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PRECISION MEASUREMENT AND THE GENESIS OF PHYSICS TEACHING LABORATORIES IN VICTORIAN BRITAIN

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ABSTRACT

This thesis examines the ascendance of Victorian academic physics laboratories as institutions primarily devoted to undergraduate instruction in the techniques of precision measurement. The genesis of these teaching laboratories in established centres of higher education is shown to be specific to the period 1866-1874. This temporal specificity is analysed in terms of the confluence of an industrially-generated "demand" for practical scientific education and an independent research-generated "supply" of academic expertise in precision measurement.

Case studies assess the variation of pedagogical practices according to local institutional factors and the biographical characteristics of incumbent professors of experimental physics in seven English and Scottish laboratories. Three sub-groups of British academic physicists are thereby identified: i) non-analytical experimentalists in London and Oxford, who supplied comprehensive and regimented courses of training in the techniques of exact measurement; ii) Scottish natural philosophers in Glasgow, Edinburgh and Manchester, who characteristically enlisted undergraduate assistance in professorial measurement researches; and iii) Cambridge analysts, who allowed at most only an elite of the indigenous mathematics graduates to pursue independent investigations in their laboratories.

The tensions between traditions i) and iii) are illustrated in the divergence of the Cambridge analyst James Clerk Maxwell from the pedagogical interests and practices of the majority of academic laboratory physicists as represented by the Physical Society founded in 1873-74. Maxwell's dissidence is demonstrated in a brief account of his management of the Cavendish Laboratory as a generically unique centre of postgraduate research from 1874-79.

Nevertheless, it is shown that laboratory precision measurement, as the universally recognized vehicle for progress in physics between the 1860's and 1890's, was practised in common by Maxwell and the distinct community of academic experimentalists which existed "before and beyond" the Cavendish Laboratory.

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CONTENTS

PREFACE

The modern laboratory is a really new institution, the evolution of which still awaits its historian...

Science: 1884 editorial "The Laboratory in Modern Science" [Science,3,173].

The initiation and proliferation of physics laboratories in the academic institutions of Britain between 1865 and 1885 is an established feature of Victorian science [Phillips,1983;Sviedrys,1976]. However, neither of the two existing modern accounts have adequately documented the predominant function of early academic physics laboratories as centres for *teaching,* emphasizing instead the less typical research activities of the Glasgow and Cambridge University laboratories. Hence these accounts have attempted to explain, somewhat misguidedly, the genesis of these laboratories by reference to the stimuli of professionalized *research* programmes, instead of considering the contemporary growth in demand for the professional laboratory teaching of physics [Phillips,1983, 498-9; Sviedrys,1976,409-415 & 422-427]. In failing to consider such physics laboratories in terms of the political economy of British education, these accounts have also failed *a fortiori* to correlate this development with the contemporaneous extension of laboratory teaching to other scientific disciplines, a movement dubbed as a laboratory "revolution" by later 19th century commentators.

In presenting an alternative historical analysis of academic physics laboratories as centres of teaching, this thesis will locate their genesis not only in the British socio-economic context of the late 1860's and 1870's, but also in the contemporaneous "internal" context of laboratory precision measurement. As the predominant activity of experimental physicists in this period, the practice of precision measurement was developed interactively with the pedagogical function of academic laboratories. The aim of this thesis will thus be to characterize the symbiosis of precision measurement and laboratory teaching in Victorian experimental physics, focussing attention on developments between 1866 and 1874 since the majority of laboratories documented in chapters 2-8 were created in this period.

In exploring the character of this symbiosis, the following questions will serve to provide directives for our discussion:

1) What was the context of physical research underlying the ascendancy of exact measurement as the generic activity of experimental physicists?

2) How did physicists view the role of precision measurement in relation to the future progress of their discipline?

3) How was the contemporary progress of physics correlated to the ascendance of academic experimental physics and the genesis of its distinctive institutional artefact: the teaching laboratory?

4) Why did the immediate socio-economic context generate specific audiences for the practical teaching that these experimentalists offered?

5) In what ways did these physicists express their disciplinary commitment to precision measurement in the operation of their teaching laboratories?

6) What inter-institutional connections sustained the diffuse population of laboratory physics teachers as a coherent community with common commitments to measurement practices and laboratory training?

To achieve answers to these questions, we will first of all develop a contextual overview of British academic physics between 1860 and 1900 in Chapter 1 to establish some general arguments about the relation between measurement physics and the genesis of laboratory teaching. In institutional case-studies of laboratories in Glasgow, Edinburgh, London, Oxford and Manchester, these arguments will be explored and refined by reference to the local circumstances of each laboratory and to the biographical characteristics of the incumbent professor of physics.

These institutions chosen for our case studies are those which existed with a specific tradition of natural philosophy/physics prior to the creation of their physics laboratory. Subsequent academic laboratories at Newcastle-upon-Tyne, Bristol, Liverpool, Birmingham etc were initiated integrally with the foundation of their *new* parent institution in the 1870's and 1880's and as such deserve a separate historical analysis [Sviedrys,1976,433]. Although erected in an established university within the nominal time span of this thesis, the Cavendish Laboratory does not qualify for a separate case-study because no undergraduate teaching took place in it during the incumbence of its first professorial director: James Clerk Maxwell. The contrasts between the laboratory ideology of Maxwell and that of his colleagues (as discussed in Chapters 2 to 8) will be analysed in chapters 1, 8 and 9 in order to differentiate his work in Cambridge from this independent tradition of academic experimental physics.

Although the order in which the case studies have been laid out is approximately chronological (as far as it possible to assign a unique date to the initiation of a laboratory) the sequence give in the table of contents has been chosen specifically to emphasize the social and biographical connections between successive case studies:

Chapters 2 and 3 relate to the Scottish context of natural philosophy;

Chapters 3 and 4 discuss laboratories created to emulate Thomson's Glasgow model;

Chapters 4 and 5 document metropolitan laboratories affiliated to the University of London;

Chapters 5 and 6 cover the laboratories of two contemporary Wranglers and former students of Stokes, viz.Adams and Clifton;

Chapters 6 and 7 are connected by Clifton's successive tenure of chairs at Manchester and Oxford;

Chapters 7 and 8 chronicle laboratories at two relatively new institutions created in the early 1850's which both served industrially-linked audiences: Owens College and the Royal School of Mines.

After an analysis in Chapter 8 of how the metropolitan Physical Society operated as the distinctive common nexus for the physicists and laboratories in Chapters 2 to 8, Chapter 9 will conclude by drawing upon evidence from all these case-studies to answer the six central questions posed above and thereby characterize the British tradition of academic experimental physics which existed "before and beyond the Cavendish Laboratory".

CHAPTER 1

PRECISION MEASUREMENT, THE LABORATORY REVOLUTION AND THE INDUSTRIAL CONTEXT

...as early as 1858 a practical beginning of definite electrical measurement had been made, in the testing of copper resistances, insulation resistances, and electrostatic inductive capacities of submarine cables. But fifteen years passed after this beginning was made, and resistance coils and ohms, and standard condensers and microfarads had been for ten years familiar to the electricians of the submarine-cable factories and testing stations, before anything that could be called electric measurement had come to be regularly practised in almost any of the scientific laboratories of the world...I doubt whether the resistances of one in a hundred of the coils of electromagnets, galvanometers, and other electromagnetic apparatus, in the universities, and laboratories, and lecture establishments of the world were known to the learned professors whose duty it was to explain their properties, and to teach their use, to students and pupils. But we have changed all that...

William Thomson: 1883 Lecture on "Electrical Units of Measurement" to the Institute of Civil Engineers [Thomson,1883,82-83].

This introductory chapter will serve to place the central themes of this thesis in the interactive contexts of precision measurement, laboratory science teaching and industrial science education in Victorian Britain.

The first section will document the role of exact physical measurement in the British Association for the Advancement of Science and the telegraph industry during the 1860's, relating this practice of precision physics to the disciplinary progress perceived by physicists and also to the ascendance of the new academic sub-community of laboratory physicists.

The second section will correlate the growing network of academic physics laboratories with parallel contemporary developments in the practical teaching of other experimental sciences collectively known as "the laboratory revolution".

The third section will document the creation of various clienteles for an education in experimental science in the industrial context of electrical telegraphy and the 1867 Paris Exhibition. Focussing upon the debates over the non-quantitative manufacturing practice colloquially known as "rule-of-thumb", it will be argued in relation to the first section that the precision techniques taught in academic physics laboratories were specifically advocated as a scientific "antidote" to this traditional workshop practice.

SECTION I: Precision physics and the genesis of teaching laboratories

1): Exact measurement, the B.A.A.S. and progress in physics in the 1860's.

During the late 1860's and 1870's there was a consensus amongst British physicists that a major transformation in both the theories and practices of their subject had taken place in the preceding two decades. As President of the Mathematics and Physics Section (A) of the B.A.A.S. in 1868, John Tyndall declared at the Association's annual meeting: "partly through mathematical and partly through experimental research, physical science has of late years assumed a momentous position in the world" [B.A.A.S. Report, 1868, (Part 2) 2]. The mathematical and experimental researches which had generated this disciplinary progress were widely identified by physicists both with developments in energetics such as the principle of energy conservation and the "electrotechnics"[1] associated with the telegraph industry.

For example, as President of Section A in 1877 George Carey Foster discussed the recent "conquests" in thermodynamics by Thomson, Helmholtz *et al*, remarking that the ideas of Work and Energy "have been found to have a most far reaching significance and to have exerted a transforming effect upon every branch of physics" [B.A.A.S. Report, 1877, (Part 2) 5-6]. In his speech as President of the Institute of Telegraph Engineers in 1884, Foster's metropolitan colleague W.G. Adams further articulated the nature of this transformation in *terms of* the *disciplinary unification* effected by practitioners of "the new science of energy":

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[1] A phrase widely used to refer to the contemporary technology associated with the telegraphic science in both academia and industry - see for example The Electrician.

> ...all the physical sciences may be regarded as linked together and as forming branches of this new science - the science of energy...The accurate measurements of Joule, of Peltier and of Thomson have shown the definite relations which the sciences of motion, of electricity, and of magnetism bear to the science of heat. The labours of Maxwell, based on measurements of the velocity of light and of specific inductive capacities, have given us some clue to the relationship between electromagnetism and the phenomena of radiant heat and light, which must be regarded as only different effects of the same form of energy.
>
> [Adams,1884,7-8].

These physicists thus identified the mechanism of disciplinary progress to be the systematic implementation of *precision measurement* to experimental physics, most particularly to the fields of thermodynamics, electromagnetism and terrestrial magnetism, in order to establish these as interrelated quantitative sciences of energy transference.

Commenting upon these "recent advances" in physics in his role as President of the B.A.A.S. in 1871, Sir William Thomson argued that "...nearly all the grandest discoveries of science have been but the rewards of accurate measurement", declaring with contextual deference that "great service has been done to science by the British Association in promoting accurate measurement in various subjects" [B.A.A.S.Report,1871, xci-xcii]. Thomson was referring here to the collective quantitative investigations made by physicists, mathematicians etc at a range of British institutions under the aegis of the various B.A.A.S. measurement committees during the 1850's and 1860's. The activities of one of these, namely the Electrical Standards Committee, will be discussed in detail below.

Most of the laboratory physicists discussed in this thesis took an active part in the B.A.A.S. measurement committees from the 1860's onwards in subjects encompassed by Section A, undertaking for example the precise measurement of thermal quantities. In 1867 William Thomson, Balfour Stewart and George Carey Foster of the Electrical Standards Committee were joined by James Joule and P.G. Tait to form a committee "for the purpose of executing a remeasurement of the Dynamical Equivalent of Heat" using accurate electrical measuring equipment developed by Thomson et al for the former committee. Also in 1867 Maxwell, Thomson, Stewart and J.D. Everett were among a large committee formed to "investigate the rate of increase of underground temperature downwards in various localities, of dry land and water" [B.A.A.S. Report,1867,lxii]. P.G.Tait and John Tyndall joined Stewart in the 1868 Committee "for the purpose of repeating Principal J.D. Forbes' experiments on the Thermal Conductivity of Iron, and extending them to other metals [Ch.4][B.A.A.S. Report,1868, xlv]. Under the aegis of the B.A.A.S. Kew Committee, the Superintendent of Kew Observatory, Balfour Stewart, supervised i) the systematic measurement of geomagnetic declination, dip and force; ii) the standardization and operation of meteorological recording apparatus iii) the mapping and measurement of solar activity especially sun-spots [Ch.7].

The longest-running and most influential of these, however, was the Electrical Standards Committee, founded in 1861 and operational until the mid-1880's. Whilst Sviedrys discusses the work of this Committee in some detail, he fails to consider three important aspects of this work in his account of the nascent tradition of academic physics laboratories [Sviedrys,1976,422-27]. These aspects are: 1) the significance of the Committee's communal activities in electrical *measurement* as the common context (in concert with the other measurement committees) for the academic laboratory practices of professorial physicists such as Thomson, Maxwell, Foster and Stewart; 2) the Committee's seminal integration of contemporary measurement researches in both thermodynamics and telegraphy, an amalgamation physically embodied in their standardized apparatus for absolute electrical measurement, and 3) the general distribution of such standardized precision measurement apparatus throughout the laboratories discussed in this thesis. These omissions from Sviedrys' account will be remedied below.

Following up work undertaken by Faraday, Thomson and his laboratory students investigated the retardation of signals in telegraphic cables due to resistance and capacitance effects during 1857 and 1858. The Glasgow professor then successfully persuaded the telegraph industry in 1857-58 to adopt his precision testing of the electrical conductivity of the highly variable copper samples used in cable manufacture [Thompson,1910, 350-51]. In the widespread adoption of Thomson's precision electrical techniques during the subsequent three years there was, however, such a proliferation of electrical measurement systems in the telegraph industry that two prominent industrial telegraphic engineers, Latimer Clark and Sir Charles Bright, presented a paper to the B.A.A.S. in 1861 in which they argued for "the desirability of a [single] set of standards of electrical measurement" appealing for "the aid and authority of the B.A. in introducing such standards into practical use" [Bright & Clark ,1861,37-8]. Thomson immediately put a motion on this matter to the B.A.A.S. General Committee which appointed Thomson, Wheatstone, Fleeming Jenkin and others as a "Committee to report upon Standards of Electrical Resistance" [<u>B.A.A.S.Report</u>, 1861,xxxix].

The brief of this committee was initially to determine what would be a generally convenient unit for measuring electrical resistance and the means of realizing this unit in reproducible "standardized" apparatus.

However, within its first year of existence Thomson *et al* had extended their brief to investigating a complete system of electrical measurement and for this they incorporated Weber's scheme of absolute electrical units framed with reference only to measurements of mass, length and time. This absolute system had been forcefully advocated by Thomson since the start of his thermodynamic researches in 1847 because of its compatibility with the other monument of Victorian measurement research due to Thomson, Helmholtz *et al.* viz. the law of energy conservation. The parameters of resistance, current, electromotive force and electric quantity could all be expressed in terms of the unit of energy as a common denominator e.g. a unit of current does one unit of work in passing through a unit resistance in unit time - c.f. Adams' comments above. Hence the committee argued that the unit of resistance, like the other units of the absolute system should be construed to bear a definite relation to the unit of work: "the great connecting link between all physical measurements", thereby integrating contemporary thermodynamics into the Committee's programme of standardizing electrical measurements [B.A.A.S. Report,1862,125-127].

To fulfil their broadened objective of cultivating a complete theoretical and practical system of absolute electrical measurement, Thomson, Wheatstone *et al* were joined by James Clerk Maxwell, the Kew Superintendent and meteorologist Balfour Stewart, and the telegraph engineer C.W. Siemens to become the "Committee on Electrical Standards". To dispel considerable doubts among members of the Committee "as to the degree of accuracy with which this admirable system could [in reality] be...reduced to practice" Stewart, Jenkin and Maxwell were commissioned to attempt a determination of absolute resistance according to a method devised by Thomson and put into effect in Maxwell's professorial suite at

Kings College, London between April and August of 1863 [Ch.5]. Although the Committee were satisfied that their measurement of the absolute "ohm" was in reasonable agreement with the previous determinations of Weber, Siemens and Thomson, they deferred the official[a] production of standards until after the second determination of 1864 [B.A.A.S Report,1864,345].

By 1865, however, the Committee had arranged for the free distribution of 20 ratified sets of resistance standards to the directors of public telegraphs in France, Austria, Belgium, Spain, Italy, Portugal, Prussia, Sweden and Norway, Russia, India, three states of Australia and also to Professors Neumann, Kirchhoff and Weber in the Germanic states. Copies were also sold to Faraday at the Royal Institution and to industrial concerns such as the Atlantic Telegraph Company who, under Thomson's jurisdiction, employed it in the laying of the Atlantic cable [B.A.A.S. Report,1865,308-313; Smith & Wise,1989, Chs 19 & 20].

Thomson had been commissioned to construct absolute electrometers and electrodynamometers to facilitate standardized absolute measurements of current and electromotive force although his work on the Atlantic Cable in 1865-66 had ironically brought a temporary halt to this work. However, by 1867 he had developed a range of electrometers for measuring absolute potential differences between 1/400 and 10,000 Daniell Cells with the assistance of his "corps" of laboratory students [Ch.2]. One such electrometer was used by Joule in his high precision re-determination of the "mechanical equivalent of heat", further illustrating the

[a] Such was the demand for the B.A.A.S. standards among telegraphic practitioners, however, that unofficial resistance coils based upon the 1863 determination were made by Siemens and Halske of Berlin for the Superintendent of the Government telegraph lines in India, Col. Douglas, and by Elliott Bros. of London for those unwilling to wait for the second determination [B.A.A.S Report,1864,345].

institutionalized interdependence of electrical and thermodynamic measurements through the B.A.A.S absolute system [B.A.A.S. Report,1867, 474-75]. Of the Glasgow professor's instrument-making enterprise the Committee eulogized: "Sir William Thomson has not ceased to invent better and simpler forms until the instruments now supplied surpass every expectation of practical electricians and furnish, indeed, a new engine for electrical research" [B.A.A.S Report,1867,474-490]. The significance of this last remark will be made clear in subsequent chapters as we see how Thomson's instruments and measurement methods were adopted in the research and teaching of academic physical laboratories beyond Glasgow as an "engine" for the progressive vehicle of exact measurement. In the context of this we can therefore comprehend Thomson's assertion in his 1871 address as President of the B.A.A.S.:

> Those who perilled and lost their money in the original Atlantic Telegraph...little thought that...when the assistance of the British Association was invoked to supply their electricians with methods for absolute measurement... [that]..they were laying the foundation for accurate electric measurement in every scientific laboratory in the world.
>
> [B.A.A.S.Report,1870,xciii].

2): Precision measurement and the "*closure*" *of physics*

Whilst Thomson in his guise as an experimentalist emphasized to the B.A.A.S. the progress of physics through exact measurement in relation to laboratory practice, the more mathematically-oriented tradition in the Section A of the B.A.A.S. naturally interpreted the significance of this progress specifically with respect to their own specialism. For example, Clerk Maxwell as President of Section A in 1870 had argued that the improvements in accurate physical quantification (i.e. measurement) had brought more of scientific enquiry under the jurisdiction of the mathematician:

> As science has been developed, the domain of quantity has everywhere encroached upon that of [mere] quality, till the process of scientific enquiry seems to have become simply the measurement and registration of quantities, combined with a mathematical discussion of the numbers thus obtained. It is this scientific method of directing our attention to those features of phenomena which may be regarded as quantities which brings physical research under the influence of mathematical reasoning.
>
> [B.A.A.S.Report,1870,(Part 2) 4]

Maxwell considered the effect of such progress in exact measurement to be that "physical researches are continually revealing to us new features of natural processes"; hence by bringing these new features "under the influence of mathematical reasoning" new areas of research for theoretical physicists would be generated [B.A.A.S.Report, 1870, (Part 2) 8]. And as a mathematical practitioner concerned to extend the prerogative of mathematics in physics Maxwell was not alone in these views; this is clear from the reaction, for example, of the Oxford mathematician H.J.S. Smith, as President of Section A in 1873, upon the publication of Maxwell's Treatise on Electricity and Magnetism [Maxwell,1873]. Smith argued that the Treatise demonstrated the future prospects which could be entertained by mathematicians of elevating electro-magnetism to the status of astronomy as an "exact science":

> the great practical importance of telegraphy has enabled the methods of electric measurement to be rapidly perfected to an extent which renders their accuracy comparable to that of astronomical observations...It must be considered fortunate for mathematicians that such a vast field of

> research in the application of mathematics to physical enquiries should be thrown open to them at the very time when the scientific interest in the old mathematical astronomy has for the moment flagged...
>
> [B.A.A.S. Report,1873, (Part 2) 4]

If mathematical physicists held that the systematic deployment of exact measurement methods would bring about further progress in their own specialism by enabling them further to subjugate experimental physics to their theoretical analyses, it is important to note the contrasting conviction which prevailed among experimentalists in the 1870's. So extensive had the progress of measurement-based physics research been in the 1850's and 1860's that many experimentalists believed physics to be approaching "closure", i.e. was almost complete, so that the laboratory research that remained to be done by them was merely to improve the accuracy of standard measurements already made. As Arthur Schuster evocatively recalled the common perceptions of those who, like himself, had been laboratory students in the 1870's:

> I think I interpret correctly the recollection of those who passed through their scientific education at the time when I say that the general impression that they received , was that, apart from theoretical work, a reputation could only be secured by improved methods of measurement which would extend the numerical accuracy of the determination of physical constants. In many cases the student was led to believe that the main facts of nature were all known, that the chances of any great discovery being made by experiment were vanishingly small, and that therefore the experimentalist's work consisted in deciding between rival theories, or in finding some small residual effect which might add a more or less important detail to the theory.
>
> [Schuster,1911,7]

In the context of these opposed disciplinary perspectives on the function of measurement between mathematical and experimental physicists, we can assess the significance of Maxwell's denial of "closure" in his

Introductory Lecture as Professor of Experimental Physics at Cambridge in 1871. In this oft-quoted lecture on the operations of his projected "Cavendish Laboratory", he argued that the propriety of laboratory measurement lay in expeditiously quantifying physical phenomena to serve the interests of mathematicians, denying the view that the only progress which such measurements could now effect was the marginal improvement of accuracy in a virtually complete corpus of physical knowledge:

> This characteristic of modern experiments - that they consist principally of measurements, - is so prominent that the opinion seems to have got abroad, that in a few years all the great physical constants will have been approximately estimated, and that the only occupation which will be left to men of science will be to carry on these measurements to another place of decimals...But the history of science shews that even during that phase of her progress in which she devotes herself to improving the accuracy of the numerical measurements of quantities with which she has long been familiar, she is preparing the materials for the subjugation of new regions...
>
> [Maxwell,1871,244]

Maxwell therefore denied that the institutional rationale of the Cavendish should be measurement *for its own sake,* arguing that "if this really is the state of [closure] to which we are fast approaching, our Laboratory may perhaps be celebrated as a place of conscientious labour and consummate skill, but it will be out of place in the University, and ought rather to be classed with the other great workshops of the country [viz. industrial factories]" [Maxwell,1871,244].

With regard to Maxwell's views on the function of laboratory measurement Schuster remarked that "there were no doubt great differences of opinion depending upon the temperament of the teacher as to how far increased accuracy of measurement was an object desirable in itself or only a means to an end..."[Schuster,1911,7]. Commenting from the vantage point of 1908, when recent discoveries in radio-activity and "Roentgen rays" had led physicists to be "suspicious of the soundness" of nineteenth century physics [Schuster,1911,112], Schuster was able to dub Maxwell's denial of closure in the 1870's as "the most progressive view of the time, but unfortunately only a few students came under his direct influence" [Schuster,1911,11]. The relative influence of Maxwell's "progressive" view of the future of physics upon apprentice physicists at the Cavendish and elsewhere will be explored in chapter 9.

Schuster's remark hints at the divergence of Maxwell from the views and practices of the experimental physicists who were his contemporary counterparts in the laboratories analyzed in this thesis. This divergence was most clearly manifested in his alienation from the institutional body which was founded by Frederick Guthrie in 1873-74 and whose membership was constituted by precisely these counterparts of Maxwell who were laboratory teachers of physics: the Physical Society. In the final case-study in this thesis it will be demonstrated from a study of the membership and activities of the Physical Society from 1874 onwards that there was clearly a self-identifying group of experimental physics teachers based in metropolitan and provincial university colleges that held interests divergent from those of mathematical practitioners based in Cambridge and the *Royal* Society, there being conflict in particular between Guthrie as the founder of the Physical Society and the Cambridge Professors J.C. Maxwell and G.G. Stokes [Ch.8].

The intermediate institutional case-studies which follow will provide material for analysing the anatomy of this conflict in the British community of physicists. In the concluding chapter a comparative assessment will be made of the manner in which Maxwell's counterparts in

Glasgow, London, Oxford and Manchester constructed and operated their academic physics laboratories as centres of progressive exact measurement, cultivating exact measurement either "as an object desirable in itself", or further to bring experimental physics under the reign of mathematical analysis. Prior to this it is important to elucidate the historical background of these tensions in the British community of physicists in the context of the contemporary measurement-engendered progress in physics by examining the institutional ascendance of the sub-community of academic laboratory physicists in the 1860's and 1870's alongside the pre-existent sub-communities of Scottish Natural Philosophers and Cambridge mathematical analysts.

3): Exact measurement and the rise of the academic laboratory physicists.

> Natural Philosophy has been long taught in two very different ways. One method is to begin by giving the student a thorough training in pure mathematics..[and].. the progress of science according to this method, consists in bringing the different branches of science in succession under the power of the calculus....
>
> The other method of diffusing physical science is to render the senses familiar with physical phenomena, and the ear with the language of science, till the student becomes at length able to perform and to describe experiments of his own. The investigator of this type is in no danger of having no more worlds to conquer [sic], for he can always go back to his former measurements, and carry them forward to another place of decimals...
>
> There is, however, a third method of cultivating physical science in which each department [viz. mathematical and experimental] in turn is regarded....The book before us shows that the Professors of Natural Philosophy at Glasgow and Edinburgh have adopted this third method of diffusing physical science...
>
> 1873 review of Elements of Natural Philosophy by Thomson and Tait in [Nature,7,399-400].

In this very sympathetic assessment of Thomson and Tait's elementary version of their Treatise on Natural Philosophy [Thomson & Tait,1867] the author strategically overstates the longevity of the second "laboratory method" of teaching physical science in order to establish the relative novelty of Thomson and Tait's pedagogical text. Despite this distortion, as a contemporary commentary on the pedagogy of physics this review enables us to identify three distinctive but overlapping traditions in British physics education: the Cambridge school of analytical theorization, the laboratory practitioners, who pursued physical measurements to ever further decimal places, and the Scottish practitioners, who fused both approaches into a unique national style of Natural Philosophy.

From the nostalgic testimony of one laboratory practitioner, A.J. Fleming, in 1939 we can identify some members of each of these subsections of the physics community as follows:

1) Cambridge analysts
George Gabriel Stokes, James Clerk Maxwell,

2) the laboratory practitioners
John Tyndall, James Prescott Joule, William Crookes, John Hall Gladstone, Frederick Guthrie, Robert Bellamy Clifton, William Grylls Adams, George Carey Foster and Balfour Stewart.

3) Scottish Natural Philosophers[3]
William Thomson, Peter Guthrie Tait.

[Fleming,1939,99]

[3] Fleming himself does not employ this category, but he is nonetheless equivocal in identifying Thomson and Tait as theoreticians whilst emphasizing at the same time Thomson's "brilliant experimental researches and practical inventions" and Tait's "very important contributions to experimental physics" [Fleming, 1939,99]. This leads us to an extension of Fleming's scheme to a third group viz. Scottish Natural Philosophers according to the primary source in Nature cited above, a recategorization which will be justified further by material presented in chapters 2 and 3.

The institutional and disciplinary interconnections between the figures in these categories are as follows:

1) Cambridge University: Maxwell, Adams, Clifton, Thomson and Tait were all high Wranglers in the Mathematics Tripos [Sviedrys,1976,432] and all except Thomson were also students of Stokes, who was the Lucasian Professor of Mathematics at Cambridge from 1849 - four years after Thomson's graduation. The common grounding in experimental physics which these men received during the 1850's in Stokes' lectures will be documented in chapters 5 and 6.

2) Edinburgh University: Maxwell, Tait and Stewart were students of the Edinburgh Professor of Natural Philosophy J.D. Forbes and all carried out experimental work in his private laboratory - see Chapters 3 and 7.

3) Laboratory chemistry: Tyndall, Crookes, Gladstone, Guthrie, and Foster all began their scientific careers as experimental chemists, Guthrie and Foster, for example, studying in the laboratory of A.W. Williamson at University College London before undertaking the conventional tour of French and German chemistry laboratories after graduation - see Chapters 4 and 8. As undergraduates, Thomson, Stewart Tait and Maxwell all studied in the chemical laboratories at their respective institutions: William Thomson under Thomas Thomson at Glasgow, and Stewart, Tait and Maxwell under Gregory and Playfair at Edinburgh - see Chapters 2, 3, and 7.

With regard to this almost universal[4] training in practical chemistry amongst novice men of science, Fleming - who had been laboratory assistant to the chemist Edward Frankland and also worked in Guthrie's physical laboratory at the Royal School of Mines in 1873 - remarked in 1924 that many physicists up to the 1870's began their scientific training as chemists [Physical Society,1924,17]. Indeed, as Lord Rayleigh often complained to his son of his experiences in Cambridge during the 1860's, the chemistry laboratory was often the *only* place in England where an aspiring man of science could acquire a practical training [Strutt,1924,43]. In the Scottish universities, however, there was a somewhat different extant tradition of student co-operation with professorial researches for both Thomson under J.P. Nichol at Glasgow and for Tait, Maxwell and Stewart under Forbes at Edinburgh - see chapters 2, 3 and 7 for a discussion of this independent tradition.

Breaking the monopoly of academic chemistry upon practical laboratory training in England from the late 1860's onwards, Clifton, Adams, Foster, Stewart and Guthrie created physics laboratories upon their appointment to newly created or promoted Chairs of experimental physics/natural philosophy, laboratories in which their students could receive a specialist training in the contemporary techniques of precision measurement. The creation of these new professorships in experimental physics and of their characteristic pedagogical vehicle - the physical teaching laboratory - can be taken as an index of the institutional ascendancy of experimental physicists during the late 1860's to 1870's.

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[4] Although they were never students in chemistry laboratories, Adams and Clifton collaborated with professorial colleagues on chemical researches respectively at Kings College, London and Owens College, Manchester - see Chapters 5 and 6.

This ascendancy was manifested in two ways: firstly in the division of academic labour through the fission of chairs of natural philosophy into separate professorships of mathematical and experimental physics, this being an institutional acknowledgement of the recently expanded *scope* of physics teaching; and secondly recognition was given to the pedagogical importance of the *methods* of experimental physics by integrating laboratory work into the institutional curriculum of natural science.

For example, the Chair of Natural Philosophy at University College, London was divided in 1865 by the College Council into a Professorship of Mathematical Physics and a Professorship of Experimental Physics in explicit recognition that the duties of teaching the corpus of physics recently enlarged by research in thermodynamics and telegraphic electricity was too great a workload for a single man to achieve. By 1866, within a year of his appointment as University College's new Professor of Experimental Physics, George Carey Foster had negotiated the creation of a small laboratory to teach his students the techniques of precision measurement [Ch.4].

Also in 1866, Robert Bellamy Clifton was appointed to the Oxford Chair of Experimental Philosophy, which had been elevated from a Readership to a full Professorship in 1860, and thereupon borrowed a room in the University Museum to teach practical physics to undergraduates in the Natural Sciences School. Appealing for money for a purpose-built physical laboratory two years later in 1868, Clifton argued that in order to teach experimental physics properly by enabling "a student of physics to become acquainted, by actual experience with accurate physical processes" in the wake of the great expansion of the subject since the Museum was built in the late 1850's, it was now necessary for him to give more than public lectures [Ch.6]. This advocacy of laboratory work as a prerequisite for teaching experimental physics was thereupon taken up by Clifton's Cambridge contemporary at King's College London, William Grylls Adams; as Adams remarked in 1871 of the Oxford Professor's negotiations in the late 1860's: "I believe that Prof. Clifton was the first to propose, more than three years ago, that a course of training in a physical laboratory should form part of the regular work of every student of Physics...The system was at once adopted and put into action at Kings College" [Adams,1871d,323] [Ch.5].

Clifton had originally made similar demands for a teaching laboratory in 1864, towards the end of his tenure as Professor of Natural Philosophy at Owens College Manchester, and upon the departure of Clifton's successor William Jack in 1870 the Owens Trustees agreed to establish such a laboratory. Upon the advice of William Thomson that the requisite amount of teaching and research appropriate to a natural philosophy department was now so great that it could only be carried out if shared between two professors, the Trustees divided the Chair in two and appointed Balfour Stewart as Senior Professor in charge of the physical laboratory [Ch.6].

To encapsulate the intimate relation between the progress of physics through exact measurement and the creation of new teaching laboratories by the ascendant generation of professorial experimentalists, we can cite the forceful demand for laboratory facilities in a speech made by Frederick Guthrie to a meeting of the Society of Arts in April 1870, which was to organize the forthcoming international exhibition of apparatus for practical science teaching. Guthrie was Professor of Physics at the Royal School of Mines in Jermyn St.but unlike his Profesorial counterparts in London, Oxford and Manchester he did not (yet) have his own physics teaching laboratory; thus he argued:

> I trust that we will have to examine and judge of a far greater number of educational appliances than such exhibitions have hitherto brought together, for the exact and experimental sciences [viz. physical sciences] are now so much more fully developed that it is impossible to remain any longer contented with attempting to teach an experimental class by means of a blackboard and a piece of chalk; it is now necessary to have an efficient apparatus for teaching these subjects...
>
> [Journal of the Society of Arts,18,440][5]

Such was the currency accorded to this rhetoric of teaching physics by practical laboratory work that the assembled meeting voted unanimously in support of Guthrie's resolution [Journal of the Society of Arts,18,440].

To conclude our preliminary discussion of the foundation of academic physics laboratories it is important to see how academic experimentalists architectonically institutionalized the measurement practices seminal in the rise of their sub-profession in the fitting and construction of their laboratories.

4): Precision measurement and laboratory architecture

The first generation of British academic experimental physicists in the 1860's and 1870's did not all enjoy an institutional position which enabled them to negotiate financial resources for purpose-built laboratories in which to teach and research their measurement specialism. Those who enjoyed this luxury were William Thomson, whose laboratory in the new Glasgow University was built 1866-1870 [Ch.2]; Robert Clifton

[5] Quotation re-constructed from a report of his speech written in the third person in the Journal of the Society of Arts.

whose Clarendon laboratory at Oxford was constructed 1868-1870 [Ch.6]; Balfour Stewart whose laboratory at the Owens Extension College was built 1870-1873 and Maxwell whose Cavendish laboratory at Cambridge was erected 1870-1874 [Ch.9]. Nonetheless all of these professors had originally been obliged to operate in "makeshift" laboratories by converting existing institutional space into an environment amenable to their measurement operations; similarly Foster, Adams, Tait and Guthrie had to utilize the accommodation contingently available when their demands for a physical laboratory were met by their parent institutions between 1866 and 1872 [Ch. 2-9].

The essential architectonic issue faced by all these men, whether converting or constructing institutional laboratory space, was the creation of an environment amenable to *undisturbed measurement:* an environment which isolated the all-important measurement operations from mechanical and electromagnetic vibrations respectively inimical to the desired accuracy of instruments such as pan-balances and galvanometers. This was the predominant issue to be addressed by laboratory architects and practitioners throughout the rest of the nineteenth century, as is instanced in the testimony of Edward Robins, an English architect specializing in the design and construction of laboratories, who held that "the great desideratum of a physical laboratory is a steady working table, and this is difficult to secure...In a physical laboratory steady supports must be had, independent of any shaking of the room due to traffic outside the building or to people walking around inside the building itself" [Robins,1887,116].

Structural insulation devices were developed co-operatively by physicists and architects to achieve the requisite stability of measurement surfaces; for example, to achieve mechanical stability "floating" concrete supports for laboratory tables independent of the parent building's structural foundations were employed. To enable electromagnetic laboratories to operate without the potential disturbance of induction currents and stray magnetic fields, non-magnetic substitutes were developed for iron heating and water pipes as well as cast-iron structural reinforcements [Forman *et al.* ,1975,105-114; Robins,1887,116]. The political negotiations engaged upon by academic physicists to acquire such facilities to isolate their physical laboratories from the immediate institutional environment will be discussed in Chapters 2-9.

To gauge the level of zealotry with which protagonists of laboratory science campaigned against external threats to this institutional isolation it is informative to cite the bitter opposition of Norman Lockyer, the editor of Nature, to the plans in 1890 to run an underground railway directly underneath the Royal College of Science in South Kensington. Lockyer was the Professor of Astronomical Physics at this institution and since the Royal College's laboratory for astronomical physics was under his jurisdiction we can apprehend the motives behind his belligerent Nature editorial in which he vehemently condemned the proposals:

> Shall the [students] come to find a rumbling earthquake led from early morn till night close to the very foundations of the very pillars that have been erected at considerable cost in order to secure for the instruments placed on them freedom from even the vibration caused by passing footsteps? On concrete foundations, 13 feet below the level of the street , rest many pillars of 9 feet square, each quite separate from its neighbour and from the floor on which the student stands; and the upper portion of each pillar is stuffed with a thick cushion of wool , so that the instrument resting upon it may give as unwavering a decision as the Lord Chancellor on the woolsack...As it is the hosts of Mammon now threaten the domain of science in Exhibition Road, for it is actually proposed to make an

> underground railway, with trains running at frequent intervals, right under Professor Rucker's laboratory....
>
> ...the approach of a cart at the other end of Exhibition Road [will thus be] foreshadowed, long before it can be heard, by the uneasy trembling of the very delicate galvanometer needles....[hence] to study one set of vibrations in the laboratories in Exhibition Road, when another disturbing set is superimposed by the vibration of the building itself, will be like listening to a violin solo when a brass band is braying in the neighbourhood.
>
> [Lockyer,1890,145]

The cognitive significance of physically partitioning laboratories from the external world can be seen in Lockyer's metaphorical reference to the *judicial* objectivity of measurements made by instruments on suitably insulated structural pillars. In the light of this we can comprehend the rhetorical claims made on behalf of the laboratory as a place where undisturbed *objective and absolute* knowledge of Nature could be acquired. Consider for example the conviction held by the Glasgow Professor of Natural Philosophy upon the pursuit of *truth* in the laboratory:

> There is one thing I feel strongly with respect to investigation in physical or chemical laboratories - it leaves no room for shady , doubtful distinctions between truth, half-truth, whole falsehood. In the laboratory everything tested is found either true or not true. Every result is true. Nothing not proved true is a result; there is no such thing as doubtfulness. The search for absolute and unmistakeable truth is promoted by laboratory work in a manner beyond all conception.
>
> [Thomson,1885,411]

The force of this laboratory rhetoric of isolated objectivity is clear from Lockyer's description of the perennial conflict between the sensitivity required by the physicist of his measurement laboratory and the inimical intrusions of the city centre environment:

> Even at the Cavendish Laboratory, in a quiet back street in the University of Cambridge, Lord Rayleigh's most accurate work on the electric units [of measurement] had to be done at night; and Wheatstone's galvanometer magnetic needles at Kings College followed the penny iron steamers during

the day rather than the electric currents he was measuring.

[Lockyer,1890,145]

The importance attached to this concern of procuring cognitive objectivity in the laboratory through purely structural devices for achieving accuracy in sensitive measurements will be explored in the individual case studies which follow. To conclude this section it is important, however, to demarcate the historical endpoint of the period in which such concerns with exact measurement predominated over all other issues of laboratory practice.

5): New physics and the end of the "era of measurement"

In their survey of institutional physics *circa* 1900 Forman *et al* note that the concern for "freedom from vibration" universally predominated in the architectonics of measurement in physical laboratories throughout Europe and America. This predominance lasted however only up till about 1914 by which time Forman *et al.* present evidence that many of the architectural artefacts of measurement physics e.g. the tower for large scale delicate experiments on pendula, metallic elasticity and the meteorological observatory etc, no longer took precedence in laboratory construction [Forman *et al*, 1975,105&111]. Forman *et al.* do not analyse the reasons for this change of laboratory rationale but it is clear from the testimony of Arthur Schuster that the decline in the institutional supremacy of measurement physics thus expressed in laboratory architecture reflected a shift in the interests of experimentalists towards qualitatively new domains of physics which had emerged in the late 1890's.

Documenting the recent growth of interest in X-rays, atomic decay and the Michelson-Morley experiments in 1908, Arthur Schuster criticized the experimentalists of the 1870's who had predicted that there were no such major discoveries left to be made. After describing the consequent obsessive manner in which these men devoted themselves simply to measuring established physical constants, *contra* Maxwell, merely for the sake of increased accuracy, Schuster reflected: "looking back now on this period, when Roentgen rays and radio-activity were undreamt of, we may well learn to be cautious in our own predictions of the future" [Schuster,1911,9-10 &112]. Thus what J.A.Fleming dubbed the "era of quantitative measurement" in physics, an era which had fostered the genesis of academic physics laboratories in Britain, was evidently on the wane when the next generation of laboratory physicists took up the study of qualitatively new domains of physics at the close of the nineteenth century [Fleming,1919, 239].

To illustrate something of the backlash against "measurement for its own sake" we can cite a Presidential speech by J.J. Thomson to Section A of the B.A.A.S. just six months after Roentgen laid claim to the discovery of his eponymous rays in 1896 - Section A of the B.A.A.S. ironically having been the setting for unqualified advocacy of measurement physics by earlier Presidents. As a successor to Maxwell at the Cavendish Laboratory, Thomson took up the Maxwellian mantle in criticizing the prevailing tendency in teaching laboratories *merely* to give students a training in precise measurement for its own sake:

> I think...that in the teaching of physics at our universities, there is perhaps a tendency to make the course too complex and too complete. I refer especially to the training of those students who intend to become physicists. I think that after a student has been trained to take accurate observations, to be alive to those pitfalls and errors to which all experiments are liable, mischief may in some cases be done if, with a view of

> learning a knowledge of methods, he is kept performing elaborate experiments the results of which are already well-known...If the student once tastes the delights of the successful completion of an [original] investigation [he] will be better equipped for investigating the secrets of nature than if, like the White Knight of "Alice in Wonderland" he commences his career knowing how to measure or weigh every physical quantity under the sun, but with little desire or enthusiasm to have anything to do with any of them.
>
> [B.A.A.S. Report, 1896, (Part 2) 471].

We have now characterized the following features of experimental physics between 1860 and 1890:

i) the progress effected through the practices of precision measurement in the fields of thermodynamics and electromagnetism since the 1850's;

ii) the status of closure perceived by the majority of practitioners;

iii) the ascendance of the academic experimentalist and his laboratory;

iv) the architectonic expression of physicists' commitment to exact measurement;

v) the end of the "era of measurement" upon the emergence of new research subjects in the closing years of the nineteenth century.

Having thus depicted the context of the ascendance of laboratory physics in the last third of the nineteenth century it is now important to link this phenomenon with the contemporaneous extension of laboratory teaching methods to a range of other experimental sciences generically referred to as "the laboratory revolution".

SECTION II: Experimental Physics and the "Laboratory Revolution"

British and American commentators in the mid-1880's observed that a "laboratory revolution" had taken place in the teaching of a wide range of sciences, a revolution in which new methods of laboratory instruction had come to displace the traditional pedagogical practices of lectures and textbook rote-learning. Of this proliferation of academic laboratories the American journal Science declared in 1884:

> The material circumstances under which scientific discovery is prosecuted have been completely revolutionized during the last 40 years.....40 years ago there were very few, more properly no laboratories which we of today would consider even tolerable. Now every university of importance and high repute the world over, has large suites of rooms for each department of science and often numerous great buildings within whose walls thousands and thousands of students are daily brought face to face with the facts and laws of nature.
>
> [Science,3,172]

To give some examples of this innovation and proliferation of practical science teaching in a chronological perspective, laboratories for physiology teaching were founded at Oxford in 1860 by Henry Acland, and in 1873 at both Cambridge University and the Royal School of Mines by Michael Foster and T.H. Huxley respectively [Acland,1870,q2881; Geison,1978,117-118; Minutes of Council, R.S.M. 14/6/1873]. As we saw above, laboratories for physics teaching were introduced in 1866 both at Oxford by Robert Clifton and at University College, London by George Carey Foster [Ch.4 & Ch.6]. In 1878 a geological teaching laboratory was initiated at the Royal School of Mines by Prof. Judd and in 1879 the first British engineering laboratory was created by A.B.W. Kennedy at University College, London [Minutes of Council, R.S.M 6/7/1878; Kennedy: UCL College Correspondence, 18/2/1879].

Such laboratory-based professors gave systematic practical teaching in these subjects to undergraduates on an institutional scale by bringing them "face to face" with the facts and laws of Nature. To understand the extent to which this was a "revolutionary" use of the laboratory it is important to comprehend the radically different character of the laboratory 40 years before the "culmination" of the revolution.

1): The prehistory and invention of the physics teaching laboratory

In the early nineteenth century, laboratories were exclusively associated with the subject of chemistry, existed only in private institutions and did not have any recognized didactic function in training students. For example, The Cyclopedia of 1819 had defined the "Laboratory" as "a place furnished with chemical apparatus and entirely devoted to the different operations of chemistry whether on the scale of chemical manufacture or for the purpose of experimental research" [The Cyclopedia,1819]. The introduction of a formalized *didactic* role to the institutional functioning of a (chemistry) laboratory was effected shortly after this in the 1820's by Thomas Thomson at Glasgow University and Justus Liebig at Giessen [Morrell,1972,1-46].

Although Liebig's innovation was perpetuated by an international "research school" including British academic chemists such as Lyon Playfair, A.W.Williamson and Henry Roscoe [D.N.B] it is interesting to note the reaction of one of his school to the innovation of the physical laboratory. William Thomson recalled that soon after he set up his unprecedented experimental room for natural philosophy in the University of Glasgow in the early 1850's one of the Liebig school of chemists asked him "what was the object of a physical laboratory?" [Thomson,1885,410]. Clearly the extension of laboratory methods from chemistry to physics was not perceived as a "natural" or "self-evident" step to make amongst Thomson's contemporaries. Similarly, although Thomas Thomson[6] had been teaching laboratory chemistry at Glasgow University for more than three decades, William Thomson remarked that when he began his experimental work the term "physical laboratory" was a "name then unknown" to his colleagues [Thomson,1885,210].

As late as 1867 we find the term "laboratory" used in an almost exclusively chemical sense in a journal entitled The Laboratory which ran from April to September 1867. In its first issue on April 6th the editor John Cargill Brough projected a wide readership of laboratory experimentalists:

> We believe that the large and important class which includes the cultivators and students of Experimental Science has not, up to the present moment, been presented with a journal fully reflecting that which is being accomplished in the public and private laboratories of the United Kingdom....We purpose that original communications shall constitute the staple contents of The Laboratory , and we shall constantly seek the cooperation of eminent chemists and physicists...
>
> [The Laboratory,1,7]

Nevertheless, the material covered by Brough's journal was almost entirely related to issues in chemistry and the laboratory centred sub-disciplines of metallurgy and pharmacy . Of the cultivators of science regularly contributing to its columns in its six month run, the only "physicists" were G.C.Foster and Charles Hockin who as former chemists had their researches for the B.A.A.S. Committee on Electrical Standards reported alongside those of their chemical colleagues A.W.Williamson and A. Matthiessen. The Laboratory was thus largely a vehicle for chemical

[6] William Thomson was taught inorganic chemistry by Thomas Thomson in 1840-41 [Thompson S.P.,1910,10]

rather than physical interests, thereby continuing the general identification of laboratories with the discipline of chemistry.

It was therefore an unequivocal *innovation* for laboratory methods to be extended from the single discipline of chemistry to physics, geology, physiology and engineering in the decade immediately succeeding the demise of The Laboratory. This point was made by the author of the article on "the laboratory" in the 1879 edition of the Globe Encylcopedia in his comment that the term "laboratory....has long been familiar in reference to chemistry, and till comparatively recently has been used almost exclusively in this connection. Now, however, laboratories are recognised as essential to the complete study of every natural and physical science" [Globe Encylopedia, 1879].

In the provincial university and technical colleges that were opened from the early 1870's onwards [Sanderson,1972,Ch.3] the teaching laboratory became an increasingly commonplace part of physics departments. Indeed quite a number of such laboratories were fitted up or constructed *integrally* with their parent institution and set to teach physics practically at their opening: University College, Bristol 1876; Mason College, Birmingham 1880; University College, Liverpool 1881; University College, Bangor 1884 [Sviedrys,1976,433; Silver & Teague,1970]. By the 1880's this phenomenon was widely acknowledged amongst physicists as can be seen in the declaration by William Thomson to students at the opening of the Bangor laboratory in 1885 that "the physical laboratory system has now become quite universal. No University in the world can live unless it has a well-equipped laboratory"[Thomson,1885,412].

Having established that the evolution and institutional recognition of the physics laboratory was integral with the wider phenomenon of the "laboratory revolution" in the teaching of a wide range of experimental sciences, it is important now to understand how this recognition was achieved by advocates of laboratory education such as the "biologist" T.H.Huxley, the chemist H.E. Roscoe and the "physicist" William Thomson. These protagonists of the "laboratory revolution" made two central rhetorical claims about the unique cognitive value of practical science teaching in order to legitimate the novelties of laboratory study against the pedagogical practices of rival educational groups. The nature and target of these claims will be explored in the next section.

2): Bringing about the "laboratory revolution"

> Physical science will have its great divisions of physical geography, with geology and astronomy ; physics; chemistry and biology; represented not merely by professors and their lectures, but by laboratories, in which the students, under guidance of demonstrators, will work out facts for themselves and come into that direct contact with reality which constitutes the fundamental distinction of a scientific education.
>
> [Huxley,1876,240]

The widespread adoption of practical teaching in the 1860's and 1870's so forcefully advocated, for example, by Huxley in his 1876 lecture "On University Education", was met by both indifference and opposition from the educational establishment. Institutional teachers of science had to be convinced that their methods of teaching by blackboard and textbook were inadequate in order for them to believe that a teaching laboratory was really *necessary* and thence to seek the requisite institutional funding and accommodation for such a laboratory. Furthermore, there was overt opposition from educators whose *professional interests* were threatened by the "laboratory revolution". Two specific examples of this opposition were 1) the "cram" tutors who claimed that laboratory experimentation was too *demanding* of the pupils that they coached by book-work and 2) Isaac Todhunter, the eminent Cambridge mathematics tutor, who attacked laboratory teaching for subverting the didactic *authority* of the Oxbridge clerical don.

To counter this indifference and opposition, proponents of the "laboratory revolution" developed two specific rhetorical claims, both embodied in Huxley's oratory above, about the unique cognitive propriety of the laboratory as the *generic* vehicle of scientific education. For example, against the blackboard didacticism of the conventional teacher and the "paper-science" cultivated by the book-oriented "cram tutors" they argued that:

i) study in the laboratory was the *only means* for cultivators and students of science to apprehend the facts of science by investigating and quantifying for themselves the reality underlying the superficial appearances of "Nature".

Against Todhunter's view that the use of laboratory experiments as the primary means of demonstrating scientific facts was an impertinent denial of the *authority* of a teacher to impart the same knowledge, they argued:

ii) students carrying out their own experimental manipulations in the laboratory had a unique first hand experience of scientific facts which they could accept solely upon the *uniquely objective authority* of "Nature" itself.

2i): <u>locating "Nature" in the laboratory</u>

In claiming that the laboratory as a vehicle of scientific education was "fundamentally distinctive" in bringing students into "direct contact with reality", Huxley implicitly argued that this "direct contact" could not be attained either from professorial lectures, textbooks, the "cramming" of the private tutor nor even from the great outdoors where natural historians had traditionally collected their specimens for study. To establish the laboratory as the *definitive* location of scientific endeavour entailed a break with the extant tradition of science as practiced "out in the field" by naturalists, botanists, geologists etc. In his Bangor address of 1885 William Thomson took up this theme, arguing that the man of science in the field had to bring his specimens back to the laboratory to study the reality which underlay their surface appearances:

> The laboratory of a scientific man is his place of work. The laboratory of the geologist and of the naturalist is the face of this beautiful world. The geologist's laboratory is the mountain, the ravine and the seashore. The naturalist and the botanist go to foreign lands, to study the wonders of nature, and describe and classify the results of their observations. *But they must do more than merely describe, represent and depict what they have seen.* They must bring home the products of their expeditions to their studies and have recourse to the appliances of the laboratory properly so-called for their thorough and detailed examination...The naturalist in his laboratory with his microscope and appliances for the keenest examination, learns to know more than can be learned by merely looking at external beauties. The geologist...brings his crystals to the physical laboratory to be examined as to [i.e. to measure] their physical properties, their hardness, the angles between their faces, their optical qualities. Some people might think this an ignoble way to deal with crystals. But it is not so to the trained eye and deeper thought of the scientific man. The scientific man sees and feels beauty as much as any mere observer...But he also sees something *underlying* that beauty...Th[is] *necessity* for study below the surface seems to have been earliest recognised in anatomy...
>
> [Thomson,1885,409]

In this passage then we see Thomson transposing the natural historian's aesthetic appreciation of the beauty of outdoor "Nature" into the quantitative study of the underlying reality of matter, a "reality" uniquely accessible in the indoor laboratory.

Henry Roscoe made a similar comparison between the cognitive value of professorial lectures and laboratory work in a topical speech made at the opening of his new chemistry laboratory at the recently built Owens Extension College in 1873. Roscoe argued that:

> lectures serve as giving a general view of the main features of a subject; the laboratory work brings the student into direct contact with Nature and gives him an insight into her processes, which can only thus be obtained ...It is thus with the study of chemistry; the laboratory is the place where the details are really mastered.
>
> [Roscoe,1873,539].

This view of the relation between lectures and laboratory work in chemistry was not, however, controversial, as we in the testimony of Edward Pickering, an American physicist who had introduced a scheme of laboratory work in physics at the Massachusetts Institute of Technology in 1867. Juxtaposing this established hierachy in chemical education with his innovation in physics teaching in a Nature article of January 1871:

> It is well known that chemistry can be taught far better by a laboratory in which the student performs the various experiments, than by any system of lectures. Now although for many years physicists have been in the habit of instructing their special students and assistants in this way, but only recently has the same plan been tried with large classes in physics...By..handling..instruments [the student] acquires a facility in using them and comprehension of their construction which he could never obtain from lectures...[and through these classes]...the value of a knowledge of physical manipulation is becoming daily better appreciated and it is evident that instruction of this kind can be properly given only in a physical laboratory.
>
> [Pickering,1871,241]

Whilst Pickering's views of the *unique propriety* of the physics laboratory as a place for learning experimental physics through tactile contact with Nature were gaining currency amongst academic physicists in the early 1870's, another battle of rhetoric was taking place between laboratory propagandists and the private "cram-tutors".

2)ii: Laboratory tuition vs. cramming

A vociferous source of opposition to the instigation of laboratory science teaching from the late 1860's onwards can be found in the complaints of a number of private tutors against the character and standard of the examinations which the protagonists of laboratory teaching were successfully enforcing upon the institutional study of natural science during the 1860's and 1870's. These private tutors were disparagingly dubbed the "cram" tutors by their detractors, for their characteristic practice was to offer rapid instruction in a range of academic subjects - at whatever expense - to those wishing to pass examinations for the Civil Service, the Department of Science and Art or matriculation at the University of London. Their "cramming" method was to "drill" or "grind" their pupils into the rote-learning of information that would furnish a range of standardized answers to a typical range of examination questions with the minimum of time and effort. Since a systematic practical training in science was incompatible with this personalized domestic tuition and since the tutors anyway were generally private individuals independent of any institution, they had no access to the institutional resources that were necessary to establish a teaching laboratory. The cramming fraternity thus reacted with consternation to the institutional proliferation of laboratory teaching for it was a form of tuition in which they could not drill or grind their pupils.

A sizeable correspondence on this topic was generated in 1867 in the medical journal The Lancet [The Lancet,40 519,583&776]. For example, in June of 1867 a reader wrote in, complaining bitterly that the examiner in chemistry for the University of London, Professor A.W Williamson of University College, was offering a course in *practical* chemistry as a means of preparing for the matriculation examination. The "Inquirer" alluded to a distinct nepotism in this "convenient arrangement" whereby the examiner acquired a guaranteed clientele for his classes and at the same time imposed an unfair "severity" on the University examinations - insofar as they demanded more than a training in text-book chemistry [The Lancet,40, 722]. The editor of The Laboratory, John Cargill Brough, very soon afterwards identified the 'Inquirer' as a cram tutor threatened by the ascendance of laboratory teaching methods: "the writer in the Lancet fears for his craft, for none can be in doubt that he belongs to.....the cramming fraternity and so throws out base insinuations against a Professor". Brough then responded to this attack on Professor Williamson's laboratory teaching by fiercely defending the prerogative of laboratory teachers to set their own standards of science examinations according to standards of experimental expertise :

> We do not have to look far beyond Inquirer's letter to find a reason for its publication. At University College *Practical Chemistry* is taught, as he tells us; and if he knows anything about chemistry he will feel that laboratory teaching is the *only* teaching by which a real and useful knowledge of the science can be imparted..he feels no doubt that his "craft is in danger", that the sound teaching at University College will upset, as it is no doubt intended to do, those "coaches" that convey idlers and noodles through examinations which are intended as proofs of sound knowledge, not of cramming, as public safeguards against quackery and imposture....[These examination questions] are set by men determined to test the nature of the candidate's knowledge and to upset the unprincipled system of cramming. And this no doubt is why Inquirer tries to throw discredit on one who has been foremost in putting down "grinding" by teaching his pupils in such a manner that they cannot need it.

[The Laboratory,1,214]

Brough took up a similar theme in advancing the cause of laboratory teaching in a subsequent issue, arguing that the "remedy for cramming" was to make the system of practical examinations in science quite universal, thereby giving the practitioners of laboratory teaching a monopoly on scientific education:

> Were all examiners, with one accord so to frame their examinations as to make them touchstones of real knowledge, the whole system of cramming would disappear within a twelvemonth...Let the examinations in physics and chemistry consist wholly of a repetition by each student of fundamental observations and [measurement] experiments and cramming will be known no more, as far as these sciences are concerned.
>
> [The Laboratory,1,286]

This controversy between the cram-tutors and the experimental teachers became a tactical weapon not only in the advocacy of laboratory teaching but also in negotiating financial support for laboratory construction. We will see in chapter 3 how the physicist P.G.Tait railed on the subject of "cram" in 1874 while addressing the students at Edinburgh University on the nascent scheme for extending the University, with particular reference to the new laboratories that were to be built. He argued that the facilities would thereby be acquired for teaching science practically "as it ought to be taught and thus tend to extinguish paper-science [i.e.textbook learning], a term which conveys to all who are *really* scientific men an impression of the most unutterable contempt"[Nature,9,501-02]. Tait thereby tellingly differentiated the profession of crammer from that of science by asserting that the only *real* men of science were those who practised in the laboratory. In the next section we will see how Tait defended with equal vigour the integrity of laboratory teaching against the reactionary strictures of his former

Cambridge tutor Isaac Todhunter upon the issue of didactic *authority* in the teaching of science.

2iii): <u>the authority of Nature in the laboratory</u>

In the 1869 lecture "Scientific Education: Notes from an After Dinner Speech" T.H. Huxley strategically denigrated the effectiveness of traditional pedagogical media in order to press home the claims of laboratory education. His specific complaint was that the education provided by schools formed a poor preparation for a training in science because it characteristically inculcated students into an undue reliance upon the authority of textbooks and teachers. A recipient of a traditional schooling would thus not only be:

> ...devoid of all apprehension of scientific conceptions, not only...fail to attach any meaning to the words "matter", "force", or law in their scientific senses, but worse still [have] no notion of what it is to come into contact with Nature, or to lay his mind alongside of a physical fact, and try to conquer it in the way our great naval hero[7] told his captains to master their enemies. His whole mind has been given to books, and I am hardly exaggerating if I say they are more real to him than Nature. He imagines that all knowledge can be got out of books, and rests upon the authority of some master or other.
>
> [Huxley,1869,116-117]

Huxley asserted by contrast that "Nature" as studied practically should be the *sole authority* in compelling belief in the laws of science, so that:

> when teaching a boy physics or chemistry...you must be careful that what he learns he knows of his own knowledge. Don't be satisfied with telling him that a magnet attracts iron. Let him see that it does. Let him feel the pull of the one upon the other for himself. And especially tell him that it is his duty to doubt until he is compelled

[7] Note here how Huxley promotes the *moral* qualities of an experimental education in science by reference to a Nelsonian heroism in conquering the facts of Nature.

by the absolute authority of Nature to believe that which is written in books.

[Huxley,1869,127]

This polemic of Huxley's against the effects of traditional educational methods was still very much in action 14 years later in his speech at the opening of the new laboratories at the Royal School of Science in South Kensington. From his own teaching experience in the earlier biological laboratory there he related that:

> Nothing is more surprising to me than to find a number of instructed persons coming here for scientific education, and to discover that they cannot observe. They have been so accustomed to take statements on credit from books and word of mouth that they have almost lost the faculty of seeing things for themselves. I remember after having given a lecture, accompanied in my ordinary way by drawings on the blackboard, that I went to look through the microscope, and see what one of the students who had heard this lecture was drawing. To my astonishment, I saw that his drawing was the thing I had drawn on the blackboard not the thing under the microscope. I said to him, What is this? this is not at all like what is under the microscope. No, he said, that is what is on the blackboard. He did not believe nature, he believed me; and the great lesson I have tried to teach, which is the fundamental basis of scientific teaching, is do not put too much faith in your teacher, but do believe nature.
>
> [Nature,26,234]

As regards the teaching of physics at the Royal College of Science we can give a similar example of this style of rhetoric in an 1885 article by G.W.von Tunzellmann - G.C. Foster's laboratory assistant at University College, London in the late 1870's to 1880's - entitled "South Kensington examinations and the teaching of physics". As an examiner himself, Tunzellmann articulated the Department of Science and Art's policy as follows:

> The student of physics is not asked to accept his facts upon the authority of a teacher or a book. He is brought face to face with the experimental demonstrations and compelled to accept them by the evidence of his senses. If the teacher does his work

> at all as it should be done , the learner is taught to question every statement until Nature herself has satisfied him of its truth.
>
> [Tunzellmann,1883,127]

In order to replace the traditional reliance upon authority of the textbook and/or pedagogue by an equivalent cognitive source germane to laboratory work, Huxley and Tunzellmann thus invoked an *alternative* authority unique to the laboratory and free from human subjectivity: the authority of "Nature". This creation of "Nature" as an anthropomorphic cognitive authority enabled Huxley and Tunzellman to retain the notion of an independent source of objective scientific knowledge, thereby avoiding the necessity of explicitly devolving didactic authority to the students themselves in their apprehension of scientific facts in the laboratory. However, Tunzellmann argued further that this unparalleled cognitive authority of "Nature" could, to some extent, be communicated to the science master if the science master was taught directly by "Nature" in the laboratory: "it is only when taught by one who has himself sat at Nature's feet and learned something of her ways that the study of physics can produce its proper effect on the mind of the learner" [Tunzellmann,1883,126].

To understand the purpose of such rhetoric as a tool against the opposition of established academics who had professedly not learnt "at Nature's feet" we can cite the case of the Cambridge mathematics tutor and clergyman Isaac Todhunter. At Cambridge in the 1870's, the Cavendish Professor of Experimental Physics, Clerk Maxwell, once offered to give Todhunter an experimental demonstration of the optical phenomenon of conical refraction only to be told by the latter "I have been teaching it [i.e. conical refraction] all my life and I do not want to have my ideas upset by seeing it" [Schuster,1911,25]. To expound his caricature of traditionalist pedagogy, Todhunter wrote The Conflict of Studies in 1873, and in this book he argued that laboratory education was not only unnecessary but positively subversive in a course of formal education [Todhunter, 1873].

In this work Todhunter defends the traditional pedagogy of mathematics and classics against the propaganda of Huxley *et al* in complaining of "the assault which has been made made in our time on the monopoly enjoyed by the older studies [which] seems to have been a combined movement in favour of chemistry, natural philosophy and natural history". Directing his counterattack specifically at "Experimental Philosophy...a subject which may be considered one of the most fashionable elements in education at the present time" [Todhunter,1873,15], he argued first that the alleged pedagogical value of teaching by *experiment* was totally spurious, his view being that the *proper* role for experiments lay in investigation, not education:

> Experimental Science, viewed in connection with education, rejoices in a name which is unfairly expressive. A real experiment is a very valuable product of the mind, requiring great knowledge to invent it and great ingenuity to carry it out....When Perrier ascended the Puy de Dome with a barometer in order to test the influence of change of level in the height of a column of mercury, he performed an *experiment* ... [but] when a modern traveller ascends Mont Blanc and directs one of his guides to carry a barometer he cannot be said to perform an experiment in any very meritorious sense of the word. It is a *repetition* of an observation made many thousands of times before and we can never recover any of the interest which belonged to the first trial.
>
> [Todhunter,1873,16]

Todhunter, however, not only considered it *unnecessary* for a student to see an experimental demonstration of a physical fact but went so far as to denounce such demonstrations as subversive of a teacher's authority.

Citing specifically the demonstration of free-fall in a vacuum in which a feather and a sovereign fell equal spaces in equal times, Todhunter exclaimed:

> It may be said that a boy takes more interest in the matter by seeing for himself, or by performing for himself ..by working the handle of the airpump: this we admit while we continue to doubt the educational value of the transaction [for although] it may be said that the fact makes a stronger impression on the boy through the medium of his sight, that he believes it the more confidently. I say that this ought not to be the case. If he does not believe the statements of his tutor - probably a clergyman of mature knowledge, recognised ability, and a blameless character - his suspicion is irrational and manifests a want of the power of appreciating evidence, a want fatal to his success in that branch of science which he is supposed to be cultivating.
>
> [Todhunter,1873,16-17]

Arthur Schuster later argued that Todhunter was not representative of a broad movement of clerical opposition to experimental teaching, remarking that the Cambridge don "was a freak who differed from his type in having the courage of his opinions" [Schuster,1911,27]. Nevertheless Todhunter was a highly respected author of elementary mathematics text-books such as Algebra [Todhunter,1858] and Euclid [Todhunter,1862] and so widely read were these works that whole generations of Victorian youth received their entire education in mathematics, both at school and at University, from Todhunter's textbooks [D.S.B.]. The extent of the readership which Todhunter could thus have expected for The Conflict of Studies was recognised by his former tutee Peter Guthrie Tait as extremely wide and constituted by precisely the same audience to which he and the other protagonists of the "laboratory revolution" sought to convert to the kind of practical teaching so vehemently condemned by Todhunter.

To counteract the great damage which Todhunter's influence could thus effect upon the nascent British culture of physics teaching laboratories, of which his Edinburgh model was an early component [Ch.3], Tait wrote a scathing review of The Conflict of Studies for Nature in early 1874 - a review which reflects Tait's deep immersion in the characteristically democratic culture of Scottish Education [Ch.2 and 3]. Tait indeed upheld Todhunter's status "as one of the most erudite and voluminous of British mathematicians" but whilst acknowledging the high opinion of him held by teachers, school pupils and mathematics students, he argued to Nature readers that Todhunter was not qualified to pass such an *authoritarian* judgement upon the propriety of practical teaching: "such a man speaks with great authority, on many points; and therefore his dicta upon a point with which he shows himself to be totally unacquainted are especially dangerous. And I feel that it is my duty to point out to you and warn you against, errors or absurdities connected with physics, whenever they come from one whose statements are, on other grounds, worthy of attention" [Tait,1874,323].

Whilst pointing to the manifest "absurdity" of Todhunter's argument, Tait's strategy in this critique was not even to bother addressing the issue of didactic subversion raised in The Conflict of Studies, explaining instead that verbal comment on the passage cited above "would be altogether superfluous". Tait instead demonstrated the "self-evident" pedagogical *effectiveness* of experimental illustration and to achieve this he described in detail the use of Hope's apparatus for measuring the maximum density point of water, an experiment which Tait simply asserted as being one which "wonderfully assists you in understanding" the nature of density variation with temperature. At the end of his review Tait reiterated his view that this demonstration of the didactic authority of experiment obviated any need to answer Todhunter's strictures, explaining that "the only practical comment which I am disposed now to make is to proceed at once to further *experimental* illustrations of the subject before us" [Tait,1874,323].

Although Tait here made somewhat short shrift of The Conflict of Studies, his defence of experimental physics teaching against Todhunter's "dangerous absurdity" was raised in several of his public discussions on the methods of scientific education, such as in his Introductory Lecture to students at Edinburgh University in 1877. A version of this lecture, again quoting Todhunter verbatim, was published in the popular journal The Contemporary Review but this time Tait argued explicitly against Todhunter; Tait asserted that experiment was the *only* way of comprehending some of the more abstruse facts of Nature and therefore that Todhunterian authority, for all its clerical dignity, could simply never be *sufficient* for educating students on these subjects:

> Many facts cannot be made thoroughly intelligible without experiment; many others require no illustration whatever, except what can be best given by a few chalk lines on a black-board. To teach an essentially experimental science without illustrative experiments may conceivably be possible in the abstract, but certainly not with professors and students such as are to be found on this little planet.
>
> [Tait,1876b,306]

Whilst Tait reacted in a characteristically Scottish fashion to defuse Todhunter's extreme Cantabrigian authoritarianism, it is important to note how other academic physicists effectively reacted to Todhunter's other main objection to the teaching of practical science: that it was essentially *unexaminable*. Discussion of this issue will serve, by a conspicuous historiographical irony, to introduce us to the national system of science examinations which, as a major vehicle of the "laboratory revolution", was put into effect by the Department of Science and Art several years before the publication of The Conflict of Studies.

2)iv : Examining "Nature" in the laboratory

In the Conflict of Studies Todhunter strongly defends the precedence of the traditional academic subjects of mathematics and classics against the curricular incursion of the experimental sciences on the grounds of their relative *examinability*. Todhunter's main gambit was to argue that "examinations can be brought to bear on what is most important in the [older] subjects" and hence that "classics and mathematics are strongly to be recommended on the ground of the *accuracy* with which they can compare the relative performance of the students" [Todhunter,1873,6-7]. By contrast he argued that the distinctive features of the experimental sciences were not thus amenable to quantitative and meritocratic examination:

> I have had much to do with examinations but not exclusively in pure and mixed mathematics; and my experience is that nothing is so hopelessly worthless as the products of examination in experimental science. Nowhere else is the proportion of what is intelligible and true to what is absurd and false so small. Often after encountering a mass of confusion and error the disheartening conviction has been forced upon the examiner that the candidates must have derived positive harm from their attempts. *Experiments* indeed strictly so called can scarcely be introduced in an examination room; in other words, the distinguishing characteristic of the subjects cannot be subjected to test. I have heard it said by an eminent professor that the intelligent use of instruments is a most essential part of natural philosophy,[but] that it is almost impossible to examine a large class in the matter; it would be dangerous to trust a good instrument in the hands of an average candidate.
>
> [Todhunter,1873,8-10]

This "eminent professor" was almost certainly the Oxford Professor of Experimental Philosophy, R.B. Clifton, whose common context with

Todhunter was their training in Cambridge mathematics. Clifton was indeed well-known for his reluctance to allow any but the most advanced students to use his delicate apparatus [Ch.6] and Todhunter's endorsement of such views suggests his position as a clerical mathematician opposed to practical teaching was not so far removed from that of Clifton who, as the most conservative laboratory practitioner discussed in this thesis, taught practical physics only to a small elite of mathematics students. Although conservative in this respect as the Oxford Professor of Experimental Philosophy in the late 1860's he adminstered, *contra* Todhunter, the practical examination of physics for the University of Oxford B.A. in the School of Natural Sciences [Ch.6]. These examinations he carried out through a didactic medium which readily met Todhunter's desideratum that the relative performance of students in examination be amenable to "accurate" comparison: the medium of precision measurement.

Having seen how laboratory protagonists such as Tait dealt with the opposition of Todhunter and the cram-tutors it is thus important to see the struggle for the recognition of laboratory work as a conflict between a number of professional sub-groups in the educational world who were mutually competing for a common clientele of natural science students. In the next section we will therefore consider how the laboratory was promoted by teachers of experimental science to *displace* these other didactic practices. Firstly we will discuss the activities of the B.A.A.S. Committees involved in the public canvassing of experimental science teaching; secondly we will refer to the activities of T.H. Huxley and his colleagues at the Department of Science and Art in promoting the centralized *experimental training* and *certification* of school science teachers through courses of laboratory practice.

SECTION III: the industrial context of laboratory teaching

1): Campaigning for experimental science in education.

All of the laboratory physicists discussed in this thesis except one[8] were active from the late 1860's onwards in a continuous sequence of B.A.A.S. Committees created to monitor and promote the level of experimental science teaching in British schools and colleges. The first of these committees was formed at the Nottingham meeting of 1866 and on to this committee were elected John Tyndall and the ubiquitous T.H.Huxley, both Professors at the Government School of Mines and Examiners for the Department of Science and Art. In 1867 this committee created the B.A.A.S. "manifesto" on which to promote publicly the interests of experimental science teaching, arguing that experimental science provided "the best discipline in observation and collection of facts, in the combination of inductive with deductive reasoning, and in *accuracy* of both thought and language" [B.A.A.S. Report,1867, xxxix-xl]. In arguing that physics was of paramount importance in the teaching of experimental science we will see how Thomson, Foster, Clifton *et al,* argued that the specific value of their subject in a "liberal education" lay in its facility to furnish students of the subject with *exact* habits of observation, *precise* habits of thought and *accurate* modes of reasoning - skills derived directly from the practices of laboratory measurement [Ch. 2-8].

In the following year, 1868, the issue of physics teaching in schools was pursued further by a committee for "inquiring into the present methods of teaching the elements of Dynamics, Experimental Physics, and Chemistry

[8] This exception was the dissident Frederick Guthrie who promoted the interests of experimental physics teaching instead through the Physical Society which he created in 1873-1874 [Ch.8].

in schools of various [social] classes" and to this committee were appointed the physicists George Carey Foster and John Tyndall. Following this up in the next session they presented a petition to Parliament pleading that "in the opinion of the Association, the study of Natural Science, whether as a means of disciplining the mind, or for providing knowledge useful for the purposes of life, is of essential importance to the youth of this country; and that it ought to form a part of education in all secondary schools" [B.A.A.S. Report,1869, xlii-xliii].

This canvassing of science as a constitutive part of a school curriculum was a strategic move by the B.A.A.S. committee since at this juncture the British education system was undergoing a major expansion from the bottom level upwards as a consequence of the national system of compulsory primary education being introduced by Forster, the Liberal Minister of Education. In an article entitled "Supply of Teachers for the New Schools" in January 1871 the Journal of the Society of Arts discussed a long letter from Sir James Kay-Shuttleworth arguing that the 35,000 elementary school teachers required to fill posts at the new primary schools was 10,000 more than the existing training colleges could supply. Kay-Shuttleworth argued that there was "every reason to encourage the apprenticeship of pupil-teachers" and at the same time a need to establish new training colleges in order to meet this demand; the debates on how to achieve these goals were continued in this journal throughout the following three years [Journal of the Society of Arts,21,129 & 682].

In this phase of national expansion the B.A.A.S. Committee had clear motives, then, for attempting to assimilate an education in experimental science into the curriculae of the new schools and hence to enforce the *practical* training of the ascendant generation of school-masters. Indeed the majority of the academic laboratory physicists discussed in this thesis were engaged to some considerable degree in the training of school teachers[9] and thus naturally sought the extension of their own specialist methods of laboratory teaching to the school classroom. This is clear from the report of another educational committee appointed in 1873 to investigate the teaching of physics in schools which consisted of Adams, Stewart, Foster, R.B. Clifton, and the Ireland-based physicists J.D.Everett and W.F. Barrett. Their report was an explicit promotion of laboratory work framed in terms of the rhetoric discussed above:

> [The committee] think it of the utmost importance that the first teaching of all branches of physics should be, as far as possible, of an experimental kind. Whenever circumstances admit of it, the experiments should be made by the pupils themselves, and not merely by the teacher; and though it may not be needful for every pupil to go through every experiment, the Committee think it essential that every pupil,should at least make some experiments himself.
>
> For the same reason, they consider that the study of text books should be entirely subordinate to attendance at experimental demonstrations or lectures, in order that the pupil's first impressions may be got directly from the things themselves, and not from what is said about them.
>
> [B.A.A.S. Report, 1874,71-73]

This committee recommended that the practical teaching skills of these school-masters should be cultivated by learning these skills directly from "the leading teachers of Physics in the universities, colleges and schools

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[9] As educationalists themselves they were all involved in promoting the interests of scientific teaching as is evident in a further Committee appointed in 1871 to "consider and report on the best means of advancing science by lectures" consisted of the majority of the laboratory physicists discussed in this thesis: Adams, Foster, Stewart, Thomson, Clifton, Tait along with the august company of H.E. Roscoe, T.H. Huxley and the editor of Nature J.N. Lockyer [B.A.A.S. Report,1871,lxxiii]

of the United Kingdom" through acting both as their pupils and assistants.

Ironically, the one physicist who was not a member of these B.A.A.S. educational committees, Frederick Guthrie, had been active with his other colleagues at the RSM since 1869 in promoting the practical training of the large body of school teachers employed by the Department of Science and Art. Employed by the DSA to supervise the Government's national scheme of science examinations established in 1859 for the "self-improvement" of the industrial classes, Huxley the biologist, Frankland the chemist, and Guthrie the physicist played a direct role in effecting the "laboratory revolution" in British schools and colleges. Through their positions in the DSA, these men were able to restructure the examinations to require a greater element of *experimental* teaching of the candidates entering for them, and by this means insisting upon the practical training of school teachers. Their subtle reconstrual of the DSA examinations led to the creation of a centralized scheme for the *laboratory* training of school and college science teachers as will be documented in the next section. [Devonshire Commission 2nd Report, 1872, xix-xxviii].

2): The DSA examinations and the "laboratory revolution"

The scheme of examinations run by the Department of Science and Art was a particularly useful resource for the protagonists of the laboratory revolution because the pupils in day schools, night schools and city colleges who were training for these examinations constituted by far the largest single body of science students in Britain during the late 1860's and 1870's. During the 1860's a large number of school teachers had suffered financial hardship when the curricula of Government-run elementary day schools and training schools was narrowed to "the three

R's" in the Revised Code of 1861 since this Code effectively removed institutional funding from the teaching of other subjects. To augment their reduced salaries many teachers joined the scheme in order to benefit from the DSA policy of payment by examination results. This was a major factor in the expansion of the DSA system during the 1860's, although we will see shortly the socio-industrial foundation for the accelerated growth of the scheme from 1867 onwards as depicted below:

Year	Number of schools in DSA scheme	Number of students under DSA instruction
1860	9	500
1862	70	2543
1864	91	4666
1866	153	6835
1867	212	10,230
1868	300	15,010
1869	523	24,865
1870	799	34,283

[Devonshire Commission 2nd Report,1872,xi-xx]

As the scheme grew to this level in the late 1860's Huxley, Frankland *et al* took the opportunity to increase the "stringency" of the examination setting and marking as part of their plan forcibly to introduce practical teaching into the scheme; the Devonshire Commissioners reported the rationale of their plan as follows:

> ..from the considerable proportion of failures which occur, as well as from the character of the answers given, the examiners are under the impression that a very large part of the instruction is drawn from books...and that it is not often illustrated by specimens or experiments, the use of apparatus or [even] the outdoor study of nature... we have it in evidence that not only is scientific apparatus wanting, but that too often teachers confine their instruction to the same routine of book-learning and class-questioning...[by] which they received their own imperfect elementary knowledge.
>
> [Dev.Comm. 2nd Report, 1872, xxv]

Articulating this complaint further by invoking the rhetoric of laboratory work that we discussed earlier, Professor Frankland reported of the chemistry examinations that:

> the chief defects noticed in the papers were obviously due to the want of efficient laboratory training in practical chemistry....it was also evident that the candidates had depended too much upon mere book work and oral instruction; they had not been brought sufficiently into contact with the phenomena themselves through the aid of experimental illustrations, performed either by them personally or by their teacher...
>
> [Frankland,1870,q766]

Of the examiners' attempts to improve the preparation of DSA candidates, Frankland expressed the problem facing them in terms of a fundamental conflict between the established traditions of schooling for literary subjects and the new laboratory methodology of teaching science by contact with "Nature":

> The long-continued exclusively literary training which has obtained in our schools and colleges, makes both teachers and pupils slow to learn that a training in experimental science does not contemplate merely the reading and committing to memory of the thoughts of others, but much more, an actual contact of the student with the phenomena presented by the objects that surround him.
>
> [Frankland,1870,q766]

In this scenario the Examiners Frankland, Huxley and Guthrie used their position in the Department to further the causes of laboratory science teaching by advocating a system of practical training for teachers which would be supervised by the Examiners themselves. As Frederick Guthrie later related in a lecture given to the Society of Arts on the reforms effected in his own subject :

> Put to the test of examination it appeared that such [orthodox] instruction [chiefly betrayed] rather an acquaintance with books than a knowledge of things. Hence it was decided to introduce practical instruction to the teaching of physics of such an elementary but always exact

> kind as would give more reality, vitality and interest to the instruction and enable the teachers of science in elementary schools, in their turn to make the instruction which they gave to their pupils more real and impressive.
>
> [Guthrie,1886c,660][10]

Huxley, Guthrie and Frankland thus persuaded the Department in 1869 to introduce financially assisted summer courses of experimental lectures for teachers at the Royal School of Mines and at South Kensington which were "specially meant to instruct teachers in the art of teaching, making their experiments, etc". In total 253 provincial science teachers attended one or both of Guthrie's course on light and a joint course by Huxley and Michael Foster on animal physiology, whilst Frankland gave a course of laboratory instruction at the Royal College of Chemistry. After a similar arrangement the following year the particular success of Frankland's laboratory training prompted the DSA to initiate similar practical courses for biology by Huxley and physics by Guthrie in 1871 [Dev.Comm.2nd Report, 1872,xxi]. This systematic and centralized scheme of laboratory training for teachers thus cultivated for DSA teachers by the powerful coterie of RSM professors was moved to the Department's laboratory complex at South Kensington in the summer of 1872. At this juncture Guthrie and Huxley respectively opened physics and biology laboratories for the full-time instruction of "teachers-in-training" and students of the RSM [Ch.8].

The success of the DSA scheme that had been so carefully stage-managed by the RSM professoriate as a means of inculcating a whole generation of science teachers in the techniques of laboratory training was celebrated as follows by Nature in 1875:

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[10] For a very similar account by Huxley of the parallel changes in the teaching of biology see Huxley quoted in [Nature,12, 208].

> The improvement in the quality of the education given by the science teacher is already making itself felt. The reports of the [DSA] May examiners for recent years have shown that "while the general average has been maintained throughout, the instruction had in some subjects decidedly improved". But it will necessarily take up a few years to lift up so large a constituency. Surely and slowly it is being done, and the masses of the country are gaining a sound elementary knowledge of science. Whilst the magnificent laboratories of the Universities of Oxford and Cambridge and Dublin are nearly empty, Owens College and the [practical] classes under the Department are crowded with active and earnest workers.
>
> [Nature,12,206]

The DSA scheme of centralized laboratory training was successful because it was founded upon a confluence of interests between the protagonists of the "laboratory revolution" and the extant population of school teachers: Huxley *et al* manipulated the requirements of the DSA examinations to bring about the laboratory training of school teachers on a national basis and these school teachers made use of the financial incentives offered them for both DSA teaching and the scheme of laboratory training. However it is important to note also that this confluence of interests in laboratory training extended to a large third group: the "industrial population". This educational clientele, at whom the DSA examinations were primarily directed, were concerned to acquire a training in *experimental* science for reasons which relate to the debates on "technical education" immediately following the Paris Exhibition of 1867. In the discussion which follows we will see how the generically precise practices of contemporary experimental physics had particular relevance to the campaign for a scheme of scientific training intended to eliminate the non-precise manufacturing methods of "rule-of-thumb" prevalent in British industry.

3):The 1867 Paris Exhibition and the "laboratory revolution"

The late 1860's and early 1870's saw a general expansion in many areas of British education, not only in the 1870 provisions for universal primary school education but especially also in the level of science teaching to the industrial population[11]. The Nature article cited above documented a five-fold increase in the number of students attending DSA science classes from 10,230 to 48,546 between 1867 and 1873, explaining that the "international exhibitions have been at the bottom of this" [Nature,12,205]. The international exhibitions in question had been held alternately in London and Paris in 1851, 1855, 1862 and 1867, and were forums for the public adjudication of relative industrial progress amongst the manufacturing nations of Europe [Danvers,1867,488-499]. The Paris Exhibition of 1867 in particular, however, instigated considerable controversy about the connection between industrial progress and the systematic scientific education of the manufacturing population; in this section we will explore how protagonists of laboratory science used this controversy as a resource to promote the teaching of experimental science to the British industrial population.

At this Exhibition in Paris, the expertise of British manufacturers received a far smaller proportion of the official accolades than they had at preceding Exhibitions, most memorably at the Crystal Palace in 1851. Scientific lobbyists, particularly Lyon Playfair, argued that this debacle had resulted from British "rule-of-thumb" manufacturing methods being

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[11] The industrial classes were defined here as being "all those in receipt of weekly wages, small tradesmen whose income does not exceed £200 per annum, the children of any of these, all attendants at Public Elementary Schools, together with the teachers and teacher pupils of such, and the students in the Training Colleges which receive grants from the Education Department" [Nature,12,204].

superseded by the industrial techniques of the scientifically-educated foremen and management in France, Austria, Germany, Switzerland and Belgium [Select Committee on Education Report,1868, "Correspondence Relative to Technical Education"]. Playfair was not, however, the only scientific figure who identified the central issue in Britain's loss of prestige as being the "anachronistic" crudeness of British workshop practices. Playfair's intense canvassing for the scientific lobby led to the formation of a Select Committee in the year after the Exhibition to investigate the education of the nation's industrial workforces, and men of science and educationalists interviewed by the Committee declared similar views upon the manufacturing expertise of the nation. For example, the Registrar of the University of London, Dr. W.B. Carpenter, argued that there were a number of cities such as Sheffield and Birmingham in which the use of mere "manual dexterity" in manufacturing trades tended to exclude any working on scientific principles:

> Take the manufacture of implements on the old method, it is mere rule-of-thumb work; of course for the manufacture of Bessemer steel a very much higher scientific knowledge is necessary to carry it out thoroughly and effectively. It has always struck me that where the mere rule of thumb method constitutes the staple of the work, there is less demand for [scientific] intelligence.
>
> [Carpenter,1868,q2116]

A graphic description of the nature of these "rule-of-thumb" practices was given, albeit much later in 1882, by Alfred Mundella, the vice-president of the Committee of Council on Education:

In every part of England [the people] are tired of working by rule of thumb......I have heard a Dyer explain how he got certain results, he tried his alkalies and acids by dipping his thumb[sic] into them and tasting them, and when he found the components for a particular dye , he took a shovelful of this and a shovelful of the other, and so arrived at certain results which he could rarely arrive at with *precision* again, but which was mere guesswork, rule of thumb, chance, and accident... (emphasis added)
[Nature, 16, 236]

However, there was considerable controversy immediately after the Paris Exhibition over the conclusion of the scientific lobby that the poor showing of the British contingent resulted exclusively from the use of "rule-of-thumb" *vis-a-vis* scientific methods in industry. Of the twelve British jurors who had adjudicated upon the Exhibition in Paris as many as eight considered that Britain's alleged industrial regress involved more than *just* a lack of scientific practice in the nation's industry. These argued that British manufacturing expertise had not been fully represented at the Paris Exhibition, and some believed that the apparent regress of Britain's industrial contingent was caused by continental manufacturers systematically copying English designs and English machine tools in order to rival their products [Select Committee on Education Report, 1868, "Correspondence Relative to Technical Education"]. A valedictory article in the Quarterly Journal of Science written in similar vein by Frederick Charles Danvers at the close of the Exhibition in October 1867 declared:

Since 1862 France and Belgium have wonderfully improved in the manufacture of iron and steel, so that this country is not now so far ahead in its iron manufacture as was formerly the case. Foreigners now, also, make more of their own tools and machinery than heretofore; and although their best specimens are generally copies from English models, they can now, for all practical purposes, turn out as good machinery as could be obtained in this country; in excellency of design, however, and in finish, there is still no country that has come up to the standard of

> English manufactures. Alarmists have raised the cry that England is not keeping pace with the advancements of other countries, but we are disposed to believe the truth to be that whilst we steadily advance, other countries, which a few years since were much behind us have made themselves acquainted with all that we possess, and thus are able to make more rapid strides, and to lessen the distance between us and themselves.
>
> [Danvers,1867,499]

Nevertheless Playfair and allied pro-scientific "alarmists" subsequently succeeded in ensuring that media debate on the Paris Exhibition centred upon the issue of improving Britain's industrial position by educating the manufacturing population in *experimental* science. For example, in early 1870 George Gore published an article entitled "On Practical Scientific Instruction" in the Quarterly Journal of Science, in which he argued that those who were "best-informed" upon the industrial progress and widespread "diffusion of scientific knowledge" in America and Prussia thought that "unless great efforts are made in this country to ensure a general and widespread knowledge of science, the prosperity of our manufacture must speedily decline". Explicitly relating his main theme of "practical" scientific instruction to the generic practices of British industry Gore argued that "we must not trust to genius only and the 'rule of thumb' as we have hitherto done, but judiciously impart scientific instruction to minds of ordinary capacity as well as to others" [Gore,1870,215-216].

Gore characterized "rule of thumb" in terms very similar to those of Mundella, speaking of the "blind following" of routine and non-quantitative empirical method chiefly manifested in the indiscriminate use of resources which led to the "painful experience of the great and almost incessant variation that occurs in the quality and properties of materials". One example he cited of this was the deleterious effect of traces of arsenic upon the conductivity of copper telegraph cables, arguing later that the use of such defective materials in "unscientific management" had serious economic consequences since the discarding of imperfect products led to the higher pricing of saleable ones. Gore thus argued that "many of these difficulties arise from the *inaccuracy* and carelessness of the workmen, and would be lessened by the more general diffusion of scientific knowledge", and to emphasize the significance of scientific knowledge as the "accurate" substitute for rule of thumb in the pursuit of business efficiency he commented tersely: "in science, the great aim is truth and accuracy; in art and manufacture the chief object is to produce the best practical result at the lowest possible cost" [Gore,1870,217-219].

In the context of Gore's example of contaminated copper cables as an illustration of how the traditional avoidance of quantitative control and testing in manufacture led to uneconomic production it is important, therefore, to note that William Thomson had advocated exact laboratory methods of testing the conductivity of such cables since 1858 [Ch.2]. In his subsequent involvement with the Atlantic Telegraph Company Thomson used arguments of business economics to negotiate the introduction of his techniques of exact electrical testing and measurement into the company's manufacturing and cable-laying policies, thereby displacing the non-quantitative and ineffective "rule of thumb" methods characteristic of the Company's resident engineer E.O. Whitehouse [Smith and Wise,1989,661-665]. As we saw above, the development of "scientific telegraphy" as a prototypical measurement-based industry in the 1860's played a major role in the genesis of institutional laboratory physics, and in a later section we will explore the operation of the earliest British teaching laboratories as centres of training for telegraph engineers in precision electrical practices.

Although the debates about rule of thumb and its industrial displacement by precision training in laboratory science were current throughout the 1860's, it is important to observe how the advocacy of a laboratory training in science was given an especial impetus in the aftermath of the Paris Exhibition. This we can see from the proceedings of a conference organised in early 1868 by the Society of Arts that was intended to unite British politicians, aristocrats, industrialists, men of science and teachers in finding a solution to Britain's conspicuous loss of industrial leadership.[12] At this conference it was unanimously resolved to promote a broad campaign to extend the availability of "technical education" to the various sectors of the industrial population, construing "technical education" to consist of "general instruction in those sciences, the principles of which are applicable to various employments of life" [Journal of the Society of Arts,16,633].

In casting plans for this scheme in order to match industrial science

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[12] The delegates present included: Earls Granville, Russell, Lichfield; MPs of all parties including Bernard Samuelson, Dr Lyon Playfair, C.B.; Henry Cole, C.B.(Secretary of the Science and Art Department); Lieut.-Col.A.Strange; representatives of Chambers of Commerce from Wakefield, Macclesfield, Manchester, Birmingham, Huddersfield; Coventry; Academics and men of science including Prof.W.G.Adams (King's College London) Prof J.G. Greenwood (Principal, Owen's College, Manchester); Prof. Huxley F.R.S. (Royal School of Mines); Prof G.D. Liveing (University of Cambridge), Michael Foster and Prof.Fleeming Jenkin F.R.S. (University College London); Prof W.J.M. Rankine (Glasgow University) [Journal of the Society of Arts, 14, 183-184].

education on the continent, the Society of Arts' advisory sub-committee[13] recommended practical laboratory instruction especially in the *physics laboratory* , as *essential* for the first year of training for professions associated with:

1. agriculture and gardening
2. chemical manufactures
3. metallurgy
4. mining
5. civil engineering
6. naval architecture and marine engineering
7. mechanical engineering and machining
8. architecture

[Journal of the Society of Arts,16,633-637].

In chapter 5 we will see a specific example of how one industrialist with a reputation for *accurate* machine engineering viz Sir Joseph Whitworth, specifically sponsored the laboratory training of engineering apprentices in precise scientific practices in the wake of this conference between 1868 and 1871. We will conclude this introductory chapter, however, by linking these debates on the industrial importance of experimental science teaching to a broader analysis of the emergent clientele for a laboratory education in physics.

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[13] This sub-committee consisted of T.H. Huxley and Edward Frankland - Frederick Guthrie's colleagues at the Royal School of Mines as well as Fleeming Jenkin, A.C. Williamson and T.A. Hirst - George Carey Foster's colleagues at University College, London [Journal of the Society of Arts, 16,62].

4): <u>The laboratory revolution in its socio-industrial context</u>.

Sviedrys' account of the rise of physical laboratories in Britain emphasizes the "major stimulus" of electrical communications in the 1860's: "the growth of electrical telegraphy...demanded instruction in theoretical electricity and practical training in electrical precision measurements and hence encouraged the establishment of special laboratories for these purposes" [Sviedrys,1976,409]. However, in this institutionally simplistic argument Sviedrys fails to explain why the industrial "demand" for practical training in precision measurements which "encouraged" the foundation of physics teaching laboratories was so specifically effective in the late 1860's and not earlier. Furthermore he fails to note that trainee telegraph engineers were one of several distinct clienteles for laboratory physics teaching generated in the socio-economic context of the late 1860's to 1870's.

As regards the first point, the telegraphic industry and its associated institutions of professional training were given an appreciable stimulus after 1866 because, as W.G. Adams informed the Devonshire Commission five years later, the depression in civil engineering during 1866 severely limited the openings in that older branch of the profession. Many prospective engineers in that year were thus tempted to train instead for appointments in the Indian Telegraphic Service which hitherto had been insufficiently lucrative to countenance by comparison with the remuneration of posts with domestic civil engineers [Adams,1871, q6901, 6930,6938,6941]. Institutions such as King's College subsequently found a considerable growth in the number of students undertaking courses in electrical science that prepared them for the competitive Telegraphic Service examinations, W.G. Adams for example finding that a considerable proportion of the practical students attending during the first three years of his laboratory's operation aimed to join the Indian Telegraph Service:

Appointment/qualification	No. of Adams' students
Indian Engineering Service	10
Natural Science Exhibition, St Johns Cambridge	2
Natural Science Scholarships, Merton Oxford	2
Hons in University of London 1st BSc examinations	2

[Adams,1871,q6892]

Although Sviedrys appropriately emphasizes the extent to which Adams' laboratory at King's College and Thomson's laboratory in Glasgow were involved in the training of telegraph engineers for the Indian civil service, he does not acknowledge that the majority of students in the King's laboratory were in fact training to be civil engineers, nor does he fully comprehend the integration of Thomson's laboratory work within the tradition of Scottish "liberal education" [Sviedrys,1876,409-415 &418-419; Ch.2 & Ch.3]. He similarly overstates his case in claiming that George Carey Foster at the University College London laboratory also "strove to meet the needs of civil service candidates"; as Foster described his perceptions of the demand for a training in electrical telegraphy to the Devonshire Commissioners in 1870:

> The only demand for physical knowledge, that I am aware of, to any considerable extent, is in connexion with telegraph works. That is now coming to be a rather an important sphere, but that is quite recent....I have had several students who have been specially preparing for the Indian Telegraph Service, and some of the have taken very high places in the examinations, but I do not remember distinct cases where they have gone into private employment as telegraph engineers or anything of that sort.
>
> [Foster,1871,q7793-94]

Foster instead emphasized the expanding constituency of schoolmasters as the major clientele for laboratory training: "I think there is a rapidly growing demand for teachers of physics in the better class of schools, and I believe that the demand which already exists cannot be supplied" [Foster,1871,q7795]. Indeed the practical training of science teachers was the predominant activity of the London and Oxford laboratories during the 1860's and 1870's [Ch.4,5,6 & 8], especially as the training of telegraph engineers and electricians shifted to specialist establishments during the 1870's and 1880's [Sviedrys,1976,414-15].

Thus it will become clear that Sviedrys' emphasis upon the importance of the telegraphic industry as a "major stimulus to the growth of the physical laboratory in Britain" is somewhat misplaced [Sviedrys,1976,409]. Moreover his historiography of laboratory creation as a "passive" response to an industrial demand for training in scientific techniques is manifestly oversimplified; from the above discussion we can identify instead a number of separate groups whose *mutual* interest in cultivating the academic physics laboratory as a centre for teaching experimental physics was the primary factor underpinning its widespread institutional acceptance:

1. <u>the academic physicists</u> who established these laboratories as

   i) symbols of the institutional recognition of their new sub-profession of experimental physics,

   & ii) vehicles for the perpetuation of their research tradition in precision measurement;

2. <u>the educational institutions</u> which were persuaded by physicists to finance the fitting up/construction and equipment of these laboratories:

   i) as recognized prerequisites of secondary/tertiary instruction,

ii) to compete effectively against similarly equipped institutions in the free market of Victorian education;

3) <u>the science teachers</u> training in science to occupy the new school posts created in the expanding education system after the 1867 Paris Exhibition sought a training in laboratory physics in order:

i) to receive the training "at Nature's feet" which was canvassed by Huxley *et al* as essential to the *competent* teaching of experimental science in all institutions from artisan evening classes up to the public school.

ii) in the case of the burgeoning contingent of DSA teachers, to meet the stringent demands placed upon their pupil-candidates by the RSM-based examiners for evidence of being taught by "contact with nature" and thereby to reap the financial benefits of successful examination results.

4) <u>telegraph engineers</u> who turned to electrical science in the wake of the 1866 depression in civil engineering:

to receive a training in the techniques of precision measurement which William Thomson, Latimer Clark *et al* had made into the central practices of contemporary industrial telegraphy since 1858.

5) <u>mechanical, civil engineers, manufacturers etc.</u> who responded to the debates on the British manufacturing practices of "rule-of thumb" in the wake of the Paris Exhibition:

by taking a practical training in the methods of precision science in order to improve upon the traditionally low business efficiency of non-quantitative workshop methods and thus more effectively compete with continental industry.

Conclusion

Physics laboratories appeared between 1865 and 1885 as a result of a conjunction of an enormous internal growth in the discipline itself during the previous two decades - giving rise to new subjects of study viz thermodynamics and electrical telegraphy, as well as considerable optimism amongst the physicists themselves - and an industrial 'crisis' resulting from the Paris Exhibition of 1867 which scientific lobbyists successfully portrayed as resulting from an inadequate training in the techniques of precision measurement. Physicists used their expertise in precision measurement in schemes of liberal and industrially-oriented education which they and colleagues such as T.H. Huxley promoted with a highly-developed species of 'laboratory rhetoric' to attract audiences to their new educational medium and legitimate its novel character against opposition from established traditions of natural science teaching.

This expertise in precision measurement was rooted in their common participation in the B.A.A.S. committees of the 1860's to 1870's and was cultivated in their own institutional laboratories of teaching and research. These were specially designed and fitted up to isolate their delicate measurement operations from sources of disturbance beyond the laboratory walls, thus enabling the directors and their students to pursue the last decimal place of their precise physical determinations to a further degree of exactitude and impartiality.

Nature declared of all this laboratory activity in 1875: "We trust the time is not too far distant when the pressure of public opinion will lead men and women to feel but half educated if they have no aquaintance with the living facts and solid ground of nature...[Nature,12,247].

CHAPTER 2

William Thomson and the Glasgow University physical laboratory

A large part of the work of a physical or chemical laboratory must be measurement...The difficulties to be overcome in physical science in mere measurement are teeming with interest...

> William Thomson: 1885 lecture "Scientific Laboratories" at the opening of the laboratories of University College, Bangor [Thomson,1885,411]

Introduction

A study of William Thomson's laboratory at Glasgow necessarily forms the starting point of this thesis. Apart from being *merely* chronologically prior to the laboratories in London, Edinburgh, Manchester and Oxford, Thomson's laboratory both acted as the specific source of expertise in precision measurement and furnished the precedent of student experimentation for all these laboratories. Although Thomson's former students testified that their habitual voluntary assistance of his professorial researches was not a *prototype* for the more formalized courses of undergraduate laboratory training typical of the teaching laboratories discussed in chapters 3 to 8, Thomson's personal influence upon physicists of the 1860's onwards was unmistakeable: the laboratory physicist W.E. Ayrton described him in 1908 as the "inspiration of our lives" [Ayrton,1908,268].

More directly a model for the laboratory teaching practices of his later professorial contemporaries was his specialized work in *precision measurement* . The tradition of precision measurement practices which epitomized the activities of laboratories from the late 1860's onwards had originally been nurtured and propagated predominantly by Thomson from his student-assisted work in the Glasgow laboratory. These generic laboratory practices of exact physical measurement were first specifically cultivated by Thomson in his thermodynamic investigations of the 1840's to 1850's and we will locate the foundation of his University natural philosophy laboratory in the context of his related researches on thermo-electricity during the early-mid 1850's. Thomson further nurtured his characteristic practices of laboratory precision measurement in the telegraphic exploits which he undertook immediately afterwards in the late 1850's and 1860's and through his directly related work on the exact determination of electrical standards for the B.A.A.S. Committee on Electrical Standards from 1861 onwards Thomson communicated his ethos and expertise in precision electrical measurement to the British community of physicists. Subsequently laboratory work in accurate electrical measurement predominated in the teaching and research activities of both Thomson and his English counterparts during the 1860's to 1880's, the period essentially covered in this thesis.

To locate the contextual origins of Thomson's laboratory measurement practices we will next analyse the development of his early career as a synthesis of interests and expertise from Glaswegian natural philosophy and chemistry, Cambridge mathematics and Parisian precision *physique* .

1): Glasgow, Cambridge and Paris: training as a natural philosopher.

Unique amongst the secondary literature relating to the case-studies in this thesis, the institutional and biographical background to Thomson's career as a laboratory natural philosopher at Glasgow has been well-documented by 20th century historians, particularly in [Thompson,1910] and [Smith & Wise,1989]. Whilst no useful purpose would be served in duplicating comprehensive and widely available accounts of Thomson's early years as undergraduate, savant and youthful Professor immediately prior to his initiation of a physical laboratory, it is important briefly to acknowledge the contribution made by these and other accounts towards our understanding of the subject.

The tradition of experimental philosophy teaching at the University of Glasgow can be traced to the 17th century, and the practice of giving experimental demonstrations in natural philosophy lectures we can trace back at least to the tenure (1727-1757) of the first formally-appointed Professor of Natural Philosophy, Robert Dick Senior [Murray,1927,110-113]. The continuing essence of this Glaswegian tradition as perceived by the undergraduate William Thomson can be discerned from the notes he took of the Natural Philosophy lectures of Professor Meikleham in 1839-40: "the foundation..of natural philosophy..is experiment" in which mathematics was the instrument of "reason" [Smith & Wise,1989,Ch.4]. From John Pringle Nichol, the Radical Professor of Astronomy who soon took over the course of natural philosophy lectures from the debilitated Meikleham soon after their commencement in 1839, Thomson learnt that "no branch of physical science is perfect until it is reduced to number or quantity" - a creed central to Thomson's later practices as the archetypal measurer and quantifier in ninteteenth century physics. In Thomas Thomson's chemistry laboratory William Thomson was taught the application of this creed to experimental practice through the Professor's course of precision inorganic analysis [Smith & Wise,1989,Ch.4; Nature, 68(1903), 623-624; Morrell,1972, 15,21&45]

In 1841, the 17-year-old William was despatched by his father, then the Professor of Mathematics at Glasgow College, from this essentially egalitarian and medically-oriented Scottish University to read for the Mathematics Tripos at the anti-utilitarian and conservative University of Cambridge. Placed in this alien environment in order to achieve his father's desideratum of obtaining the best available disciplining in mathematics, the young Thomson soon came under the influence of William Hopkins, a private tutor with the reputation of being "senior wrangler maker." Under Hopkins' tuition, Thomson was drilled between 1842 and 1845 in the Tripos syllabus of calculus and mixed mathematics which included, for example, planetary theory, hydrostatics and hydrodynamics. [Smith & Wise,1989,Ch.3]

As his undergraduate mentor, Hopkins also encouraged Thomson to undertake a more *experimental* study of the subject than was required of a prospective Wrangler by advising him to attend the lectures of James Challis, the Plumian Professor of Astronomy at Cambridge. As William wrote to his father on April 22nd 1844:

> Today Professor Challis's lecures on Practical Astronomy and Astronomical instruments commenced, wh[ich] I am attending, along with the rest of Hopkins pupils, and most of the principal mathematical men in our year. After the Astronomical lectures are over we shall have an experimental course on Natural Philosophy. I do not know whether they are the same as I attended last year, but even if they are, Hopkins says I should attend them again, as I shall have an opportunity of seeing and handling the apparatus.
>
> [William Thomson to James Thomson Snr, 22/4/1844, T257, ULC]

This acquisition of practical experience in handling apparatus was of great significance to both Thomson and his father, for the latter was now orchestrating a campaign throughout Glasgow University to elect his son to the Professorship of Natural Philosophy upon the imminent demise of Professor Meikleham: a year previously he had informed William of the prevailing view that his "experimental acquirements" were to be of paramount importance in competing for this Professorship against such established experimentalists as the occupant of the Edinburgh Chair, J.D. Forbes [William Thomson to James Thomson Snr, 20/4/1843, T236, ULC; Smith & Wise,1989, Ch.4].

Thus immediately after graduating as Second Wrangler and First Smith's Prizeman in 1845, William took the advice of Nichol and others in travelling to France: they considered that the laboratories of the

Parisian savants were the best place to cultivate his skills as an experimental philosopher to a level comparable with his manifest expertise in the mathematical practice of natural philosophy. Thus in the spring of 1845 Thomson went first to the elderly Biot, who in turn introduced him to Victor Regnault, the eminent *physicien* at the College de France and possessor of a comprehensive *cabinet de physique*. Regnault immediately adopted Thomson as his laboratory assistant and occupied him daily in preparing experiments for lectures and entrusting him with complex "manipulations" for his thermodynamic researches. As the result of this training he many years later attributed to Regnault's guidance the acquisition of a "faultless technique, a love of precision in all things, and the highest virtue of the experimenter - patience." And through acquiring these skills in accurate measurement the young Thomson put Nichol's dictum that a "perfect science" was one made quantitative and exact in to the form of a laboratory practice [Smith & Wise, 1989, ch4; Thompson, 1910, 113-133 &1154].

However, on returning from France to his native Glasgow after several months of laboratory labours, William found that the other competitors for the Chair his father so ardently sought for him had for a variety of reasons withdrawn from "running". Hence after Meikleham's death in May 1846 the young Thomson's unique combination of experimental and mathematical skills - to which Thomson senior devoted much publicity - were sufficient to win William the unanimous election to the Glasgow Professorship of Natural Philosophy on September 11th 1846 [Smith & Wise, 1989,Ch.4].

2): New professorial apparatus

Prior to Thomson's election and assumption of office on October 13th 1846, his former teacher Professor Nichol "primed" the Faculty of Arts for a considerable development in the experimental resources that his former *protege* was to inherit in the Natural Philosophy Chair. On 25th August 1846 Nichol proposed to the Faculty that:

> ...in consequence of the advance of discovery in many important departments of physical science it is open to serious question whether the arrangements that were adequate in former times are now sufficient to exhibit so full an exhibition of the various branches of Natural Philosophy during the Ordinary University Course as their position and relative importance unquestionably demand..
>
> [Faculty Minutes 25/8/1846, 85, 277]

After due consideration of Nichol's appeal to replace the worm-eaten mahogany instruments that dated back to the tenure of Robert Dick [Thomson,1885,410], and that had long been "neglected" by the infirm Meikleham [Fac.Min.26/11/1847,85,351], the Faculty decided on the very day of William's election to appoint a committee to supervise the purchase of new apparatus [Fac.Min.11/9/1846,85,281]. This committee consisted of Principal McFarlan, William Ramsay (Professor of Humanity), Nichol himself and Thomson's father James and on 1st of December reported in favour of Nichol's scheme of updating: "a large portion of the apparatus is so old that it can prove of hardly any service in illustrating the physical sciences in their present advanced stage and those instruments which are of a more recent date are in such bad order that they require a thorough repair. Under these circumstances the expenditure of a considerable sum of money on the part of the Faculty appears unavoidable" [Fac.Min.1/12/1846,85,296].

Over the next five years this Committee allowed Thomson a total expenditure of £550 on such "valuable and delicate instruments" as he needed for his teaching and investigations, purchasing most of these from Pixii and Marloye in the Parisian haven of experimental philosophy [Smith &Wise, 1989, Ch.5]. This indulgence diplayed by the Faculty Committee indicated their satisfaction with his progress in the Chair of Natural Philosophy: at the beginning of his second session it was announced that "the Committee view his ardour and anxiety in the prosecution of his profession with the greatest pleasure, and...heartily concur in their anticipations of his future celebrity..." [Fac.Min. 26/11/1847],85,351].

Nonetheless, Thomson had previously encountered some discountenance from one particular conservative Professor of the University very soon after taking up his new Professorial mantle in 1846: the cantankerous Dr Fleming, occupant of the Chair of Moral Philosophy, had complained to the Faculty on the 4th December 1846 of the "public students who had been admitted against the law and practice of the University to the Natural Philosophy Classes". The committee appointed to report to the Senate on this alleged infelicity of Thomson's nevertheless evidently let the matter drop as there is no record of the complaint being investigated in the Minutes of the Senate [Fac.Min.4/12/1846, 85, 300].

However, uncharitable as Fleming was towards the young Professor Thomson, his complaints about the subject of the next section - the natural philosophy laboratory - will prove highly informative. After two years in office that were relatively unproblematic apart from this minor imbroglio with Fleming, Thomson embarked on intensive work in formulating the classical theory of thermodynamics (1848-1853), and the genesis of his natural philosophy laboratory can be traced to a series of thermal investigations in the last phase of this activity.

3): Student-assisted researches.

In making a detailed investigation of the genesis of the Glasgow natural philosophy laboratory, it is important to note a striking disagreement between all secondary sources of Thomson's career upon the actual *date* of its foundation. The table below summarizes the variety of dates that have been assigned to this initiation of the earliest British academic physics laboratory:

Author	Laboratory dating	Source
Ayrton	1849	[Ayrton,1908,262]
Sviedrys	1850	[Sviedrys,1976,410]
Thompson	1850	[Thompson,1910,297]
Gray	1851	[Gray,1908,70n1]
Bottomley	1852	[Bottomley,1872,29]
Smith and Wise	1852	[Smith and Wise,1989, Ch.5]
Coutts	1855	[Coutts,1909,385]
Tait	1855	[Tait,1875,387]

The confusion amongst these sources is vital to resolve, not merely for the mundane purpose of establishing an accurate chronology for Thomson's career as a Professorial experimentalist, but also to locate precisely the genesis of his laboratory in the context of his researches on thermoelectricity. One source of this existing confusion is the general conflation by these authors of three identifiable stages in the evolution of his student laboratory:

1) selective invitations for students to assist him in research in 1852,
2) "appropriation" of laboratory space for his general class c.1855,
3) negotiation of an official cellar laboratory in 1857.

To identify the character of these three stages and to locate the context of research in which his laboratory was created we can fruitfully cite Thomson's own well-known nostalgic account of its foundation in his 1885 address to the students of the newly opened University College, Bangor. Despite the contextual distortions of this account - distortions typical of the anecdotal biographical retrospective - it is a valuable starting point for our discussion:

> Soon after I entered my present Chair in the University of Glasgow in 1845[sic] I had occasion to undertake some investigations of certain electrodynamic qualities of matter, to answer questions which had been suggested by the results of mathematical theory, questions which could only be answered by direct experiment. The labour of observing proved too heavy, much of it could scarcely be carried on without two or more persons working together. I therefore invited students [stage 1] to aid in the work. They willingly accepted the invitation, and lent me most cheerful and able help. Soon after [stage 2], other students, hearing that some of their class fellows had got experimental work to do, came to me and volunteered to assist in the investigation. I could not give them all work in the particular investigation with which I had commenced - "The electric convection of heat" - for want of means and time and possibilities of arrangement, but I did all in my power to find work for them on allied subjects (Electrodynamic Properties of Metals, Moduluses of Elasticity of Metals, Elastic fatigue, Atmospheric Electricity etc).
>
> [Thomson,1885,410-11]

In this brief allusion to the first stage of his laboratory's genesis, we find an explicit identification of the investigation for which Thomson created his scheme of laboratory assistance in his interview with the Devonshire Commission in 1870:

> The various investigations that I have carried on have been aided very materially by the voluntary assistance of laboratory students; indeed the laboratory system which has now in some degree taken root and promises permanency in the University of Glasgow originated altogether with volunteer students, who helped me in the investigation of the results which are published in the transactions of the Royal Society under the title of "Electro-dynamic qualities of metals".
>
> [Thomson,1870b,q2707]

A paper by Thomson entitled "On the Electro-dynamic Properties of Metals" was read to the Royal Society as his Bakerian lecture on the 28 February 1856. In this paper Thomson wrote very significantly of the extent and importance of supportive work carried out by his students: "the author has to acknowledge much valuable assistance in the various experimental investigations described in this paper, from his assistant Mr McFarlane and from *Mr C.A. Smith, Mr R. Davidson, Mr F. Maclean, Mr John Murray and other pupils in his laboratory*" [Thomson,1856,189]. The involvement of the first two of these students we can trace back to about 1852 from an acknowledgement in Thomson's first experimental study of the "electric convection of heat" [Thomson,1854].

A Report of this first study can be found in an earlier paper by Thomson read at the Royal Society on May 4th 1854 entitled "Account of Researches in Thermo-electricity". In a section headed "On the thermal effects of electric currents in unequally heated conductors" Thomson explained how a thermo-electric theory which he had developed in his study of heated metals during 1851 [Thomson,1851] had led him to the conclusion that "an electric current must exercise a convective effect on heat in a homogeneous metallic conductor of which different parts are kept at different temperatures". In applying this theory to the construction of a thermo-electric junction between two *different* metals, viz. copper and iron, he found, however, that the direction of this electrical convection of heat was *reversed* when the junction was heated to a sufficiently high temperature. Thomson therefore drew upon the earlier work of Becquerel to formulate three hypotheses to explain this phenomenon by reference to the respective thermal properties of "Vitreous" and "Resinous" electricities in different metals:

> Vitreous electricity carries heat with it in an unequally heated conductor whether of copper or iron; but more in copper than in iron.
>
> Or Resinous electricity carries heat with it in an unequally heated conductor whether of copper or of iron; but more in iron than in copper.
>
> Or Vitreous electricity carries heat with it in an unequally heated conductor of copper, and Resinous electricity carries heat with it in an unequally heated conductor of iron.

He continued: "immediately after communicating this theory to the Royal Society of Edinburgh [in December 1851], I commenced trying to ascertain by experiment which of the three hypotheses is the truth, as Theory with only thermo-electric data could not decide between them" [Thomson,1854,49-52]. Here we see the identity of the questions concerning "certain electrodynamic propeties of matter" which Thomson declared in 1885 "could only be answered by direct experiment" [Thomson,1885,410].

Of the assistance which he called upon in 1852 to aid him in arriving at what he regarded as an unexpected substantiation of the third hypothesis, he related:

> with the able and persevering exertions of my assistant Mr. McFarlane, applied to the construction of various forms of apparatus and to assist me in conducting experiments, the research has been carried on, with little intermission for more than two years. Mr Robert Davidson and Mr Charles A.Smith, and other friends have also given much valuable assistance during the greater part of this time, in the various experimental investigations of which are now laid before the Royal Society.
>
> [Thomson,1854,52]

Davidson and Smith were thus students whom Thomson had originally invited as early as 1852 to act as "volunteer assistants," and in the final report he made on electrical convection to the Royal Society, in the guise of the 1856 Bakerian lecture, we can ascertain why the "labour of observing proved too heavy" for Thomson to the extent that "much of it could scarcely be carried on without two or more persons [e.g. Davidson and Smith] working together" [Thomson,1885,410]. Dating the first series of experiments on thermoelectric behaviour to the academic session 1852-1853, Thomson wrote in 1856 that "many experiments, both on the iron and the copper conductors, were made from October 1852 to March 1853 and the results of the observations [were taken] on each of the two principal thermometers, either every half minute or every quarter minute, during an experiment of two hours" [Thomson,1856,204]. Both of these thermometer readings were taken to an accuracy of four-significant figures (three decimal places) at this high frequency and for this long period of observation [Thomson,1856,261 *et seq.*]. It was this attempt to match the fastidious precision of his thermodynamic experiments which he and Joule had made during the preceding five years that impelled Thomson to invite Smith and Davidson to give their assistance.

The observations recorded by Thomson during October 1852 to March 1853 and then also from October 1853 to April 1854 coincide closely enough with the period of the University sessions for us to deduce that Thomson only carried out these exhaustive sequences of experiments when he had ready recourse to undergraduate assistance - such was his dependence upon the availability of Davidson and Smith. Nevertheless, we can note that Smith also made observations for Thomson during the May, August and September of 1854 [Thomson,1856,261-262], and from this we can surmise that Smith was the first of the special assistants that Thomson paid to carry out investigations during the summer months by drawing upon the annual research funds he was granted by the Royal Society - see [Smith & Wise,1989, Ch.5; Thomson to Stokes 7/2/1860, ULC K111]. Similarly we know that another student acknowledged by Thomson in the Bakerian lecture, John

Murray[1], was similarly employed in the summer of 1856[2] and was thus probably a student in Thomson's class during the session 1855-56.

Having thus established that the earliest student-assisted investigations that match Thomson's biographical account of 1885 took place in the autumn of 1852, it is now important to analyse the development of these student-assisted researches into a large scale system of laboratory work accessible to all Thomson's natural philosophy undergraduates - a transformation unambiguously alluded to in his 1885 retrospective. This establishment of such a general student laboratory relates closely to the specific *location* of the laboratory since a large specifically-allocated domain of experimental activity was an institutional prerequisite for a whole "volunteer corps" of students to undertake Thomson's practical research work. In our discusson below of the institutional politics of the space occupied by Thomson's laboratory, the second and third stages of the laboratory's genesis will be established.

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[1] Of the other student acknowledged in the lecture footnote, viz Mr F.McClean, we learn nothing of his role or chronology from the content of the lecture although Thomson *does* acknowledge assistance from two other students: G.Chapman and J. Cranston for some concluding work on his thermoelectric experiments during January and February 1856 - immediately before Thomson gave the Bakerian lecture at the Royal Society on the 26th February.....[Thomson,1856,261].

[2] This infomation is contained in a note added to the Lecture for the purpose of finalizing the results for inclusion in his Mathematical and Physical Papers [Thomson,1856,261]

3): <u>The student laboratory</u>

In his 1885 account of his laboratory's formation Thomson conflated, either through nostalgic oversimplifiction or by political diplomacy, the second and third stages of his laboratory's genesis, viz: the creation of his volunteer corps of student researchers and their accommodation in the famous cellar laboratory. As we shall see below there was a gap of two years, an intermediate laboratory and much institutional controversy between these two stages. Thomson's simplified account reads:

> ....To meet my requirements for my new volunteer laboratory corps, the "Faculty" (then the governing body of the College) allotted to me an old wine-cellar, part of an old professor's house, the rest of which had been converted into lecture rooms. This, with the bins swept away, and a water supply and a sink added, served as a physical laboratory (a name then unknown) for several years till the University Commissioners came and abolished a certain old function of Glasgow University, the "Blackstone Examination." The examination room was left unprotected, its talisman, the old "Blackstone Chair" [having been] removed. I instantly annexed it (it was very convenient, adjoining the old wine-cellar and below the apparatus room); and as soon as could conveniently be done, obtained the sanction of the Faculty for the annexation. The Blackstone room and the old wine-cellar served well for [a] physical laboratory till 1870 when the University was removed from its old site embedded in the densest part of the city, to the airy hill-top on which it now stands.
>
> [Thomson,1885,411]

Andrew Gray, the former student and laboratory assistant by whom this oration was commissioned, comments in his biography of Thomson that one sympathetic colleague at Glasow considered Thomson to have had "a great faculty for annexation" [Gray,1908,297]. This esteemed skill in institutional colonization was first exercised by Thomson c.1855 in his annexation of the room next door to his natural philosophy class to convert it into a student physical laboratory. From the circumstantial evidence presented below it would appear that this laboratory on the first floor of the Old College's inner quadrangle was the one created to accommodate the rest of the students in Thomson's class who wished to join their specially selected colleagues in assisting the Professorial investigations [Thomson,1885,410-11]. Through a major historiographical irony we only know of this pre-cellar laboratory, one which thus first accommodated the full "volunteer corps", through another complaint made by Professor Fleming about the conduct of Thomson's classes.

The elderly Professor of Moral Philosophy was manifestly not sympathetic to Thomson's proclivity for annexation, and objected vigorously to the latter's colonization of the room immediately above his Moral Philosophy class-room. Fleming thus put a motion to the Faculty on the 1st of May 1857 that: "the room adjoining the Natural Philosophy Class be no longer occupied as it has been during the last two years" and reiterated his demand that the unpaid account incurred by Thomson in fitting up the room should "be not paid until the inconvenience is rectified"[3].

A committee was thus appointed to resolve Fleming's discountenance at the disturbance he experienced from Thomson's bustling laboratory upstairs and this committee was invested with the official prerogative of considering "the best means of forming a laboratory on the ground floor in connection with the Natural Philosophy classroom in place of one on the upper floor now in use" [Fac.Min.1/5/1857 & 5/8/1857,87, 359-60 & 366-68]. This committee reported back on the 5th of August to recommend that:

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[3] The respective locations of Fleming's and Thomson's classrooms from which this interpretation is drawn are explained and illustrated in [Murray, 1927, 126,131 & 134-35].

...on obtaining the concurrence of the Professor of Natural Philosophy, who was unavoidably absent from this meeting, that the unoccupied cellar on the ground floor situated beneath the natural philosophy class-room should during the remaining months of the [summer] recess or as soon as thereafter possible be rendered available for a laboratory or experimental room to be allotted to the Professor of Natural Philosophy.

This "rendering available" was considered to involve the construction of a trap-door between the class-room and the projected laboratory below it[4], building a separate entrance from the South Court, removing some internal walls to be replaced by supporting pillars and the asphalting of the floor etc. The Faculty assented to this proposal and granted £40 to the purpose [Fac.Min. 5/8/1857, 87, 366-68]. After these recommendations had been carried out, the committee reported to the Faculty on the 6th of November that "...the apartments on the floor immediately beneath the Natural Philosophy Classroom have been thrown into one and otherwise repaired so as to render them suitable as a laboratory and experimental room." And despite Fleming's objections to the issue of the unpaid bills on Thomson's original and illegitimate laboratory, £43 was granted by the Faculty towards the fitting up of the cellar as a working laboratory [Min.Fac. 6/11/1857, 88, 3].

Presumably this laboratory was in operation by the end of 1857 or by the beginning of 1858 and Murray gives us a clear account of the constitution of Thomson's "cellar" laboratory. Whilst the Natural Philosophy class-room occupied the first and second floors of one section of Glasgow College's inner quadrangle, the laboratory was on the ground floor under the west end of this class-room and was initially an

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[4] The plan for a trap-door was passed over in favour of using the pre-existent turret store stairs [Min.Fac.6/11/1857,88,3].

amalgamation of three rooms:

1) a room immediately underneath Thomson's "retiring room"; and two rooms directly beneath the classroom which had originally been a wine cellar when it had been part of a professorial house.

2) the "James Watt" room, the eponymous mechanic having carried out experimental work there in the previous century,

3) a adjacent room containing a large galvanic battery.

[Murray,1927,132-133]

In 1862 the Blackstone room was also added to this suite of rooms when the ancient Blackstone examination was abolished upon the recommendation of the Scottish University Commission which made its official report in that year. Against S.P. Thompson's implausible dating of 1868 for this annexation, we can cite letters from William to Helmholtz firstly from November 23rd 1862 in which he wrote "the next time you come to Glasgow... you will find a great improvement in my working place. From the beginning of the session (about a month ago) I have had a really convenient and sufficient laboratory for students"; in another letter of 16th March 1863 Thomson enthused "I have got a great improvement in my laboratory recently, which gives me, what I never had before, [i.e.] space for allowing the students to work in a systematic manner" [Thomson to Helmholtz 23/10/1862 & 16/3/1863 in Thompson,1910,425 &428; c.f. Thompson, 1910,297].

We have now located the three stages in the creation of Thomson's laboratory in the inter-related contexts of his thermo-electrical research and the territorial politics of his University. We can now consider the Professor's operation of this student research laboratory within the democratic traditions of the nineteenth century Scottish Universities.

4): Thomson's laboratory in the Scottish "democratic" context.

> ....a students' laboratory [is a place] where they can meet together for the practical study of the various departments of science, where they will be brought together to use their eyes and hands - their eyes otherwise than in reading books and looking at pictures or drawings; their eyes to observe accurately, and their hands to experiment, in order to learn more than can be learned by mere observation. To teach students to so work and so learn is the object of a scientific students' laboratory.
>
> [Thomson,1885,410]]

Considering Thomson's highly innovative step of introducing a laboratory in which to practise experimental natural philosophy in 1855, it must have appeared more radical still to his contemporaries for a Professor to invite undergraduates to assist him in laboratory investigations. It was more radical still that the students he invited to work with him on researches intended for publication in elite journals such as the Transactions of the Royal Society were not only the elite of his students but from 1855 the general membership of his natural philosophy class. By contrast, Thomson's close correspondent and fellow experimenter in Cambridge, Professor G.G. Stokes allowed no students to work in his laboratory: even when Lord Rayleigh as aristocratic Senior Wrangler and First Smith's Prizeman offered in 1864 to assist him in his researches, Stokes would not even countenance his help in "getting out and putting away the apparatus" [Strutt,1924,38]. Although Stokes represents an extreme case of such conservatism in Professorial experimentation, we will see that laboratory physics teachers in the 1860's and 1870's were far more cautious than Thomson in allowing students to handle their research apparatus, especially Stokes' former student R.B. Clifton [Ch.6].

Considered as an institutional expression of his heritage of family, civic and University traditions [Smith & Wise,1989,Ch.2; Davie,1964] the democratic constitution of Thomson's laboratory audiences persisted throughout the remaining years of his tenure. The non-elitist character of his laboratory audience in 1870, for example, is clear from the following extracts from his interview with the (highly sympathetic) Devonshire Commission:

> 2698 *(Dr.Sharpey.)* [54] ...might I ask whether in your laboratory you have a class of working students? - Yes.
>
> 2699 Who can enter the laboratory as they like? - Yes a class that originated gradually by natural selection...
>
> ...2702 You speak of natural selection, but I suppose you also aid in the selection of these gentlemen? - Those who do the work regularly are encouraged, and those who do not do any good, or who are irregular or show no intelligence or spirit, fall off [in attendance].
>
> 2703 Then the physical laboratory is open to such men as desire to receive practical instruction and to carry on research? - Yes, no applicant is ever refused a trial.
>
> ...2760 So anyone who presents himself would be allowed to try his hand? - Yes.
>
> [Thomson,1870b, q2698-2670]

Further in his evidence, Thomson explained that up to thirty students, i.e. between a quarter and a third of those in his lecture class, applied to work in the laboratory every year although only 15-20 ended up working consistently throughout the session [Thomson,1870, q2763-64]. Indeed, the laboratory was so popular amongst his students that its original function had in effect been inverted so that "instead of [students'] positions being that of volunteer assistants for investigations which I wished to make, I have had [such] a great many

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[5] William Sharpey, originally trained as a medical practitioner, was jointly Secretary of the Royal Society with G.G. Stokes.

applicants for experimental work, [that] I have had to make work for them" [Thomson,1870b, q2708].

Thomson appears to have developed his habit of providing "customized" experimental problems for his students into a more definite scheme of practical investigation around 1860, this despite his heavy load of lecture preparation, long-term researches and private enterpreneurial work. As he wrote to Stokes of the multifarious activities in his laboratory during that year:

> The general experimental work of my laboratory includes not only attempts to investigate new truth...it involves as the primary and essential work the preparation of illustrations for my lectures during the winter six months. I have besides instituted a system of experimental exercise for laboratory pupils in which I am induced to persevere, devoting a great deal of time to it...
>
> [Thomson to Stokes 7/2/1860, ULC K111]

A brief indication of the extent to which Thomson sytematized this student laboratory work, especially after his annexation of the Blackstone room in 1862, is given in the University Calendar. Although the earliest extant edition of this dates back only as far as 1863, Thomson's descriptive note in the Faculty of Arts syllabus for the Natural Philosophy Class of that year was maintained in the same form until 1869: "the laboratory in connection with the Class is open daily from 9am to 4 pm for Experimental Exercises and Investigations, under the direction of the Professor and his Official Assistant" [University of Glasgow Calendar,1863-64,31].

Nevertheless, after Thomson had created appropriate research tasks for his students to pursue, in the form of "Experimental Exercises and Investigations" as an optional adjunct to their Class work, he delegated the task of supervising this system to his assistant Donald McFarlane. As is clear, however, from an account by David Murray of the work he undertook as a laboratory student between 1862 and 1863, the Professor's democratic instincts still prevailed whenever he found time to join his students:

> The Professor told McFarlane what was wanted and how he wished the investigation to be carried out and left the rest to him. McFarlane distributed the work amongst the students according to their ability, but little instruction was given as to the method or the manipulation. He however could always be consulted; he was most ready to give assistance, and his directions were clear and easy to follow. The professor often came in to see what we were doing and what progress we were making. He would sometimes assist, taking part as if he was one of ourselves, often explaining what he had in view and discussing the problems involved. He had none of the air or manner of a superior, but treated us as if we had been Faradays or Joules.
>
> [Murray,1927,135-136]

It is important to emphasize here that the manner in which Thomson thus joined in his students' laboratory researches, effectively on equal terms with them as "Faradays" and "Joules", was very different to the more strictly disciplined and authoritarian approach of Foster, Adams, Clifton *et al* as we shall see in later chapters. Indeed his egalitarian determination led him further still from the working practices adopted by these English professors in making his laboratory accessible to all students[6] through subsidizing their practical work to the extent that he

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[6] The average undergraduate Scot was a great deal poorer than his English counterpart as this extract for the Report of the 1858 Scottish Universities Commission illustrates: "It is undoubted that a very large number of the Students in the Scotch Universities are in exceedingly poor circumstances. Many of them engage during the summer in teaching and other employments in order to gain the means of supporting themselves at the University during the winter; and the Professors receive in the last few weeks of the Winter Session, frequent application from Students to dispense with their longer attendance, on account of their scanty funds being already exhausted" - cited in [Seventh Report of the Devonshire Commission,1874, II 5]

did not demand a fee for the materials and apparatus which they used - at least not until 1878 [Glasgow University Calendar,1878-79,45]. Although by the later 1860's Thomson had a considerable private income from his entrepreneurial activities in the telegraph industry etc. he still found that his student's non fee-paying laboratory work involved "a larger expenditure out of my private resources than I feel altogether consistent with other claims[7]"; from this we can see one reason why students such as W.E. Ayrton who attended Thomson's class in 1867-1868 felt that "*entree*" to his laboratory "was a great privilege" [Ayrton,1908,263;Ayrton,D.N.B.].

Yet again, though, from this testimony of Ayrton's in 1908, we can reiterate the extent of the essential differences between Thomson's laboratory and those which succeeded it:

> There was no special apparatus for students' use in the laboratory, no contrivances as would to-day be found in any polytechnic, no laboratory course, no special hours for the students to attend, no assistants to supervise or explain [sic], no marks given for laboratory work, no workshop and even no fee to be paid...[for instance i]f for some test a student wanted a resistance coil, or a Wheatstone's bridge, he had to find some wire, wind the coil, and adjust it for himself.
>
> [Ayrton,1908,262-263]

In 1869, however, two years after Ayrton's attendance and upon the eve of the University's removal to Gilmorehill, Thomson invested his laboratory schedule with a structure both more comprehensive and more explicitly related to the content of his concurrent lecture course. The "main divisions" of the Natural Philosophy course read as follows:

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[7] These other claims generally consisted of the large scale and long term researches which he discussed with a view to Government sponsorship in [Thomson to Stokes 7/2/1860, ULC K111] and in both his interviews with the Devonshire Commission [Thomson,1870, q2678-2772; 1872, q10678-10740].

1. Abstract Dynamics (including elements of Physical Astronomy).
2. Properties of Matter.
3. Thermodynamics.
4. Illustration.

Illustration is conducted partly through examples and calculation: partly by experiment. The course of detailed Experimental Illustration and Demonstration is extended over two years thus:

1869-70.	1870-71.
1. Capillary Attraction.	6. Magnetism and Electromagnetism.
2. Sound.	7. Thermo-electricity.
3. Heat.	8. Elasticity.
4. Electrostatics.	9. Hydraulics.
5. Galvanism and Electrolysis.	10.Light and Radiant Heat.

[UG Calendar,1869-70,34]

The most obvious inference to be drawn from the structure of this experimental course is that the perceived necessity for extending it over a period of two years reflects the enormous disciplinary expansion of physics discussed in Ch.1 - an expansion that was partly a result, at least for categories 3-8, of Thomson's own experimental researches during the preceding twenty-two years [Smith and Wise,1989, Chs. 9, 10, 19 & 20]. However, the status of the practical work delineated above does not appear to have been concomitantly augmented to render it a *compulsory* element of the University of Glasgow M.A.: although we know from Thomson's evidence to the Devonshire Commission that he considered no scientific teaching could be "thorough" without laboratory work [Thomson,1870b, q2828-2832], his description of the laboratory audience as evolving by "natural selection" [Thomson,1870b, q2699&2702] leaves us in no doubt that they were not formally obliged to undertake investigative work in the laboratory.

From the testimony of Sir William Ramsay, who attended Thomson's class during 1870 we also know that, even after the restructuring of the laboratory work described above, his experimental "syllabus" did not resemble those of his English counterparts as being an examinable course of systematic elementary training in quantitative laboratory techniques. We find instead that Thomson integrated original research into the teaching work of his laboratory in a manner subsequently followed by his compatriots Tait and Stewart [redacted] expressing a uniquely Scottish form of natural philosophy pedagogy that maintained no strict demarcation between teaching and research. The English position on the subject was generally more allied to Whewell's hyper-conventionalist view that "it is not desirable ...to require or suppose in our students a knowledge of *Original Investigations*" before a thorough inculcation in the received wisdom of standard textbooks [Whewell,1845,69]. By contrast with Whewell's strictures Ramsay related of Thomson's laboratory teaching through measurement research:

> In the laboratory Sir William was a most stimulating teacher, though his methods were not those which have since been introduced into physics laboratories. I remember my first exercise, which occupied over a week, was to take kinks out of a bundle of copper wire. Having achieved this with some success I was placed opposite a quadrant electrometer and made to study its construction and use. I was made to determine [i.e. *measure*] the potential difference between all kinds of materials, charged and uncharged and among others between the external and internal coatings of a child's balloon, black leaded internally and externally and filled with hydrogen...
>
> ...In short we had little systematic teaching, but were at once launched into knowledge that there is an unknown region where much is to be discovered, and we were made to feel that we might help to fathom its depths.
>
> [Ramsay,1908,94-95]

Although Thomson's ideology of investigation as education was thus at something of a methodological remove from the English ethos of physics teaching to be discussed in chapters 4,5,6 and 8, the laboratory curriculae of Foster, Clifton *et al.* were closely allied with his experimental teaching practices of precision measurement which form the subject of the next section.

5): <u>the Old College laboratory</u>

In his 1885 Bangor address on the work of the laboratory, Thomson articulated his own idiomatic version of what was by then a widespread creed of laboratory precision measurement:

> A large part of the work of a physical or chemical laboratory must be measurement. That might seem rather trying work; "harsh and crabbed" shall we say? Who cares to measure the length of a line in land surveying, or of a piece of cord, or of ribbon, or of cloth? These may not be in themselves essentially interesting occupations; but ...what do you think of a measurement of something you can only gauge by inference from the performance of apparatus tested some peculiarly subtle way?...if it becomes necessary to measure something smaller than can be seen with the eye, the measurement itself becomes an object to inspire the worker with the greatest ardour. Dullness does not exist in science....The difficulties to be overcome in physical science in mere measurement are simply teeming with interest[8].
>
> [Thomson,1885,411]

In documenting Professor Thomson's commitment to the laboratory practices of precise laboratory measurement, Wise and Smith have located the origins of this commitment in a confluence of two interactive contextual elements: the business ethos of precisely optimized economic efficiency characteristic of his native Glaswegian environment, and his own formative work in Regnault's laboratory of 1845 in which he carried out high-accuracy measurements of pressure to effect the conditions of

[8] Taken as it is from a somewhat discursive and digressionary public speech, this quotation has been slightly re-ordered to clarify the cogency of Thomson's arguments upon the *rationale* of laboratory measurement.

maximum work output from a steam-engine [Wise & Smith,1986, 155 & 163-64]. In chapter 1 we saw how Thomson applied the precision measurement practices thus nurtured in the dual Glaswegian-Parisian context, to developing a dynamical theory of heat [1848-1854], to optimizing the transmission characteristics of submarine telegraph cables [1855-1874], and to the B.A.A.S. scheme for standardized electrical measuring instruments [1861-1884]. The result of Thomson's collaborative measurement "programme" with such men as James Joule, Balfour Stewart, Fleeming Jenkin, Cromwell Varley, Charles Wheatstone, James Clerk Maxwell *et al.* was to establish a vastly expanded corpus of physical theory and thus a widespread committment amongst physicists to the view that laboratory precision measurement was *the* vehicle of experimental research through which they could effect the progress of their subject.

Smith and Wise have illustrated the way in which Thomson articulated this efficacy of high accuracy researches to his undergraduate students through the medium of his Professorial lectures. On the 17th January 1850, for example, Thomson revealed to his assembled class the results of his experimental endeavours with "a very delicate thermometer...assuredly the most delicate that was ever made" to verify his brother's quantitative prediction of the high pressure lowering of the freezing point of water: the agreement of one experiment was to within 1/3000 of a fahrenheit [Smith & Wise, 1989, Ch.9]. In this section we will illustrate how Thomson extended this indoctrination[9] of his students in the efficacy of contemporaneous practices of precision measurement from the domain of the

[9] With regard to his deep sea cable-laying equipment Thomson wrote to Professor Andrews in Belfast in May 1873: "I have now to get sounding apparatus, and one of my laboratory students *indoctrinated* in the use of it..." [Thomson to Andrews, 25/3/1873 in Thompson,1910,635].

Professorial lecture theatre into the context of the student research laboratory.

We saw earlier how two years after this 1850 lecture upon thermometry, Thomson undertook long-term precision studies involving his students upon the electric convection of heat, thermo-electric inversions, the effects of mechanical strain and of magnetisation on the thermo-electric qualities of metals and the effects of tension and magnetisation on the electric conductivity of metals between 1852 and 1856. The laboratory was thus founded in the context of his students embarking upon long-term researches in precision measurement, and in his 1885 Bangor oration at the opening a new physics laboratory, under the charge of his former student and laboratory assistant Andrew Gray, Thomson laid claim to the historical character of his native Glaswegian laboratory as a major international centre of measurement operations. In the passage below Thomson makes clear the extent to which investigative and comparative measurements including the routine determinations assigned to the novice experimenter, had constituted the generic function of his student laboratory ever since its foundation:

> The physical laboratory at Glasgow has, I believe, been, more than most others devoted to whatever work occurred in physical investigation, measuring properties of matter, comparing thermometers, electrometers, galvanometers, and doing other practically useful work. We at once put the students into investigations, and let them measure and weigh whatever requires measurement and weighing in the course of the investigation.
>
> [Thomson,1885,411-412]

For example the work he undertook on the mathematical theory of elasticity in 1855-56, concurrently with his student-assisted work on the electrodynamic properties of metals, formed the basis of an on-going programme of student measurement which ten years later in 1865 gave rise to more explicitly exact researches by Thomson himself:

> Among the experimental exercises performed by students in the physical laboratory of the University of Glasgow, observations on the elasticity of metals have been continued during many years. Numerous questions of great interest, requiring more thorough and accurate investigation, have been suggested by these observations; and recently they have brought to light some very unexpected properties of metallic wires.
>
> [Thomson,1865,289]

In this paper Thomson reveals another case of his characteristic facility for institutional annexation, this time explicitly for the purposes of detailed measurements upon elasticity: "several accurate determinations of Young's modulus have been made upon wires of different substances hung in the College Tower of the University of Glasgow....which by giving 80 feet of clear protected vertical space, afford great facilities for the investigation[10]" [Thomson,1865,293]. Thus Thomson had no compunction in carrying his ethos and practice of laboratory measurements into even the most hallowed domains of the University beyond the immediate confines of his formal laboratory setting.

Whilst the College tower was the venue for the culmination of these fine measurements on elasticity, Thomson's famous cellar laboratory is recorded by W.E. Ayrton to have been the "birth-place of the siphon recorder". In this latter episode we see the converse process of contextual interaction between Thomson's laboratory and its immediate

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[10] To give an idea of the level of precision which Thomson sought and obtained from these measurements in the College Tower, he obtained respective values of Young's Modulus for two differently treated copper wires of 1159 gcm^{-2} and 1153 gcm^{-2} [Thomson,1865,296].

environment: the introduction of principles of business economy and efficiency, not to mention those of patents and capital interest, into student measurement work connected with the development of telegraph technology. This portrayal of economic principles in the practices of laboratory measurement is discernibly analogous with the analysis of Thomson's use of the economic steam-engine as a metaphor for measurable energetic systems in [Wise & Smith,1986,152-155].

Professor Thomson characteristically shared in the midwifery of the siphon recorder with Ayrton and others whom Thomson commissioned to investigate an electric motor which could provide the requisite steady flow of ink to this machine. As Ayrton related:

> To find out what sort of electrical machine should be used for this purpose Thomson suggested that we should measure the efficiency of frictional electric machines. We did so , and brought him the result - viz, efficiency equals some small fraction of unity. He replied "I cannot degrade a man by asking him to use his energy so wastefully; I must design something better." And he did, the influence machine; then when, by carrying out his suggestions, a fellow student and I had constructed an influence machine and got it to work, he sent us to the Glasgow patent office to see whether anyone had thought of this principle before. And we found Varley's and other anticipations [but quite uniquely] Thomson's was a machine that worked on the compound interest law, starting with an infinitely small initial capital. This led not only to the "mouse mill" and the "replenisher", but to the class working on all kinds of problems on investments at compound interest.
>
> [Ayrton,1908,262]

Thomson's laboratory capitalism as exhibited in Ayrton's reminiscences of the telegraphic siphon recorder, was first manifested in 1856-7 when the Professor measured the typical resistances of the commercial copper used in telegraph cables. His results showed that there was an economically unrepresentative discrepancy in conductive power between cables manufactured by different companies, and after convincing the Atlantic Cable Company of the commercial importance of employing accurately checked high conductivity copper in transatlantic telegraphy he was commissioned to join the cable laying expedition of 1858 [Thomson,1857,550-555, Thompson, 1910,348-357]. S.P. Thompson informs us that through the first six months of 1857 Thomson had employed his laboratory corps upon his investigations of copper conductivity, and Murray notes that one of Thomson's earliest laboratory students J.B. Russell accompanied him on the cable-laying "Agamemnon" in 1858 and assisted in the laying of the cable [Thompson,1910,340; Murray,1927,132].

Indeed, Thomson's laboratory students seem to have been participants in much of Thomson's subsequent electrical work arising from his dealings with the telegraph industry. The B.A.A.S. Committee on Electrical Standards was one such venture initiated by Thomson [Ch.1] and Thomson's main role was the construction of high accuracy electrometers for measuring potential differences in connection with the first set of B.A.A.S. standards of resistance. In his letter to Helmholtz declaring his annexation of the "Blackstone room" he wrote of the engagement of his extended class upon B.A.A.S. work: "Out of about 90 who attend my lectures, about 30 have applied for admission to the laboratory, and of these 20 or 25 will work fairly. I hope I may have half a dozen who will do good work. Some of them are at present at work on electrometers which you would not recognize" [Thomson to Helmholtz 23/11/1862, in Thompson, 1910,425]. Of the radically smaller and more sensitive instruments which Thomson and these students produced over the following five years, the B.A.A.S. committee reported in 1867 that a whole range of electrometers capable of measuring absolute potential differences between 1/400 and

10,000 Daniell Cells had been developed at the Glasgow laboratory [Ch.1]; from this we see the significance of his 1885 comments that his students had always been "devoted to whatever work occurred in physical investigation....comparing...electrometers, galvanometers, and doing other practically useful work" [Thomson,1885,411-412].

This 1867 B.A.A.S. report also alluded to Thomson's use of his new electrometers in ascertaining the value of *"v"*, the ratio of the electrostatic to the electromagnetic constant, which significantly for Maxwell possessed the dimensions of a physical velocity. In 1864 Maxwell had drawn upon the close numerical agreement between this figure and the most recent observed speed of light to argue that light and electromagnetic waves governed by these parameters were one and the same, but Thomson was extremely sceptical of this claim and according to S.P. Thompson "desired newer and more exact measurements" to convince him of this identity. Having established the reliability of his new absolute electrometers, Thomson naturally set two of his pupils W.F. King and J.D. Hamilton to redetermine the value of *"v"* using the B.A.A.S. commissioned equipment in the session 1867-68. As S.P. Thompson reported of the Professor's fastidious demands of his students' preparation for this experiment:

> The whole work of that winter was preparatory, and consisted in setting up of the electrodynamometer, and accurate measurements of the various parts of it required for the calculation of the necessary constants. It might seem a long time to expend on mere preliminary work, but Thomson insisted upon the utmost accuracy; and it must be remembered that keys, batteries and resistance boxes were commercially non-existent in those days, and it was part of the laboratory training of the students to make these instruments and to adjust them.
>
> [Thompson,1910,525-26]

Dickson and King obtained the value of $284.6x10^8$ cms^{-1} and their results were delivered at the B.A.A.S. meeting in 1869 and incorporated by

Fleeming Jenkin into the Association's Reports of Electrical Standards in the same year [Jenkin,1869]. However, evidently not satisfied with this result either, Thomson continued the work with a further improved electrometer over the next three years; from his work made "at intervals from 1870 to 1872" in the Glasgow laboratory Dugald McKichan presented a final value for "*v*" of 293.0×10^8 cms^{-1} to the Royal Society in May 1873 [McKichan,1873; Nature,8,134-35].

6): the 1870 Gilmorehill laboratory

Between fastidious measurements with Thomson's absolute electrometers and electrodynamometers by King and Dickson in 1867-68 and McKichan's between 1870 and 1872, there occurred an important transition in the circumstances of the Glasgow laboratory. Almost a decade after the Scottish University Commission had recommended the removal of the College away from its "densely peopled and polluted" environment in the centre of the city in 1863, the University of Glasgow was transferred to the "airy hilltop" of Gilmorehill and opened on November the 7th 1870 [Coutts,1909,442-46]. As S.P.Thompson wrote of the new accommodation for natural philosophy: "In the new [lecture] theatre were installed many of the time-honoured appliances used in the old College; and to the well-lighted new rooms provided for laboratories were brought the treasured instruments and paraphernalia which had been accumulating in the dark cellar and the deserted Blackstone room of the earlier time" [Thompson, 1910,569].

In this move to what was then a quiet wooded suburb of Glasgow, Thomson was able to secure the experimental environment which would enable him to take his pursuit of delicate physical measurements to even greater levels of accuracy in the new purpose-built physical laboratory. The old college laboratory, contrived as it was from a wine cellar and miscellaneous rooms adjacent to it, had provided Thomson with an environment not unamenable to the accuracy of measurement that he sought; although the old college was sited in the busiest part of Glasgow Murray explained that "the laboratory was at a considerable distance from the public street and there was no question of vibration or of electric disturbance in those days" [Murray,1927,134]. In 1863, however, when plans were evidently afoot (after the recommendations of the Scottish University Commission) to reconstruct the University elsewhere, Thomson began to sketch his ideal laboratory and lecture theatre for the new site.

In a personal note dated September 1863 we find the following specifications from Thomson:

> <u>Memorandum - Natural Philosophy Lecture Room, Apparatus Room and Laboratory</u>
>
> The laboratory to be on ground floor, and to admit of tables being supported on posts driven into the earth, independently of the flooring.
>
> The lecture-room and apparatus-room to be on the same floor with another but to be above the laboratory. Easy access required from lower laboratory to lecture room and apparatus room...
>
> [Memorandum ULG,T171,1]

From the second point we see that Thomson was attempting to recreate the congenial arrangement of the three rooms which he had enjoyed since his territorial battle in 1857 with Professor Fleming. The first point shows that Thomson was clearly intent upon achieving the maximum stability of the students' working tables by arranging for them to be structurally independent of vibrations from the laboratory floor - a prototypical version of the architectonic device for maximizing the accuracy of laboratory measurement discussed in Ch

In further elaborating this plan in a second memorandum of October 1863 we see how he aimed, by a similar device, to achieve a comparable accuracy in his lecture demonstrations - to match the precision with which these had been specially prepared in the laboratory: "a massive stone pier, independent of the flooring, (as in an astronomical observatory) to come up from the foundation below the laboratory, through the laboratory ceiling into the lecture room, in the centre of the professor's platform table" [ULG,T171,2].[11] The model that Thomson was drawing upon here is clearly the delicate telescope mounting incorporated at the University's Horslehill observatory by his early Glasgow mentor, the Professor of Astronomy John Pringle Nichol [Coutts,1909,388-91][12]. These features envisaged by Thomson in the mid-1860's were indeed incorporated into the new Gilmorehill laboratory, as we shall see below.

J.T. Bottomley[13] gave a detailed description of the new suite of Natural Philosophy rooms at Gilmorehill in 1872; these were now situated less incongruously than hitherto in the vicinity of the allied departments of mathematics, engineering and astronomy, and also occupied a greatly

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[11]As regards the lecture theatre Thomson hinted at his envy of the facilities possessed by his colleague P.G.Tait in specifying that it should be a copy of "The Natural Philosophy Room of the University of Edinburgh" which with certain improvements "would be very satisfactory" [ULG, T171,2]. For a plan of the lecture theatre that was envisaged by Thomson in about 1865, showing its connection to the laboratory below see [ULG, T171, 3].

[12] For Thomson's own views of the significance of Nichol's Astronomical Observatory see [Nature,68(1903),623-624].

[13]James Thomson Bottomley was the nephew of Professor Thomson and also his laboratory demonstrator; in 1885 he became the Deputy Professor of Natural Philosophy [Thompson, 1910, 630 & 650].

increased academic territory - reflecting the heightened institutional status indirectly conferred upon the discipline by the knighthood of its Professor in 1867. Apart from the as yet uncompleted tower[14], Thomson's private sitting room and a number of store-rooms, this suite included the lecture theatre and the laboratory immediately below it on the ground floor [as per the 1863 plan], an additional lecture and experiment room, the principal apparatus room and museum, and two more rooms for additional apparatus storage and occasional experimentation [Bottomley,1872,29].

Bottomley explicitly described the way in which the ground floor laboratory met with Thomson's prerequisite conditions of stability to the end of achieving accurate measurements:

> Three quarters of the floor is wood, the remainder concrete, covered with Portland cement; but in order to get perfectly steady tables, piers of masonry, built on the foundation, rise through the floor, and on them the feet of the tables rest. The flooring does not touch the piers at all, and thus however much the floor may shake, the table remains comparatively steady. This arrangement gives far greater steadiness than a complete stone floor. Besides these piers there are two somewhat larger stone constructions, which are unconnected with the flooring; one of these is intended for a large steady table; and on the other there is a massive stone erection on which is to hang a pendulum for a clock, or for experiments on the forces of gravity[15]. It is intended that the point of suspension of the pendulum shall be perfectly free from vibration.
>
> [Bottomley,1872,29]

With regard to the lecture theatre Bottomley described an arrangement closely resembling that specified by Thomson in 1863 to ensure the non-disturbance of delicate measurement experiments utilized in lecture

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[14] In this tower Bottomley followed up Thomson's 1865 work on the elasticity of metals during 1879 and 1886 [Gray,1897,490].

[15] Andrew Gray notes that this "stone erection" was used by George and Horace Darwin in their first attempt to determine directly the attraction of the moon on a body at the earth's surface [Gray,1897,487].

demonstrations: "for instruments that require a very steady support there are two pillars, one at each end of the lecture room table. These are unconnected with the flooring. They pass through it without touching the [floor] boards, and rest on stone arches that cover a gateway beneath". In addition, to complement the architectural link with the University Observatory suggested above, it is interesting to note that one of the two clocks on the walls of the lecture theatre was connected by *telegraph wires* to the high-precision clock in the Observatory, electrical signals from which were intended to maintain the accuracy of the former [Bottomley,1872,30].

Bottomley continues his account of the newly opened laboratory with a lengthy discussion of the type of batteries employed to provide a reliable and accurately quantified source of current for the electrical measurement activities so predominant amongst the student's exercises. To see both the degree of importance which Thomson attached to a reliable source of electric current, and also the intense dedication with which his students communally acted upon his requests for extensive measurement work, we can cite S.P.Thompson and Andrew Gray on the Professor's first encounter with the Faure accumulator - a form of secondary lead cell unfamiliar to Thomson before May 16th 1881 [Thompson,1910,765].

Thompson reported that "Sir William...was immensely seized with this new invention and fell upon it with more than his [usual] wonted energy" and although initially obliged to be absent on telegraphic business the work of the "volunteer corps" was unheeded. Thomson wrote to Dr J.H. Gladstone, the former President of the Physical Society of London, on the 17th May to say that " "the box of electricity" is being kept under [his students'] continued tests and measurements by James Bottomley in my absence. It is splendidly powerful but I have yet to find whether it does the whole amount of work specified by [Faure and others] and how much actual work must be spent on it each time to renew the charge" [Thompson,191,765-76].

As Gray described this episode:

> A supply of sheet lead, minium and woollen cloth was at once obtained, and the whole laboratory corps of students and staff was set to work to manufacture secondary batteries. A small Siemens-Halske dynamo was telegraphed for to charge the cells, and the ventilating steam-engine of the University was requisitioned to drive the dynamo through the night. Thus the University stokers and engineer were put onto double shifts; the cells were charged during the night and the charging current and battery potential measured at intervals.
>
> Then the cells were run down during the day, and their output measured in the same way. Just as this began, Thomson was laid up with an ailment which confined him to bed for a couple of weeks or so; but this led to no cessation of laboratory activity. On the contrary, the laboratory corps was divided into two squads, one for the night, the other for the day, and the work of charging and discharging and of measurements of expenditure and return of energy went on without intermission...
>
> [Gray,1908,81-82]

Perhaps as a final indication of the emphasis which Thomson placed on laboratory work of *measurement* , especially *electrical measurement* , for both himself and his students, we can note the level of public exposure which he devoted to publicizing the lattermost activity. Between 1874 and 1883 he gave three major orations which were essentially paeans of praise to the virtues and efficacy of electrical measurement: 1) his Presidential Address to the Society of Telegraph Engineers on 14th January 1874 [Thomson,1874]; 2) "Electrical Measurement" given to the Section of Mechanics at the Conference connected with... Scientific Apparatus on May 17th 1876 [Thomson,1876] and 3) "Electrical Units of Measurement" a lecture delivered to the Institute of Civil Engineers on May 3rd 1883 [Thomson,1883]. We can conclude this section with a declaration made by Thomson in the last of these, in which he laid out his view of the advances made by his own laboratory students in the accuracy of electrical measurement in the nine years since his first such lecture, and in the eleven years since his move to Gilmorehill:

> I doubt whether, ten years ago, a single scientific-instrument maker or seller could have told his customers whether the specific conductivity of his galvanometer coils was anything within 60 per cent of that of pure copper; and doubt whether the resistances of one in a hundred of the coils of electro-magnets, galvanometers, and other elctro-magnetic apparatus, in the universities, and laboratories, and lecture establishments of the world were known to the learned profesors whose duty it was to explain their properties, and to teach their use, to students and pupils. But we have changed all that; and now we know the resistances of our electromagnetic coils, generally speaking, better than we know their lengths; and our least advanced students in physical laboratories are quite able to measure resistances through a somewhat wide range with considerable accuracy. I should think that with the appliances in ordinary use, they are more likely to measure resistances of from 100 to 10,000 ohms to an accuracy of 1/10 per cent, than they are to be right to one millimetre in a metre in their measurements of length.
>
> [Thomson,1883,84]

Conclusion

William Thomson's career was one which imbibed from his background at the University of Glasgow the experimentalist heritage of the Chair of Natural Philosophy and the tradition of social egalitarianism germane to the nineteenth century Scottish University. These were synthesized with the rigorous mathematical training he received for the Cambridge Mathematics Tripos from his coach William Hopkins, and the incidental practical acquaintance with apparatus he received through both Thomas

Thomson in the Glasgow chemical laboratory and from Professor Challis at Cambridge. This experience in experimental philosophy was then substantially augmented by his apprenticeship in precision measurement in the Parisian laboratory of Regnault to the extent that his qualifications for the Glasgow Professorship of Natural Philosophy in 1846 were unique and effectively uncontested.

Having acquired the Glasgow Chair, he replenished the University's stock of philosophical apparatus and employed this from 1848 to 1854 in his (collaborative) revolutionary work on theory of heat, in pursuing a side-line of which Thomson began a system of inviting his best students to assist him in his research in late 1852. Within two to three years of this he was persuaded to open such researches on the electrodynamic theory of metals to the assistance of his whole class and to facilitate this he appropriated a room next to his own lecture classroom in about 1855. After some controversy, the sympathetic University Faculty of Arts negotiated for him the use of a ground floor cellar laboratory and a few other adjacent rooms in late 1857. Immediately after this acquisition of an official university physics laboratory Thomson began his work on electrical measurement for the nascent telegraph industry; with co-operative work from his students over the next decade he built up an expertise in scientific cable-laying technology which brought him both a knighthood and widespread acceptance of his methods of exact laboratory measurement in the sphere of commercial telegraphy.

Building upon his student-assisted work in constructing ever more accurate electrometers, electrodynamometers and galvanometers etc for the B.A.A.S. Committee on Electrical Standards throughout the 1860's, Thomson continued his constant refinement of electrical measurement equipment into the more congenial surroundings of his grandiose purpose-built laboratory on Gilmorehill in late 1870. Whilst Thomson was cultivating this tradition of electrical measurement in the 1860's to 1880's laboratories at other institutions were being set up by former pupils such as Ayrton (Tokyo and London) and Gray (Bangor) and by colleagues such as Tait (Edinburgh) and Foster (London). These men drew "inspiration" from Thomson's role as a laboratory research mentor in creating their own, generally more formalized, student laboratories to inculcate them in the practices of precision measurement which they themselves had originally learnt in the unique environment of Thomson's Glasgow laboratory; the laboratories of Tait and Foster will be the two case-studies next discussed in this thesis.

Thomson's professorial measurement activities continued at Glasgow - with the conferral of a Peerage in 1892 - until his retirement in 1899 when a former student, laboratory assistant and fellow expert measurer[16] Andrew Gray took his place in the Professorial Chair. Gray's eloquent tribute to the tradition of physical laboratory work created by Kelvin two years before taking up his Chair can be found in [Gray,1897]. It is thus appropriate to end this discussion with a quotation from the speech given at Gray's behest at the opening of the latter's Bangor laboratory in 1885; in this passage Thomson gives his view of the educational importance of laboratory measurement within the broad context of the University of Glasgow M.A.:

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[16] Gray's first publication in 1883, the year before his appointment to the Chair at Bangor, was entitled Absolute Measurement in Electricity and Magnetism and dealt almost exclusively with work being carried out by Thomson and his fellow laboratory workers on the practical realization of Thomson's long-vaunted system of *absolute* measurement [A.R., 1925].

> As a matter of general education for those not going to practise medicine, was it of any use entering a chemical or physical laboratory? I found as many as three-quarters of the students were destined for service in the religious denominations in after-life. I have frequently met some of those old students who had entered upon their professions as ministers, and have found that they always recollected with interest their experimental work at the University. They felt that the time they had spent in making definite and accurate measurements had not been thrown away, because it *educated them into accuracy*....
>
> [Thomson,1885,411]

CHAPTER 3

Peter Guthrie Tait and the Edinburgh University laboratory of Natural Philosophy.

> ...I may say a word or two about what has been so persistently croaked against the British Association, viz. that it tends to develop...what are called Scientific Heresies. No doubt such charges are more usually brought against other Sections than this; but Section A has not been held blameless. It seems to me the proper answer to all such charges will be very simply and easily given, if we merely show that in our reasonings from observation and experiment we invariably confine our conclusions to matter and energy...things which we can weigh and measure...
>
> Peter Guthrie Tait: 1871 Presidential Address to Section A of the B.A.A.S. [B.A.A.S. Report, 1871, (Part 2) 6-7].

The career of Peter Guthrie Tait as a Professorial teacher and investigator of both experimental and mathematical physics at the University of Edinburgh has been documented by his former pupil and assistant Cargill Knott [Knott,1911]. Knott's account does not, however, give us a coherent account of the prehistory and development of Tait's Edinburgh laboratory since none of the valuable primary sources that he cites are subjected to any constructive historical analysis. What follows will be an attempt to remedy Knott's omission by incorporating these unanalysed primary sources with material drawn from Tait's correspondence and reviews as well as recollections of Tait's contemporaries.

By juxtaposing this study of Tait's career with the preceding account of Thomson's work at Glasgow, this chapter will draw out the very close contextual connections between the laboratories of precision teaching and research operated by these two natural philosophers. First, Tait was the only professorial physicist to create an experimental environment which directly emulated the liberal research-orientation of Thomson's student laboratory. Although the other Scottish professors discussed in this thesis, Balfour Stewart and James Clerk Maxwell, also gave a high priority to student investigations in their work as academic experimentalists, Thomson and Tait's laboratories operated within the unique institutional confines of the Scottish Universities with their distinctive courses of study for the degree of M.A.

The Scottish M.A. curriculum not only covered a much wider range of subjects in arts and sciences than the narrower degree courses in classics and mathematics at Oxford and Cambridge, but also served a very different clientele to institutions of professional and industrial training such as

Kings College, London; Owens College Manchester; and the Royal School of Mines in London. Both Glasgow and Edinburgh Universities characteristically placed the study of natural philosophy at the pinnacle of their syllabus of liberal education and as such it was a major subject of study for the aspiring Presbyterian ministers and physicians which constituted the majority of their student population.

Davie has documented the essentially "democratic" character of the Scottish universities as expressed in their relatively non-hierarchical teaching practices and the wide range of social status and wealth amongst their student clientele. Such was the egalitarianism of the Scottish Universities, that their unique six-month winter academic sessions integrated Scottish undergraduate life with the financial and domestic commitments of their poorer students [Davie,1964]. The residual six-month summer vacations had the effect of indirectly facilitating the researches of the natural philosophy professors and any students assisting them beyond the regular session.

These distinctive social and institutional characteristics of Thomson's and Tait's university heritage were accentuated on the one hand by their mutual proximity and on the other by their geographical separation from the English universities and colleges discussed later. Thus Thomson and Tait uniquely fraternized local forums for physical research and debate such as the Royal Society of Edinburgh and Glasgow Philosophical Society, which hosted much of the native activity in natural philosophy during our period. The "common context" of Scottish experimental natural philosophy was thus effectively constituted by the work of these two men in their respective laboratories through their communications to these public bodies. This academic link between them was further cultivated in their ongoing dialogue on theories, instruments and laboratory practices, which, for example, resulted in the production of their major collaborative volume, the Treatise on Natural Philosophy [Tait & Thomson,1867].

Professorial partnerships such as that between the pair affectionately known as "T & T'" were not unique in the British community of physicists: P.G. Tait for example had a warm if remote working relationship with Balfour Stewart [Ch.7]. Nevertheless, in the length of his entire career Tait (unlike Thomson) was never a member either of the Royal Society or the Physical Society of London, the Societies that were the twin metropolitan nuclei of the British community of experimental physicists and physics teachers within the range of this thesis. Hence the only representative way of acheiving a coherent exegesis of Tait's laboratory work within the operation of this community is to relate the work of his Edinburgh laboratory specifically to Thomson's activities in the physical laboratory at the University of Glasgow. The extensive similarities between Tait's laboratory and its Glaswegian counterpart will become clear as we trace Tait's attempt to emulate the student-assisted experimental work of his professorial colleague William Thomson documented earlier. Indeed, it will be argued that Tait's laboratory was to a considerable extent an Edinburgh *annexe* of Thomson's laboratory.

1): Tait's early life: Edinburgh, Cambridge and Belfast.

Tait's educational career began in Edinburgh in the late 1830's, and co-students of his at the city's Academy were Fleeming Jenkin, James Clerk Maxwell and Lewis Campbell[1]: Jenkin was later Tait's colleague as Professor of Engineering at Edinburgh, and although a year ahead of Tait, Maxwell was a life-long friend and correspondent from 1845[Knott,1911,4-5; Tait,1880,332]. After considerable success in the Academy's mathematics prizes, Maxwell and then Tait moved on to Edinburgh University where the two enrolled in the mathematics class of Kelland (a Cambridge Wrangler), and the natural philosophy class of Principal Forbes. Maxwell entered the intermediate year of Forbes' class in 1846[2] and thus became class-mate of Tait when the latter less modestly joined the Natural Philosophy course at its senior level in 1847 [Knott,1911,6]. In this one year together at the University, both Tait and Maxwell did experimental work under the aegis of the Professor of Natural Philosophy: whilst Tait assisted Forbes in constructing lecture-demonstration models (e.g. of catenaries), Maxwell - as Forbes' "favourite" - was allowed "free use of the class apparatus for original experiments" [Knott,1911,7; Tait,1880,332][3].

In 1848 Tait left Edinburgh for Cambridge, leaving Maxwell behind to continue his experimental work with Forbes' and Playfair's apparatus until he too departed southwards in 1850, both men matriculating in turn at St Peter's College. Not only did Tait thus enrol at the same college as had William Thomson in 1841 but he was also coached for the Mathematics Tripos

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[1] Campbell was later Professor of Greek at St Andrews and Maxwell's co-biographer - [Campbell & Garnett, 1882].
[2] Maxwell arrived just after Balfour Stewart graduated and left for Australia [D.N.B.:Balfour Stewart] - [Ch.7].
[3] For a more detailed discussion of Forbes' teaching see Balfour Stewart's remniscences in [Ch.7].

by the same private tutor, William Hopkins [Tait,1880,332, Knott,1911,8]. In addition, Tait attended the lectures on experimental physics given by G.G.Stokes, the Lucasian Professor of Mathematics, who by this time was a friend and constant correspondent of William Thomson [ULC ADD 7656 NB1].

Subsequently these indirect connections with Thomson became tangible when Tait befriended fellow Peterhouse student, William Steele, who had come to Cambridge straight from the classes of William Thomson[4] and his father at the University of Glasgow. In 1852, shortly after Tait graduated Senior Wrangler (and First Smith's Prizeman) with Steele as Second, Thomson had his first[5] encounter with his future collaborator on a summer visit to Cambridge, as we know from a contemporary letter to his sister:

> Peterhouse has earned great credit by two students from Scotland, one of them, Steele, being a former pupil of Papa's and mine, who were senior and second wrangler last February. They were both here for some time after I arrived, and one will in a few weeks be a Fellow of the College.

[William to Elizabeth Thomson 7/6/1852, in Thompson,1910,231]

The Fellowship appointment to which Thomson alluded was one given

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[4] Steele was apparently a student assistant to William Thomson before the latter undertook the student researches on thermo-electricity of 1852-56 discussed above, for as Knott points out, Thomson's 1848 paper "On the Absolute Temperature Scale" cites one William Steele as having assisted in comparing the scale investigated by Thomson with that of an air thermometer [Knott,1911,9n1].

[5] In citing this letter S.P. Thompson himself notes that the Senior Wrangler in question was Tait but later confuses this dating of the first meeting between Thomson and Tait by arguing that Tait was "personally unknown" to Thomson until the former was elected to the Edinburgh Chair in 1860 [Thompson,1910,449]. It would be more appropriate to argue that the long-term friendship and collaboration of "T & T'" did not begin at this juncture - see later.

first of all to Tait although Steele was also made a Fellow not long afterwards. As a young Fellow Tait tried to establish himself as a coach, although he only ever taught one pupil[6]: partly perhaps because of his nascent aversion to "Cramming"·[Ch.1] - a practice to which coaching for the Mathematics Tripos necessarily exposed him - but more probably owing to a heavy commitment to his first academic treatise. As *alumni* of Peterhouse Tait collaborated with Steele on writing The Dynamics of a Particle almost immediately after graduating in 1852 although Steele died before its eventual publication in 1856 [Knott,1911,10-11; Steele&Tait, 1856].

Before this work was published, however, Tait was appointed Professor of Mathematics at Queen's College, Belfast in September 1854, and although nothing is known of the circumstances of this appointment we know something of the formative nature of his ensuing six years in Ireland. In Belfast Tait not only met William Rowan Hamilton whose volume on quarternions captivated him as both Cambridge Fellow and Edinburgh Professor, but also encountered William Thomson's brother, James, as the Professor of Engineering, and most importantly he fell in with the Professor of Chemistry, Thomas Andrews, the man to whom much of Tait's free time was to be dedicated between 1854 and 1860 [Knott,1911,12].

Some impression of the relation between Andrews and Tait can be judged from a review of Andrews' career, possibly by Tait's own hand, which was first published in the Northern Whig of 1879 and which Tait (anonymously) republished in Nature:

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[6] This pupil was one rejected by Hopkins: after Tait's coaching placed him above all of Hopkins' men in the Tripos the young Scot is alleged to have remarked of his pedagogical success that he could "teach a coal scuttle to be Senior Wrangler" [Knott,1911,11].

> As a teacher of science, Dr Andrews has been most successful...while his faculty of popular experimenting was of the most delicately accurate and attractive character. He had a peculiar power of gathering about him the *elite* of the best men of the year; wherever there was a man endowed with somewhat of the true scientific spirit, he was sure to gravitate towards the laboratory; and it is an interesting fact that the great majority of Dr Andrews' most trusted laboratory students have turned out successful men in after life.
>
> [Tait,1879a,507; Crum Brown & Tait,1889,xxxiii & xxxv-vi]

Tait evidently became a member of this *elite* soon after arriving in Belfast in 1854 since within two years Andrews and Tait had published collaborative research on the structure of ozone which was in turn a direct continuation of Andrews' earlier work on this subject. Tait assisted Andrews not only in carrying out the complex calculations that were involved but also in constructing much of the apparatus used; indeed, Knott records that Tait "proved such an apt pupil in the art of glass-blowing that ere long Andrews gave that part of the manipulation over to his eager and energetic companion" [Knott,1911,13]. If Andrews provided Tait with an introduction to the practical expertise required of the laboratory researcher he also inculcated Tait in the practices of precision measurement; as the reviewer in the Northern Whig remarked of Andrews' skill in determining chemical heats of combination: "considering the difficulties of this inquiry, as shown by the preposterous results which have sometimes been given even by able experimenters, the simplicity of Andrews' methods and the recognised accuracy of his results form a striking tribute to his care and skill" [Tait,1879,507].

It is highly significant, then, that in a lecture to the British Association meeting at Glasgow in 1876 - the year of Andrews' presidency - Tait identified the source of his commitment to precision laboratory practices as follows: "my old friend, Dr Andrews, [was the man] in whose laboratory I first learned properly to use scientific apparatus, and whose sage counsel impressed upon me the paramount importance of scientific accuracy..."[Tait,1876,463].

2): Tait's application for the Edinburgh Chair: 1860

When Tait applied for the Edinburgh Chair of Natural Philosophy in 1859 (upon the resignation of his former mentor J.D. Forbes), Professor Andrews furnished Tait with a testimonial which tellingly emphasized his skills as an experimentalist of some accuracy - an essential virtue for the successor to a man with so great a reputation for exactness as was generally attributed to Forbes [D.N.B.:Forbes].

> ...I am anxious to bear testimony to the extent and accuracy of his knowledge of general physics and in his skill in managing apparatus and performing experiments. In these points I speak very positively as I have had the advantage of his able co-operation for the last three[7] years in an extended physico-chemical enquiry...
>
> Professor Tait appears to me to combine to an unusual extent the mathematical knowledge and experimental skill required in Professors of Natural Philosophy...

Andrews also underlined the "very high opinion" held by all his colleagues of Tait's qualifications "as a man of science and of his talents as a teacher", adding not only Rowan Hamilton's praise of his mathematical skills but also emphasising the value of the experience Tait gained between 1857 and 1859 in conducting the senior class in natural philosophy

[7] We know however that as early as the summer of 1855 Tait had visited Paris to purchase instruments for joint experiments between Andrews and himself [Tait to Andrews 21/9/1855, cited in Knott,1911,64].

at Queen's College [Andrews to Tait,13/12/1859, ULE Gen.2169 11].

In connection with his application for the Edinburgh Chair, it is important to note that Tait wrote to William Thomson asking for a similar reference in late 1859 - a request that Tait would obviously not have made had Thomson not been familiar with his work at Queen's College Belfast. Judging from the content of the testimonial cited below it is very likely that his brother James Thomson - Tait's colleague as Professor of Engineering - had kept him informed of Tait's activities in Ireland:

> The manner in which Mr Tait has devoted himself to the cultivation of science since he took his degree at Cambridge affords in my opinion a much stronger evidence of his qualification for a professorship of Natural Philosophy than even the high distinction which he there acquired. The eminent ability and great zeal and perseverance with which he has worked, not only in some of the most difficult problems of mathematical physics, but in elaborate and refined experimental researches also, mark him as possessed to no ordinary degree of the qualities required in the successful prosecution of scientific investigations. I have not had the means of judging from personal knowledge regarding his powers as a teacher, but I believe they will be well attested by his colleagues in Belfast.
>
> [Thomson to Tait 30/11/1859, ULE Gen.2169 81]

From the personal letter that Thomson enclosed with this testimonial it is clear that Tait's original letter to him had expressed concern over whether Thomson would also be making an application for the Edinburgh Chair. Thomson had in fact held no intentions of doing so and was "glad... to be of any service" to Tait in his application, warning him, however, that he had heard "of a great many candidates ahead" [Thomson to Tait 30/11/1859, ULE Gen.2169 80].

There were indeed a considerable number of candidates for the Natural Philosophy Profesorship including Tait's Cambridge contemporary E.J. Routh, Fuller and Swan the Natural Philosophy Professors, respectively, at King's College, Aberdeen and the University of St Andrews, a certain Edward Sang of Edinburgh and Tait's old school-friend James Clerk Maxwell [Knott,1911,16]. Although Sang was apparently a popular candidate in the city of Edinburgh, William Thomson evidently favoured Tait, for in writing to his brother James (Tait's colleague) in Belfast on Febrary 14th 1860 he exclaimed: "I expected to see Prof. Tait before this on his way to or from Edinburgh. I was very much disgusted, but not excessively annoyed, to hear the other day that it is supposed a Mr. S___[8] (a mere nobody) has a good chance for the Chair vacated by Forbes. I hope however, this is not true" [William to James Thomson 14/2/1860, cited in Thompson,1910,408]

Maxwell was however Tait's primary rival for the Chair, given especially that Maxwell's chair at Marischal College had just been abolished upon the institution's amalgamation with the other Aberdeen College viz. King's [Campbell&Garnett,1882,277]. An informative comparison of the two candidates can be found in an article published in the Edinburgh <u>Daily Courant</u> on May 3rd 1860, shortly after Tait had won the appointment. Very significantly Maxwell was praised for the "almost intuitive *accuracy* of his ideas" which the <u>Courant</u> considered to be advantageous to his candidacy since such qualities guaranteed "a sure and valuable guide to those who came with partial knowledge requiring direction and *precision* in the study of natural philosophy."

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[8] Edward Sang, as the only candidate from Edinburgh cited by Knott, is obviously the target here - it was against the etiquette of biographies such as Thompson's overtly to identify persons thus attacked. The relevance of Thomson's concern for Sang's local popularity was that it could influence the decision of the Edinburgh Town Council which played a major role in making University appointments - see [Tait,1878,441].

Notwithstanding such qualities, Maxwell's failure to win the appointment was attributed by the journal to his evidently well-publicized inability to lecture effectively to any but the most highly knowledgeable audience. Tait, on the other hand, was deemed to have "very much of that habitual accuracy" which Maxwell allegedly possessed by intuition, but also possessed what Maxwell did not in commanding "great powers of impressing and instructing an audience such as his class will consist of [viz. of an elementary level] [Daily Courant(Edinburgh) 3/5/1860,in Knott,1911,16-17].

In interpreting Tait's election to have resulted from his unique combination of professional "accuracy" and oratorical skill it is thus pertinent to note that former colleagues and students who made Tait's acquaintance at both ends of his career at Edinburgh were specific in their comments upon the former of these virtues. John Chiene, Edinburgh Professor of Surgery 1860-1907 looked nostalgically back to his appointment in the same year as the Professor of Natural Philosophy as the occasion on which he had first met "Tait the precise physicist" [University of Edinburgh,1908,47]. In 1897, to undergraduates who first encountered Tait thirty-seven years after Chiene, the house journal The Student gave the following advice: "The aim at simplicity, together with the aim at accuracy, is the great characteristic of Tait's work...If those students, who in future years may forget all else that Tait has taught them, remember the maxim *Accuracy first, and simplicity next* , Tait's chief lesson will have been learned even by them" [The Student, 11(N.S.),170].

3): <u>The negotiation of a laboratory: 1860-1868</u>

In his inaugural lecture as Edinburgh's Professor of Natural Philosophy on November 7th 1860, Tait laid out a vision of physics closely resembling that of William Thomson [Smith&Wise,1989,Ch.4]. In emphasising that natural philosophy was "strictly a science of observation and experiment", to which mathematics was however a vital adjunct, and in espousing the central importance of the "conservation of energy" in contemporary natural philosophy, Tait explicitly aligned himself with the views of his Glaswegian counterpart [Tait,1860,5&25-34]. We can see one reason then why his correspondence and working relationship with William Thomson seventy miles westward in Glasgow began in earnest.

In December 1861, for example, Tait wrote to Thomas Andrews upon the genesis of a book that six years later was to be published as <u>The Treatise on Natural Philosophy</u>: "I ha[ve] agreed to write a joint book on Physics with Thomson. In fact I had nearly arranged the whole matter with Macmillan, when Thomson, to my great delight offered to join in" [Tait to Andrews, 18/12/1861,cited in Knott,1911,177]. This primary source can be complemented by Lord Kelvin's somewhat distant reminiscences of 1901 (shortly after Tait's death) in which he makes plain the extent of their common commitment to utilizing the concept of energy as the principal theme in their work:

> It must have been either before [Tait's] election or very soon after it that we entered on the project of a joint treatise of Natural Philosophy. He was then strongly impressed with the fundamental importance of Joule's work, and was full of vivid interest in all that he had learned from, and worked at with Andrews. We incessantly talked over the mode of dealing with energy which we adopted in the book, and we went most cordially together in the whole affair.

[Lord Kelvin to George Chrystal 13/7/1901, cited in Thompson,1910,452]

Before their collaborative work on the Treatise got underway in 1861, it is clear, however, that Tait became conversant with the working of Thomson's student laboratory at Glasgow and developed aspirations to have one similar at Edinburgh. For example, Tait informed the Devonshire Commission that after his appointment in 1860 "for nearly eight years I was exceedingly desirous to obtain space for a laboratory. I saw that it was excessively desirable that such a thing should be established in Edinburgh" [Tait,1872, q9416]. The earliest extant primary source that indicates anything of Tait's "excessive desire" for a physical laboratory is a letter from Tait to Thomson of January 1861, after Tait had spent three months in the Chair. From this letter it is clear that J.D.Forbes, now Principal at St Andrews had asked Thomson for a transcript of the evidence the the latter had given on the subject of his physical laboratory to the Scottish Universities Commission; Forbes evidently used this transcript in advising upon Tait's claims for a similar laboratory. Tait compared his own demands with those made by Thomson for financial support of his laboratory:

> The novelties to me were the laboratory assistant, and the fund for maintaining the laboratory. My demands have been so much milder than yours that I shall lose no time in supplementing them - though I am not prepared to go so far as you have done - one good reason being that even had I laboratory funds and an assistant, I have no laboratory.
>
> [Tait to Thomson, 1/1/1861, ULC ADD 7342 T6a]

There is unfortunately no extant evidence of Tait's subsequent attempts to emulate Thomson's aggressive demands for laboratory facilities although it would seem indeed that Edinburgh's extremely crowded accommodation and difficult financial position precluded any prospect of Tait receiving a laboratory in the early 1860's [Devonshire Commission Seventh Report,1875]. This was evidently a source of some regret to Tait as is illustrated for example by Tait's lament to Knott that the lack of such a laboratory had prevented him from being able to "make use of" the experimental skills of the young James Dewar whilst the latter was one of Tait's undergraduates in the early 1860's[9] [Knott,1911, 51].

Thus although Tait was a frequent visitor to Thomson's laboratory during the ensuing five years to discuss the development of the Treatise and purchase Thomson's high-resolution galvanometers and electrometers, e.g. in December 1861 [Tait to Thomson 12/12/1861, in Thompson,1910,453; Tait to Andrews, 18/12/1861 & 15/1/1862 in Knott,1910,67], his plans for a laboratory to emulate Thomson's Glaswegian prototype effectively lay fallow. Nevertheless after five busy years on the Treatise with Thomson, and four years work on the heating of rotating discs in vacuo [Ch.7], Tait became more optimistic about his prospects of acquiring a laboratory: in 1866 the following notice appeared in the University Calendar:

PHYSICAL LABORATORY

> Professor Tait is endeavouring to obtain the means of establishing a laboratory in which Students may acquire a practical knowledge of the construction of, and manner of using, physical apparatus; with the mode of conducting experimental enquiries. Further details on this subject will be published, whenever the state of the University Funds admits of the attempt being made.
>
> [Edinburgh University Calendar,1866,57]

[9] Dewar did however become laboratory assistant to the Professor of Chemistry, Lyon Playfair, upon Tait's explicit recommendation in 1867 [Knott,1911,51; D.S.B.: Dewar].

That such a speculative note relating to a contingency of University finance could be published in an official University publication suggests that Tait's appeal for a laboratory had now won a commitment from the University Court and Senate to sanction the creation of a laboratory when sufficient funds were at their disposal. This inference that the use of University funds for the laboratory was imminent is borne out by the fact that six months later, in April 1867, the University Senate passed a motion granting Tait the requisite funds for this purpose; Knott plausibly suggests that David Brewster, then Principal of the University, was very probably a leading figure in winning Senate support for Tait's scheme for a laboratory [Knott,1911,70].

Having obtained funding for his laboratory, however, it was another matter still for Tait to procure the requisite accommodation, and in these circumstances he was obliged to make another optimistic yet indefinite entry to the University Calendar regarding the future creation of his laboratory [Edinburgh University Calendar,1867,57-58]. This problem was solved in late 1867 as we know from a letter Tait wrote to Andrews on the 20th December:

> I am about to get a laboratory for practical students. The money has been voted. Henderson [the Professor of Pathology] has been induced to give up his classroom (which is situated just over my apparatus room) and during the holidays it will be put in order for work...
>
> [Tait to Andrews 20/12/1967, in Knott,1911,70]

In this small attic room Tait initially envisioned a more systematic course of instruction than was precedented in Thomson's voluntary research assistance at Glasgow, for in December 1867 he wrote to both Andrews and Maxwell for recommendations on appropriate [measurement] experiments for his students:

> I want to ask you if you can give me hints as to good subjects of experimental work for practical physical students, not subjects that require a Faraday [i.e. great originality], still less that require a Regnault [i.e. extreme accuracy].
>
> [Tait to Andrews 20/12/1967, cited in Knott,1911,70]

Although Andrews' reply is not extant we do have the benefit of Maxwell's characteristically idiosyncratic reply:

> You wrote [to] me about experiments in the Laboratory. There is one which is of a high order but yet I think within the means and powers of students, namely, the determination of Joule's coefficient by means of mercury. Mercury is (13.57/0.033) times better than water so that about 9 feet would give 10°F...[Plan described for obtaining a vertical fall of mercury and measuring temperatures above and below] ..I think it a plan free from many mechanical difficulties, and in a lofty room with plenty of mercury and strong ironwork, and a cherub aloft to read the level and the thermometer and a monkey to carry up mercury to him (called Quicksilver Jack), the thing might go on for hours, the coefficient meanwhile converging to a value to be appreciated only by the naturalist.
>
> [Maxwell to Tait 23/12/1867, in Knott,1911,215]

These enquiries indicate that Tait had plans for teaching a systematic course of "standard" measurement experiments and this was a feature evidently not drawn from the predominant operations of Thomson's laboratory. Nevertheless, we shall shortly see that when Tait's laboratory grew to maturity, this course of routine measurement training for students was complemented by an unequivocally Thomsonian mode of investigating physical problems at Tait's behest.

From a letter to Thomson in January 1868 we get a glimpse of the nascent laboratory being "put in order", although Tait's financial position was still not very favourable as regards laboratory equipment:

> I can't well tell you whether I shall be in a position to purchase the large electrometer. I have ordered several things from White and others; and if they all come at once I shall be a good deal out of pocket myself.
>
> Meanwhile my laboratory is being fitted up with every requisite - save instruments - but funds for them will not, I hope, be long in coming.

[Tait to Thomson 18/1/1868, ULE Special Collection Gen.2169 192]

These instruments were evidently some time in coming as can be judged from Tait's applications to the Court on April 27th 1868 for "permission to be given to commence a practical class in the physical laboratory (already fitted up for the purpose by the *Senatus Academicus)* as soon as the Special University Fund will admit of a grant for the necessary apparatus. For this practical class the fee charged would be 2/3 of that for Practical Chemistry [ie £2][10]" [ULE Minutes of Court 27/4/1868,358-59].

Tait's entry for the 1868-69 University Calendar indicates both that by September 1868 this new laboratory was fully equipped, and that he also intended the laboratory to be one of *voluntary research* , following the Glasgow prototype after all: "The Laboratory will be open for five or six hours daily, under the personal superintendence of the Professor and his Class-Assistant[11] . It is hoped that, with the valuable collection of apparatus in the Museum *results of real use to science may be obtained"* [ULE Calendar,1868-9,59]. He described the operation of his laboratory after its subsequent opening in autumn 1868 to the Devonshire Commission

[10] Although this contrasts with Thomson's practice of demanding no fee from his students (at least not until 1879 - see above) from Knott's testimony below we will see that Tait charged no fee to any student who returned to carry out research in his laboratory after the first session.

[11] W. Robertson Smith was appointed as Tait's class-assistant on July 24th 1868 and was trained for the job by the Professor during the summer of that year [ULE Minutes of Court 24/7/1868, 409; Knott,1911,71-72].

in 1872, and in his evidence we see how the preliminary training given to his students in the routine methods of *measurement* led them directly into a carbon copy of the research management employed at Glasgow:

> ...with the help of my assistant I put each student as he enters the laboratory through an elementary course of the application of the various physical instruments, the primary ones. For instance, I begin by practising them in measuring time, estimating small intervals of time, then measuring very carefully by means of a micrometer and sphereometer, length, angle, curvature; and by other appropriate instruments, temperature, electric current, electric potential and so on...
>
> When I find they have sufficiently mastered those elementary parts of the subject I allow them to choose the particular branch of natural philosophy to which they wish to devote themselves, and when they have told me that, it is not by any means difficult to assign to them, if they carry it out properly, what may be excessively useful and even valuable work.
>
> [Tait,1872, q9411-412]

From the reminiscences of Knott as an early laboratory assistant we see more specifically how Tait was attempting to emulate the operation of Thomson's laboratory:

> Lying quite outside any recognised course of study th[e] purely voluntary course of practical physics offered no inducement to the ordinary student intent on getting his degree. Tait's idea was to attract men who wished to familiarize themselves with methods of research. This he did by giving every encouragement to the man who had thought of some physical question worthy of investigation, or (as was more frequent) by suggesting some line of research to the eager student. Whoever showed real aptitude had all the resources of the Department placed at his disposal; and beyond the initial fee of two guineas for the first winter session no other charge was made, no matter how long the student continued to work in the physical laboratory. Those students whose interest in the subject brought them back after the first session of their enrolment were nicknamed "veterans"; and on their enthusiastic help Tait largely depended for the successful carrying out of his many ideas.
>
> [Knott,1911,22]

From this we can discern four separate points of similarity between Tait's and Thomson's laboratories which corroborate this interpretation of the latter as a prototype for the former:

1) laboratory work was voluntary in the sense of being "extra-curricular",
2) Tait intended it to "attract" potential researchers (rather than primarily those seeking a "liberal" education).
3) the "veterans" assisting him in research paid him no fee,
4) these researchers were an integral part of Tait's major experimental investigations.

A fifth similarity with Thomson's ethos of laboratory administration can be discerned from Tait's interview with the Devonshire Commissioners in which he declared "I have made the laboratory open to all comers[12]" - another characteristically Scottish expression of "democratic" education [Tait,1872, q9407].

For conclusive evidence that Thomson's laboratory was the prototype employed by Tait consider the following passage from Tait's opening lecture of the 1868-69 session:

> In several respects the present session may be expected to differ for the better, as regards the class of Natural Philosophy, from at least the last eight during which I have been connected with this University...From the miserable resources of the University enough has been granted me to make at least a beginning of what will, I hope, at no very distant time, form one of the most important features of our physical education. A room has been fitted up as a physical laboratory, where a student may not only repeat and examine from any point of view the ordinary lecture experiments, thereby acquiring for himself an amount of practical information which no mere lecturer can pretend to teach him; but where he may also attempt original work, and possibly even in his student

[12] This was qualified however by the constraints of his laboratory's size - see [Tait,1872, q9407-9408] and below.

> days make some real addition to scientific knowledge. That this is no delusive expectation is proved by the fact that in Glasgow, under circumstances and convenience far more unfavourable than I can now offer, Sir William Thomson's students have for years been doing excellent work, and have furnished their distinguished teacher with the experimental bases of more than one very remarkable investigation. What has been done under great difficulties in the dingy old buildings in Glasgow, ought to be possible in so much more a suitable place as this.
>
> [Knott,1911,70-71]

4): Laboratory space: the rhetoric of expansionism

It is interesting to note that in other circumstances Tait found it politically expedient to *play down* the comparative luxury which he enjoyed in contrast to the laboratory accommodation at Thomson's disposal in the old Glasgow laboratory. When interviewed by the Devonshire Commission in 1872 Tait disparagingly described the original cirucmstances in which he acquired his laboratory: "...I could not get space until 1868, and then the space which I got was a small classroom which had come to be disused, entirely unsuitable, or at least by no means very suitable for almost any class of experiments" [Tait,1872,q9416].

Nonetheless we know from Knott, in more sanguine mood, how Tait effected the adaptation of this room to give sufficient scope for accuracy in the "class" of measurement experiments that he envisioned:

> In this small upper room stripped of its benches, but with the terraced floor left intact, the men were put through a short course of physical measurements, such as specific gravities, specific heats, electrical resistance, and the like. Any who showed talent were soon utilised by Tait in carrying out original research; and to facilitate this kind of work, every possible corner of the old suite of rooms of the Natural Philosophy Department was adapted by means of slate slabs built into the thick steady walls for the installation of galvanometers and electrometers. The small room which Forbes had used as his sanctum became the

centre of experimental work. In this room Forbes had made his classical researches in polarization of heat; and here also Tait, with the help of successive sets of students, made his novel discoveries in thermo-electricity.

The large classroom was used also used as a research room, especially during the summer when (at least until the seventies) no class met. Two slate slabs were built into the wall, one on each side of the blackboard; and on these were placed the mirror galvanometers and electrometers necessary for delicate electrical investigations.

[Knott,1911,72]

Nevertheless, after solving the structural problem of creating an environment conducive to accurate measurement, Tait still faced problems of experimental accuracy and versatility engendered by the diversity of investigations which had to be carried out in the confined space of his laboratory and classroom suite:

...my rooms, such as they are, are so close together that if I wished to perform experiments on diamagnetic bodies, where a powerful electro-magnet is required, the moment I set to work the galvanometers (as well as electrometers and other instruments depending on magnetic force) in all the rest of my rooms are disturbed, so that it is impossible to carry on investigations with them...

...[similarly] it is impossible for me to allow one man alone to make [optical] experiments for example in a darkened room, because unless there were a special room which could be darkened for him, his darkening of the room would prevent any others continuing their experiments.

[Tait,1872, q9417]

Whilst arguing on the one hand that his classroom, apparatus room and laboratory were too close together for sufficient accuracy and even mutual co-existence of experiments by different students, he then claimed on the other hand that they were *too far apart* for effective administration:

> I may mention that as regards space I am perhaps worse off than any [other] professor in the University of Edinburgh.. When I have more than eight or ten attending the laboratory at once it is absolutely essential, in order to accommodate them, that I should have some of them working amongst my collection of apparatus, and others working in my classroom whenever it can be put at their disposal; and the superintendence of groups scattered about with stairs to ascend [between classroom and laboratory], and passages between them, is a matter of considerable difficulty.
>
> [Tait,1872, q9416]

To mitigate this claim, one that conflicts with that made in [9417], Tait pinpointed problems with the laboratory staff that helped him administrate the "far-flung" parts of the laboratory. The teaching assistants who marked examination scripts, gave tutorial lectures and superintended the laboratory in Tait's absence stayed no more than two or three years[13] as a result of their low salary viz. £100[Tait,1872,q9422-9423]. Secondly, his current mechanical assistant was a somewhat geriatric man over the age of 70 who had been resident in the University for more than 50 years, originally as doorkeeper and mechanical assistant to Forbes' predecessor Sir John Leslie [Tait,1872,9420].

Moving on from this "deficiency of assistance" Tait complained that the sheer rapidity of *progress* in experimental physics caused him problems too:

> I have not only utterly inadequate space for my laboratory but barely space for my collection of instruments, and that in trying to keep my instruments as well up to the advancement of natural philosophy as I can, every new addition prevents my access to what is already there; in fact my instruments are in one or two places piled in strata about the room, and the additional labour of finding what I want at a moment's notice forms of itself a very serious inconvenience.
>
> [Tait,1872, q9419]

[13] Between 1868 and 1879 Tait's laboratory teaching assistants were W.Robertson Smith, D.H. Marshall, P.R.Scott Lang and from 1879 it was C.G. Knott [Knott,1911,86].

Tait's complaints of insufficient accommodation were matched by other menbers of the Edinburgh professoriate, Crum Brown the Professor of Chemistry arguing, for example, that (like Tait's laboratory) his own "was never intended for the purpose [being] dark and ill-ventilated [and] altogether unsuitable" [Crum Brown,1872,9356]. Acknowledging that this widespread problem in laboratory accommodation for the experimental sciences lay essentially in the "extreme poverty" of the University [Tait, 1872,9413], a point upon which the Devonshire Commissioners emphatically agreed [Devonshire Commission:Seventh Report,1875,7-15], Tait argued that funding for new chemistry and anatomy laboratories would resolve his problems. "I should have no difficulty whatever in extending my space provided the University obtained funds enough to remove from the present University buildings, the departments of anatomy and chemistry. Once those were removed from the present buildings there is the whole space occupied by anatomy in immediate contiguity to my present department and in it I should have no difficulty in getting the amount of space required" [Tait,1872, q9418].

A highly sympathetic report from the Devonshire Commissioners in early 1874 [Devonshire Commission:Seventh Report,1875] initiated a civic campaign to procure funds for new University buildings. At the graduation ceremony in April of that year Tait gave a speech in which he explicitly declared that the buildings resulting from this scheme would be an effective combatant to the "evils of cram": the antithesis of laboratory work discussed in [Ch1]. As Nature reported this oration: "[Tait] spoke of 'the scheme for extending Edinburgh University and the facilities which would thereby be acquired for teaching science practically, as it ought to be taught, and thus tend to extinguish "paper-science" a term which "conveys to all who are real scientific men an impression of the most unutterable contempt" [Nature,10,502].

Tait's aggressive rhetoric in favour of laboratory science finally brought results five years later when, in the session 1879-80, the Anatomical Department moved into new premises - as Tait had suggested in 1872 - and the four vacant rooms were given over to Natural Philosophy. This gave Tait sufficient accommodation to have separate rooms for magnetism and optics [c.f. Devonshire Commission interview q9417 cited above], and a special laboratory for teaching junior students as well as three cellars for storing heavy equipment [Knott,1911,86].

Given that Tait thereby aquired more than the laboratory facilities for which he had appealed in his 1872 Devonshire Commission interview, the conclusion which Romualdas Sviedrys draws from an apparently cursory reading of this source that "Tait's hopes were not fully realized; his laboratory did not eclipse or even challenge Thomson's" is manifestly inappropriate [Sviedrys,1976,417]. Sviedrys evidently fails to discern that Tait's negative portrayal of his laboratory work in his 1872 interview was a *rhetorical* device to win the sympathy and support of the Commissioners and not a disinterested account of his alleged "failure" to emulate Thomson. The contingently *strategic* character of Tait's testimony to the Commissioners is obvious both from the internal inconsistencies in his evidence e.g. that the rooms he used for experimenting were too far apart for convenient control but simultaneously too close together to avoid mutual interference, and also from the telling contrast yielded by comparison of his 1872 interview with his optimistic lecture of 1868 [Tait,1868]. Sviedrys' failure to interpret Tait's remarks in this political context is exacerbated by his further failure to note that

Tait's laboratory facilities from 1880 onwards were comparable in scale and sophistication with those of Thomson's Gilmorehill laboratory from 1870.

As an additional counter to Sviedrys, it is important to show that even in the decade between the upgrading of the two laboratories Tait's laboratory was far from run down and ineffectual; it is equally important to demonstrate that Tait was not attempting to "eclipse" or "challenge" Thomson's laboratory as Sviedrys insinuates the Edinburgh man attempted but failed to achieve. The final section of this chapter will thus be devoted to illustrating how Tait's laboratory operated from 1870 to 1880 rather as an institution essentially imitative of Thomson's, but nonetheless not lacking in vitality nor distinctive character.

5): <u>Laboratory research and the Glasgow model 1870-1880.</u>

Soon after the foundation of his laboratory in 1868, Tait established a tradition of student research which embodied important parallels with Thomson's laboratory in both the mode and subject of experimental investigations. Throughout this period we will see that Tait incorporated the assistance of his students, much after the manner of Thomson, in a considerable number of his major experimental investigations, of which several stemmed directly from earlier researches undertaken in the Glasgow laboratory. In contrast, however, to Thomson who regularly travelled and published in journals throughout Britain and Europe, Tait left Scotland on only very few occasions during his tenure of the Edinburgh Chair and apart from acting as reviewer and leader writer for the London-based <u>Nature</u>, Tait published his laboratory researches almost exclusively in the

Proceedings of the Royal Society of Edinburgh[14] - an institution in which both Thomson and Tait took leading roles[Thomson,1901,503].

Immediately before the opening of the Edinburgh laboratory, in the session 1867-68, Tait had studied the experimental work undertaken by his predecessor J.D. Forbes in establishing that the thermal conductivity of an iron bar varied inversely with its absolute temperature and communicated his confirmation of Forbes' results to the RSE. He subsequently considered the extension of this investigation to other metals and to this end formed a B.A.A.S. Committee to carry out these researches together with his periodic *bete noir* John Tyndall, of the Royal Institution, and Forbes' former assistant and now Tait's close ally [Ch.7] Balfour Stewart [Tait,1869a,175]. Through this work of Forbes and Tait, measuring the thermal conductivity of metal bars became a research activity indigenous to Edinburgh and thus during the 1870's and 1880's repetitions of Forbes' original measurement became a regular feature of the work of laboratory "veterans" [Knott,1911,80-81]. In 1873, for example, Tait used a method due to Angstrom to measure the flow of heat waves down the metal bars and commissioned two "veterans", A.L.Macleish and C.E. Greig, to make measurements for a paper published by Tait in the RSE Proceedings [Tait,1873][15].

As early as 1869 however, Tait's thermal researches produced "spin-off" investigations for his students to undertake, e.g. in a letter to Thomson in July he reported: "one of my students has attained great skill in finding [i.e. measuring] specific heats; and has found that of best conducting copper to be slightly above that of bad, but to rise more

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[14] Hereafter referred to as the RSE.
[15] Like Thomson, Tait normally acknowledged the assistance of students in published articles.

slowly with increase of temperature" [Tait to Thomson 5/7/1869, cited in Knott,1911,75]. In the following year he institutionalized such student laboratory researches in the Proceedings of the RSE by instigating a regular article under the title "Notes from the Physical Laboratory" which summarized experimental results obtained by Tait and his students. In the first set of "Notes from 1870, for example, Tait reported work by J.W. Nichol on experiments directly connected with Balfour Stewart's work on "the radiation and convection of heat from blackened and bright surfaces at varied gas pressures"; A.Brebner continued investigations on "electrolytic polarization" that had been initiated by Tait and published in [Tait,1869b]; P.W.Meik and John Murray measured the change in the electrical resistance of copper upon mechanical loading [Tait *et al*,1870a].

From 1870 till about 1876 eight of the twelve sets of "Laboratory Notes" contained the results of research into thermo-electricity - a subject that predominated in Tait's work throughout this period. This work focussed upon the very subject that had been intimately involved with the formation of Thomson's corps of laboratory volunteers: "the electrical convection of heat" by now known eponymously as the "Thomson effect". Early on in fact Tait delegated much of this research to his students, as we see from a letter to Andrews in January 1871: "...I have barely time for any private work during the winter session now-a-days. However, I have got some students who are able and willing to work and I have handed over my apparatus to them to make the best of it. At present I am entirely engaged with "l'effet Thomson"" [Tait to Andrews 17/1/1871, cited in Knott, 1911,77]. Students involved in the investigations of 1871 were May and Straker, and when the B.A.A.S. met in Edinburgh that summer with Thomson as President and Tait as President of Section A the results obtained by these students were evidently incorporated into the article on the "Thomson Effect" read and published by Tait in the Sectional Proceeding of the Report [Tait,1871].

In 1872 John Murray and R.M. Morrison were similarly employed by Tait, and in 1873 it was the turn of Greig and Knott himself. After having spent one winter session in the laboratory Greig and Knott were instructed by Tait to "investigate by one and the same method the thermo-electric properties of some twenty different metals paired in a sufficient number of different ways; and these experiments which were made in the Natural Philosophy Class-room formed the basis of the "First Approximation to the Thermoelectric Diagram"[Knott,1911,77]. The Thermoelectric Diagram was a means of showing the variation of the Thomson effect with temperature and was indeed itself an invention by Thomson himself [Knott, 1911,79].

Greig and Knott's work for Tait on the Thermoelectric Diagram formed the subject of the Rede Lecture Thermo-electricity" in Cambridge in May 1873 - one of Tait's rare excursions to the South. Tait's numerous (17) references to Thomson, particularly with regard to the latter's applications of the First and Second Laws of thermodynamics to the thermal behaviour of currents leave us in no doubt that Tait's investigation of thermo-electricity was inspired by his Glaswegian collaborator. Similarly, the wide variety of metals incorporated in the "thermoelectric diagram" - indicate the extent to which Tait employed the Thomsonian mode of student assistance in carrying out his routine measurements for his publicly digested scientific research [Tait,1873].

One final piece of evidence of the great extent to which Tait's laboratory was in effect an annexe of Thomson's laboratory, insofar as

Tait carried out such Thomsonian researches with Thomsonian methods, can be cited from Knott's personal reminiscences of an occasion on which Thomson visited Tait's laboratory in the late 1870's. In 1877 Tait aquired a "Gramme Machine" to use as a current source for further thermoelectric experiments [Knott,1911,80], and here we see explicitly an example of Thomson "commandeering" Tait's students and laboratory for work on electrical measurement as if they were simply an extension of his own facilities - indeed we are reminded again that much of the the electrical equipment used by Tait viz. electrometers and galvanometers, had been designed, built and marketed by Thomson with his manufacturing partner White:

> Once on a Saturday morning in summer when two of us were working with electrometer and galvanometer in the Classroom Tait arrived in excitement and said "Thomson will be here in half an hour on his way to London. He wishes to try some experiments with our Gramme machine and will need your co-operation with electrometer and galvanometer." Sir William soon appeared, and we were immediately commandeered into his service. And then followed the wildest piece of experimenting I have ever had the delight of witnessing. The Gramme machine was run at various rates with various resistances introduced, and simultaneous readings of the quadrant electrometer and shunted mirror galvanometer were taken. The electrometer light-spot danced all over the scale...Full of impatience and excitement Thomson kept moving to and fro between the slabs on which the instruments stood, suggesting new combinations and jotting down in chalk on the blackboard the readings we declared. Tait stood by, assisting and at the same time criticising some of the methods. At length Sir William went to the further side of the lecture table and copied into his note book the columns of figures on the blackboard. After a few hasty calculations he said: - "That will do, it is just what I expected." Then he hurried off for a hasty lunch at Tait's...As they withdrew Tait looked back on us with a laugh and said "*There's* experimenting for you"....
> [Knott,1911,31-32].

This acknowledgement of Thomson's *definitive* experimenting in Tait's own territory is an eloquent illustration of Thomson as a role model for Tait in the operation of his physical laboratory.

Conclusion

We have seen, then, how after an early training in natural philosophy under Forbes, Tait's Cambridge education brought him indirectly into contact with William Thomson through St Peter's College, Stokes' lectures, his coach Hopkins, and through William Steele - an early student of Thomson. Moving on to Queen's college Belfast in 1854 Tait met William's brother James and Thomas Andrews who was a former pupil of William's father James Thomson Sr. From Andrews he learnt the skills of laboratory manipulation and was inculcated by him in the creed and practice of precision in experimental investigation.

After such immersion in a web of indirect contact with William Thomson, and after acquiring a commitment to energetics and laboratory measurement, Tait formed an alliance of both literary and laboratory skills after his appointment to Glasgow's sister university. Eight years later and after considerable negotiation in the impecunious Edinburgh environment, Tait was eventually able to create the laboratory that he had wished since first encountering Thomson's original model in 1860.

Tait devotedly nurtured his laboratory throughout the succeeding decades, democratically incorporating student's voluntary assistance into his own measurement researches, thereby adopting the Thomsonian method of teaching through experimental investigation. The Edinburgh investigations drew directly upon Thomson's own seminal researches of the 1850's and utilized the electrical equipment developed by Thomson himself in the 1860's. Tait's experimental work thus *merged* with Thomson's, for through their common research and as a result of constant contact between the two men, Tait's institution became in effect a physical *annexe* of Thomson's prototypical physical laboratory.

CHAPTER 4

George Carey Foster and the physical laboratory at University College, London.

No detail was too small or insignificant. He believed that accuracy was the soul of science, and he made his pupils reverence accuracy as a sacred duty...

> Rev. Henry Crow: funeral oration for George Carey Foster, 14/2/1919 [papers of Mrs E.M Cooper].

Introduction

During his tenure as the Professor of Physics at University College, London from 1865-1898, George Carey Foster created one of the earliest English laboratories for the formal teaching of practical physics in 1866. In this chapter we will document the negotations which Foster undertook in the next three decades in order to gain recognition of his laboratory teaching and research in the curricular and architectural hierarchies of University College.

Since the requirements of the external University of London examinations were the essential curricular constraint upon the teaching of natural science at this college, we will discuss his campaign for the introduction of laboratory-based experimental physics into the University of London scheme of a liberal education and thence into the regulations of the University B.A. and B.Sc. Degrees. Within the Gower Street setting of UCL[1] itself we will also analyse the generic problems of undisturbed laboratory measurement that Foster experienced in this busy metropolitan institution of higher education. As the functional theme linking these two discussions, Foster's fundamental commitment to *precision measurement* practices will be used to characterize both the pedagogical virtues he espoused on behalf of experimental physics and also his structural and cognitive requirements of laboratory architecture.

Firstly, however, it will be necessary to document Foster's disciplinary transition from the chemistry laboratory to the physics laboratory prior to his period at University College. This account will begin with a detailed analysis of two formative aspects of his early

[1] UCL and KCL will hereafter be used as abbreviations for University College, London and King's College, London respectively.

career: his education at UCL in the 1850's and his work as Professor of Natural Philosophy at Anderson's Institution, Glasgow, from 1862-65. These episodes will document his move from chemical to physical activities through the interdisciplinary subjects of electricity, heat and optics, with a concomitant transference of his expertise in *accurate* laboratory practices from chemistry to physics. Reference will also be made to the significant proximity of William Thomson's laboratory to Foster's working environment at the "Andersonian Institution", and also to Foster's educational tour of Europe from 1858 to 1862.

1):Laboratory training

i) early education at University college

Born in 1835 to a Lancashire calico printer and Justice of the Peace [Fison,1919,413], Foster arrived at Gower Street in 1852 and enrolled in the University College Faculty of Arts and Sciences to read for the University of London B.A. [Bellot,1929,293]. Here he followed the usual UCL curriculum including Professor Potter's course in natural philosophy in which Foster was awarded second class prize in 1852-3 [Anderson's Inst. Minutes,1862,298]. However, whilst preparing for the B.A., Foster devoted much of his time to the study of chemistry under Professor Alexander Williamson and such was Foster's chemical expertise that Williamson had him assist at lectures [Fison,1919,413]. After graduating with B.A. Honours and a prize in chemistry in 1855 [UCL Report,1856,6], Foster was appointed as Williamson's assistant in the Birkbeck Laboratory and effectively ran the laboratory classes for two years [Fison,1919,413; Anderson's Inst. Minutes,1862,498].

Williamson, like so many of his contemporaries, had migrated to Giessen to study in the laboratory of Liebig in the mid 1840's, and Foster later depicted the operation of Williamson's laboratory in teaching organic chemistry as reminiscent of Giessen:

> [in the session 1853-1854, Williamson] was a splendid teacher, always in the laboratory, going from one student to another, arousing and maintaining their interest in their work, and ready to discuss with them any point on which they sought his help...

Indeed some fellow disciples of the Liebig school were also often present:

> Kekule, Odling, and Brodie were constant visitors, and in the talk of these men in Williamson's little room at the end of the laboratory the seed was planted of much of the chemical theory of the day.
>
> [Foster,1905,610-11].

Foster also reported of this session that it was particularly fruitful from the point of view that six experimental papers were published by the Professor, his senior pupils and assistants on Williamson's specialist subject of etherification. When Foster became the Professorial assistant in 1855 he too carried out extensive researches in organic chemistry but even prior to publishing any of this collaborative work he was elected a fellow of the Chemical Society in 1856 [Foster,1904,610; Fison,1919,414-416].

From 1857 until 1867 Foster communicated nine papers on organic chemistry to the journals of the Chemical Society, the B.A.A.S. and the Royal Society[2] These papers were concerned mainly with quantitative analysis and nomenclature. From his quantitative analyses in particular,

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[2] Foster became a member of the B.A.A.S. in 1857 and was elected a fellow of the Royal Society in 1859 after the publication of his second paper for the B.A.A.S. - a major report on the state of organic chemistry [Foster,1859],[Fison,1919,416&420].

Foster aquired a professional reputation as being a "zealous and *accurate* experimenter in organic chemistry" [Anderson's Inst. Minutes,1862,498] e.g. with regard to his collaborative research with Matthiessen into the constitution of alkaloids [Foster & Matthiessen,1861;1863 and 1867] one former pupil of Foster's commented particularly on the *accuracy* of their work being "amply confirmed by subsequent investigation" [Fison,1919,414-416].

However, between his chemical activities at UCL in the mid 1850's and his appointment to the Andersonian chair of *natural philosophy* in 1862 Foster's interests developed in the direction of *physics* and this too we can trace to his mentor Williamson at UCL. As was characteristic of the disciplinary boundaries between the two subjects in the first half of the century, Williamson had incorporated the subjects of Heat, Light and Electricity into his chemical lectures - apparently more fully and more practically than they were taught by his colleague Potter in the Natural Philosophy classes [Anderson's Inst. Minutes,1862, 498]. It seems likely that Foster followed the example of his mentor in exploring natural phiolosophy as an adjunct to chemistry since as we shall see shortly Foster specialized in the very three philosophical subjects taught by Williamson viz Heat, Light and Electricity. Like Williamson, Foster pursued his interest in natural philosophy in some of the academic centres of Europe.

ii) the continental tour 1858-62

We know from Foster's obituary of Williamson that while studying under Liebig, the latter had carried out a considerable number of experiments relating to his theory of galvanism, and after being granted a Doctorate by Liebig in the summer of 1845 Williamson had turned his attentions to physics and mathematics under the Giessen Professors Buff and Zamminer respectively. Under Buff's guidance Williamson was granted quite unprecedented access as a student to the Giessen Physical Cabinet: "the use of an institution to which no student at Giessen ha[d] as yet been allowed access, being only intended for the use of lecturers" (Williamson quoted in [Foster,1905,609]). In mathematics too Williamson took his studies to an advanced level, following John Stuart Mill's recommendation that he become a pupil of Auguste Comte, thus residing in Paris for three years until 1849.

Following Williamson's example, Foster left University College in 1858 to undertake a similar tour of laboratories of continental Universities to broaden his scientific education. According to A.W. Porter the young chemist studied with Williamson's friend Auguste Kekulé at Ghent, with Jules Jamin at Paris and allegedly with Georg Quincke at Heidelberg, although since Quincke was himself only a student there in mid-1858 (thereupon moving back to Berlin) [Stevens,1902,587], it is much more likely that Foster studied with Professors Bunsen and Kirchhoff, as H.E. Roscoe - one of Foster's predecessors at UCL - had done in 1853 [DNB:Roscoe]. Details of Foster's study in these Continental laboratories between 1858 and 1862 are unfortunately somewhat obscure: all that we can be certain of is that Foster was with Kekulé during 1860 since his paper on organic acids of that year is marked as 'Ghent,1860' [Foster,1860].

However, whilst little is known about the work of Jamin in Paris apart from the researches in optics in which Foster himself probably participated [see Royal Society Catalogue of Scientific Papers], we can nonetheless speculate that his experiences at Heidelberg may have been formative in his later preoccupation with measurement physics.

The important *general* characteristic of the work in Heidelberg that is at issue here is Kirchhoff's and Quincke's concern with accurate physical measurement. That Kirchhoff was deeply concerned with precision and rigour in his work is a view maintained in obituaries of him written by German colleagues: "Kirchhoff's theoretical and experimental works were recognized by his contemporaries as models of accuracy and thoroughness" [Jungnickel & McCormmach,1986,294]. Arthur Schuster recalled of his time in the Heidelberg laboratory in 1872-73 that Kirchhoff was a "very precise man" who "attached great importance to a carefully prepared scheme of observation accurately carried out" and "expected the same standard of accuracy in others which he had set for himself" [Schuster,1911,13-15].

Ironically, however, Foster was not intimately acquainted with all the research carried out by Kirchhoff and Quincke during the period 1856-58: when Foster and Lodge studied the flow of electric currents in metal plates during 1875, "we thought," Lodge declared "that this was a new subject", discovering only later that Kirchoff had written two papers on the subject during 1857 [Kirchhoff,1857a&1857b] and Quincke had written two related papers [Quincke,1856&1858] under Kirchhoff's supervision [Lodge, 1931,94]. Nevertheless, from circumstantial evidence it is clear that Foster was inculcated in Kirchhoff's school of precision measurement at Heidelberg, a training that served to direct his skills as a precision laboratory chemist into the domain of experimental physics.

2) Anderson's Institution and the Chair of Natural Philosophy.

After his continental tour was completed in early 1862, Foster had made some considerable study of physics by the time he settled in London in the summer of that year. This is clear from the report of a deputation of trustees from the Anderson's Institution of Glasgow made in July by the Institution's President, Walter Crum, after visiting the city in June to search for candidates for their chair of Natural Philosophy. Upon encountering Foster, Crum commented "I was not aware that he had given so much attention to physics, particularly to three very important and extensively applicable of its departments viz Heat, Light and Electricity," which as Crum mentioned himself were the very ones taught so "fully and practically" by Professor Williamson at University College.

On the 14th of June Foster wrote to Crum expressing great interest in the appointment although he was somewhat worried about his qualifications in natural philosophy: "As I have never as yet taught Natural Philosophy, nor made it my special study, I could of course not do much in the way of presenting testimonials[3] [Foster to Crum, 14/6/1862]. Notwithstanding Foster's reservations, Crum et al. considered that a training in a chemical laboratory in the physical subjects of heat, light and electricity was ideal for the business of teaching the medical students that along with the "higher artisans" of Glasgow made up the bulk of the Andersonian's clientele:

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[3] These were the same testimonials as those he had collected when planning to apply for a Professorship at Sandhurst earlier in 1862 [Foster to Crum 14/6/62].

"I [and my colleagues] have long been of the opinion that for our particular purposes a teacher would be most likely to be found among the young men who have studied physics in a chemical laboratory, especially as the training for teaching physics separately is extremely limited, owing to the Medical Boards being satisfied with a superficial knowledge of Natural Philosophy in their examinations....

Crum explained the propriety of a laboratory training for this purpose as follows:

> In the laboratory of the chemist the handling of apparatus is constantly practised, and general dexterity in manipulation is acquired with greater facility and greater certainty than any other position. There too, the facts are rendered intelligible to a general audience by a description given in plain language with illustrations by experiment and diagram.
>
> [Anderson's Inst. Minutes,1862,497-498]

The testimonials received from Williamson and others confirmed Crum's view that Foster's education in chemical laboratories had endowed him with such appropriate teaching skills, the only remaining requirement being that appropriate instruments be purchased for his lecture-demonstrations: "If suitable apparatus can be procured in time he is fully competent to take up the three subjects I have named [Heat, Electricity and Optics] during the coming session, and he could prepare lectures on the other most important branches of Physics for the succeeding Winter." Without further ado Foster was offered the lectureship and detailed arrangements were made to procure £100 worth of the apparatus that he desired - even before he had travelled to Glasgow to discover what equipment was already there[4] [Anderson's Inst. Minutes,1862,499; Foster to Crum 23/7/62]. Shortly after Crum presented his report on the new lecturer in Natural Philosophy

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[4] In his retrospect of 1893 Foster remarked that Andersonian's physical apparatus was mostly "conspicuous by its absence, and a good part of what did exist consisted of contrivances for producing curious or surprizing effects, veritable tricks, rather than apparatus for instruction"[Foster,1893,4]

to the rest of the Andersonian trustees on 23rd July, an extra meeting was held to determine whether he should also be elected to the Chair of Natural Philosophy against the rival applicant Alexander Herschel[5]. In giving their reasons for preferring Foster to Herschel, despite the dynastic pedigree and Cambridge distinctions of the latter, the trustee's report gave emphasis to the needs of the Andersonian's working-class clientele:

> In a sphere where mechanical philosophy with the higher mathematics are chiefly studied [Mr Herschel] might...outshine his competitor; but for the thorough and steady going work of this institution, where matters of fact have to be explained and illustrated practically as well as philosophically to the artisans and warehousemen of Glasgow...we have no hesitation...in giving a decided preference to Mr Foster[6].
>
> [Anderson's Inst. Minutes, 1862,500]

Professor Foster followed the duties commonly adopted by his predecessors in the chair in giving a weekly course of "popular" evening lectures to the "Mechanics Class" from November to April. In successive years he gave a course specifically devoted to Heat, Electricity and Magnetism, and Light and Sound - precisely the subjects of his acknowledged expertise as discussed above. [Foster,1893,3-4]. These

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[5] Alexander Stewart Herschel (1836-1907) was son of the eminent astronomer and natural philosopher John Herschel, and grandson of William Herschel. After Foster vacated the Anderson chair, Herschel junior held the Professorship (incorrectly described by the DNB as a Glasgow University lectureship) 1866-71. In 1871 he took up a similar post at the newly opened Armstrong College of Science at Newcastle-upon-Tyne and later created the first physical laboratory there in 1875 [D.N.B., Sviedrys,1976].

[6] Foster was elected by 35 votes to 5 on the 19th of August [Andersonian Inst. Minutes, 501]

subjects were also the ones for which Foster was invited by Henry Watts, the editor of the Chemical Society Journal, to contribute articles for his Dictionary of Chemistry [Watts et al,1864]. His extensive articles on Electricity and particularly on Heat established, according to A.H. Fison, "Carey Foster's reputation as a clear thinker and able exponent of physics[7]" [Fison, 1919, 413]. Thus again through a chemical avenue Foster achieved recognition as a practitioner of physics.

As a further avenue into academic natural philosophy there is strong circumstantial evidence that Foster was well acquainted with the student activities in William Thomson's Glasgow University laboratory, the (old) Glasgow University buildings being situated only a few minutes walk from the Andersonian. First of all Walter Crum was the father of William Thomson's first wife, Margaret Crum, hence it is extremely likely that the two men were introduced whilst Foster was in Glasgow. This is particularly likely in view of their common subject matter, their close institutional proximity to one another, and their common involvement with the Glasgow Philosophical Society [see Proceedings]. More specifically, although still indirectly, we know of Foster's familiarity with Thomson's laboratory through the later testimony of W.E. Ayrton[8], one of Foster's earliest students at University College in 1866, to whom Foster gave a very comprehensive description of the laboratory. Ayrton reported that "[Foster's] description of what Thomson had done at Glasgow...made my mouth water and turned my attention northwards..." Indeed Ayrton took

[7] In Fison's view Foster's subsequent election to the Chair at University College "was mainly due to the reputation he established as as the author of the articles in Watt's Dictionary [Fison,1919,414] .

[8] Ayrton was a student at UCL between 1864 and 1867 [DNB]

Foster's advice to work in Thomson's laboratory after leaving UCL in 1867 [Ayrton,1908,263].

As far as Foster's immediate response to Thomson's work at the neighbouring institution is concerned, we can only speculate that his commencement of a daytime class for about a dozen pupils on the subject of Mechanics in the session 1864-5 was perhaps an attempt to emulate his Glasgow colleague. We do know, however, that like Thomson, Foster himself had a private laboratory in which he spent his abundant free time at the Andersonian in constructing apparatus for lectures and research. From the session 1863-4 onwards, a local F.R.S., James Young, offered to pay for a "skilled mechanical assistant" to aid Foster in the construction and repair of his apparatus. Young himself also found the assistant that Foster was thus able to employ: a mechanic named William Grant.

Foster later testified to Grant's services as being of "very great value" in making and repairing such instruments as Foster required and Grant became an essential feature of his private laboratory at the Andersonian. Indeed, so great was the extent to which Grant's services were valued that Foster was determined to take the mechanic with him when, after encouragement from Williamson [Fison,1919,413-414] he resigned the Andersonian Chair to return to University College, London to take up the Professorship of Natural Philosophy in summer 1865 [Foster,1893,1-8]. As we shall see, William Grant's assistance thus gave substantive continuity to Foster's transition from the private preparatory laboratory in Glasgow and his teaching laboratory in London.

3): The context of Natural Philosophy at University College in 1865.

The revised charter of University College, instituted in 1836 after the opening of the rival Kings College in the previous year, decreed that the object of the College would be the "general advancement of literature and science by affording to young men adequate opportunities for obtaining literary and scientific education at moderate expense" [UCL Calendar, 1865, frontispiece]. At the time of Carey Foster's appointment in 1865 the "opportunities" for receiving such an education were furnished by the Faculty of Arts and Sciences[9] which provided a "liberal" curriculum of study intended to prepare students for the University of London B.A. and B.Sc., the latter having been established by new University regulations in 1858 [Bellot,1929,303]. Teaching in Natural Philosophy formed part of the curriculae for both the B.A., and from 1858 the B.Sc. also, and up until 1865 the incumbent Professor in this subject was the man who had taught Foster as an undergraduate: Richard Potter.

In establishing the context of Foster's appointment it is important to appreciate the very unflattering portrait painted of this Professor by his former pupils and other contemporary commentators. A.H. Fison, assistant to Foster in the mid-1880's, reported of Potter that he was "essentially a man of the previous generation and was unable to assimilate the developments which had taken place since the latter part of the eighteenth century." With regard to theories of heat and light he belonged to what Fison dubbed the'old school' of corpuscularianism (Potter taking as his speciality the study of geometrical optics) and he was never

[9] The Faculties of Law and Medicine served in a parallel fashion to prepare students for the professional qualification in these fields. In 1870 the Faculties of Arts and Science were separated - see later.

reconciled to the principle of energy conservation . As a "High Wrangler" and Fellow of Queen's College Cambridge, Potter was in fact deemed by H.E. Roscoe to be representative of contemporary Cantabrigian scientific pedagogy and recounted his experience of Potter's teaching in the early 1850's as follows:

> The Professor of Natural Philosophy was an extraordinary man - an enormous bulky body, with a face like a woman's and a piping voice. His method was that of the Cambridge of that day. His lectures were not experimental, and they were not appreciated by my fellow students. He generally read from his own book on mechanics, holding it up in his hand while he wrote up a formula on the blackboard and occasionally would become confused...Times have changed as regards the teaching of physics, nothing less than a revolution having occurred.
>
> [Roscoe,1906,29]

According to W.S. Jevons in the next decade, it seems that Potter took up the experimental mode of lecturing with some success [Jevons,1886,30], although Jevons very rapidly lost interest in this and reverted to private study rather than attend the professorial lectures [Jevons,1886,35-36]. By the mid-1860's (just before his resignation) Potter's teaching had apparently degenerated somewhat further, for Dr.J.B. Benson, who had then been in attendance at both his experimental and mathematical classes, related that:

> ...as a teacher in my day, he had one fatal defect. He was worn out, he had lost his memory and and not a few of his wits. In his experimental class he was mercilessly ragged. I [saw] him snowballed in the lecture-room, I [saw] him sprayed....[and] the apparatus was as worn out as the Professor. It never did what it was expected to do. Magnetic force, for example, would be demonstrated experimentally by holding a needle to what might once have been a magnet, but had ceased to attract, whilst the professor said, "You see it wants a little helping, gentlemen".
>
> [cited in Bellot,1929,263]

By July 1865 the College clearly found this situation intolerable for a committee "to consider the condition of the Natural Philosophy classes" was formed and reported that after long conferences in the Senate and discussions between Potter and the Dean of the Faculty of Arts they had arrived at the unanimous conclusion that "it would be for the interests of the College that the classes of natural philosophy be placed in other hands " . So keen in fact was the College Council to be rid of Potter that he was "retired" on full salary, "serious as such a further burthen" was deemed to be in the unhealthy financial circumstances[10] of the College [UCL Minutes of Council 1/7/65].

As soon as the Council had negotiated Potter's retirement, the committee appointed to consider the testimonials of applicants to replace him were also commissioned to consider the "expediency" of splitting the Professorship of Natural Philosophy into *two* new chairs viz: Mathematical Physics and Experimental Physics[11]. In the committee's *unanimous* decision to effect this separation, there is a strong suggestion that Potter's own apparent inability to master the teaching of both these subjects was a crucial factor:

> the duties of the two chairs, though connected with the same subject, demand decided differences of thought, reading, and talent: insomuch that it may happen, and *does happen* , that the individual of marked eminence in either may be *below mediocrity* in the other. (emphasis added)
>
> [UCL College Correspondence 1865 AM/103]

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10 See [Bellot,1929, ch8] for an account of the College's "struggle for existence" in the period preceding the appointment of Foster.

11 This transition can be interpreted as symbolic not only of the developing specialization in physics but also of the institutional obsolescence of the term 'natural philosophy'

A decade later Foster gave another perspective on the subject of Potter's mathematical eminence in relation to Foster's own subsequent appointment as Professor of Experimental Physics, his former laboratory assistant Oliver Lodge later reminiscing that:

> In the old days Carey Foster told me chafingly that he had been appointed Professor of Physics, although up to that time his best work had lain in a chemical direction, because he did not know too much about mathematics, and because his predecessors, J.J. Sylvester and Richard Potter, had known too much, and had not been very successful teachers in consequence.
>
> [Lodge,1927,13]

Ironically it was precisely *because* of the expertise in chemistry which he had first acquired at University College in the 1850's, as well as for his unique prior experience in teaching experimental physics, amongst the applicants for the two chairs (who were mostly Cambridge Wranglers) that Foster was selected by the committee for the experimental professorship[12] in August of 1865:

> There is..no question that Mr Foster is the only candidate who produces any sufficient evidence of ability to conduct a course of experimental physics. He has been employed in this manner for three years at the Andersonian Institution, with very good proof of his success.
>
> Mr. Foster is a practical chemist, and Dr Williamson bears strong testimony to the originality of his published researches. He was for many years Dr. Williamson's assistant in the laboratory and in emergencies delivered some of the chemical lectures. He is thus known as a good lecturer and a successful teacher in chemistry.
>
> The approximation of [advancing?] physics and chemistry is

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[12] T.A. Hirst, a member of the metropolitan X-Club with Huxley, Tyndall et.al. [Macleod,1970] was elected Professor of Mathematical Physics on the basis of his "reputation for mathematical ability and learning [that put him] not merely above other candidates but into another class" [UCL College Correspondence,1865,AM/103].

> close, and daily becoming closer. It is [thus] very desirable that the Professor of Experimental Physics should be a good chemist.
> [UCL College Correspondence,1865:AM/103,3-4]

The committee's conception of Foster's pedigree in practical chemistry as strongly supporting his status as an experimental physicist embodied the contemporary view of chemistry as the 'established' training-ground of experimental science [Ch.1]. However, whilst colleagues such as Guthrie continued with chemical researches throughout their professorial careers as physicsts, Foster, after one more collaborative venture in organic chemistry with his Heidelberg colleague August Matthiessen [Foster&Matthiessen,1867][13], focussed his activities at University College almost exclusively on extending the institutional position of physics.

4): The creation of the UCL physics laboratory

Although there is little extant documentation of Foster's first year of tenure we know that two major changes took place in the institutional position of his subject at the end of the session 1865-66: Foster persuaded the College to endow him with an experimental teaching laboratory and early in 1867 his professorial chair in "Experimental Physics" was subsequently restructured as that of "Physics". The latter move occurred when Augustus De Morgan retired, at which point the

[13] Foster did however continue to be active in the administration of the Chemical Society, sitting on the Council 1865-68, 1872-75, 1885-6 and acting as Vice-President 1888-1890 [Fison,1919, 416]. These ran concurrently with his frontline activities in the Physical Society from 1874 onwards [Ch.8].

Professor of Mathematical Physics, Thomas Archer Hirst, was assigned the duties of the outgoing Professor of Mathematics and thereby acquired the chair of Pure and Applied Mathematics. Although Foster, as we saw above, disclaimed any great expertise in mathematics, he took over the less advanced classes of mathematical physics requisite for students attempting the University of London B.A. or B.Sc [Ker,1898,66-67; UCL Report,1868,9].

This redistribution of Professorial labour would appear to have been something of an economy measure since three professorships were thus amalgamated into two at considerably reduced expense to the College. Nevertheless Hirst soon found his task too arduous and thus another new Professorship of Applied Mathematics and Mechanics was created (in *addition* to the pre-existent Professorship of Engineering) leaving Hirst with De Morgan's original Professorial mantle of "Mathematics" [UCL Report,1869,4]. Although no longer exclusively an experimental physicist, Foster ventured to extend his Professorial role to the provision of experimental teaching in physics.

According to John Robson, the Secretary of University College, at the time Foster was elected (in 1865) "he pointed out to the [College] Council the importance of the means of giving practical instruction in the various subjects which he had to teach, and the Council complied with his suggestions as far as they had means of doing so" [Robson,1871,q7134]. That he made this suggestion so soon after arriving from Glasgow is perhaps indicative of Foster's enthusiasm to emulate what he had seen of William Thomson's work there. W.E. Ayrton at least alluded to a very close similarity between Thomson's laboratory and Foster's initial set-up. commenting some years later that "the accommodation in Gower Street for physical work, in 1866, did not differ much from what had existed at Glasgow since 1846[sic]" [Ayrton,1908,263].

On June 19th 1866 Foster proposed a plan to the Committee of Management "for the establishment of a Physical Laboratory and courses of Practical Instruction in Physics", the laboratory to be located at the upper end of the natural philosophy lecture theatre. [UCL Committee of Management Minutes,5,196-7]. Foster was invited to present a paper on this subject at the Committee's next meeting on July 3rd but unfortunately all that was recorded of the paper's content in the Committee's minutes was that it related to a proposed engineering workshop as well as a physical laboratory [UCL Comm. Man., 5, 201].

We might surmise that Foster framed his arguments for the educational value of laboratory work in similar terms to the views that he gave to the Devonshire Commission in 1871 [see below]; a more detailed advocacy of student laboratory work can be found, however, in Foster's preface to Weinhold's Introduction to Experimental Physics of 1875. In this he argued that the most important aspect of teaching physics was to give students an initial grounding in the "concrete facts" of the subject:

> The kind of knowledge...which is really serviceable for this purpose is not such as can be got by merely reading or hearing descriptions of phenomena, or even seeing experiments made by a teacher: it needs that the student should observe and experiment for himself.....the knowledge we obtain, by seeing and handling an object for ourselves, is more vivid and complete than what can obtained second-hand through the testimony of others..[thus]...at the outset of [a student's] course it is very desirable that as far as possible his attention should be directed to things that he has seen and examined for himself; and unless he has learned by his own experience, at least in a few cases, what experimental evidence means, he will scarcely ever be able to appreciate rightly the evidence to be obtained by reading [books].
>
> [Foster,1875c,cviii-ix]

Foster's rhetoric for legitimating laboratory work was thus typical in attributing a higher cognitive authority to the personal experimentation of the student than to the statements of a physics textbook [Ch.1], although in this case Foster gave exceptional praise to Weinhold's book for giving instructions to the reader on how to make the appropriate experiments for himself [Foster,1875c,cxi].

Although we do not know the exact terms of the plan that he presented to the College in 1866, it is safe to assume that the manner in which Foster promoted laboratory work for physics students would not have changed radically between 1866 and 1875. At any rate in 1866, the College Committees were highly amenable to the reasons he gave for the creation of a physics laboratory for teaching purposes: the Committee of Management recommended that the College Council allow Foster free reign in adapting the lecture theatre to his requirements and also to employ an assistant - none other than William Grant, specially imported from Glasgow to rejoin his former master in the workshop and laboratory[14]. The College Council, four days later on the July 7th, upheld this recommendation "subject to careful examination by Professor Foster as to the *conditions of stability* in the room where the experiments are to be carried out" [UCL Council Minutes, 7/7/1865]. Clearly Foster had impressed upon them the extent to which sensitive measurement operations were to be the main activity of his laboratory, although as we shall see later Foster was never entirely satisfied with the stability that this laboratory environment offered him.

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[14]Grant's assistantship spanned the entire of Foster's tenure [Fison,1919,423].

More specific details of this development were furnished by one of Foster's later contemporaries at UCL, W.P. Ker, in his history of the College's Department of Physics. Ker described the creation of the laboratory as follows:

> The level space above the seats in the lecture-theatre had been for a good many years occupied by a very miscellaneous collection of models of inventions, which had been presented to the College by the Society of Arts and were mostly in a very decrepit condition. In consequence of the representations made to them by the Professor as to the importance of providing opportunities for experimental work by students, in addition to mere class lectures, the Council got rid of all but a very few of these models, and transferred most of the apparatus-cases to the space they had occupied, thus leaving the "apparatus room" free to be used as a room for experimental work.
>
> [Ker,1898,67]

The College authorities decision to invest in a scheme of practical physics teaching as unprecedented in England as Foster's laboratory would seem to have been intended, at least in part, as a public display of benificence in a period when UCL was in difficult financial straits [see Bellot,1928,Ch.8] the Annual Report of the College conveyed their official approval of this new development as follows:

> It is to be observed that, in consideration of the deficiency of the Receipts in recent years, extraordinary expenses were generally speaking avoided in the last session [1865-66]: the only exception was in the class of Experimental Physics. The extended mode of instruction introduced by Prof. Carey Foster both in the General Class[15] and by the establishment of a Physical Laboratory has seemed to the Council worthy of special encouragement and to justify their incurring additional expense, both present and future, to promote its success.
>
> [UCL Annual Report,1867,16]

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[15] This is probably a reference to the much broadened scope of Foster's experimental lectures by comparison with Potter's.

A quantity of new equipment was also obtained by Foster after he presented the College with a long catalogue of the additional apparatus which he considered to be indispensable for carrying on his work. The College, however, provided him then with funds sufficient only to purchase what Foster deemed "absolutely necessary",[Robson,1871,q7134] - as Foster later declared: "in order to start the physical laboratory at all I considered it of such importance that I proposed to the Council to make any additional outgoings that might be required on the income of the laboratory" i.e student's fees [Foster,1871,q7804]. Thus although Foster did succeed in negotiating further funds to purchase important apparatus from the College Committee of Management during the first year of the laboratory's operation [UCL Minutes.Comm.Man,1866-7], this was only because Foster gave up part of his income from student fees to repay the College [Foster,1871,q7804].

Nevertheless the college were more forthcoming providing funds for the laboratory's collection of apparatus when they were satisfied with the financial viability of Foster's innovation and also paid for a laboratory demonstrator from 1874 onwards - the first of these being Oliver Lodge[16] [Lodge,1931,82]. The College's motives for this continued investment can only have been related to the institutional *prestige* that they associated with their possession of a laboratory unrivalled in England except by Robert Clifton's apparatus room in the Oxford Museum. After all, Foster's laboratory classes were not made a *compulsory* element in his natural

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[16] This expenditure up till 1879 was 1866-67: £125 9s 9d, 1868-1874: none recorded, 1874-75: £160 (including £50 salary of demonstrator), 1875-76: £195 (ditto), 1876-77: £152(ditto), 1877-78: £170(ditto), 1878-79: £190(ditto) [UCL Reports 1865-1880].

philosophy courses: when Foster's laboratory first came into operation *practical* physics was not a subject examined in the University of London (BSc or BA); thus the considerable proportion of UCL students that were studying for these degrees could not feasibly have had laboratory work imposed upon them as a curricular requirement. Secondly as an *extra-curricular* activity, the fees paid by laboratory students only very occasionally gave significant profit to the College [Foster,1871,q7804]. Hence the laboratory played a relatively marginal role in Foster's *formal* teaching of physics until a decade after his courses began[17].

To understand how Foster campaigned to achieve institutional recognition of laboratory work as an *compulsory* part of his physics teaching at UCL and the rationale for the improvements he sought in the architectonics of his laboratory, we will now discuss the significance to Foster of laboratory precision measurement.

5): Teaching by measurement in Foster's laboratory

The syllabuses developed by Foster for the optional course in experimental work in the first two years of the laboratory's operation (1866-68) demonstrate Foster's commitment to a comprehensive training in the methods of precise quantitative experimentation - an approach somewhat removed from Thomson's "teaching through research". For the session 1866-67 the prospectus entry for "Practical Instruction in Experimental Physics" read as follows:

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[17] A BSc examination for in Experimental Physics was instituted in 1876/7 - see below - [University of London,1912,503].

> The object of this course is to afford instruction 1) in Pure Physics and 2) in the practical Applications of Physical Science.....Students are first taught the construction and use of the most important physical apparatus (as for example the Air Pump, Electrical Machine, Galvanic Battery) and are made practically familiar with the conditions needed for the production of the fundamental phenomena of the various branches of physics; they are taught the use of the most important measuring instruments (as for example, the Balance, Barometer, Theodolite, Galvanometer) and are practised in making accurate observations by them.
>
> [UCL College Calendar,1866-7,20-22]

Regarding the priority of the two objects in this course, Oliver Lodge later related that "in those days attention was paid to the principles of pure physics rather than to technology" [Lodge,1908,128]. Reference to matters of applied physics was generally made in relation to electrical telegraphy since this was not only a subject of great popular interest in the wake of the Atlantic Cable laying [Foster,1866b][18] but also the source of a specific element of his laboratory clientele: students preparing for the Civil Service examinations to enter the Indian Telegraph Service [Foster,1871,q7794; Robson,1871,q7134]. Also relevant more generally to industrial interests was Foster's scheme[19] which required students to construct (some) apparatus for themselves - they could learn the practical skills requisite for apparatus construction i.e. joinery, turning and the working of wood and metals, from Foster's assistant William Grant in the mechanical workshop attached to the

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[18] In his 1866 paper "Electrical Principles of the Atlantic Telegraph" Foster showed himself to be very much au fait with the contemporary state of telegraphy - unsurpising in view of his close proximity to Thomson's Glasgow laboratory in the previous three years [Foster,1866b].

[19] This scheme of apparatus construction by students prior to laboratory experimentation was later jointly codified with Guthrie and Barrett [see Chapter 1 & 8].

laboratory [UCL College Calendar,1866-7,22].

After the first year of the laboratory's operation, however, Foster significantly revised both the presentation and the content of his course, presumably as a result of his first experiences in practical teaching. Whilst mention was made in the 1866-7 prospectus of students being set to repeat some standard research experiment i.e a standard measurement upon their successful completion of the course, this suggestion was dropped in the next year's prospectus. Underlying this change was perhaps Foster's discovery of less skill in his students than he had hitherto expected, for in the 1867-68 prospectus there was a new requirement of some prior experience or qualification in physics before laboratory work could be undertaken, students now being recommended to "attend at least one of the General Classes of Physics before entering the Physical Laboratory, unless they have obtained elsewhere a fair knowledge of the principles of physics" [Calendar,1867-68,25-26]. In this context Foster urged the Devonshire Commissioners in 1871 that students would have such a prior preparation if schools organized "practical instruction in which [pupils] should make experiments themselves, which [although difficult] I think is by far the preferable method" [Foster,1871,7815-19].

However, the lack of an elementary schooling in physics amongst Foster's students was not the only problem that prevented him imposing a minimum level of expertise upon them: to the Devonshire Commissioners in 1871 he commented that he would have instituted an entrance examination for the laboratory but for the financial necessity of taking all of the small number of students that applied to work in it [Foster,1871,7813-15]. Thus we find that, although Foster restricted his laboratory to senior year students only [Lodge,1919b,xviii], his laboratory assistant certainly encountered a considerable lack of expertise amongst his charges. Of Grant's regime in the laboratory A.H.Fison recalled that "his love of the apparatus, so much of which he had constructed, and the agony he experienced in seeing it misused, made him a source of terror to all students other than those few who proved themselves worthy to be entrusted with it" [Fison,1919,423].

In addition to his new proviso on the qualifications of students, Foster also employed a rhetoric in the 1867 prospectus which was much more specific in promoting the *utility* of measurement as relevant to engineering and other practical purposes, doubtless as a move to attract the engineering clientele of Kings College [Ch.5] into his laboratory. Now the "special object" of studying laboratory physics became:

> ...to afford instruction in the methods of obtaining the numerical data which form the basis, not only of all accurate reasoning upon physical phenomena, but also of all the applications of the principles of physics to Engineering and other practical purposes.
>
> [UCL Calendar,1866,20-21]

A general idea of the kind of instruction afforded to students on "methods of accurately obtaining numerical data" may be gathered from the list of (some of) the subjects taught:-

1) The use of the Balance and methods of accurate Weighing - Modes of determining the Specific Gravity of Solid Liquid and Aeriform bodies - Measurement of the Bulk of Solid bodies, and of the Capacity of vessels, and of the calibre of Tubes.

2) Determination of the Rates of Expansion by Heat in the case of a Solid, Liquid and aeriform bodies - Methods of testing and verifying thermometers - Methods of measuring temperatures, and of determining Specific and Latent Heats.

3) Comparison of the relative intensities of different Sources of Light - Application of the Goniometer, Sextant and Theodolite - Measurement of Indices of Refraction - Applications of Prismatic Analysis and of Polarized light in Chemical Investigations.

4) Construction and use of the most important Electrical and Galvanic Apparatus - Methods of measuring Electrical Currents, Resistance, Quantity [i.e. charge], Capacity and Electromotive Force - Modes of testing Conductors and insulators for telegraphic purposes etc.

[UCL Calendar,1867-68,25-26]

Oliver Lodge testified to a very similar account of the "course of quantitative laboratory instruction through which I was myself put by Prof. Carey Foster in [1874]". Lodge made a "series of well-designed experiments on moments of inertia, on the kinetic torsion of wires, and on determinations of *g* by falling bodies and chronograph as well as by pendulums. We used to measure E.M.F. by the potentiometer method.... the absolute density of liquids,..density of gases;... the usual optical measurements and some less usual;....measurements of electrochemical equivalents etc etc all before 1875" [Lodge,1908,128].

Experimental measurements also played a central role in the formal curricular teaching that Foster gave outside the laboratory; as Fison put it: "..his lecture illustrations would often consist of the actual results of laboratory measurements[20], and the younger students...were apt to lose both attention and interest in the details of laborious computation. The more able students, however, were inspired by this very quality in their teacher. They grew to reverence exact expression and regard it as the foundation of all scientific knowledge" [Fison,1919,424].

Not only would Foster carry out all the measurements he required to legitimate the arguments in his lectures, but occasionally he would actually carry out the experiments for his students as well:

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[20] For an example of Foster's measurement-centred lecturing idiom see [Foster,1869]

> ...he would frequently come to the help of some duffer in difficulties in the laboratory, and would devote the best part of an hour to the details of a simple experiment in physical measurement. On these occasions there was a danger of him being led by his own love of accurate detail, nor only to conduct the whole experiment himself, making all the observations, but to carry out whatever computation might be involved, while the student looked on wonderingly, as from a distance.
>
> [Fison,1919,424]

After some years of operating his scheme of laboratory teaching, Foster was acknowledged by both UCL and his contemporaries as having cultivated an important centre for training in the skills of precision measurement[21]. John Robson, the Secretary of the College, told the Devonshire Commissioners in 1871 that "with respect to the [UCL] laboratory of physics, I may be allowed to remark that the application of exact measurements to various branches of science, such as electricity, is becoming a very important branch of instruction in science" [Robson,1871, q7134]. After Foster's retirement in 1898, one of his successors, Prof. A.W. Porter expanded upon this view of the former's career declaring that "in [his] exiguous quarters during twenty-five years, 1865-1890, Carey Foster built up a great reputation as a trainer of physicists and as an authority on precise physical measurements" [Porter,1925,7]. To understand further these two aspects of Foster's reputation it is important to see how closely his teaching and research in measurement practices were inter-related in the laboratory.

6): Research by measurement in Foster's laboratory

From Lodge's accounts of his work at UCL it is clear that Foster did adopt one particular trait of Thomson's laboratory instruction with his more advanced students by involving them in his research on methods and apparatus of precision measurement, particularly for the B.A.A.S. Committees on several of which he and Thomson both sat. For example Foster was appointed to the B.A.A.S. "Committee for testing the New Pyrometer of Mr Siemens, along with Thomson, Maxwell, Jenkin and others, and Foster's students assisted him in the extensive testing of this precision electrical instrument [B.A.A.S Report,1871,lxx]. Whilst Lodge was testing this pyrometer he was "impressed with the consummate care and scrupulous accuracy demanded by the Professor in this comparatively simple research"; immersed in this laboratory ethos Lodge himself pinpointed a slight loss of the instrument's accuracy resulting from an unforeseen heating effect in its central wire at high temperatures [Lodge,1919,xvi].

Another manifestation of Foster's concern with "scrupulous accuracy" in B.A.A.S. research was for the Electrical Standards Committee which he joined in 1866 [B.A.A.S. Report 1866]. Although Lodge portrayed him as being "always an important member" of this committee, often occupying the chair at its meetings [Lodge,1919,xvi], A.W. Porter counter-pointed this with the comment that "owing to the unobtrusive nature of the man, [his] valuable work will never be known as his to his successors". However, one product of Foster's research in the determination of resistance standards which was unequivocally known to his successors (as his) was the "Carey Foster Bridge".

The eponymous bridge was an adaptation of Wheatstone's original, transformed into a device that measured the *difference* between two resistances rather than their ratio; it was thus an ideal vehicle for comparing sample resistances with a standard [Foster,1872] and was used by the B.A.A.S. in distributing its duplicate standard resistances[22] [B.A.A.S. Report,1883,42; Fison,1919,417]. A.H. Fison's verdict on the importance of the new device was that:

> [it] has transformed the bridge method from being merely a fairly accurate means of measurement into one of the most refined accuracy, comparable with that attained in the use of a sensitive balance......Carey Foster's method has proved of the highest value to the science of exact electrical measurement, and made it possible to issue standards of electrical resistance of an accuracy that would otherwise have been impossible of attainment. Alike in its simplicity and its refined accuracy, the method is thoroughly characteristic of his mind.
>
> [Fison,1919,417]

In addition, Foster used his bridge as a vehicle for training students in the methods of precision measurements: according to Lodge "students were regularly familiarized with it" from 1872 onwards [Lodge,1908,128].

Lodge himself collaborated with Foster on a project that utilized the latter's bridge principle in a determination of the "flow of electricity in a uniform plane conducting surface"[Foster&Lodge,1875]; Foster's young laboratory assistant later declared that with apparatus developed they were "able to plot equipotential surfaces with much greater ease and accuracy than had previously been achieved, either on the Continent or anywhere" [Lodge,1919b,xvi; see also Lodge,1931,94-5]. This tradition of research in precision electrical measurement was continued in the next decade with another student-turned-laboratory assistant, G.W. von Tunzelmann when Foster attempted to redetermine the B.A.A.S. unit of

[22] The Foster Bridge was subsequently institutionalized in this role by R.T.Glazebrook in his custody of the National Physical Laboratory standards.

resistance in 1881 . On this occasion however the problems of delicate measurement in the environment of a busy academic institution were felt by Foster to be insuperable, the redetermination taking place at the Cambridge centre of precision physical measurement: the Cavendish Laboratory [Fison,1919,417; B.A.A.S. Report,1881].

The significance of these insuperable problems can be interpreted in the "political" context of Foster's laboratory within University College. We will deal first with his attempts to secure a laboratory environment conducive to delicate measurement operations: the improvement of laboratory architectonics. Secondly, we will consider his campaign for College and University recognition of his experimental physics teaching: the incorporation of laboratory measurement into the syllabus of the University of London BSc and hence into the derivative curriculum of UCL natural sciences teaching.

7): Laboratory politics and the architectonic position of physics at UCL

In appealing for more favourable working conditions for his laboratory Foster asked the College for two things: firstly for a greater number of rooms to provide separate accommodation for his different classes, and secondly for a measurement environment that was undisturbed by external sources of mechanical and electro-magnetic activity.

Upon its initial creation out of the old lecture theatre in 1866, very little structural alteration had been effected apart from the installation of some workbenches [Ker,1898,67]. The most radical change had been simply the introduction of new equipment into a domain previously occupied largely by experimental apparatus anyway, hence this process was therefore little more than a change in title from "apparatus room" to "laboratory", there being no evidence that the College paid for any alterations to augment the structural "stability" of the room.

In February 1867, however, the College did accede to Foster's request for additional laboratory space by relocating one of the classes held in the next room by the UC School so that Foster could have a room ensuite with the existing laboratory[23] [UCL,Minutes Comm.Man,1866-7]. Nevertheless, Foster was not contented with the experimental environment that these rooms provided: Lodge later described the location of the laboratory as being in "topographical circumstances of some difficulty" [Lodge,1908,128] As can be seen from the plan of the College's first floor [UCL Calendar,1878,frontispiece] the physical laboratory and its 1867 annexe (room 5) were in close proximity to the disturbing influences of busy corridors and staircases.

Foster took the opportunity of voicing his dissatisfaction with both the extent of the accommodation granted him for laboratory work and also the working conditions that the college were able to provide for his measurement researches during his interview with the Devonshire Commission in March 1871. With respect to the first problem Foster told the Commissioners, for example, of "the difficulty of providing accommodation for optical students. That necessitates, of course, a dark room, and as my rooms are limited in number I cannot darken one of them and put one man in

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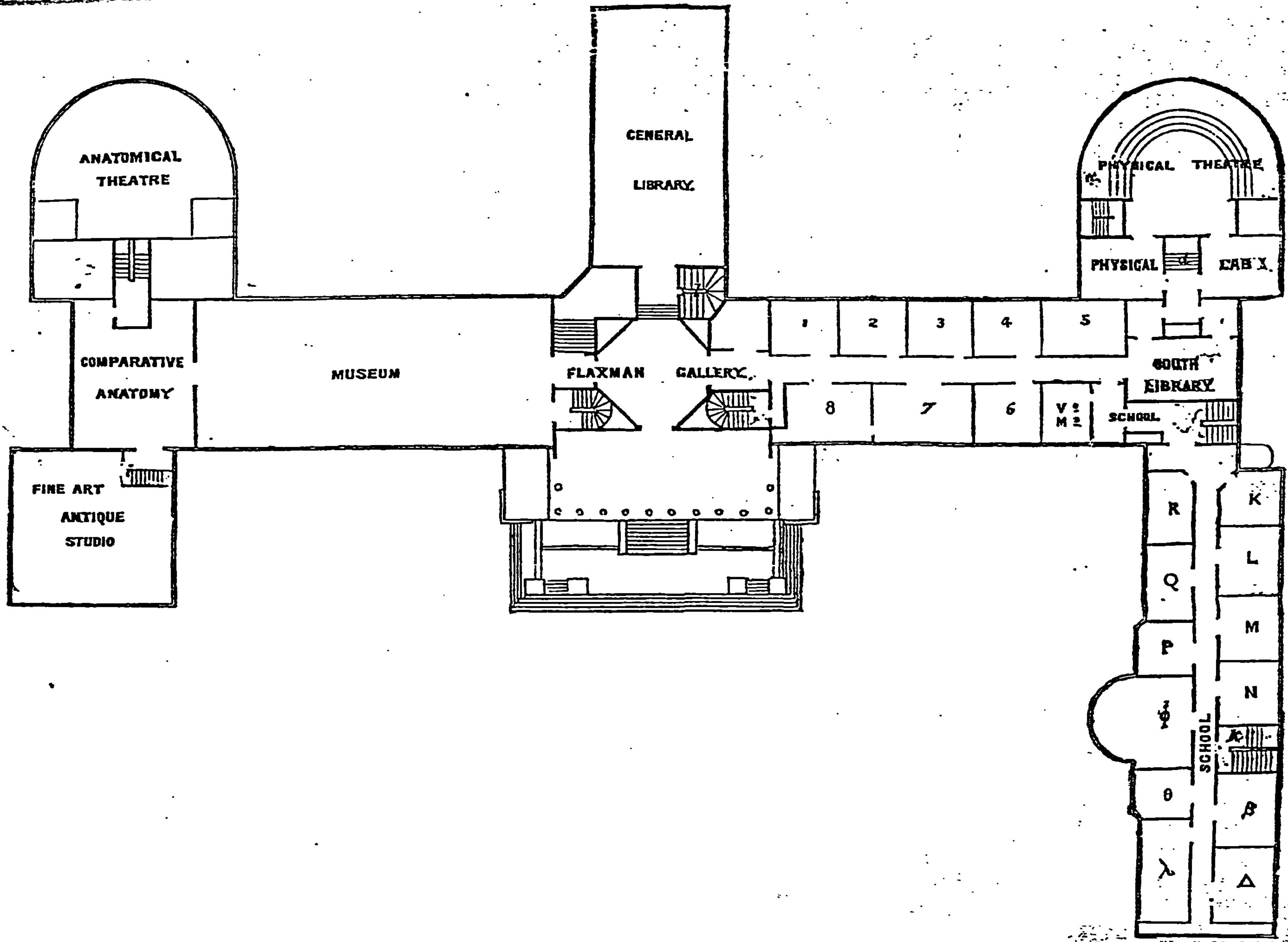
[23] The 1867 Minutes refer to this room as number 53 but it is probable that this was the room Ker referred to as "now No.5 adjoining the physical lecture theatre", added "a year or two" after the main laboratory was opened - see plan overleaf [Ker,1898.67].

FIRST FLOOR.



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the dark when the others are working in the same room at the same time" [Foster,1871,q7824]. As regards the structural instability of his laboratory's work-surfaces he complained that:

> The rooms we have are both insufficent in space and highly unsuitable; they are quite unsteady, and frequently I find that the students are quite unable to go on with their operations; and as a result of that they get discouraged, and from the inaccuracy which is unavoidable in a place that is not steady enough, they get careless in their work, and it operates very prejudicially in that respect.
>
> [Foster,1871,q7799]

In response to such complaints the College did later provide Foster with further accommodation; although the dates of this are not certain, it is likely that the rooms allocated to Foster were ones vacated by the College School upon removal of the latter's classes to newly constructed buildings[24]. It was probably a few years after the Commissioner's interview with Foster that three more rooms were granted: two were on the floor above of which one was used as a workshop for making and repairing apparatus and the third was a room in the basement underneath the Council-room. Later still, two school rooms on the top floor were amalgamated and given over to the Professor of Physics [Ker,1898,67].

Even with this extension of Foster's domain, however, matters were still not entirely satisfactory as the Professor of Architecture, T. Roger Smith recalled in 1894:

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[24] Ker confirms this to be true of two old school-rooms on the top-floor South wing of the College [Ker,1898,67] and from the Minutes of the Committee of Management we know that there was an ongoing campaign to raise money for new school-buildings from April 10th 1867 onwards [UCL Minutes of Committee of Management,5,249].

> The Professor of [Physics] was but indifferently lodged. His classes had not room enough, and they occupied parts of the building which had been designed for other purposes, and were so ill fitted to the requirements of physical observation and experiment that there was no room in his department where the equilibrium of a delicate balance would remain undisturbed if any one walked across the floor.
>
> [Foster et al,1894,281]

Subsequently it was the basement room, popularly dubbed "The Dungeon", that was deemed to be the most important addition to Foster's suite of laboratories by Ker on the grounds that "it was the only place where a steady floor was to be had" [Ker,1898,67]. A.H. Fison commented that this laboratory "was indeed a veritable dungeon.....and the privilege of working in it was reserved for the Professor and students engaged in research" [Fison,1919,423]; A.W. Porter added that along with a few research men a gas engine and a set of accumulators were accommodated in the "Dungeon". Nevertheless Porter qualified his account in a manner that illustrated how research in this laboratory was still institutionally subservient to the requirements of undergraduate teaching:

> Even as late as 1890, the present writer can testify, from his own experience, to the difficulties which research laboured under. Being able to work only a few days a week, his research apparatus was completely dismounted each week in order to make room for the undergraduate worker, and had to be built up again each following week.
>
> [Porter,1925,7]

In addition to the difficulties of this discontinuous research and the consequent problems of recalibration for the measuring instruments, it is clear also that this laboratory still did not satisfy Foster's requirement of complete electro-magnetic insulation for the most delicate measurement operations, despite being the only place where a steady floor was to be had. This was manifested in his 1881 attempts to develop an improvement in the original B.A.A.S. apparatus for determining the standard of resistance which was at that time in use again at the Cavendish Laboratory by Rayleigh and Schuster in their own redetermination of the B.A.A.S. 1867 Standard[25] [B.A.A.S. Report,1881,424 &426]. (The modification of the B.A.A.S. set-up that Foster introduced enabled a more direct "in situ" determination of the resistance of the standard coil against which the revolving sample coil was compared [Foster,1881b,426-30; Fison,1919,418-19]).

It was Foster's intention that these experiments would only ascertain the extent to which his method was capable "when employed under favourable circumstances, of giving good results" rather than necessarily to furnish the B.A.A.S. with a new determination of the standard itself[26] [Foster, 1881b, 431]. In this respect Foster considered the results "fairly satisfactory", although the Standards Committee did not subsequently adopt his modification in the Cavendish experiments. Nevertheless it was quite clear that Foster did not consider that his laboratory environment could provide the requisite "favourable circumstances" for such a determination. He complained, for example that an attempt to repeat one important magnetic measurement "was made useless by some large mass of iron being brought just outside the laboratory while [the repetition] was going on" [Foster,1881b,427]. With the privilege of hindsight, Fison commented in connection with this that Foster's results would have been more consistent

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[25] This standard had been deemed an underestimate upon subsequent re-examination by the B.A.A.S. Committee in the late 1870's and early 1880's [B.A.A.S. Report,1881,423; Fison,1919,417]

[26] Fison seems confused on this point, suggesting that Foster "abandoned" an attempt at redetermining the standard because the results were "not sufficiently consistent to satisfy [Foster's] critical judgement" [Fison,1919,419].

"if they had been repeated in a modern laboratory more completely removed from the disturbing magnetic influences of large masses of iron of constantly varying temperatures" [Fison,1919,418-19].

Foster's "circumstances" were not only unfavourable as a result of his lack of institutional control over disturbing influences in the proximity of his laboratory however: he also complained of the lack of a proper governor for the gas engine driving the rotating coil in this experiment, and of proper timing equipment and resistances for his accurate determinations. Thus lacking the working environment and the equipment that he deemed necessary for precision measurement, it is perhaps significant that after this 1881 paper Foster published material only on *methods* and *apparatus* of measurement rather than making any further attempts at accurate quantitative determinations himself. Such papers that he produced were thus for example on "a method of determining constants of mutual induction" [Foster,1887] and also "a note on the constant volume gas thermometer" [Foster,1897a].

Foster continued to campaign, however, for a purpose-built laboratory and in the early 1890's, integrated his appeals with those of his Professorial colleagues T.H Beare (Mechanical Engineering) and J.A. Fleming (Electrical Engineering) for laboratory accommodation to match that available at other academic institutions [Fleming,1934,112]. At the meeting of the UCL Senate on November 7th 1891, letters were received from Profs. Beare and Fleming "urging the Council to begin the construction of new Engineering and Electrical Laboratories without delay." At this meeting Foster successfully moved that "a committee be appointed to consider the requirements of the college in relation to the teaching and promotion of the scientific study of Physics, Electrical Engineering and

Mechanical Engineering and to report to the [UCL] Council on the best way of practically meeting these requirements" [UCL Minutes of Senate,7th Nov. 1891]. Foster used his position on the Committee subsequently formed to achieve the recommendation that not only should there be engineering and electrical engineering laboratories built at the end of the South Wing of the main building, but also that the "present engineering laboratory with an annexe to be built in the South Quadrangle should be converted into a physics laboratory" [Minutes of Senate, 23/1/1892].

The UCL Council adopted this Committee's recommendation to build a new extension housing laboratories for all three departments in early February 1892 and commissioned its Committee of Management to procure plans and tenders for their construction as well as to raise a subscription fund to meet the cost [UCL Minutes of Council,Jan-Mar 1892].

Of the laboratory building completed two years later the architect Roger Smith confessed to his own specific interest in this construction as being "to improve the lighting of what had hitherto been a dark - or at best a very unequally lighted department" e.g. "The Dungeon". Nonetheless Foster acknowledged that both Roger Smith and Elsey Smith, the contractor, had gone to "extremely great trouble...to meet his [i.e Foster's] views in the planning of the Physical Department of the College in every detail that presented itself" [Foster *et al,*, 1894,287 & 305]. The ideals which Foster thus realized in his new laboratory were those of a location removed from the disturbing influences of the external world and a construction that furnished a steady base, despite any remaining disturbances to which the measurement surfaces might be exposed:

> There is no heavy traffic within a considerable distance of the buildings, and, for a site in the heart of London, the soil is fairly free from tremors. The special precautions taken with a view to steadiness are as follows:- The floor is formed of wooden blocks laid directly on a bed of concrete 6 inches thick; stone slabs project from the walls in various places to serve as supports for instruments; short wooden beams are built into the walls near the roof, and serve as firm supports for anything that has to be suspended from above; lastly, breeze bricks are let into the (unplastered) walls at intervals, both horizontally and vertically.....by means of [which] a firm attachment to the wall can be obtained...
>
> [Foster *et al*, 1894,301]

W.P. Ker wrote in 1898 of this construction: "this building is well-lighted [c.f. the "Dungeon"] and steady and has proved to be admirably adapted to its purpose" and as a physics laboratory was "probably as complete and well-adapted as is to be found anywhere in the kingdom" [Ker,1898,68]. And by May 1893, the mechanical engineering and electrical engineering laboratories were equally furnished as purpose-built centres of physical measurement in the testing of industrial materials and electromagnetic instruments respectively [Nature,48,107].

Foster retired five years after thus successfully negotiating his claims upon University College for physical laboratories that satisfied his stringent requirements for the pursuit of undisturbed measurement in research and teaching. In his account of UCL physics, written as it was in the year of Foster's retirement, W.P Ker gives a contemporary's summary of Foster's career at UCL in a manner that appropriately concludes this section and introduces the subject of our next section - Foster's campaign for the *curricular* recognition of laboratory measurememt at UCL:

> In 1865, [physics] teaching was carried on, not only in University College, but in every College and University in the country, solely by experimental or mathematical lectures; now the systematic performance of experiments themselves constitutes everywhere an essential part of the course of instruction. The gradual increase of the space devoted to the teaching of Physics in the College and the recent [1892 and 1893] reconstruction of the accommodation afforded to this subject have resulted from the persistent endeavours of the Professor to make the practical instruction more and more complete and satisfactory. At the present time all students of Experimental Physics are expected to go through an elementary course of practical work in making simple physical measurements.
>
> [Ker,1898,68-69]

The relationship between the architectonic and the curricular status of Foster's physics laboratory throughout the preceding three decades had been an intimate one: without institutional recognition of laboratory work as a *compulsory* component of UCL physics teaching in the Faculty of Arts and Sciences, Foster could not negotiate better facilities and a more amenable environment for his laboratory's operation. The next section will thus deal with Foster's attempts to extend the curricular position of *practical* physics and thereby to increase the size of his laboratory audiences and staff. Reference will be made to both internal recognition of physics by UCL and the external growth in public demand for practical instruction in physics from trainee telegraphists and school physics teachers.

8): Laboratory politics and the curricular position of physics at UCL.

The curricular position of most subjects in the Faculty of Arts and Sciences at UCL was generally determined by the examination requirements of the University of London Bachelors degrees: in 1871 more than half the students in this Faculty were preparing for University of London examinations [Foster,1871,782]. Indeed it was certainly Foster's firm belief that the unfavoured curricular position of experimental physics at UCL was a result of the regime held over the College's teaching by the "antiquated" University BSc and BA Regulations [Foster,1874a,525]

As Dean of the Faculty in 1874 he bitterly complained of the position of Physics in the first and second examinations for the BSc degree: only slightly more knowledge of physical subjects was required for the second than the first BSc examination - the second being a *mathematical* paper prepared for in the UCL class of Applied Mathematics [Foster,1871,7831], and that the *same* paper in "Mechanical and Natural Philosophy" was set in the second examinations for the Bachelor of Science as the Bachelor of Arts degree. Foster declared "it is certainly strange that a degree in Science should not imply any greater acquaintance with the fundamental principles of Mechanics than is demanded of candidates for the Bachelor of Arts, the examination for which is in the main literary and classical" [Foster,1874a,526].

In his earlier interview with the Devonshire Commission in 1871 Foster had been particularly explicit about this:

> It appears to me that the examinations of the University tend to discourage any more than a very elementary study of the subject....the only examination in which experimental[17] physics is required at all are the first examination for the Bachelor of Science degree and the Preliminary Scientific medical examination...After that, unless they proceed to the exceptional degree of Doctor of Science, they are not, by the University regulations obliged to study physics any more.
>
> [Foster,1871,q7783]

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[17] The term "Experimental" is used here to distinguish the qualitative physics in the First BSc examination from the "Mathematical" physics in the Second BSc examination. "Experimental" did not refer as such to any requirement for *practical* work in the laboratory [Foster,1871,q7831-35].

Thus while Foster had an annual attendance of 60-70 students (1865-71) for his junior class in preparation for the First BSc. Examination, he never had more than eight students in his senior class for laboratory work: no laboratory study of physics nor in fact any other "advanced" classes in physics were required for any students going on to the Second BSc examination. In 1871 he had in fact five senior students working in the laboratory of which only *two* were preparing for any of the University Examinations [Foster,1871,q7786-7790] and neither of these two were preparing for the only University degree which did require laboratory work viz. the DSc. [Foster,1871,q7826]. Foster therefore agreed with the Commissioner's suggestion that *were* a larger amount of experimental physics to be required from a candidate for the degree of Bachelor of Science, his laboratory class for senior students would be more largely attended [Foster,1871,q7790-91]. He thus demanded that the Second BSc Regulations should include both a written and a *practical* examination in physics as was required in chemistry and as was currently required for physics in Oxford University's school of Natural Science [Foster,1871, q7832-7835].

Although Foster felt that the University Regulations themselves were a major cause of the low demand for an "extended" course of physics at UCL, he was heartened not only by the interest of apprentice telegraphists in his courses,[28] but also by the increased numbers of prospective science teachers attending his laboratory: Foster explained that the rapidly

[28] A figure of between 20 and 30 students undergoing training at UCL for the Indian Telegraph Service was alluded to in the evidence of the College Secretary, John Robson, to the Devonshire Commission [Robson,1871,q7139]

growing demand for teachers of physics could not be supplied by existing institutions [Foster,1871,q7793-95]. Naturally Foster took the opportunity of emphasizing how laboratory work could serve to improve the preparation of school-masters, especially of mathematics, who generally took to physics as a subject "which they could easily get up in a short time" [Foster,1871,q7796]. Thus in response to the Commissioner's question "Do you consider that practice in the use of apparatus and in the methods of observation is essential in a thorough course of physics?", Foster declared:

> I think it is quite as essential in the case of the study of physics as in the case of chemistry, where it is always admitted to be of importance, and especially for teachers. I think it is absolutely essential that they [i.e.teachers] should know by personal experience and familiarity the phenomena that they deal with in their teaching.
>
> [Foster,1871,q7798]

We observe here two of the typical rhetorical devices used to legitimize the innovation of laboratory work: the assertion of a parity in teaching methods between physics and chemistry, and the assertion also of the absolute necessity that physics teachers have experimental familiarity with their subject to instruct their pupils properly [Ch.1].

Such was the demand that Foster perceived for teachers of physics that he expected little difficulty in getting men aspiring to such careers to provide him with unpaid laboratory assistance:

> I believe that it will be easy to get young men to aid in the teaching of students for little or no remuneration; for instance, that men who are looking forward to employment in schools as teachers of physics would be glad of the opportunity of assisting at lectures, in the preparation of apparatus, and in experiments, for the sake of the mere experience that they would gain. I have recently had...a gentleman, who is a teacher of physics at one of the large public schools, working in my laboratory, simply for the sake of familiarizing himself with the use of the apparatus. [Foster,1871,q7845]

The demands for laboratory teaching made upon Foster were such that he considered there to be work enough for six such assistants besides himself in giving practical instruction to his students, and thus he argued for a system of paid assistance, modelled on the German plan of *Privat-Docenten* [Foster,1871,7846]. However, since Foster's laboratory had no formal position in the University College curriculum this expansion of personnel was not feasible; hence his main gambit was to promote the study of experimental physics in relation to the *liberal education* embodied in the statutes of the examining University of London.

9): Physics and the University of London liberal education

As Dean of the UCL Faculty of Sciences in October 1874, Foster gave an inaugural address in which he explicitly stated his view of the ideal position of physics within the University of London Examinations which so comprehensively determined much of the College curriculum [Foster, 1874a]. Primarily this speech was a critique of the University's enactment of the Royal Charter of 1837 which specified its purpose "to hold forth to all classes and denominations....an encouragement for pursuing a regular and liberal course of education....ascertaining, by means of examination the persons who have acquired proficiency in Literature, Science, and Art, by the pursuit of such a course of education[29]..." [Foster,1874a,506]. It was Foster's contention, as both a teacher and perforce as an examiner,

[29] In the revised charter of 1858 separate degrees in Arts, Laws, Science and Music were created and at the same time the requirement that examinees study at no other college than University or King's was also abolished.

that the examinations set by the University did not give the proper encouragement to a liberal education in physics [Foster,1874a,507]. It was his belief that these "examinations should be improved *in order* that teaching may be improved through their influence" [Foster,1874a,527].

He criticised the examinations firstly for not reflecting the advanced status of physics, in both the omission of the subject from the highest level papers in the second BSc, and also through the framing of their regulations and questions in long-outdated subject divisions. As far as the advanced state of physics was concerned Foster claimed that:

> It is of course because physical phenomena are simpler and more accessible to investigations than those of Chemistry or Biology, that greater progress has been made in the study of them, and that the explanations that have been reached are of a higher degree of certainty and generality: but it is precisely the relatively advanced stage which has been reached by it that gives to the study of physics its high value as an element in general education, and is the reason why it furnishes us with fuller and more instructive examples of scientific reasoning than other sciences.
>
> [Foster,1874a,507]

Thus according to Foster, the extent to which physics was approaching *closure* rendered the subject suitable for the *highest* position in science teaching and examinations.

Given this view of the advanced state of physics' disciplinary completeness, Foster thus objected to the "archaic" categories within which physics was examined, remarking that the subjects required for Matriculation viz: *Mechanics; Hydrostatics, Hydraulics and Pneumatics; Optics and Heat* , (recently transferred from chemistry) read like "the table of contents of an elementary treatise on Natural Philosophy published about a hundred years ago" [Foster,1874a,525]. He further objected that the quantitative methods which had brought about this state of near completeness were a *fortiori* unrepresented, for despite the fact that the First BSc examination now included the recently developed subject of electricity "there is no distinct reference to any of the quantitative laws of the science," there being only the obscurest allusion to Ohm's law and under Heat "no liberality of interpretation could detect the smallest trace of the Dynamical Theory of Heat" [Foster,1874a,526].

However, the most deleterious consequences of casting examinations in this 18th century mould were, Foster argued, that no allowance was made for "progress or improvement in the means of teaching long-known truths", which led to his greatest criticism of these examinations as a vehicle of liberal education: that they encouraged *cramming* [Foster,1874a,507]. Foster contended that these Regulations "cut up the subjects to which they relate into a number of detached propositions.....[which teachers and students then]...generally treat as independent units of knowledge each of which is to be put into a separate hole of the memory." From his experience of reading examination papers he declared that such traditional methods of pedagogy were inappropriate to the study of physics: "it is impossible not to regret that the same method[30] should be employed in learning what is called Science, as in learning the dates of accession of the Kings of England" [Foster,1874a,525-526].

Foster thus made a pointed contrast between this rote acquisition of facts and the powers of *precise reasoning* inculcated by a proper education in physics: "it may be confidently asserted that, for training the mind in habits of accurate thinking, no other study can be compared

[30] Foster reported that a "late, very distinguished member of the University" once asserted that all that could be tested of candidates in the Matriculation Examination was evidence of the "correct acquisition" of Physics [Foster,1874a,527]

with that of Physics if properly pursued" . And it was in speaking of the "exactness" with which the conditions of physical phenomena's occurence had been ascertained, and the "precision" with which the laws relating them could be described, that Foster attributed to physics a primary place in an ideal scheme of liberal education, making an unambiguous allusion here to the practice of accurate laboratory *measurement* for students as the specific medium for communicating these pedagogical virtues [Foster, 1874a,507].

As an appeal to the University of London Senate to reappraise the position of physics in the BSc regulations, this Faculty lecture was not the only medium exploited by Foster for promoting its disciplinary value in *accurate thinking* in the context of liberal education. We find similar arguments in his preface to an English translation of A.F. Weinhold's Introduction to Experimental Physics, Theoretical and Practical [Weinhold, 1875].

In this preface Foster expanded upon his approach to teaching physics with a view to explaining the difficulties he had encountered in teaching physics practically: in physics, students had to accustom themselves to a kind of knowledge that was "much more accurate and precise than we are accustomed to be satisfied [with] in matters of ordinary life." However in being obliged to adopt "new habits of learning", in for example attaching "accurately defined meanings to the terms employed in discussing physical phenomena" great benefit acrued to the student. Echoing his speech to the UCL Faculty in the previous year he proclaimed that:

> These characteristics of the study of physics, give it a value, as a method of training in habits of exact thinking, which probably no other study possesses in the same degree, [although] at the same time they make this study more than usually difficult, especially to beginners.
>
> [Foster,1875c,v]

Emphasizing the *practical* component of physics teaching he argued that:

> ...a great part of the the mental discipline which the study of physics is capable of affording depends upon our being convinced, through direct personal observation, that the general laws of the science represent conclusions truly derived from an accurate examination and comprisal of the impressions which the actual phenomena make upon our senses.
>
> [Foster,1875c,viii]

It was thus in terms of the unique mental discipline obtained from the *experimental* and *precise* study of physics, both features characteristic of his UCL courses in laboratory measurement techniques, that Foster canvassed for the wider educational recognition of his subject. Campaigning in this manner from the positions of both teacher and examiner, Foster subsequently achieved his ambition of effecting reforms in the London University BSc Regulations in 1876: within two years of his speech at the Faculty of Sciences and a year after publication of Weinhold's textbook.

The Committee commissioned by the Senate of London University to propose revisions of the BSc, reported in that year that the regulations adopted eighteen years previously were "not well adapted to the requirements of scientific education as now conducted." These regulations were designed to ensure that the holder of a BSc possessed a "general culture" in a broad range of sciences instead of being a "mere specialist"; since 1858 however almost every department of science had undergone a "higher development" rendering the mastery of of fundamental principles less easily attainable. The Committee thus recommended on the one hand that the examinations should be bifurcated into physical and biological sections (according to the apparent division of interests amongst nearly all science students) but on the other hand that both sections should additionally be examined *experimentally* since it had "come to be generally felt that scientific knowledge, to be *real*, must be *practical*, as well as theoretical" [Nature,14,332] .

In recommending a higher level of specialization for the Second BSc examination so that it incorporated Experimental Physics as one of its major options, as well as recommending the introduction "of an efficient *practical* examination in each of the subjects in which it is feasible", the fulfilment of Foster's scheme is evident. Reference to his involvement in these revisions is apparent in the Committee's comment that "several of the most able teachers in institutions connected with the University, and of its most experienced examiners (past and present) concurred" in the recommendations presented to the Senate. Nature too, in jubilantly reporting these University affairs, alluded to the recent development of laboratory teaching carried out by academic physicists such as G.C. Foster and academic biologists such as Michael Foster as formative in the University's plans:

> It is not...only experience in the examination of science students which has led to the necessity for change, but [also that] the stimulus which has been given to the teaching of physics and biology, by the founding of science degrees and otherwise, has so altered the method of teaching these subjects that what was expected to be known formerly is quite different from that taught by the most able exponents of the subjects at the present time.
>
> [Nature,14,331]

The University subsequently enacted these recommendations, thereby assimilating laboratory physics into the courses of a liberal education at associated institutions. Specifically it rendered the University College Physics Laboratory a functional part of the teaching curriculum: Foster's students at UCL soon began to compete for the BSc examination in Experimental Physics when it was first held in 1877, and between 1878 and 1886 fourteen of Foster's students were awarded First, Second or Third Classes in this subject [University of London,1912,503-507].

When Foster acquired his purpose-built suite of laboratories in 1892-3, he was then able to effect a much more general requirement for laboratory study on his pupils since there was now accommodation for practical instruction in both junior and senior classes. All the students in his general elementary course were expected to participate in one and a half hours of work per week in the basement laboratory and there was space for thirty-two such workers of whom Foster commented: "this number, working two together, all make the same experiments at once. These experiments consist almost exclusively of simple measurement operations.." [Foster et al,1894,300].

Conclusion

The overall pattern of Foster's early career as a man of laboratory science can be summed up as a transference of his skills in accurate quantitative chemical analysis to the precise practices of delicate physical measurement in the physics laboratory. These skills, as well as his interest in the interdisciplinary subjects of Heat, Electromagnetism and Light were cultivated under the guidance of Prof. Williamson in the UCL Birkbeck Laboratory. Pursuing these physical subjects further in the laboratories of Bunsen, Jamin and Thomson led Foster finally to forge these interests into a specialization in natural philosophy at the

Andersonian Institution in Glasgow. This in turn led to Foster's return to UCL, taking with him the cherished laboratory expertise of his assistant William Grant in setting up a teaching laboratory which circumstantial evidence suggests was an attempt to emulate something of the student participation that he had observed in William Thomson's laboratory at the University of Glasgow between 1862 and 1865. Although Foster only allowed experienced physics students into the laboratory and placed more emphasis than Thomson on systematically training students in standard techniques of precise measurement, Foster's work matched that of the Glasgow professor in engaging volunteer students in his laboratory researches for the B.A.A.S. measurement committees of which both professors were leading members.

As Professor of Physics at University College, London, Foster established his physical laboratory at first as only a marginal voluntary activity in the teaching of practical physics; this was accommodated in a makeshift experimental environment in which disturbing external influences from the bustling College life deleteriously intruded upon his measurement practices. However, by asserting the pedagogical values of precision measurement practices, and the importance of granting a dominant curricular position to physics in terms of its acknowledged advanced state, Foster was able over a period of several decades to negotiate with College authorities for the laboratory facilities that he desired. These were an isolated suite of rooms with foundations sufficiently stable and independent to enable the most delicate of measurement procedures to be carried out by he and his students without interference from sources of external mechanical or electromagnetic activity. This suite was sufficiently extensive for all his junior and senior students to undertake a systematic course of practical study which had been granted recognition by the College and University establishments as essential to an education in physics.

In the light of this we can conclude by citing the paean of praise given in an anonymous obituary of Foster after his death in February 1919:

> [Carey Foster] laid the foundation of the physical laboratory as it exists today. When he himself was educated, laboratory work, as we now know it, did not form part of any curriculum. But, about 1866, in two rooms in his college, he created the first physical laboratory, in which students might repeat the standard methods of measurement which were then being rapidly developed especially on the Continent - and be taught the conditions for success in such measurements. Cabinets of physical apparatus had existed before, but these were intended for the illustration of lectures. The spirit of change was in the air and physical laboratories sprang up in many directions. At the present day lectures without laboratory work are a deadly anachronism, even for, or particularly for, junior men.
>
> [Nature, 102, 489-90]

CHAPTER 5

William Grylls Adams at King's College London: Laboratory Measurement For The Engineer.

> The rapid progress in the discovery of the principles of electricity, and in its practical applications, is especially due to the fact that those who have taken the lead, both in theory and in practice, have been men who have seen the full importance of accuracy of measurement in all that pertains to physical science, and, by the instruments which they have invented, have themselves greatly contributed to the means of attaining that accuracy in the measurements of electricity.
>
> William Grylls Adams: 1884 Presidential Address to the Society of Telegraph Engineers [Adams,1884,11].

Introduction

This chapter will chronicle the creation and development of William Grylls Adams' physical laboratory which opened in the Applied Sciences Department at King's College, London in 1868. In this metropolitan laboratory a significant proportion of the ascendant generation of "scientific engineers" received a training in the techniques of precision measurement. As the laboratory-based alternative to the traditional workshop practices of "rule of thumb", this training in precision measurement served in the long term to regenerate British mechanical and civil engineering as "scientific" professions.

Attention will also be given, however, to the exceptionally broad basis of the King's College student clientele in this account, considering especially that as the offical *Anglican* rival to its sister University College, King's College effectively monopolized the section of the student population intending to go in for Oxford and Cambridge. Until the late 1860's the students taking this route to the Universities had little to do with the Department of Applied Sciences, in whose jurisdiction Adams' physical laboratory lay, since this portion of the King's clientele had hitherto involved only the students in the classically aligned Department of General Literature [Cunningham & Miller,1868,q3291]. After 1869 we will find, however, that an audience preparing for Oxbridge found its way into the physical laboratory and undertook Adams' courses in measurement techniques as part of a *liberally-oriented* scheme of education.

With respect to Adams' laboratory clientele it is important to contrast the scenario at King's with the situation described in the previous chapter at University College: since the Anglican college drew upon the lucrative Oxbridge market and the source of trainee engineers mentioned above, King's did not need to recruit as heavily as University College amongst students reading for the University of London B.A. and B.Sc examinations [Adams,1871e,6896]. Hence Adams was not subject to the same external curricular constraints as was G.C. Foster at Gower Street in his teaching of experimental physics. Moreover, since the socio-economic climate after the 1867 Paris Exhibition increasingly favoured an industrial training grounded in experimental science, Adams was able to negotiate both a strong curricular position and also substantial institutional resources for his practical physics teaching from the time that his laboratory opened in 1868. Thus whereas Foster's career was documented in the previous chapter as a long-term campaign for minimal teaching resources and a stable laboratory environment, we will see that Adams' requirements were on the whole readily met by the KCL Council.

Thus for KCL we have a considerably less controversial and convoluted laboratory history than our account of UCL and since more archival information is available upon the origins and creation of the King's laboratory than about its Gower St. counterpart, this account will specifically focus upon this aspect of academic territorial politics. Although this degree of archival detail is not matched by information from contemporaries on how they saw Adams' evolving academic regime, Adams' work will be linked with that of his Cambridge mentor George Gabriel Stokes, and with that of his undergraduate contemporary Robert Bellamy Clifton. Of Clifton's influence in the genesis of the King's laboratory, Adams very suggestively remarked in 1871 that "Prof Clifton was the first to propose, more than three years ago, that a course of training in a physical laboratory should form a part of the regular work of every student of physics. This system was adopted and at once put into action at King's College" [Adams,1871d,323].

In documenting the background to Adams' professorial career at King's we will first of all discuss Adams in relation to fellow Cantabrigians Stokes, Clifton and William's elder brother the astronomer John Couch Adams.

1):Biographical context of William Grylls Adams.

Born in 1836 to an ancient line of Cornish farmers, William Grylls followed the precedent set by his precocious elder brother John Couch Adams[1] in entering St John's College, Cambridge in 1855 to study for the Mathematics Tripos. In 1857 he attended, like many of the elite Tripos mathematicians, the experimental lectures of Stokes, the Lucasian Professor of Mathematics; as Lord Rayleigh later commented: "for many years [Stokes] ran a course on Physical Optics, which was pretty generally attended by candidates for mathematical honours" [Rayleigh,1903,211].

Although the Natural Sciences Tripos had been extant since 1849, Stokes' lectures were the only source of experimental science that mathematics students experienced at Cambridge[2]. Hence in considering Sviedrys' analysis of those Wranglers who subsequently became academic

[1] John Couch Adams won a sizarship to St John's in 1839 and subsequently graduated Senior Wrangler in 1843 as well as 1st Smith's prizeman. He became a Fellow and tutor of the college shortly afterwards whilst working on astronomical observations which led, after some controversy, to his recognition as co-discoverer of the planet Neptune in the mid 1840's. William Grylls edited his brother's scientific papers into two volumes after the latter's death in 1892 [DSB: John Couch Adams].

[2] See the comments of Clifton to the 1868 Select Committee [Ch.6].

laboratory physicists it is vital to go beyond Sviedrys' emphasis on the credentials of these laboratory physicists as *Wranglers* [Sviedrys,1976, 432] to note that whilst training for their Wranglership, almost all[3] had attended Stokes' lectures on experimental physics:

Name	Laboratory	Year attending Stokes' lectures
P.G. Tait	Edinburgh,1868	1851;
J.C. Maxwell	Cambridge,1874	1853;
R.B. Clifton	Oxford,1870	1856;
J.W. Strutt(Rayleigh)	Cambridge,1879	1864;
W. Garnett	Nottingham,1882	1871;
W.M. Hicks	Sheffield,1883	1872;
J. Larmor	Galway, 1880	1879;
A. Schuster	Manchester, 1887	1879;
J.J. Thomson	Cambridge,1884	1879;

[Stokes,1850-1880,NB1; Sviedrys,1976, 416 & 432]

To explain the spawning of these physicists from the population of Cambridge mathematics students, it would be more relevant to characterize Stokes' as the scientific mentor of this generation of Wrangler-physicists. As P.G.Tait eulogized in 1875:

> Prof.Stokes may justly be looked upon as in a sense one of the intellectual parents of the present splendid school of Natural Philosophers which Cambridge has nurtured - the school which numbers in its ranks Sir William Thomson and Prof. Clerk Maxwell....when [these Natural Philosophers] were able, as it were, to walk without assistance, they all (more or less wittingly) took Stokes as a model. And the model could not but be a good one: it is all but that of Newton himself. Newton's wonderful combination of mathematical power with experimental skill...lives on in his successor.
>
> [Tait,1875,201]

The status of Stokes as a mentor to this generation of academic physicists is borne out by the size of the extant correspondence between

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[3] W. Thomson(Glasgow) graduated before Stokes began his appointment as Lucasian Professor although he nevertheless cultivated a very close dialogue with Stokes in all his physical investigations; Alexander Herschel (Newcastle-upon Tyne,1875) does not appear in Stokes' course register for any of the years that he was in Cambridge as an undergraduate.

Stokes and his former students[4]; yet although his lectures furnished them with a "model" of interactive experimentation and mathematics [Ch.6], Stokes did not give his students the impetus to "experiment for themselves" as we noted from Rayleigh's reminscences in chapter 2. Indeed, as Stokes' pupils Adams and especially Clifton followed his conservatism in limiting the access of their laboratory students to sophisticated apparatus to a far greater extent than their Scottish contemporaries Thomson and Tait. However, a specific characteristic of Stokes' experimental work which was used as a "model" by his students lay in the optical demonstrations which he gave during the months of spring when natural sunshine was available for theatre experiments. As J.J. Thomson recollected of his Cambridge youth:

> The lectures I enjoyed the most were those by Sir George Stokes on light. For clearness of exposition, beauty and aptness of the experiments, I have never heard their equal. *He had only the simplest apparatus at his command, no light* but that of the sun, no assistant to help him. He prepared the experiments himself before the lecture and performed them himself in the lecture, and they always came off.
>
> [Thomson,1936,48]

Elsewhere Thomson wrote that Stokes' lectures were "more physical" than those of Professors Cayley and J.C. Adams, and that these physical experiments "succeeded with a precision that I have often envied" [Fitzpatrick, Whetham *et al*, 1910,79]. This special expertise in optics and the didactic "precision" with which they were executed will be themes running throughout this chapter and chapter 6 when we consider how Adams and Clifton "wittingly" took Stokes as their model of a mathematician-experimentalist.

Adams attended Stokes' Lucasian lectures in 1857, the year after

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[4] See ULC ADD MSS 7656.

Clifton and thereby became versed in experimental hydrostatics, hydrodynamics and optics [Stokes MSS,NB1; Adams to KCL Council, 1865, KA/IC/52] graduating Twelfth Wrangler in 1859. Adams later intimated that his position might have been higher had he not been ill during the final Senate House examinations [Adams to KCL Council, 1865, KA/IC/52]. His colleague and later obituarist George Carey Foster relates that after leaving Cambridge he acted for a year as Vice-Principal of the Peterborough [Teacher] Training College and was then mathematics master at Marlborough College from 1860 to 1863 [Foster,1915,lxiii]. Although he continued his mathematical studies at these institutions, writing a paper on "the rectangular hyperbola" for the Messenger of Mathematics [Adams, 1862], it is clear that Adams' interests shifted away from pure mathematics to the Stokesian domain of natural philosophy, for in the summer of 1863 Adams applied for a lectureship in natural philosophy at King's College, London - a post supportive of James Clerk Maxwell's teaching as Professor of Natural Philosophy.

On July 21st of that year, G.R. Smalley, the incumbent lecturer in natural philosophy[5] had resigned his position and ten applications were subsequently received for the vacancy [KCL Council Minutes,1(1863),165 & 169-70]. In applying for this post Adams later commented:

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[5] George Robert Smalley, like Adams and Maxwell , was also a Cambridge man, 28th Wrangler in 1845. Smalley was appointed in 1862 to the lectureship created in that year to "lighten the labours of the Professor of Natural Philosophy" (see later). [Domb,1985,71; KCL Report,April,1862].

> I was induced to do so, not by the remuneration offered (for in doing so, I made a sacrifice of more than £100 a year) but because the subject of Natural Philosophy was more attractive to me as being more interesting in the teaching, and as affording a wider field and greater opportunities for research.
>
> [Adams to KCL Council,1865, KA/IC/52]

Although there are no reasons recorded in the Minutes of KCL Council for their final choice of candidate, it is quite possible that they appointed Adams instead of the only other major contender, the Rev Robert Dell of Corpus Christi College, Cambridge, because of this manifest enthusiasm and determination to broaden his vistas in teaching and research. Indeed, after his taking up his post at King's in October 1863, he apparently turned his attention "more especially to the higher branches of Natural Philosophy, and, to obtain a more practical acquaintance with the subject...attended Professor Tyndall's experimental lectures [at the Royal Institution] in order to prepare for the evening class lectures that it was his duty to provide at the college" [Adams to KCL Council,1865, KA/IC/52].

In his centenary history of King's College, F.J.C. Hearnshaw suggests that Adams was appointed to be lecturer under Maxwell in 1863 to alleviate the discipline problems that the latter experienced in conducting the natural philosophy classes, or as Hearnshaw put it: "so as to relieve him [Maxwell] of the presence of the more turbulent disturbers of the peace" [Hearnshaw,1929,247]. No evidence is presented by Hearnshaw, however, to substantiate the claim that Adams was appointed specifically for his skills as a disciplinarian. Domb in particular makes further criticisms of Hearnshaw's account, particularly with regard to the circumstances attending Adams' appointment as professor after Maxwell's resignation in

1865[6] - see below [Domb,1985].

Hearnshaw relates that after Adams' appointment "things, however did not greatly improve, and there was obviously not enough going on, apart from noise, to give employment to two teachers; hence early in 1865 it would appear, an intimation was conveyed to Clerk Maxwell that he should resign". Although Maxwell's resignation on February 1865 was amicably accepted by KCL Council, the Council's unincriminating resolution on the matter was interpreted by Hearnshaw as being "as eloquent in its omissions as in its expressions" [Hearnshaw,1929,247-248]. This allegation that the Council was discreetly satisfied at Maxwell's departure rested on the 1927 reminiscences of Grylls Adams' first laboratory demonstrator of 1868, Richard Abbay, the uncritical use of which evidence Domb takes great exception to in his account of Maxwell's tenure at King's College. Domb declared it to be "surprising" that Hearnshaw should give such a "completely negative [sic][7]" assessment of Maxwell's association with King's in the centenary history of the College.

As the only piece of recent scholarship on physics at King's College in the period concerning this thesis, Domb's account of Maxwell's tenure at King's College deserves detailed consideration. Domb's paper is useful to consider for three reasons:

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[6] The narrative gap here between Adams' appointment in 1863 and his promotion in 1865 is due to a dearth of documentation for the two years he spent as lecturer. The only extant evidence for this period is that to be found in Adams' letter of application to KCL Council in 1865 - see below.

[7] Hearnshaw's assessment of Maxwell's work in London was that he was "undoubtedly one of the most distinguished of all the great men associated with King's College. His investigations into the nature of the ether and his theories respecting the electric field, indeed, place him among the foremost of the pioneers of nineteenth century science" [Hearnshaw,1929,247].

1) to connect the work of William Grylls Adams with the already well documented career of Maxwell;

2) to link Maxwell's inability to teach undergraduates, alluded to in his appliction for the Edinburgh Chair in 1860 [Ch.3], with his formulation of the Cavendish in 1871-74 as a laboratory of postgraduate research and not undergraduate instruction;

3) as a means of contrasting the hagiographical idiom of Domb's account with the more socially integrated study of less "eminent" experimental physicists in this thesis.

To these ends the next section will briefly discuss Domb's attempt to rehabilitate Maxwell's reputation as a teacher at King's College.

2): <u>Maxwell, Adams and the Domb-Hearnshaw controversy.</u>

The passages in Abbay's letters pertaining to Maxwell's resignation from which Domb and Hearnshaw drew their divergent interpretations are given below:

> ...It was difficult to keep order in those days, especially when the blackboard was much used, but possibly the disorder led to the greatest scientific discovery of the century. It was believed by scientific men that the Governors had asked Clerk Maxwell to resign his Professorship of Physics because he could not keep order that then he went back to Cambridge and quietly worked out his theory of the aether[1]. I met him afterwards at Oxford. He was a quiet and rather silent man and it seems not unlikely that the students were too much for him......I am afraid this is only a gossipy letter and of no use to you[2] ...
>
> [Abbay to Hearnshaw, 15/2/1927] in Domb,1985,101-102]

> ...It was Professor Clifton who told me, I think in 1869, that he had heard that Clerk Maxwell had been asked to resign because of the disorder at his lectures. Clifton was a F.R.S. and a

former Smith's Prizeman and must have known Clerk Maxwell in the Early Sixties at Cambridge, as Clerk Maxwell was also a Smith's Prizeman[3]. If there were no truth in the rumour it is strange that it should have existed *before* Maxwell became eminent[4]. I have no other authority for the statement and I think it would not appear in the Minutes of the College meetings in 1868 and 1869, as the suggestion would probably have been made privately out of respect for Clerk Maxwell[5].

[Abbay to Hearnshaw, 18/2/1927] in [Domb,1985,102-103]

Domb firstly implies that the factual inaccuracy of [1] casts doubt on the reliability of Abbay's evidence. However, on closer inspection Abbays's claim here is quite comprehensible: although Maxwell spent most of the period 1865-1871 at Glenlair casting his work on field theory (the aether) and heat into a publishable form, he did, in fact, visit Cambridge virtually every year. Campbell and Garnett point out that in the years 1866,1867,1869 and 1870 he was "either Moderator or Examiner in the Mathematical Tripos at Cambridge, where his influence was more and more felt" [Campbell & Garnett,1882,320 & 324]. At a 60 year remove from these events it is not therefore surprising that Abbay's memory telescoped Maxwell's career between his professorships at King's College and Cambridge in this relatively insignificant chronological aberration.

Domb's next criticism of Hearnshaw is based on the status of [3] as being mere hearsay which Domb then briskly dismisses since it is not independently substantiated in two major biographical studies of Maxwell composed not long after his death[Niven,1890;[redacted] 1882]. Of Domb's citations, however, we can note that the accounts chosen are as hagiographical in character as his own and thus equally unlikely to give credence to any material deleterious to the academic reputation of their subject.

Domb's next critical comment is to question whether Clifton was actually "in a position to know" about the circumstances of Maxwell's resignation and is indeed correct in pointing out that Maxwell was never resident in Cambridge at the same time as Clifton [Domb,1985,94]. However there are two avenues of correspondence through which Clifton could have learnt of Maxwell's position at King's: either through his own correspondence with Maxwell and his friends or via the extensive network of correspondence that G.G. Stokes maintained with Wrangler former-pupils in British academic institutions. Although no extant correspondence between Clifton and his professorial colleagues can be found that discusses the matter, it is highly plausible that he was informed of Maxwell's position by a source such as Stokes at the heart of the Cambridge network of Wrangler-physicists.

Abbay then commented that Clifton's story was unlikely to have been a mere rumour since at the time of his resignation Maxwell had not [yet] attained "eminence" and hence no political gain could have been achieved by manufacturing such a slander. Domb whiggishly contradicts this judgement of one of Maxwell's contemporaries and instead imposes his own evaluative criteria in declaring that Maxwell *had* in fact attained eminence by 1865 through his work in 1863-4 on the B.A.A.S. Electrical Standards Committee [Domb,1985,103(b)]. This he asserts on the basis of his interpretation of Maxwell as a "major driving-force" in the report of one of the E.S.C. sub-committees in 1863, although since he fails to document the extent to which other individuals such as Thomson and Wheatstone were also "major driving forces" the relative eminence of Maxwell's work for the E.S.C. is not convincingly established.

Domb next argues that the bland minuting of Maxwell's resignation at the meeting of the College Council in February 1865 cannot be interpreted as displaying what Hearnshaw referred to as "eloquent omissions" upon the delicate subject of Maxwell's competence as a teacher [Domb,1985,94]. However, in so doing Domb ignores Abbay's own comment [5] that reference to this would not appear in the Minutes of the College Meetings as the suggestion would probably be made privately out of respect for Clerk Maxwell: it is likely that Abbay's knowledge of mid-Victorian academic protocol was greater than that possessed by Domb.

As a final attempt to repudiate Hearnshaw's attack on Maxwell's pedagogical abilities, Domb attempts to demonstrate that the disciplinary skills of his successor W.G.Adams were no better by citing evidence from an obituary of Adams' own successor H.A. Wilson that Adams himself had difficulty in dealing with rowdy students. What Domb fails to point out is that when Wilson made these observations in 1904, Adams was a man of 78 and on the verge of retirement and that Wilson himself had had to take quite drastic measures to maintain order in teaching the very same classes [Royal Society, 1965,190].

It would appear also that matters were somewhat different when Adams was first appointed to the professorship in 1865, for in recommending his promotion from lecturer to professor in natural philosophy, the Dean of the Department of Applied Sciences, Rev. T.S. Hall had cause to praise Adams for "the good discipline he has maintained in the classroom [Hall to KCL Council, 10/3/1865]. The emphasis in Hall's letter of recommendation on Adams' disciplinary skills can be interpreted as evidence that such abilities were acknowledged by the Dean of the Faculty as an important criterion in the evaluation of candidates for the professorship. Taken with reference to Abbay's comments on the circumstances of Maxwell's resignation this can be seen as evidential support for a claim (such as

Hearnshaw's) that classroom discipline was at least one of the central issues in the departure of Adams' predecessor. Considered as a conjunction of two independent factors, there would appear to be few problems however in interpreting Maxwell's departure as a coincidence of both the College's interests in procuring a better disciplinarian than Maxwell, and of Maxwell's own interests (as interpreted by Domb) in resigning to devote more time to the penning of his treatise on electricity and magnetism [Domb,1985,95, Maxwell,1873].

3): W.G.Adams application for the Chair of Natural Philosophy in 1865.

Adams' application for the professorial vacancy in natural philosophy at King's came shortly after his election to a fellowship at St John's Cambridge in November 1864. The original letter of application that Adams wrote during February or March 1865, capitalizing upon his newly won academic accolade, has fortunately been preserved in the College archives and is a document informative about the way Adams perceived his own progress in the 18 months since his appointment as lecturer. It also reveals the manner in which he projected the extension of natural philosophy as the foundation of much engineering practice, thereby making explicit his commitment to the Department of Applied Science's *raison d'etre* of providing a scientific training for engineering students; and perhaps in alluding to the *respect* he had won amongst his class-students Adams was drawing a contrast with other College staff (e.g. J.C. Maxwell) who had not achieved this valuable pedagogical quantity:

I venture to think that I have been successful not only in teaching, but also in winning the confidence and respect of the students, and in creating in the mind of many an earnest desire to obtain a sound knowledge of Mechanical Science as a groundwork for Engineering. The number of students in my classes has greatly increased, and the reality of my success has been fully borne out by the results of College Examinations...

[Adams to KCL Council,Feb/March 1865,KA/IC/A52]

Apart from strategic use of the rhetoric of "scientific engineering" in this application, Adams showed considerable sensitivity to the financial interests of the Department of Applied Sciences: although the full course in the Department of Applied Sciences covered three years, it would seem that financial expediency often moved students to stay no more than two years. Adams had evidently made efforts to deal with this problem and quantified his success by reference to the popular appeal of the course he had recently introduced in the Stokesian domain of optics:

As an additional inducement to students to remain longer than two years[8] at King's College I am at present delivering an extra course of lectures on optics to the students of the Third Year. I may add that out of <u>five</u> matriculated students of the Second Year, who were in my class during the Lent and May terms of last year, <u>four</u> have returned and are now Third Year students of the College.

[Adams to KCL Council,Feb/March 1865,KA/IC/A52]

To further support his application, Adams outlined his plans for extending the position of natural philosophy as the *groundwork* of scientific engineering within the Department of Applied Science's vocational courses; this plan was again made so as to encourage students

[8] For example in 1865, students who wished to apply for posts in the Government-run Engineering Establishment in India need only have spent two years in the KCL Department and a third apprenticed to a civil or mechanical engineer to be eligible. [KCL Calendar,1865,126].

to stay a third year:

> ...it will be my aim, first, to make the groundwork sure, and then to lead the older and more able students into the more advanced fields of Natural Philosophy. Also...I should divide the higher branches of mechanics into two courses of lectures [running in *alternate* years] one course forming the groundwork of Arts of Construction more especially, and the other of Manufacturing Art and Machinery. By this means students would have a still greater inducement to remain for the third year [i.e to attend the second course of specialist higher mechanics in their third year]...
>
> [Adams to KCL Council,Feb/March 1865,KA/IC/A52][9]

Finally, Adams stressed his commitment to scientific research in engineering: since he had acquired his Fellowship in the previous November, he had been "labouring for the advancement of Mechanical Science" by investigating the hydrodynamic problem of applying the principle of the screw to the floats of paddle-wheels [Adams,1865]. Adams' interest in this problem was clearly, at least in part, a cross-fertilization between his Stokesian training in hydrodynamics and his institutional obligations to "mechanical science."

In supporting Adams' application for the Professorial Chair the Dean of the Faculty, Rev T.S. Hall, enclosed a copy of this paper for the council's consideration and declared his own view that it referred "to a subject of national importance" viz. marine engineering. Indeed Hall expressed the conviction of his colleagues that, in addition to his skill in maintaining good discipline in the classroom [see above] "Mr Adams' election would give them satisfaction and the Council would have in him a faithful and diligent Professor" [Hall to KCL Council, 10/3/1865]. With

[9] This was indeed a plan that Adams effected later in his professorial tenure [Adams,1871e,6890].

this level of internal support and similarly positive references from senior Cambridge men including the Praelector, Bursar and Senior Fellow of St John's College, Isaac Todhunter and Adams' Fellowship Examiners, the Principal of KCL concurred with Hall that Adams was the fittest of the three candidates (all known to the College) to be the new Professor of Natural Philosophy. Thus Adams was appointed Professor of Natural Philosophy on the 10th March 1865, and in so doing took on a considerably greater workload than Maxwell had done[10] by agreeing, as a financial expedient to the college, to amalgamate his existing duties as lecturer with the new ones attached to his chair [KCL Council Minutes,1,10/3/1865].

Prior to analysing Adams' campaign for a physics teaching laboratory it is important first of all to characterize the institutional context of the Applied Sciences Department in 1865, and the manner in which both the constitution of this department and the measurement practices of his predecessor framed the development of Adams' laboratory.

4): Applied science and the Maxwellian tradition of measurement

During the 1860's the King's College Professorship of Natural Philosophy functioned solely within the college's Department of Applied Sciences. Hence at the time of Adams' appointment to this position at King's in 1865, his professorial role was to teach "applicable" aspects of natural philosophy to a clientele largely consisting of students intending to enter manufacturing or engineering works. This express function of all

[10] Maxwell had apparently worked at the College for three days a week whereas Adams was now obliged to attend all five days. [KCL Calendar, 1865-66,40].

the professorships[11] was seen by the Department as follows:

> The object in view in this Department is to provide a system of general education, practical in its nature, for the large class of young men who, in after life, are likely to be engaged in commercial and agricultural pursuits, or in professional employments, such as Civil and Military Engineering, Surveying, Architecture, and the higher branches of Manufacturing Art.
>
> The whole course occupies three years, and forms an appropriate introduction to that kind of instruction which can only be obtained within the walls of the manufactory, or by the actually taking part in the labours of the Surveyor, the Engineer or the Architect.
>
> [KCL Calendar,1865, 126]

In the first three years of his tenure Adams' duties thus consisted primarily of lectures to the first and second year trainee engineers in the Department of Applied Science and also in providing evening lectures for the local working population [[KCL Calendar,1866-68]. It is highly significant therefore that when Adams adopted Maxwell's professorial mantle, he also adopted his predecessor's *measurement-oriented* syllabus of theoretical and experimental physics [Domb,1985,75]. In Maxwell's first lectures in 1860 he had begun his courses with an exposition of the importance of measurement as applied to the properties of matter, in contrast to the entirely qualitative approach to the same subject by his predecessor Goodeve - see [Domb,1985,73-75]. Thus Maxwell's 1860 syllabus opened as follows:

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[11] The Professorships held in the Department of Applied Science were:(compulsory Anglican) Religious Instruction; Mathematics; Natural Philosophy & Astronomy; Arts of Construction; Manufacturing Art & Machinery; Land Surveying & Levelling; Geometrical Drawing; Chemistry; Practical Chemistry; Geology; Mineralogy; Photography *and* Workshop Instruction. [KCL Calendar, 1865, 126-127].

> *Properties of matter: Measurement of Quantities* On the application of Mathematics to the Study of Nature; Quantities occurring in Mechanics with their definitions and measures; the Standard Measures of Length, Time and Mass.....
>
> [KCL Calendar, 1860-1; Domb,1985,74]

In the Maxwell/Adams common syllabus for 1864 and 1865 respectively, the theme of measurement was further developed and explicitly portrayed as a matter of the greatest concern to the engineer. Lectures to first year students began with the following overview:

> Relation of Mechanical Science to the practical work of an Engineer; Necessity of making Measurements, Different Kinds of Measurements; Use of Diagrams and of Calculations in combining the Results of Measurement.

In all the topics that followed, the measurement of the quantity under consideration was addressed before all other pertinent issues:

> *Elementary Principles.*- Measurement of the relative Position of Points;....
>
> *Statics.*- Definition of Force, its absolute Measurement, and its Measurement in pounds weight;....
>
> *Theory of Motion.*- Measurement of Velocity;.....
>
> *Work.*- Measurement of work;...
>
> *Dynamics.*- Absolute Measurement of Forces;....
>
> *Hydrostatics.*- Definition of a Fluid; Measurement of Pressure;....Measurement of Gases; Measurement of Heights by the Barometer.
>
> [KCL Calendar, 1864-66, 129-130]

The second year course which succeeded this gave greater stress to application of the same areas of mechanics to practical engineering situations e.g. the application of laws of friction to the stability of structures; the calculation of a shearing force in a rivet; dynamical balancing of the motions of an engine to avoid tremor etc. However, upon the introduction of extra subjects to the syllabus we find a comparable stress on the fundamental importance of measurement practices:

> *Theory of Heat.*- Measurement of Quantities of Heat;....
>
> *Optics.*- ...Means of obtaining accuracy in Levelling and other Measurements.
>
> *Electricity.*- The Electric Current; How to measure it;....

Although no lecture notes are extant to explore further the measurement-centred content of Maxwell's and Adams' lectures it is evident from the above course-outlines that the syllabus common to Maxwell's last and Adams' first year of professorial lectures establishes a continuous tradition of measurement-orientation in the teaching of KCL physics.

With regard to the issue of electrical measurement, we can note also that when Adams first arrived at King's College in 1863, Maxwell was engaged (with Fleeming Jenkin) in making a precise determination of the absolute unit of resistance for the B.A.A.S. Committee on Electrical Standards [B.A.A.S. Report, 1863, 115-176]. When Maxwell and Jenkin repeated their measurement in the College Museum in 1864 to ascertain the accuracy of their result, it is highly probable that Adams, as Maxwell's teaching assistant, would have observed the proceedings at close quarters although there is unfortunately no explicit evidence of this [B.A.A.S. Report, 1864, 345-367]. With regard to these activities in the B.A.A.S Electrical Standards Committee we can also evince a Maxwellian tradition of precise electrical measurement at King's during the mid-1860's into which Adams would have been deeply immersed upon his professorial appointment in 1865.

It is important, therefore, to appreciate how the twin pronged

Maxwellian inheritance of measurement from the Departmental curriculum and the B.A.A.S. electrical researches was manifested in the measurement practices that Adams incorporated into the everyday working of the physical laboratory that he started up a few years later.

5) The Genesis of the Physical Laboratory: February to May 1868.

The institutional context for the establishment of Adams' physical laboratory in early 1868 was one of increasing numbers of students in the Department of Applied Science arising from growing public demand for its educational fare; consequently there was considerable optimism about the future of the Department despite the existence of a depression in the mechanical and civil engineering professions since 1866. This much is clear from the annual report published in the College's Calendar of 1867:

> A larger influx of students [than in previous years] may be expected as the facts become known, first, that there is an ever-increasing demand for *high scientific engineering* both at home and in the British Dominions abroad, especially in Australia and India; and secondly, that some of the most eminent men in the [engineering] profession show their sense of the value of the training in this Department, by not only recommending pupils to it, but also by placing their own sons there. (emphasis added)
>
> [KCL College Calendar,1867-8,43]

Indeed by the following year, such was the success of the Department that it was experiencing problems accommodating its burgeoning clientele:

> The Applied Sciences Department continues to flourish, keeping up its numbers and its popularity. Some of the lecture-rooms are already overcrowded.....and with a view of providing additional accommodation to meet the present and prospective wants of this Department, the Council are about to order a redistribution of the lecture-rooms, as well as the enlargement of the workshop.
>
> [KCL College Calendar,1868-9,44]

The "present and prospective wants" of the Department on the one hand referred to the creation of workshop facilities for the College to be eligible to hold the remunerative Whitworth scholarships [redacted]. On the other hand the College was also keen to establish itself as a pioneer of the physical laboratory for providing *scientific* instruction to trainee engineers, and it was in order to create space for a general extension in laboratory instruction that led the Council to "redistribute" the lecture-rooms in the summer of 1868. To understand the origins of this move we need to return to February of that year to consider a joint plea from Adams and his chemical colleague W.A. Miller to effect just such a redistribution.

On February 14th the Principal of King's College, Dr Self, resigned his position and shortly afterwards moved out of his official residence in the precincts of the College [KCL Council Minutes, 14/2/1868]. Adams and Miller evidently saw an ideal opportunity to extend their disciplinary domains into this newly vacated institutional territory, a move which they had clearly desired for some time - perhaps since Adams' Professorial appointment:

> For some years past we have felt strongly the necessity for more complete means of teaching Physical Science in this College, but have hitherto been deterred from pressing the matter upon your attention for want of the space necessary.....At the present juncture however, several circumstances concur to induce us to urge the subject upon your notice.

These immediate circumstances were that i) the imminent vacancy of the Principal's house would engender suitable accommodation for the desired laboratories; ii) that the School attached to the College wished to restructure its teaching around a more science-centred curriculum; iii) the Universities of Oxford and Glasgow, UCL and Owens College, Manchester, were in the process of extending their facilities for scientific education [Adams & Miller to KCL Council 14/2/1868, KA/IC/A52]. Having pinpointed the importance of this issue, Adams and Miller specified two audiences for whom these new facilities would be of great importance:

> Amongst the motives for this increased activity may be mentioned the demand for scientific teachers in the higher schools. We may further mention that it is within our knowledge that on several occasions at this College we have lost pupils for want of due means of instructing them. Recently several amongst the Candidates preparing themselves for service in the Telegraph Department in India applied here, but left us for want of special practical instruction in Electrical Science and have since entered themselves at other institutions. (Authors' own emphasis on the word "practical").
>
> [Adams & Miller to KCL Council 14/2/1868, KA/IC/A52]

Whilst deferring a full account of the facilities that were required for this "special practical instruction" and also for the related science-teaching in the school till a later date, they sketched out their joint demands as follows:

> Not only is space required for practical instruction in the separate branches of Physical research such as General Optics, Spectrum Analysis and Heat, Electricity and Magnetism, which will require special rooms for the purpose, but additional accommodation is and has long been greatly needed for the Class of Practical Chemistry, and for Physiological research.
>
> We therefore...seize the present opportunity of urging upon you the necessity of acquiring the additional space which will be at your disposal when the present Principal quits his house...
>
> [Adams & Miller to KCL Council 14/2/1868, KA/IC/A52]

Upon receipt of this letter from Adams and Miller, the Council referred their demands to the Committee of Enquiry that had been established to appoint a replacement for the outgoing Principal, since the two issues were now related by the suggestion that the latter's house be converted into a "School of Science" [KCL Council Minutes,14/2/68,155-56]. Apparently in response to this Committee's enquiries Adams submitted a more detailed proposal in March, a plan much more specific in its details now that Adams was sure of the Council's level of support for his scheme [Adams to KCL Council, March 1868, KA/IC/A52].

In this proposal for a physical laboratory we can discern an attempt by Adams to broaden the basis of his natural philosophy teaching, beyond the confines that the engineering-centred curriculum had hitherto imposed upon him, by introducing the study of heat and electricity. He argued that because the "chief and almost the only aim" of students in the Department of Applied Sciences was to achieve "a thorough scientific and practical training in....the principles of engineering the teaching in Natural Philosophy has [hitherto] been almost entirely confined to those branches ...which bear on the Theory of Structures, Earthworks etc and on Machinery":

> It was felt that to give lectures in the other branches of Natural Philosophy such as heat and electricity, subjects of the utmost importance even for Engineers, without demonstrations, in illustration of the principles, and actual applications of the principles by the students themselves would be of very little practical utility, and could never stir up any very deep interest in or love for these sciences.
>
> [Adams to KCL Council, March 1868, KA/IC/A52]

Adams argued that the demand for "such training as shall give this thorough knowledge is now rapidly increasing" and reiterated the point of his previous letter that other competitive institutions of higher education were "actively exerting themselves to supply the demands of the public in this respect". Hence he declared that to prevent King's falling behind in the competition for engineering students:

> it will be necessary to establish a Physical Laboratory and workshop where there can be demonstrations before a class of students, and where they could individually do practical work under the guidance of the Professor both to ground themselves in the principles [of physics] by making experiments for themselves, and to apply the knowledge so gained to the construction of apparatus in the different branches of Physics.
>
> [Adams to KCL Council, March 1868, KA/IC/A52]

At this point Adams emphasised his commitment to inculcating precision practices in training students to make experiments for themselves by highlighting a logistical difference of praxis between the physical laboratory and the chemical laboratory:

> A student could not, as in Chemistry, do all his experiments at his own table, for many instruments which he would have to use must be accurately fixed, and various sources of error arising from their position must be allowed for, and hence those instruments must always remain in the positions in which they are used.
>
> [Adams to KCL Council, March 1868, KA/IC/A52]

Closely related to the issue of stable measuring conditions in the laboratory was Adams' demand that different branches of physics required independent and separate facilities for their study to preclude the mutual interference of their measuring operations. Adams' emphatic demands on these issues very closely match those being made at the same time by R.B. Clifton in Oxford and employ an identical vocabulary - evidence that Clifton and Adams collaborated in developing their desiderata for a physical laboratory [Ch.6]:

It is necessary that separate rooms should be set apart for the several branches of Physics, and when it is remembered that Physics includes Mechanics, Pneumatics, Acoustics, Spectrum Analysis, Heat, Radiant Heat, Magnetism, Statical and Dynamical Electricity, and that experiments in many of these subjects cannot be conducted in close contiguity to one another owing to the mutual interference, it will be seen that in a Physical Laboratory there should be considerable space.

The scheme of rooms that Adams laid out involved one large general laboratory for student work as well as a number of smaller laboratories for the distinct subjects as outlined above. In the general laboratory students would carry out experiments in mechanics and hydrostatics and the most elementary investigations of heat and pneumatics; this room Adams considered to be manageable "to some extent" as a chemical laboratory - insofar as the apparatus used in it was intended to be relatively individualized and mobile. Apart from this general room Adams wished for "a room for the more delicate [exact] kinds of workmanship, and storerooms for materials used in the laboratory" and separate laboratories[12] for:

1. Pneumatics and Acoustics.
2. Heat.
3. Radiant Heat. ¦ with a
4. Light. ¦ south
5. Spectrum Analysis. ¦ aspect[13]
6. Magnetism.
7. Statical electricity.
8. Dynamical electricity.

[Adams to KCL Council, March 1868, KA/IC/A52]

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[12] Adams later mentioned a requirement also for a Natural Philosophy Lecture Room with a small private laboratory next to it presumably for the purpose of lecture preparation.

[13] Note that since electric lighting was not generally available until the 1880's most optical and quasi-optical experiments relied upon a ready supply of natural sunlight, a supply best acquired from a south-facing window.

These Adams deemed to be the requirements for a "good Physical Laboratory" and as such he considered that "there exists at present no physical laboratory worthy of the name in England." Whilst demonstrating Adams' appreciation of Thomson's laboratory in Glasgow, this comment belies Adams' view that Foster's recently-formed laboratory at UCL was not up to the standard that Adams stipulated for a "good Physical Laboratory". Considering our discussion of the complaints made by Foster about his working conditions in chapter 3 one supects that Foster might reluctantly have agreed with Adams on this point.

Given the scale of the demands being made here, Adams was at pains to stress the advantages of investing in such a physical laboratory during the contemporary economic depression in the engineering profession. Whilst the number of engineering students enrolling with the Department of Applied Science had continued to rise, Adams argued that in order "that our numbers should not diminish, owing to the dearth of Engineering work in England, it is necessary that we prepare our students for other walks of life." In addition to reiterating the burgeoning opportunities in telegraphy and teaching[14] mentioned above, there was one particular field in which the physical laboratory was likely to become important in preparing King's' students for "other walks of life" namely in preparing for entrance at the Universities af Oxford and Cambridge.

These Universities were described by Adams as "opening our way by giving encouragement to Physical Science", meaning of course that the examinations recently instituted in experimental physics at Oxford and those about to be established at Cambridge acted as incentives for

[14] Adams added that training teachers d physics to meet the currently unsatisfied demand was "urgent" and a case of "national importance".

students to enroll at King's to acquire the necessary preparation from Adams et al. From what has been said above about the relation between the two men, it is quite probable that Adams sent Clifton a number of students to study in the makeshift laboratory that the latter operated prior to the completion of the Clarendon in 1870 [Ch.6]. In the letter under discussion here, however, Adams was more specific in relation to the typical flow of students from KCL to Cambridge:

> ...at Cambridge it is [currently] proposed to introduce the subjects of Heat, Electricity, and Magnetism into the examinations for Mathematical Honours for those who enter the university after June 1869, so that it will be necessary to introduce these subjects experimentally into the teaching of the General Literature and <u>Science</u> Department, if the students who go to Cambridge are to distinguish themselves as much as their predecessors have done.
>
> [Adams to KCL Council, March 1868, KA/IC/A52]

Thus Adams argued his case for his prospective physical laboratory in both the Department of Applied Sciences and the Department of General Literature and Science, and in the latter effectively staked a claim for a certain autonomy of natural philosophy teaching from the pragmatic requirements of engineering students.

Shortly after this letter was received by the investigating committee, it seems that Adams was requested to make specific suggestions as to how his requests for laboratory accommodation could be met: it would appear that Adams no longer had recourse to using the Principal's house for this purpose. His plan appears in an undated (presumably late March to early April) manuscript in the college archives which bears the suggestively dramatic title "<u>Rooms which are urgently needed, and without which it is almost impossible to teach Physical Science experimentally and</u>

practically in King's College." Adams conception of rooms that were *sine qua non* for teaching experimental physics were as follows:

1. A Lecture Theatre - 40 x 30 ft.
2. A Laboratory - 30 x 15 ft, for the Professor.
3. A General Laboratory 40 x 30 ft, for Students.
4. A smaller Laboratory 30 x 20ft, for special experiments. This room should be capable of being darkened.
5. A store room for glass, chemicals and other materials.

For 1. Adams suggested appropriating the Theological Lecture Theatre since this had a South aspect "very suitable" for optical experiments. For 3. and 4. he considered the Drawing Classroom and an adjoining room to be suitable since these were sometimes, both then and previously, used for Natural Philosophy teaching [Adams MSS, March/April 1868, KA/IC/A52]. These two rooms had the special advantage that they gave direct access to the Museum of King George IV around whose every wall there were cases full of philosophical apparatus [Adams,1871e,6886].

The suggestions in this document along with Adams' notions on how to find new accommodation for the subjects whose rooms would thus be appropriated were probably the ones presented[15] by Adams at a subsequent meeting of KCL Council on April 3rd. For this meeting a plan was also prepared by the Secretary of King's, John Cunningham, with the guidance of Adams and Miller "for the purpose of providing additional room for Practical Chemistry and for [Physical] Science within the college, [to]

[15] From the very similar proposals that were finally agreed upon by the Council on May 8th (see later) we can be fairly certain that Cunningham's plan must have been quite similar to Adams' scheme in this document.

obviat[e] for the present the necessity of extra buildings" [KCL Council, 3/4/1868]. Letters on the subject were read from the parties concerned and it would seem that Adams was supported in his claims by no less than Dr Thomson, the Archbishop of York [Adams, c1905]; after "considerable discussion", the matter was again referred to a committee but from the details available it is likely that the point of debate was not whether Adams was to have the laboratory, but rather how the rooms were to be redistributed to bring his scheme into effect.

However, there is evidence that the entire subject was the cause of some disquiet amongst those of Adams' colleagues who were likely to lose some of their institutional territory if the scheme for his laboratory went ahead. In a letter written by Adams to (presumably) the Principal on the day after the Council meeting (April 4th) we can see the extent of Adams' institutional imperialism and get some idea of the controversy he caused in his attempt, for example, strategically to colonize a room suitable for pursuing his Stokesian interests in optics. In this letter Adams was evidently attempting in part to resolve some unspecified "difficulty which arose today [5/4/1868] ", by suggesting another possible re-arrangement of teaching-rooms[16]. What Adams wished to procure in this negotiation was the private room of J.G. Lonsdale, the Professor of Classics, in which he could carry out his optical experiments, this being a room in which afternoon sunlight could be obtained in the summer and one which could otherwise be easily darkened [Adams to KCL Principal, 4/4/1868, KA/IC/52].

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[16] In a postscript he referred also to Prof. Hall having "quieted" Mr Cock, a mathematics teacher, by offering to the latter the use of his own lecture room.

The crux of this little controversy was that Adams wanted to have this room available to him in the summer term of that year "so that I might prepare for the coming year and also make a beginning with the present Third Year Students to let it be known what we are going to do [in the laboratory]". In addition Adams stressed the importance of this room as a place for him to carry out his own researches:

> The Professor will have to be at the College up to 4 o'clock in the day so that he ought to have a room in which he can make experiments apart from the students for it will not be necessary that both he and the demonstrator should be in the students' laboratory always and he must have some time and place to make his own experiments [particularly because] there will be many delicate experiments in weighing and measuring [which cannot be done in either of the two student laboratories]
>
> [Adams to Principal,4/4/1868, KA/IC/52].

There are no indications however that Adams was successful in this particular ploy for new territory. After this episode however we know from Adams' reminiscences after retirement that in the Easter of 1868 he made a trip to Paris to inspect physics laboratories upon which to model his own:

> I saw Professor Cornu at the Sorbonne, but I found that he had no Laboratories for Teaching Students but only apparatus for his own investigations. Professor Cornu introduced me to his colleague M. Jamin at the Sorbonne, and I found that at the Sorbonne there were already established by M.Jamin a very complete Laboratory where students were already engaged in the determination of Physical Constants. I very much admired the weighing room of the Laboratory which was set apart from the main Laboratory. In 1868 this was the only Physical Laboratory in <u>Paris</u> or in <u>France</u> for the training of students in Practical Physics[17].
>
> [Adams,c.1905,KA/IC/52].

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[17] Adams also claimed that there was no laboratory for students at this time in either Germany or America although he was clearly misinformed on these points since Kirchhoff was operating such a laboratory in 1868 [Jungnickel & McCormmach,1986] and Edward Pickering had begun his teaching laboratory at MIT in 1867 [Pickering, 1871].

According to Adams, the only other source from which he was able to acquire any useful information was from Clifton at Oxford although in Easter 1868 what Adams referred to as Clifton's "very excellent Laboratory" was only just undergoing the earliest stages of construction. Thus when he declared that the Oxford and Sorbonne models were the "only Laboratories for Physical Work from which I could get any ideas as to what a Physical Laboratory should be," Adams very probably meant he had seen Clifton's plans for the Clarendon after their completion in late November 1867 [Ch.6].

After Adams' return from his exploratory tour, the Finance Committee of the College reported to the Council on their proposals for setting up the physical laboratory on May 8th. Their recommendations were in fact very largely what Adams had wished for, granting him the Drawing classroom for the main student laboratory and a small room for his professorial researches between the general student laboratory and the King George IV Museum. Adams' professorial colleague in chemistry, W.A. Miller fared less well, however, being granted only one of the three sets of students' rooms that he had applied for - since it appeared to the Committee that "the Professor of Chemistry already enjoys a very large amount of accommodation." Nonetheless, the committee authorised a total expenditure of £2000 on both physical and chemical laboratories, and arranged a loan to cover the cost of this capital outlay. Adams' satisfaction with the arrangements made is clear from his later remarks to the Devonshire Commission that "considering the means at their command, the Council responded liberally, and made very satisfactory arrangements for the teaching and practical study of physics [KCL Council Minutes, 8/5/1868;

Cunningham,1871, q7038; Adams, 1871e, q6886 & 6967].

One final decision taken by this committee in relation to the new physical laboratory was to create an appointment for a man to act as both demonstrator in the laboratory and also to teach natural science in the King's College School from October 1868 [KCL Council Minutes, 8/5/1868]. After advertising the post on May 18, three applications were received and one of these was from Richard Abbay, a recent graduate of Exeter College, Oxford, who was one of the first men to go through the course of practical physics run by R.B. Clifton at the Oxford Museum prior to the completion of the Clarendon in 1870 [KCL Committee of Delegacy, 18/5/1868, KA/CS/M3]. In reviewing his application the College Council were informed that

> Mr Abbay has the advantage of being sufficiently young (he is 24 years of age) to adapt himself to the position and to take suggestions from others. He brings evidence of high ability as well as of extensive theoretical and practical knowledge of the subjects which he will have to teach and has gone through a complete practical course in a physical laboratory.
>
> [KCL Council Minutes, 3/7/1868]

Abbay himself later commented that "there was scarcely anybody who had been trained in a Physical Laboratory then and I had been with Prof. Clifton little more than a year....I believe I was almost the only candidate who had had any experience with scientific apparatus[18]" [Abbay to Hearnshaw, 15/2/1927, in Domb,1985,101]. As such Abbay was deemed to be the best suited to the post of Demonstrator and was appointed on 3rd July [KCL Council Minutes, 3/7/1868].

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[18] Abbay added "I had never given a lecture nor had pupils [hence the Governers asked me to give a lecture to them. Dr Jelf and Miller were present and I explained the air-pumps etc. Dr Miller got up in the middle and said that was sufficient and I was appointed" [Abbay to Hearnshaw, 15/2/1868 in Domb, 1927, 101].

Thus granted the requisite finances, accommodation and personnel, Adams' laboratory was furnished over the summer vacation of 1868 and was ready for operation at the beginning of the following term [Adams, c1905].

6): <u>laboratory measurement and the careers of KCL students</u>

Upon the opening of the physical laboratory in October 1868 Adams began a course of "Practical Physics" for third year students in the Department of Applied Sciences and a number of occasional students who had received some previous training in physics: like Foster but unlike Thomson and Tait he set a minimum standard of seniority and experience for students to be allowed into the laboratory domain. According to the prospectus in the 1868-69 Calendar, laboratory students were taught to "make experiments for themselves in certain branches of the following subjects: - Mechanics, Pneumatics, Heat, Light, Electricity and Magnetism; and to construct the more simple apparatus required for those experiments" [<u>KCL Calendar</u>,1868,128]. When Adams opened the doors of his laboratory for this course he found himself so inundated with suitably qualified applicants for this course that he had to apply for further financial support from the College; acknowledging the "very great success [which] had attended the opening of this new Department of College work" the College Finance Committee granted him a further subsidy of £300 towards the end of his first term [<u>KCL Council Minutes</u>, 13/11/1868 & 11/12/1868].

To clarify the level of "success" that gave Adams so much bargaining power with the Finance Committee we can note that he was "surprised" to find that although he had expected no more than about 10 students to attend his first laboratory class, there were in fact 15-16 in attendance during the first week of its operation, in keeping with the general expansive trend in the Department's student audiences in this period. Thus he and Abbay were busy in the laboratory mostly with "occasional" students until 4pm every day, especially Abbay who had to give three experimental lectures in the school and four in the college every week, and was expected to arrange all the apparatus in the laboratory while Adams carried out all the teaching work. Abbay later commented that his was "very interesting work but difficult" [Adams,1871e,q6882; Minutes of College Committee of Delegacy,18/5/1868, KA/CS/M3; Abbay to Hearnshaw, 15/2/1927 in Domb,1985,101].

The syllabus of the practical course that Adams taught was very much centred upon the use of measurement apparatus and the repetition of standard measurements, reflecting his commitment to measurement practices as a primary medium of physical pedagogy even more than did his lecture courses discussed above:

The Use of the Barometer.
The Thermometer.
The Hygrometer.
Determination of Specific Gravities.
The Cathetometer.
Measurement of volumes of Gases under varying pressures.
Barometric Manometer.
Measurement of Expansion of Solids.
Measurements of Expansion of Liquids and Gases.
Measurement of Specific Heat.
Measurement of Latent Heat.
Determination of points of Solidification and Boiling.
Measurement of conducting Power of bodies for Heat.
Measurement of Indices of Refraction of various substances.
Spectrum Analysis.
Double Refraction - Interference.
Polarization.
Radiation, Absorption and Reflection of Heat.
Magnetism and the making of Magnets.
Diamagnetism.
Manipulation of Electrical Machines and Apparatus connected...
Different methods of developing currents of Electricity.
Measurement of the Strength of Currents.

Measurement of Electro-motive force and Resistances.
Induction.
Thermo-electricity.
The Electric Telegraph.

[KCL Calendar, 1868, 128-29]

This measurement-orientation of Adams' approach was a feature that he particularly emphasized in an article he wrote on the subject of "Physical Laboratories" for Nature in February 1871, having especial reference to the operation of his own laboratory at King's College during the previous two and half years. In this article his intention was to follow up the piece written by his American colleague, Prof. Pickering of M.I.T. , in a previous issue on practical physics teaching in the USA [Ch.1] and to "trace the similarity between the methods employed by different teachers" [Adams,1871d,322]. Since the methods he discussed were exclusively those relating to his own practices of teaching by measurement at King's College, it is evident that the specific similarity he was attempting to trace out was the common pursuit of pedagogical measurement at Glasgow, UCL, KCL, and MIT. Adams thus described the measurement work in his own laboratory:

> *Fixed* tables in both large rooms are supplied with water and gas, and with pipes..[of]..oxygen and hydrogen, also with thick copper wires...passing to the battery room...The principal instruments have their *fixed* places on the tables and a description of the measurement to be made is given to each student, and while in progress his work is examined by the professor or demonstrator.....When, as has sometimes been the case, there are twelve or more students beginning their laboratory work at the same time, it is necessary to deviate from the [prescribed order] of the regular course, at to set some to begin with heat, some with light, and others with electricity. For some experiments such as the determination of the relation between the pressure and volume of a gas, or the measurement of the expansion of a gas for given changes in temperature, requiring the use of the manometer and cathetometer, it is found to be better to have two students working together, each student making in his turn and so checking every part of the measurement or determination...
>
> [Adams,1871d,323]

For the lucrative market of telegraph engineers which, as we saw in chapter 1, were a major source of income to King's in the early years of the laboratory Adams developed a special course of electrical measurement. These trainees telegraph were evidently the "occasional students" to whom Adams and his demonstrator devoted most of their time [see above]:

> Besides the students pursuing the regular course there are several who wish to devote their attention to some one branch, such as Electricity. In this subject, after making determinations of Resistance, Strength of Current and Electromotive force with simple galvanometers, they pass to more delicate measurements with Thomson's Galvanometers and Electrometers, such as the experimental determination of equipotential lines on a conducting surface uniting two poles of a battery, and perform all the tests and measurements required in connection with Telegraph lines and cables.
>
> [Adams,1871d,323][19]

Note here how Adams employed Thomson's electrical equipment developed for the B.A.A.S. Electrical Standards Committee in training telegraphists.

Although telegraphists were a prestigious and lucrative source of laboratory audience, Adams gives us this overall perspective on the relative proportions of his student clientele:

> ....some are students who have been sent home from India by the Government to get up theoretical knowledge of telegraphy, and these work principally at electrical work. We have had several of these students[20]. And then there are others, matriculated students who pay the occasional fees and do work in the physical laboratory [following their own customized course of subjects]. Then there are other students that do not attend the regular lectures at the College, but come to the physical laboratory for practical work in different branches of physics, confining themselves generally to one or two branches. Some study all the branches of physics, and intend to compete for Natural Science Exhibitions at Oxford and Cambridge, and to become teachers of physics.
>
> [Adams,1871e,q6888]

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[19] See [Adams,1871e,q6887] for a similarly measurement-centred account of his laboratory teaching.

[20] Between 1868 and 1882 Adams reported that he had at least 30 such students [Adams,1882,q2674].

Of Adams' students who had already won Oxbridge Exhibitions, it is pertinent to note that two of these had been at Adams' *alma mater* St. John's College Cambridge, and that the other two were at Merton College, Oxford - where Robert Clifton was a Fellow [Adams,1871e,q6892]. Thus there is evidence that Adams despatched students directly from his own laboratory to that of undergraduate associate and experimentalist colleague in Oxford. Others of Adams' audience, like Foster's, were also entrants for the University of London BSc examinations but unlike the Gower St. clientele they did not constitute the bulk of Kings studentship, there being as many students winning honours in the UL 1st BSc up to 1871 as were separately awarded Exhibitions to Oxford and Cambridge [Adams, 1871e, q6892; Ch1].

Considering the other major element of the Kings' clientele, Adams claimed that there was good direct industrial engagement between the college's laboratory trained engineers and British manufacturers: "It is a fact that they are advanced to posts of responsibility after they leave us...in two or three cases I can think of at the present moment, there are old students who have only left us for two or three years....and who are entrusted to make out plans and estimates for their employers, and to have the entire superintendence of the execution of important engineering works; others who go abroad, are at once appointed to important posts, and sometimes with very good salaries [Adams,1871e,q6891]. To explain the aptness of King's students for such positions, Adams asserted that "we have every means at King's College that can be provided for the education of engineers, except the practical engineering itself, and for that it would be necessary that students go to a practical engineer to finish acquiring a knowledge of the profession, but we have everything preparatory to that in our present course at King's College [Adams,1871e,q6892].

Adams here aligned his views with the contemporary British consensus that practical engineering skills *qua* professional practices could not be taught in the environment of the academic laboratory in which the scientific basis of these practices could alone be comprehended[21]. Nevertheless, Adams did advocate an integration of college laboratory and workshop practices to introduce the practices of precision measurement into the engineering workshop, commenting in 1871 that "students are encouraged to combine their work in the Physical Laboratory with their work in the Mechanical Workshop,[22] and are enabled to design and construct apparatus, and their inventive apparatus, and their inventive powers are exercised often with great success"[23] [Adams,1871d,323]. Since his students were thus able "to use tools, *and* even to *make machines* and steam engines for themselves before they leave us", Adams argued that they were "probably preferred [by employers] because they are better educated" for

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[21] Elsewhere he reiterated: "a student can only learn the practice of engineering under a practical engineer" [Adams,1871e,q6969]. See the evidence of W.J.M. Rankine and Fleeming Jenkin to the Devonshire Commission for a detailed discussion of this functional and professional exclusivity between academic laboratories and industrial workshops [Jenkin,1870; Rankine,1872].

[22] For details of the operation of the workshop see the account of it given by the College Secretary, J.W. Cunningham to the Select Committee on Scientific Instruction in 1868 [Cunningham, 1868, 3354-3400]. Apparently the public reception of this academic workshop had changed over the preceding 20 years from being a laughable absurdity to becoming an accepted part of the College's training for engineers [Cunningham,1868,3397].

[23] To the 1882 Royal Commission on Technical Instruction Adams cited the case of one student who combined both sources of manual expertise in the design and construction of a "dynamo-electric machine" and a "delicate galvanometer" [Adams,1882,q2705].

work in industry [Adams,1871e,q6933].

As a reflection of this popularity of his course amongst employers, Adams was able to cite ever growing numbers of students undertaking such practical work: after starting off with 15-16 laboratory students in autumn of 1868 he had 23 students by the same time in 1871. In fact such was the level of activity being carried out there that his "more advanced students" were being set to "carry on investigations" such as *measuring* the effect of heat in altering the magnetic polarity of diamagnetic bodies or *measuring* its effect on the plane of polarization of polarized light passing through a sugar solution [Adams,1871d,323]. These student researches on the subject of polarization were evidently an extension of Adams own interests in the area for in 1871 he published two papers on the reflection and or refraction of polarized light from collections of parallel plates [Adams,1871b &1871c]. Thus Adams, like Foster, cultivated the assistance of laboratory students in his measurement researches, but again unlike Thomson selected only the elite measurers for this task.

Nonetheless Adams' laboratory rhetoric in 1871 held that his course of laboratory precision measurement work had benefits for *all* his students through both the mental exactness and the Smilesian morals that it inculcated:

> The accuracy of the results obtained has been very great, and is an evidence of the interest[24] taken in the work by the student, and of the value of such a course of study as mental training, to say nothing of the actual knowledge gained...It will [also] be seen that to the student of Electricity or any branch of physics...that the advance which may be made by him is dependent only on *his own exertions*.
>
> [Adams,1871d,323]

7): Epilogue - the Wheatstone Laboratory

As we saw above,the general popularity of his laboratory courses gave Adams an appreciable financial leverage over the College Council in accommodating the demands his practical classes, a luxury not afforded to Foster at UCL. As King's professor of experimental physics Adams was even more fortunate in 1875 when the death of one of his predecessors, Sir Charles Wheatstone, resulted in a bequest of all the late Professor's scientific instruments, books and honorary awards being made to Adams and the Natural Philosophy Department [KCL Council Minutes,10/12/1875].

Wheatstone's executor Robert Sabine had apparently wished to meet Adams to discuss what of Wheatstone's large collection of apparatus and his library would be of use to the College [Sabine to KCL Council, 16/11/1875[25]], but being apparently indisposed, Tomlinson, Adams' assistant and lecturer in science accepted the collection in its entirety along with 1500 of Wheatstone's books and a £500 cheque for the purchase of any further apparatus that was needed. Tomlinson estimated the

[24] To the Devonshire Commission in the following month Adams reiterated this point: "The work of the physical laboratory is very important because the effect of it has been to give students a thorough interest in physics, who did not get the same interest from lectures; when a student works in a physical laboratory he gets interested in his work and makes rapid progress" [Adams,1871e,§6887].

[25] The MSS letter is strangely erroneous in being dated 16/11/1876.

collective value of the bequeathed apparatus at about £1000 and pointed out that Wheatstone's bequest endowed them with precisely the apparatus that Adams and he were in need of at that particular time [KCL Council Minutes, 10/12/1875]. Thus Adams' laboratory received the very same measurement equipment that Wheatstone had used to bring prestige to natural philosophy at the institution in the early decades of its existence.

Subsequently on March 28 1879, after a proposal made by Adams to the College Council, the laboratory was christened the "Wheatstone Laboratory" thus formalizing the historical continuity of measurement physics at Kings throughout the middle of the nineteenth century [KCL Council Minutes, 28/3/1879].

8): Conclusion

William Grylls Adams, was a Cambridge mathematician converted to experimental physics through the family precedent of his astronomer elder brother, Stokes' inspirational lectures on experimental physics and the Royal Institution demonstration lectures of John Tyndall. Placed in the somewhat alien context of an institutional Department devoted to the training of engineers, and supporting the somewhat ill-placed J.C. Maxwell, Adams rose to fill the chair of his singular predecesor and in so doing adopted a Maxwellian heritage of precision measurement in his scientific pedagogy and laboratory practice.

In this context, following the immediate inspiration of his undergraduate associate Robert Bellamy Clifton at Oxford and also of Jules Jamin in Paris, Adams successfully negotiated the creation of a well-endowed metroplitan laboratory in 1868. In this laboratory he cultivated an educative expertise in precision measurement amongst science teachers, students intent on the revamped Oxbridge liberal education in sciences and most impotantly of all, the ascendant generation of scientific engineers and telegraphists. To conclude this chapter we can cite Adams' definitive view of how training in his laboratory prepared King's engineering students for a career in industry, as given to the Royal Commission on Scientific Instruction in 1882:

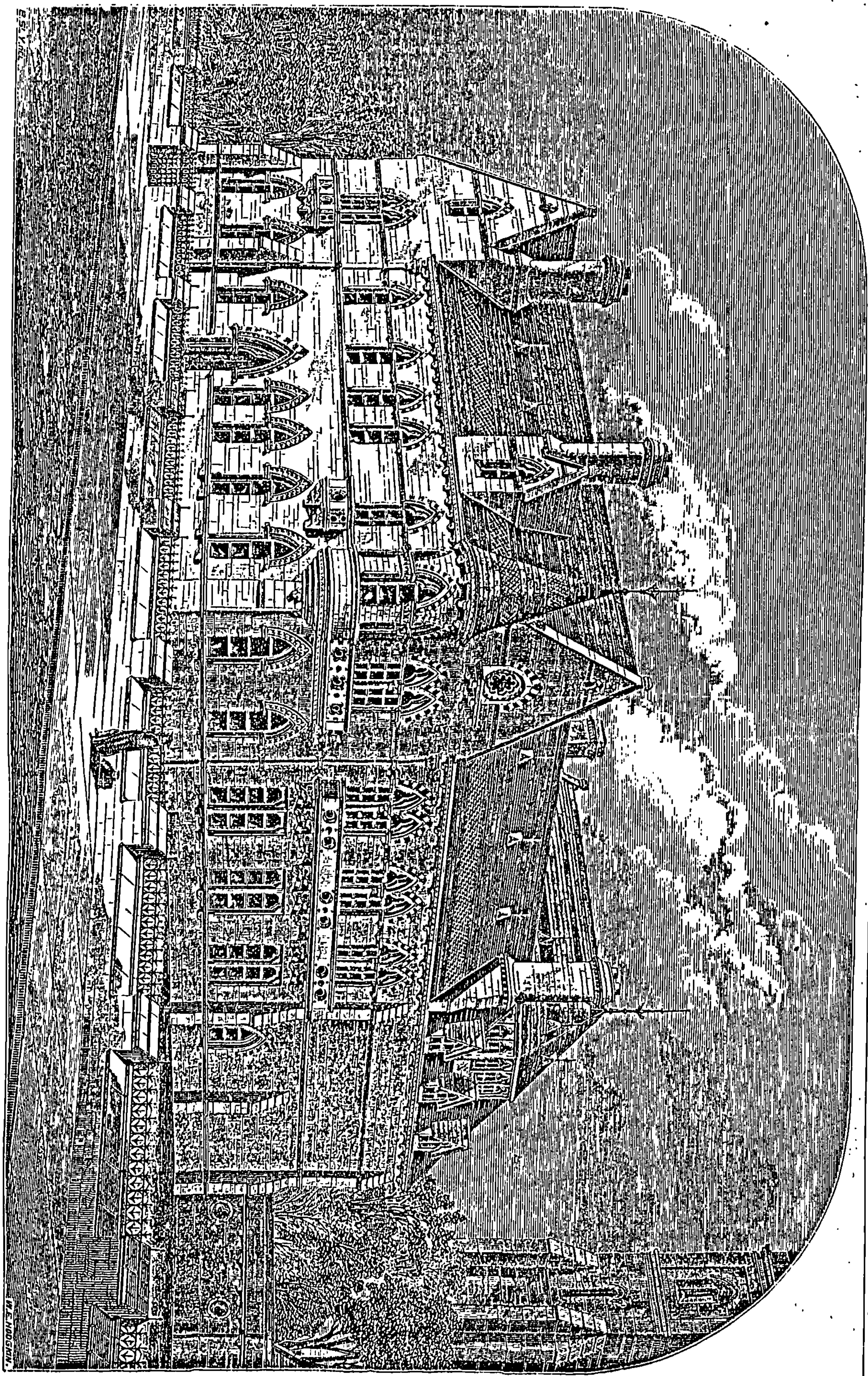
> [King's students] shall have [a] complete education in pure science; but at the same time that their education in pure science is going on, they have the opportunity of learning to use their hands, and of applying their knowledge of pure science to practice by going through courses of practical work in the physical, chemical, and metallurgical laboratories, and the mechanical workshop, so that they may be useful men....
>
> [Adams,1882,2648].

CHAPTER 6

Robert Bellamy Clifton and the Clarendon Laboratory at Oxford.

At the present time especially the progress of physics seems to me to depend on the progress of methods of exact measurements.

> Robert Bellamy Clifton: 1877 interview with Oxford University Commission [Clifton,1877,q451]



THE CLARENDON LABORATORY, OXFORD MUSEUM.—Mr. T. N. Deane, Architect.

Introduction

Robert Bellamy Clifton held the Chair of Experimental Philosophy at Oxford for a fifty year period between 1865 and 1915 and masterminded the creation of the University's physical laboratory, the Clarendon. At its opening in 1870, the Clarendon was unique in Europe as a laboratory purpose-built for the practice of high accuracy precision measurement, and this institutional function was systematically fostered by Clifton in the popular practical courses that he taught throughout his tenure. Nevertheless, Clifton himself has attracted more criticism for the character of his work at the Clarendon than any other contemporary physicist of professorial stature because his entire career was spent primarily as a *teacher* of physics rather than as a *researcher*.

Thus on the one hand he has attracted vituperation from twentieth century physicists for "failing" to meet their ideal of the physicist's professional role as one primarily of research: in Birkenhead's biography of Clifton's successor, F.A. Lindemann, we hear of the latter's view that the "moribund" state of the Clarendon in 1919 reflected "great discredit" upon Clifton. Birkenhead interpreted Lindemann's "depressing inheritance" of "scarcely any apparatus suitable for carrying out research, no facilities for electric power and, above all no staff of research physicists" as a firm indication that Clifton had been "entirely opposed to research" [Birkenhead,1961,90].

On the other hand Clifton has received indifference from historians of science due to their historiographical penchant for documenting the more tangible and ostensibly laudable products of the research physicist. In this chapter, however, such a preoccupation will be displaced in favour of a more empathetic contextual analysis of Clifton's self-professed role as a *teacher* of physics. This historiographical perspective is vital to this study of Clifton's career since it was Clifton's pedagogical skills that won him appointments to Professorships of physics at both Manchester and Oxford in the 1860's, and indeed his academic prerogative in these chairs was the *teaching* of experimental physics, not formal research.

Nevertheless, as the role of the academic physicist *qua* researcher developed during the 1870's to 1890's, Clifton showed himself willing to adopt this new mantle as a major secondary function of his Professorship yet was manifestly frustrated in this aim throughout his career. First of all Clifton bore an inordinately heavy burden of laboratory teaching which precluded much other activity, and later, as the affluence of Oxford University declined along with its sympathy for science in the impecunious decades of the 1880's and 1890's, Clifton was denied the very research resources that Birkenhead denounces him for failing to cultivate.

This account of his long career will therefore document the development of Clifton's interactive practices of teaching and laboratory measurement through his successive institutional contexts at Cambridge, Manchester and Oxford. Whilst some emphasis will be placed upon the idiosyncratic extremes to which he pursued his symbiotic commitments to precision measurement and teacher-training at the Clarendon, ongoing comparisons with his professorial counterparts in physics laboratories at other British institutions will reveal that Clifton was far from being the anachronistic reactionary that his detractors have consistently portrayed him as being. By contrast, the operation of the Clarendon and the nature of Clifton's practical teaching at Oxford will be interpreted as prototypical for the ascendant British community of laboratory physics teachers.

1): Early life and education at Cambridge.

Robert Bellamy Clifton was born on March 12th 1836 as the only son of Robert Clifton, a gentleman-landowner of Gedney, Lincolnshire. He received his early education at University College, London, prior to matriculating as a "pensioner" of St. Johns College Cambridge in Michaelmas term 1855 to read for the Mathematics Tripos. Clifton's subsequent career at Cambridge was a successful one, firstly winning a college scholarship in 1856 and then becoming 6th Wrangler in the Tripos examinations of 1859; he completed his undergraduate career by winning 2nd Smith's Prize in the same year [Venn,1940,68].

Whilst at Cambridge, however, Clifton's interest in natural philosophy was forged beyond the "mixed mathematics" required in the Tripos in attending the lectures of the Lucasian Professor of Mathematics, George Gabriel Stokes. From Stokes' register of students we know that Clifton was present at these lectures on experimental natural philosophy during his second undergraduate year (1856-7), and from Stokes' extant lecture-notes of that year we also know what experimental physics[1] Clifton learnt from Stokes' demonstrations. In October 1856 Stokes lectured on mechanics and dynamics; in November on hydrostatics and hydrodynamics; in December and January on Heat and from January until Easter on miscellaneous aspects of electricity and magnetism. All of these were regularly punctuated with experiments judiciously chosen by Stokes to demonstrate principles and phenomena of special interest, employing for example Atwood's machine to illustrate Newton's laws of dynamics and a barometer to illustrate Boyle's law. Throughout the summer term, however,

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[1] Stokes was explicit in calling this subject "physics" instead of employing the hitherto predominant term of "natural philosophy".

Stokes was generally able to give a wide experimental coverage to his favourite subject of optics by harnessing the seasonal availability of natural sunlight[2], occasionally using such opportunities to divulge his most recent optical researches e.g. showing the spectrum of horse-chestnut bark (in solution) during his lecture course of 1852 [Stokes, 1852-57, NB2 & NB7].

Nevertheless, Clifton's perceptions of Stokes' lectures were that the demonstration experiments were subservient to the essentially mathematical rationale of Stokes' teaching; as Clifton told the 1868 Select Committee on Scientific Instruction:

> I had nothing whatsoever to do with science [at Cambridge], except so far as it was included in the mathematical course ..I may correct that by saying I attended some experimental lectures, which were however of an essentially mathematical character. Professor Stokes lectured to us from an entirely mathematical point of view; he illustrated his lectures by experiments, but the main point was the mathematical theory.
>
> [Clifton,1868b,q2608-2609].

In this chapter we will see how Clifton attempted to emulate Stokes' "model" lectures of mathematical analysis as applied to the results of *in situ* experimental demonstrations, especially in their common specialism of optics. By exploiting the extant correspondence between the two men we will further characterize Stokes' role as mentor, friend and confidant to Clifton as undergraduate and professorial physicist.

After his graduation in 1859, Clifton was awarded a Fellowship of St John's College and published a paper presumably connected with this award

[2] Such seasonal limitations were a characteristic constraint upon optical demonstrations until the proliferation of electric filament lighting in the 1880's.

entitled "On the conical refraction of a straight line" [Clifton,1860]; Clifton presumably lived on his Fellowship stipend until he took up a post as the first Professor of Natural Philosophy at Owen's College, Manchester in September 1860. The controversy surrounding the creation of this Chair, in the year preceding Clifton's election will be discussed in the next chapter on the "extension" of science at Owens College, hence we can now immediately discuss Clifton's early experiences of teaching and research in the Manchester context.

2): <u>Teaching and Research at Owen's College 1860-1865</u>

Clifton's first major duty in his Chair at Manchester was to give the customary inaugural lecture of a new Professor to the assembled college, this oration bearing the title: "Some Points on the History of Natural Philosophy". In this lecture Clifton surveyed the development of all the subjects he had encountered in Stokes' Lucasian lectures of 1856, viz. mechanics, dynamics, statics, hydrostatics, hydrodynamics and optics. The rationale of physics that he expounded in so doing was as follows:"

> In the study of natural philosophy.....we are permitted, if I may be allowed to use the expression, to enter into the workshop of the Creator, to discover the Laws which regulate His works, to trace these laws to their consequences, and hence to become more and more that for which we were created, viz to rule over the the rest of his Creatures in this world, and in a sense to become fellow workers with Him, by applying the Knowledge He permits us to acquire to the benefit and advancement of our fellow men.
>
> [Clifton,1861,26]

Thus Clifton launched his academic career at Owen's College with a creed that amalgamated an orthodox Cantabrigian theology of Nature with the Mancunian industrial rhetoric of harnessing workshops to the purpose of improving mankind.

Taking up the role as lecturer of experimental physics immediately after his inaugural lecture, Clifton's first Professorial act was to apply to the College Finance Committee for funds to augment his collection of lecturing apparatus [MTP,1/10/1860], and after some deliberation the Committee granted him £100 for this purpose [MTP,26/10/1860]. At the same time Clifton negotiated the creation of both his own classroom and also a private laboratory, both rooms being forged from the old college library and reading room; thus Clifton was able to prepare his demonstration experiments in close proximity to the room where his recently-won apparatus was to be stored.

Although the laboratory space required by Clifton was not ready until the second term of his first session, he nonetheless launched into a impressive series of lectures on experimental physics with what temporary facilities he could procure, for Principal Greenwood reported at the end of October that:

> The institution of the class of Natural Philosophy has met with a success at least equal to my expectations for the first session....The trustees will I think have reason to congratulate themselves on having selected a Professor of this subject not only of conspicuous ability but of great zeal in the performance of his duties.
>
> [MTP, 25/10/1860,358]

Although Clifton was immediately deemed a success as a lecturer of physics, he nonetheless faced considerable problems relating to the level of prior education amongst his students. One historian of Owens informs us that the level of preparatory mathematical education in Manchester was such that the average College student had great difficulty in either using decimal notation or in apprehending the concept of a fraction [Fiddes, 1937,43-44]. This issue compromised Clifton's execution of his prescribed lecture courses on experimental physics and mathematical physics to the extent that he formed the latter class on only one occasion and even then he was not obliged to hold the course since the number of participating students, i.e. the six who had received adequate mathematical preparation, was technically insufficient to meet the College quota. Nevertheless, Clifton pursued the task of giving lectures in experimental physics with a rigour and *accuracy* intended to match what he had wished for in the mathematical course; as he told the Select Committee on Scientific Instruction in 1868:

> I advertised every year two courses [one experimental and one mathematical]; by [experimental] I mean a course of lectures in which the premises of the arguments were put forward mainly depending upon experiment, and then the consequences of these premises were deduced as accurately as possible, and in most cases with the same accuracy as could be attained in the mathematical study of the subject, the only difference being that in one case the premises would be deduced from general hypotheses, while in the other experiments would be used in illustration of these premises. The deductions, however in the experimental course, though not going so far, would be stated as distinctly and as accurately as in the mathematical course.
>
> [Clifton,1868b,q2694]

Thus Clifton's mathematical scruples on the accuracy of scientific arguments were vigorously applied in his technique of experimental lecture demonstrations, and as we shall now see he pursued the limits of scientific accuracy in his research to an even greater level.

As his chemical colleague, H.E.Roscoe, similarly reminisced of Clifton's appointment: "Clifton soon became most popular, his lectures were admirable, and enabled me to dispense with teaching any portion of his subject" [Roscoe,1906,109]. Although Roscoe thus deferred all instruction in physics to Clifton and thereby generated an institutional demarcation between his own chemistry teaching and that of Clifton's natural philosophy classes, the Professor of Chemistry nonetheless "tried to persuade [Clifton] to start a research on spectrum analysis" in conjunction with Roscoe himself [Physical Society 1924,30].

Clifton and Roscoe's collaborative research was a study of the extent to which the spectra of metallic vapours were dependent upon temperature, this investigation arising from Roscoe's first-hand acquaintance[3] with related work done by Kirchhoff and Bunsen at the University of Heidelberg in the 1850's and fuelled by Clifton's interest in optical spectroscopy. The product of this research was essentially a precision analysis of the superposition of spectra from vapourized metals and their oxides with a delicate spectrometer closely modelled on the apparatus used by Kirchhoff for his experiments on solar spectra [Clifton&Roscoe,1862].

First publicized at a meeting of that flourishing Mancunian institution of civic science: The Manchester Literary and Philosophical Society in 1862, this first example of Clifton's experimental research clearly demonstrates the fastidious attention to details affecting the accuracy of his work that was characteristic of all his subsequent teaching and research. However, a later anecdote by Arthur Schuster reveals something of Clifton's discontentment with the level of precision that he could attain in his laboratory on the top floor of the Quay St building:

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[3] On March 1st 1861 Roscoe gave a lecture on "Bunsen and Kirchhoff's Spectrum Observations" at the Royal Institution" [Roscoe,1906,69].

> Clifton was a very careful man, who would only take one step at a time, and when he looked through his spectroscope he found that every lorry that passed through the street underneath shook the telescope, so that instead of one point he saw a line vibrating about. He started a research, therefore, in order to see whether, by the manner in which it shook, he could distinguish between a lorry of one kind and a lorry of another kind and an ordinary cab. He never got beyond that stage.
>
> [PhysicalSoc.,1924,30]

Although Schuster was evidently unaware that Clifton did in fact get beyond this stage in submitting a paper to the Manchester Literary and Philosophical Society, it is clear, however, that the problems Clifton experienced in this early pursuit of precision measurement were sufficient for him to abandon his attempts at further experimental researches. In his remaining time at Manchester, Clifton published only one paper which was a theoretical analysis relating to this laboratory work on spectroscopy and in this paper he attempted to apply fundamental aspects of the kinetic theory of gases to the atomic production of spectra: "An attempt to refer some phenomena attending the emission of light to mechanical principles" [Clifton,1865]. (This was not dissimilar to contemporaneous work by James Clerk Maxwell on the dynamical theory of gases [Maxwell,1866]).

The extent to which the structural flaws of the Owen's building compromised the functional integrity of his laboratory was evidently widely known amongst his colleagues and correspondents: we know from the testimonials written for him in 1865 that eminent contemporaries such as G.G. Stokes, William Thomson and H.E.Roscoe hoped that at Oxford he would find "greater scope for research than had been possible at Manchester" [Gunther ,1921,vi]. Clifton's teaching of physics was, however, as much compromised by the architecture of the Owens building as was his research.

In May 1865 he wrote to Principal Greenwood complaining of cramped and unhygienic conditions in his classroom: the overcrowding that had resulted from the ever growing numbers attending the natural philosophy lectures had rendered his small, unventilated and gas-lit room quite injurious to the health of his students [MTP,25/05/1865; Thompson,1886,246-248]. After submitting a plan to the trustees for rebuilding the room with a higher ceiling and rearranged fittings he was able to seat 50 students rather than the previous maximum of 35, and all additionally with a better view of the lecture table vital to the effectiveness of Clifton's elaborate experimental demonstrations [MTP, 27/07/1865][4].

The problems that Clifton thus encountered in accommodating his lecture-students were something of an ironic comment on the popularity of his lectures alluded to above: the number of students paying the fee of £3 3s to attend his experimental courses in successive sessions manifested a progressive increase until his resignation and departure for Oxford in 1865, (except of course during the years of the American Civil War when the consequent cessation of Manchester cotton imports resulted in financial hardship for many employed in the cotton industry)[5]. After he

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[4] The accommodation problem at Owen's was such that Clifton was not able however to provide laboratory space for his students to experiment in; from his later perspective at Oxford he commented that "we were not able to have such classes at Owen's College, for there was not room for them there" [Clifton,1868,q2703].

[5] Class attendance figures, available unfortunately only for his evening classes are as follows:

1860-1	Elementary Mechanics	(no record)	
1861-2	Optics, Heat, Electricity	21	
1862-3	Heat, Electricity	22	
1863-4	Physical and Geometrical Optics	14	(Years of the
1864-5	Acoustics	14	cotton distress)
1865-6	Statics and Dynamics	26	

[Clifton,1868,q2695]

left Owens in November 1865 a Manchester paper favourably contrasted his lectures with those of his successor William Jack, emphasising "that easy grace which marked Mr Clifton, making him most interesting and dignified when lecturing from a seat on the table, or when, having thrown away his gown, he worked with a vigour quite astounding to the audience at some laborious experiment[6]" [Gunther.,1921,viii].

Clifton evidently gained popularity and respect for his lectures at Owen's, since his resignation on the 27th November 1865 led the trustees to minute their regret at the "prospect of being deprived of the services of so valuable and esteemed an officer" [MTP,30/011/1865]. Far from leaving on grounds of dissatisfaction with his position in Manchester, Clifton declared that "a chance of promotion has occurred which may never again happen to me" and thus felt obliged to take up the Chair in Experimental Philosophy that the University of Oxford had recently offered him. Nonetheless, his sense of a continuing obligation to Owen's College moved him to negotiate with Oxford that he should continue teaching in Manchester and finish his courses by Easter 1866, and then also return to supervise the examinations in Natural Philosophy at the end of the following term.

Before analysing the manner in which Clifton transferred his expertise in teaching and laboratory research in precision experimental physics from the Mancunian environment to the Oxford context in the spring of 1866, it is important first of all to understand the Oxonian tradition of experimental physics into which his career was now transplanted. Thus in the next section we will briefly document Clifton's predecessors in the

[6] Clifton sometimes also spent several hours preparing the experiments prior to lecturing with them [MTP,25/05/1865].

Readership of Experimental Philosophy, especially that of his immediate forerunner Rev. Robert Walker and his role in raising the profile of experimental physics in the academic hierarchy of Oxford.

3): Robert Walker and the Oxford Readership in Experimental Philosophy

Experimental natural philosophy had been taught at Oxford since the last decade of Newton's lifetime[7] prior to the creation of the Readership of Experimental Philosophy in 1749 as a complement to the mathematical Chair of Natural Philosophy known as the Sedleian Professorship founded in 1621 [Devonshire Commission 3rd Report,1873,xv] . At the start of the 19th century the duties of the Experimental Readership were evidently none too arduous since several other major University positions were often held in combination with it:

Thomas Hornsby	Reader in Experimental Philosophy	1763-1810
	Savilian Professor of Astronomy	1763-1810
	Radcliffe Observer	1772-1810
	Sedleian Professor of Natural Philosophy	1782-1810
	Radcliffe Librarian	1782-1810
Stephen P. Rigaud	Reader in Experimental Philosophy	1810-1839
	Savilian Professor of Geometry	1810-1827
	Savilian Professor of Astronomy	1827-1839
	Radcliffe Observer	1827-1839

[DNB: Hornsby, Rigaud]

This practice of *cumul* was perhaps necessary because the Reader's stipend was a miniscule £30, augmented by a contingent quantum of student fees; nonetheless the sheer extent of Hornsby's and Rigaud's pluralism leads one to suspect that none of the above positions held heavy duties.

[7] John Whiteside c.1714-29; James Bradley (with astronomy) 1730-1760; Nathaniel Bliss c.1760-1762 [Simcock,1984,20].

However, the Rev. Robert Walker who succeeded Rigaud upon the latter's death in 1839, was considerably more scientifically active in his tenure of the Readership, his only other major occupation during this period was holding the Vicarage of Culham from 1848-1865. Walker was also a more eminent physicist beyond the scientific professoriate of Oxford than Hornsby or Rigaud as instanced in the prestigious position he held as President of the Mathematics and Physics Section (A) of the British Association for the Advancement of Science in 1856. Walker's researches on Foucault's pendulum were, for example published in the B.A.A.S. Report of 1851 [Walker,1851] and he was also a member of several B.A.A.S. research committees including one which investigated the variation of atmospheric temperature by means of high-altitude balloons [Walker,1860].

So far as the teaching of experimental philosophy was concerned, Walker was highly energetic in attempting to improve the position of his subject at Oxford: indeed in the very year of his appointment in 1839 he was co-signatory of a petition presented to the Vice-Chancellor "in favour of physical and mathematical science." At this juncture the only appreciable amount of natural science required for the B.A. examinations was a modicum of book learning in mixed mathematics for the School of Mathematics and Physics, the School of Medicine lying moribund and Litterae Humaniores requiring no scientific tuition whatsoever [Taylor, 1952]. Thus Walker and several of his Professorial colleagues sought to extend the position of physical science in the University's examinations in a petition arguing that "in the present times most especially, some knowledge of physical and mathematical science ought to form an indispensable part of a liberal education": their specific suggestion was that four of the books of Euclid required to be learnt for the B.A. should be replaced with "geometry, algebra, arithmetic or some branch of natural philosophy" [Bodleian MSS, W.P.B.2(9)]. Although the petition was not acted upon at that date, nine years later Walker wrote again, this time independently, to the Vice-Chancellor advocating physical science to be made a compulsory element of all undergraduate study:

> It is, to say the least, discreditable that anyone should go forth from us in utter ignorance of the laws that have been impressed upon matter, and unable to explain the commonest phenomena;....[for example that a student] should suppose that earth, air, fire and water are the four elements of which the world is composed, and that the communications of the electric telegraph are made by pulling wires.
>
> [cited in Taylor,1952,94]

This time Walker's plea met a more immediate reponse for although his party ultimately lost the controversy in Convocation over the issue of compulsory physical science, Convocation did in 1849 formally sanction the establishment of a School of Natural Science in whose B.A. examinations Experimental Physics was granted a position of some prominence when they were first held in 1853 [Papers concerning the University Museum, 1849, Bodleian N.W.2.1; Taylor,1952,95]

In the two years preceding the formation of the School of Natural Science, Walker had busied himself in promoting not only the position of physics in Oxford's B.A. examinations, but also in campaigning for laboratory facilities for science teaching. In July 1847 Walker, along with Acland, Daubeny and Duncan (Keeper of the Ashmolean Museum), had issued an appeal to all the friends of the University to support "the erection of an edifice within the precincts of the university for the better display of materials illustrative of the facts and laws of the natural world" [Acland,1870,2880]. Subsequently, in June of 1849, shortly after

Convocation had founded the new School these men formed a Committee to plan the foundation of a University Museum to accommodate and cultivate the departments of natural science: Robert Walker and Henry Acland were two of this Museum Committee's three Secretaries [Papers concerning the University Museum, 1849, N.W.2.1]. While therefore noting the active role of Walker in thus establishing the study of natural science as a major discipline in Oxford, further discussion of the origins of the Oxford Museum is unnecessary since detailed accounts of its formation exist elsewhere e.g. [Acland&Ruskin,1859; Taylor,1952].

Whilst the finance and architecture of the nascent University Museum were negotiated by the Committee during the late 1840's and 1850's, Walker continued his popular lectures on experimental philosophy as related in the reminiscences of Rev. Tuckwell:

> Experimental Philosophy....meant lectures....by a cheery Mr Walker, who constructed and exploded gases, laid bare the viscera of pumps and steam engines, forced mercury through wooden blocks in a vacuum, manipulated galvanic batteries, magic lanterns [and] airguns...
>
> [Tuckwell,1900,40][8]

From the point of view of laboratory teaching it is extremely interesting to note that some of the students attending these lectures applied to Walker *as early as the 1850's* for opportunities to carry out practical work in experimental physics. This we know from the report Walker made to the Museum Committee in May 1858 regarding his requirements for accommodation in the Museum as the building approached completion; apart from requesting a personal servant to look after the burgeoning quantity of apparatus which Walker purchased with his annual Leigh bequest of £85,

[8] Tuckwell was an undergraduate from 1848-1852.

Walker, now assuming the title of a Professor declared that:

> The Professor also desires an Assistant to give practical instruction to private classes, under the superintendence of the Professor. There is a demand for such instruction which the Professor, from want of an assistant, has found himself unable properly to supply. [The Professor would share his fees with an assistant qualified by a B.A.] for his trouble in superintending these instructions, which would be of a practical character, and correspond in some degree with the work of the laboratory in the Chemical Department.
>
> [Report of the Committee upon the Professor's requirements in connexion with the new Museum, 1858, Bodleian N.W.2.1.:33]

Students of chemistry were indeed taking laboratory courses at this early juncture since Benjamin Brodie, the new Professor of Chemistry, had taken up residence in the Museum the previous year (1857) [Taylor,1952,101]. Notwithstanding Walker's special request for a student laboratory and a teaching assistant in which to emulate these practical courses, he was not granted space for them in the final organization of the Museum complex. When Clifton alluded to this episode of the Museum's history a decade later (in making a similar application for laboratory facilities) he suggested that the Museum Committee had rejected the request made in Walker's report to them: viz. not sharing his conviction as to the necessity of a physical laboratory, they had instead considered it "necessary to provide only for lectures in physics" [Clifton,1868a,1].

In the light of this it is unfortunate that Acland later hinted that experimental philosophy was not well accommodated in the University Museum for the reason that Walker had taken "a somewhat limited view of his Department" [Acland,1870,q2881]: Walker's proposal to the Museum committee to initiate a physical laboratory within his Department in fact suggests quite the opposite about his view of his role in teaching experimental physics. Acland's additional allegation that Walker's infirmity and ill-health limited his effectiveness as a department-builder are also unfounded since it was not until after 1860, when the departmental layout and organization of the Museum were finalized that Walker retired from active scientific life. For in that year the Museum played host to the celebrated meeting of the B.A.A.S. (at which Wilberforce and Huxley disputed Darwin's "Origins"), and at this meeting Walker presented his report for the Association's committee on high-altitude ballooning. Only in the following year did Walker's debilitation cause him to withdraw from the Committee, the loss of his chairmanship resulting in the rapid demise of the Committee [B.A.A.S.Report,1861,249].

Nevertheless, when the Museum finally opened in 1860 Walker's Readership was elevated to the status of a Professorship [Birkenhead,1961, 81], and thus Walker held the Chair of Experimental Philosophy whilst his health subsequently declined until his death in 1865. At this point we can now resume our account of Robert Clifton's career at the point of his application for the new Chair of Experimental Philosophy.

4): the campaign for a physics teaching laboratory in Oxford

In applying for the Oxford Chair in 1865, Clifton made the customary arrangements for his testimonials for the position to be printed and privately distributed. In this volume there are twenty testimonials from "eminent" men of science including Professors G.G.Stokes, J.C.Adams and Whewell of Cambridge, William Thomson of Glasgow, Joule and Roscoe of Manchester and Bunsen and Kirchhoff of Heidelberg. All these men spoke of his lecturing ability in the highest terms [Kurti,1984,313] and although nothing is recorded in the minutes of the Hebdomadal Council about the selection procedures for appointing the new Professor, the impact of this unimpeachable array of references was sufficient to secure Clifton's election to the chair in November 1865.

In taking up his new mantle at Oxford in spring 1866, Clifton began work in the Museum, just as he had finished at Owen's, by rebuilding his departmental accommodation but in addition negotiating to use an adjacent unoccupied room for the purposes of laboratory teaching. As he wrote to Sir William Thomson the following year:

> I found on my appointment at Oxford that the department of Physics was provided only with a lecture room, a small laboratory attached which is completely absorbed for lecture purposes and a small office. No arrangements had been made for students to work practically themselves to gain a thorough knowledge of Physics. I borrowed a small room from one of my colleagues [the chemist Brodie] who does not reside in Oxford and...... endeavoured to let a few [students] work practically at physics, but could not receive all who applied [for this popular class].
>
> [Clifton to Thomson,16/09/1867,UL Glasgow C32]

To fund the necessary refurbishment of the lecture theatre and laboratory, purchase new apparatus and employ two servants to assist him and maintain these rooms Clifton successfully petitioned Convocation for a lump sum of £280 and annual grants totalling £200 [Bodleian G.A. Oxon C.82.91]. This was the first example of the high degree of financial support that the University authorities were to give Clifton over the first decade of his tenure, and was symptomatic of the general University sympathy for the Museum that was frequently given generous financial expression until the end of the 1870's [Howarth,1987,334 & 339].

Whilst Clifton was readily granted the necessary endowments from the University Chest, he nonetheless made plain at the same time that he envisioned much more elaborate facilities for the future development of experimental physics within the University. The Committee of Convocation adjudicating upon these financial arrangements announced that Clifton had specified his "requirements for a *complete* course of training in physics" these requirements including a large public laboratory, workshop, apparatus room and a laboratory demonstrator in addition to the (non-teaching) servants already granted to him [Bodleian G.A. Oxon C.82.91]. Clifton, however, did not press his claim so soon after arriving in Oxford; instead he arranged practical classes in the room borrowed from Brodie and taught students the methods of precision measurement as best he could.

Yet over the next year the difficulties he encountered in attempting thus to teach in the claustrophobic confines of his borrowed laboratory in the Museum became intolerable for Clifton. When he related this episode to Thomson in 1867, he told not only of his regret that he had to turn students away for lack of space but also that the quality and the all-important accuracy of his students' work was being compromised by the cramped conditions of the laboratory environment, these conditions being exacerbated by the fact that the room could not conveniently be darkened for Clifton's beloved optical experiments:

> ....those whom I received [could not] get on well in one small room (not capable of being darkened). Many experiments were altogether impossible and those which were possible in many cases so interfered with one another that accuracy was in general out of the question.
>
> [Clifton to Thomson,16/09/1867,UL Glasgow C32]

Exasperated by this situation Clifton joined forces with Westwood, the Professor of Zoology, to appeal to the Vice-Chancellor for new buildings in which to give adequate practical instruction to their respective students. The letter that Clifton wrote is a document which yields a great deal of information on his early conception of a physics teaching laboratory, and also on the arguments he constructed to justify the expenditure that his scheme would entail. A brief analysis of the document will thus be used as a running commentary to the text given *in extenso* below.

Firstly Clifton expounded the pedagogical virtues uniquely and invaluably possessed by experimental physics, over and above those characteristic virtues that this subject held in common with mathematics:

> The great advantage of the study of Physical Science, as an aid to Education, is avowedly the exercise which it gives to the faculty of reasoning, combined with that of observation, and it is in this combination alone that it differs essentially from mathematics.

Having established physics as a complementary discipline to mathematics, Clifton next argued that the laboratory was the only place where such "advantages" could be acquired, especially now that physics had advanced to a sufficient level of disciplinary coherence to be taught experimentally:

> The development of the powers of observation, which may be commenced by attendance upon experimental lectures, can only be fully produced by engaging the student himself in experimental work, and this circumstance has caused the establishment in all places where the study of science is a reality of laboratories for the use of students, a laboratory for the study of any branch being founded as soon as that branch becomes susceptible of being taught to beginners.
>
> Chemistry and physiology have for some time been recognised as thus susceptible of being practically taught, and in the University Museum ample provision has been made for making such teaching really effective.
>
> Since the erection of the museum, the rapid progress in

> physics has been recognised by the University of Oxford by requiring those candidates for honours in the Physical Sciences School who select physics as their special subject to pass a practical examination.

Given that the regulations for honours in Natural Science thus demanded such experimental expertise of physics students, Clifton next underlined how ironic it thus was that such students were often obliged to go to other academic institutions to learn the practical physics that the Oxford examinations required of them:

> For the practical study of physics no rooms are provided in the museum, nor do any rooms exist there which can be converted into physical laboratories. Hence students wishing to distinguish themselves in the Oxford school of physics must seek the instruction which is absolutely necessary in some other place of education.
>
> It may be added that some knowledge of physics must form the basis of a thorough study of chemistry and physiology; and this fact is recognized by the University inasmuch as every candidate for honours in physical science must pass in mechanical philosophy and every student in the natural science school must be examined in some of the particular sciences dependent upon it. Hence the means of instruction in Physics should be obviously as complete as those provided for chemistry and physiology.

Next Clifton hinted at the necessary architectonic complexity of a physics laboratory that could accommodate the accurate pursuit of a diverse range of experimental activities, stressing particularly the special requirements for his favourite subject of optics:

> As a Physical Laboratory must include means for the simultaneous instruction of students in various stages of progress and as different physical apparatus cannot frequently be carried on under the same circumstances - some for example requiring ample light whilst others require almost total darkness - it is impossible to meet these various wants in one room, however well arranged: the laboratory must therefore consist of a number of smaller

rooms, specially arranged for definite purposes.

> The principal operations which a student would be called upon to perform may be divided into 3 classes viz.
>
> 1) those requiring ordinary daylight
>
> 2) those requiring darkness with the exception of a beam of sunlight.
>
> 3) those requiring complete darkness.
>
> [and] each class must be separately represented....their experiments are in many cases [otherwise] liable to interfere with one another.

Finally Clifton pleaded the necessity of special storage and maintenance rooms to shield his delicate apparatus from sources inimical to its precision, and then emphasized the importance of a laboratory in which to pursue his own personal researches:

> As the director of such a laboratory will be called upon to arrange all the work for the students, and to do this without interfering with the work already in hand, he should be provided with a private laboratory, in which also he might carry out original researches......
>
> [Minutes of Museum Delegacy, Bodleian 1/02/1867]

On receipt of this letter, the University appointed a Committee to consider ways of resolving CLifton's complaints [Clifton to Thomson, 16/09/1867, UL Glasgow C32] and in their subsequent investigations a possible source of finance for Clifton's scheme was located soon afterwards in the form of the Clarendon Trust. The Clarendon Trust was bequeathed to the University of Oxford by Henry, Lord Hyde, in 1751 and consisted of the accumulated income from the sale or publication of historical papers by his grandfather, the first Earl of Clarendon. Hyde had originally intended that the Trust should fund a University academy for horse-riding [Gunther :, 1921,vii]. However, a codicil to his will stipulated that "in case no such institution should be accepted by the university" then the Trust should be put to "such other uses as his Trustees should judge to be the most for the honour and benefit of the university and most conducive to the public utility [Bodleian, G.A. Oxon c.83.332, 09/12/1867]."

Now on the 13th December 1864, before Clifton's eyes had even first alighted on Oxford, the Trustees rejected the specific object of Hyde's will whose operation thence reverted to the action of the codicil. Soon afterwards an approach was made to the Senior Trustee, one William Ewart Gladstone, to direct the trust towards a recreational park in Oxford but this was rejected by the body of Trustees in June 1865. In subsequent correspondence with the University the Trustees intimated that a scheme eligible to meet the conditions of the codicil to Hyde's will would have to "perpetuat[e] the name of the trust in connection...with some visible object [that was] as definite, tangible and complete in itself as may be" [Bodleian G.A. Oxon c.83.332, 09/12/1867].

These qualities of definiteness, tangibility, and completeness were thus the criteria under which Clifton's scheme to use the trust to finance his physical laboratory were to be scrutinized in April of 1867. Yet despite the University's general sympathy with Clifton's scheme, when the Vice-Chancellor, Lightfoot, wrote to Gladstone on behalf of the Hebdomadal Council on the 5th of April, he put forward a proposal that took precedence over Clifton's, enquiring "whether the trustees would approve funding of new examination schools for candidates' accommodation and for increased room in the Bodleian", the current provisions being "very inadequate and inconvenient for examination purposes".

As a secondary call upon the Clarendon Trust, Lightfoot gave a qualified acknowledgement of Clifton's claims in arguing that the construction of a physics laboratory was "rendered desirable, if not necessary, by the rapid development of [physics] since the foundation of the museum, and the large appliances which have consequently become requisite during the last few years". The Vice-Chancellor added also that an edifice of physics laboratories would be particularly well suited to the trustee's criteria of a definite, tangible and complete object capable of perpetuating the title of the Clarendon trust to posterity. Lightfoot made plain however that the Hebdomadal Council had not formally recommended Clifton's proposal to the University, which anyway was then "so pressed by many other claims upon the resources of its Chest". [Vice-Chancellor to W.E. Gladstone, 05/04/1867, G.A. Oxon c.83.332].

On the same day that Lightfoot sent this letter, Gladstone met his fellow trustees to discuss the matter and had "no difficulty" in recommending Clifton's scheme as the preferred option. Gladstone immediately wrote to Lightfoot informing him that the trustees were "ready, in concert with the University, to consider of the best mode of applying the funds belonging to them for adding to the New Museum a laboratory for experimental philosophy" [W.E.Gladstone to Vice-Chancellor, 05/04/1867, G.A. Oxon c.83.332].

Although Clifton had the general support of the Clarendon Trustees for his scheme, he had yet to attain the formal agreement from both University and Trustees to fund any *specific* laboratory construction. In order to furnish the information on which the Trustees could formalize their offer and the University could decide upon accepting or rejecting this offer, Clifton was requested by the Committee of the Hebdomadal Council to draw up plans for his laboratory "showing the number and arrangements of the rooms required for a moderately complete course of instruction in physics." After sketching out a plan Clifton sought advice from the foremost authority on physics laboratories in Britain, Sir William Thomson[9]. Clifton asked him to suggest possible improvements in these plans, and if Thomson agreed with the general layout to "write a few lines expressing such agreement and an opinion as to the necessity for students to receive practical instruction which I know you entertain". Such agreement would evidently support Clifton's application to the Hebdomadal Council.

The plans he sent to Thomson were for a projected two-storey laboratory of a complexity quite unprecedented in British physics[10], and in their complexity they express very clearly Clifton's conviction (expounded in Clifton 1/02/67 above) of the need for architectural partitioning between sub-laboratories devoted to different branches of physics. In fact he devised an arrangement of twelve such sub-laboratories for student teaching, situated in such a way as to prevent mutual "interference" between experiments in adjacent rooms that otherwise caused inaccuracy in the results of experiments in close proximity to one another. Five of these sub-laboratories faced South to receive direct sunlight, three rooms for general optics, and one each for spectrum analysis and radiant heat; in addition the lecture theatre was situated facing South to facilitate the use of sunlight in Clifton's optical demonstrations. The remaining seven rooms were allocated to weighing and

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[9] Clifton told Thomson he would have discussed the matter at the B.A.A.S. meeting in Dundee, had he not been ill during that summer.

[10] Clifton claimed however that his proposed building was "on about the same scale" as the Museum's existing laboratories for chemistry and physiology [Clifton to Thomson, 16/02/1867].

measuring, acoustics, two for dynamical electricity, statical electricity and two for heat facing north, augmented by a small room for mathematical physics, three private laboratories for the Professor and all built round a central court in which the apparatus was stored and displayed.

Ambitious as this plan appeared by comparison with the one or two room laboratories operated by Thomson in Glasgow, and G.C. Foster in London, Clifton mitigated the scale of his plans by using evidence that: "the demand for scientific instruction appears to be rapidly increasing here and I think the accompanying plans represent no more than will be needed in a few years tho[ugh] perhaps more than will be required immediately". Similarly for the purposes of research, he considered the nascent Clarendon far from extravagant in catering for the needs of young academics embarking upon a career in research: "as soon as proper arrangements are made I believe a class of Senior Students (young fellows of Colleges) will arise - some individuals of the class already exist who are anxious to devote themselves to physical research - and with twelve laboratories, it will I hope be possible to accommodate some of them with rooms where they may carry on their work without interruption. [Clifton to Thomson, 16/09/1867, UL Glasgow C32].

Since the plans outlined in this letter are virtually identical with the final structure of the completed Clarendon laboratory in 1870, we can deduce that Thomson can have suggested at most only the most minor of alterations to Clifton's plan: indeed it is highly unlikely that Thomson opposed any major aspect of this plan given his energetic promotion of laboratory work throughout his entire career [Ch.2]. Thus with minimal revision, Clifton's plans were submitted to the Clarendon Trustees in the autumn of 1867.

On the 22nd of November the Trustees considered the formal architectural plans prepared by one of the Museum architects, Henry Deane, to accompany Clifton's proposal. Subject to a number of financial contingencies, the trustees declared themselves "willing to apply their funds to such a building, if it be acceptable to the University" [Sir William Heathcote to Vice-Chancellor, 22/11/1867, G.A. Oxon C.83]. On the 9th of December the University thus announced that for a week prior to a vote by Convocation on whether to accept the offer, Clifton's plans were to be available for inspection by members of Convocation in the vice-chancellor's office from the 27th January 1868 [G.A.Oxon C.83.332 09/12/1867].

Two days before the plans were laid out in the Vice-Chancellor's Office, Clifton published a document that forcefully explained his need for a physical laboratory, making much more detailed and extensive arguments than in his letter to the Vice-Chancellor of the previous year (01/02/1867). This document he entitled "The Offer of the Clarendon Trustees" and arranged to have copies circulated widely throughout Oxford to canvass support amongst the members of Convocation. On the day of the vote, the 4th of February, Clifton wrote to inform Stokes of the circumstances and outcome of the vote: in apologizing for his delay in sending Stokes a copy of his marks for a Cambridge examination paper Clifton explained: "I should have sent it much sooner but I have been so extremely busy looking up friends to support my laboratory scheme...I send you a copy of a document I have circulated here. If you don't care to read it, put it on the fire. I don't want it again" [Clifton to Stokes, 04/02/1868, UL Cambridge ADD 7656 C711].

This important document is given in extenso below. It develops the themes he raised in his open letter of February 1867, but he now placed greater emphasis upon the importance of 1) meeting a national need for laboratory-trained science masters and 2) the extent of the facilities required to give students destined for such careers an exhaustive and above all an *accurate* education in the techniques of experimental physics. Nevertheless, Clifton's primary claim to the Clarendon Trust was still that the enormous recent progress in physics should be acknowledged by the University in granting his subject facilities for practical instruction. These facilities, he argued should compare with the existing accommodation in the Museum for the sibling disciplines of chemistry and physiology, and also match the developments in physics departments at other academic institutions around Britain:

<u>The Offer of the Clarendon Trustees</u>

The University Museum
January 25th 1868

> As members of Convocation are called upon to consider the offer of the Clarendon trustees, to employ the funds at their disposal in the erection of additional buildings to facilitate the study of physics, they may find it useful to have a short statement of the circumstances which render additional buildings necessary and of the nature of the accommodation required.
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> At the time when the museum was built, it was considered necessary to provide only for lectures on physics; and it was not perhaps possible at that time to give students opportunities for undertaking experimental work themselves.
>
> Physical Science has now however been greatly extended: it has now become necessary for students to achieve fuller instruction than can possibly be given by public lectures, and it is as important for a student of physics to become acquainted, by actual experience with *accurate* physical processes, as it is for students of chemistry and physiology to receive practical instruction in these departments of science.
>
> There is also another circumstance which makes the want of Physical Laboratories particularly pressing at this time, namely the demand which is now arising for skilled scientific masters in the principal

schools of the country; and as no knowledge of chemistry or physiology can be sound or accurate without some knowledge of physics, it is impossible in the absence of the means of giving proper instruction in physics, to enable members of the University to qualify themselves for supplying efficiently the already considerable and rapidly growing demand. Every term many applicants for practical instruction in physics are of necessity refused; and the few who are received have to be content with very imperfect instruction in the small rooms temporarily borrowed from another department.

This want has been recognised in other schools of science. For many years physical laboratories have been established in the University of Glasgow, and in the principal Universities of the Continent, and plans are being made actively to organize similar institutions in London [Ch.4+5] and Manchester [Ch.7].

In order to understand the accommodation that is required in a physical laboratory, it must be remembered that many instruments of precision, which are used in accurate physical experiments require very considerable time to fix them. Many days or even weeks have to be spent sometimes in fixing a single instrument so as to obtain the best results which it is capable of giving, and even a much longer time is often needed to determine, so as best to allow for, the various sources of error to which an instrument may be liable; and of course if the instrument is moved, this labour has to be undergone afresh.

Again it is impossible to carry on *accurate* physical experiments in close contiguity to one another, owing to their mutual interference; and consequently different processes need different rooms, in which these delicate instruments which are always required in a particular branch of science have to be carefully and permanently fixed.

It will be seen that a physical laboratory must consist of a number of separate rooms, these rooms not being appropriated to particular students, but to particular instruments and when any instrument is required it will be used in the position in which it has been fixed, in order to give the most *accurate* results.

It may be sufficient, in order to give an idea of the number of rooms required, to enumerate the chief branches of physics which require special accommodation, owing to their mutual interference:

1) Weighing and measuring
2) Heat
3) Radiant heat
4) Dispersion of light, Spectrum Analysis etc.
5) General Optics
6) Statical electricity
7) Dynamical electricity
8) Magnetism
9) Acoustics

Of these (5) requires one large room or three smaller rooms, and these together with those devoted to (3) and (4) should have a south aspect.

Besides the fixed instruments, there is a large quantity of moveable apparatus, which is either used with them or employed in illustrating lectures; and this must be carefully preserved from causes of deterioration when not in use; for this purpose a large room fitted with glass cases is required. A store house for chemicals and other materials used is also necessary.

After a student has performed the experiments which he needs for the matter he is considering, he has almost always a considerable amount of calculation to go through before he can obtain the desired result, and in order to avoid crowding in the laboratories it is desirable that a room should be provided to which a student can retire to finish his calulations as soon as he has completed the experimental part of his work. The same room would be used for courses of mathematical lectures on theoretical physics, which would never be attended by larger classes; it would also be used for catachetical lectures. If these were given in the large lecture room they would greatly interfere with the preparation of the public lectures.

As photography is now very much employed in multiplying results of observations, in constructing diagrams for lectures etc. and as it is in fact a branch of physics, a small photographic room is necessary, both for the general use and for studying the subject itself.

For the experimental lectures the same accommodation as is at present provided would suffice, namely, a large lecture theatre with a small laboratory attached.

The Professor would have to be on the spot during the whole day [and] would require a private room and a private laboratory, divided into two parts so that one part could be darkened, whilst the other is light, in which he would be able to arrange the general work of the establishment, as well as to carry on private research.

Such being the accommodation for the successful prosecution of physical studies, an attempt has been made to arrange a building in which those conditions are fulfilled. Although nothing unnecessary has been introduced into the plans, yet if after the acceptance of the offer of the Clarendon trustees by the university it should be found that the estimates exceed the funds in their hands, such changes must be made in the design as will bring the cost within those limits; for there is no intention of asking Convocation to contribute towards the erection of the building, even if the trustees themselves were likely to consent to having their outlay supplemented by the University Chest.

R.B. Clifton.

[Bodleian G.A. Oxon c.84.6]

This document evidently captured the imagination of the members of

Convocation, sympathetic to science as they had been throughout the 1860's, for as Clifton wrote to Stokes on the 4th of February:

> The business came before Convocation today and I am glad to say passed without any serious opposition, and what opposition there was, was withdrawn before the votes were taken. The result was that in a rather full Convocation, there was only one dissentient. [Clifton to Stokes, 04/02/1868, UL Cambridge ADD 7656 C711]

The identity of this opponent to Clifton's case is not known, although it could perhaps have been Charles Neate, described by A.J. Engel as the "cantankerous senior fellow of Oriel", who was the sole opponent of expansion in physical science interviewed by the Oxford University Commission of 1877 [Engel,1983,218-219]. Two days after the vote had successfully been taken in Clifton's favour and thus out of danger from such isolated sources of opposition, Clifton's well publicized document became the subject of a gentle parody by a young mathematics don at Christchurch: Charles Dodgson. In this document the emergent Lewis Carroll wittily transposed "The Offer of the Clarendon Trustees" into a rival appeal for the accommodation of Dodgson's own subject of mathematics:

THE OFFER OF THE CLARENDON TRUSTEES

"Accommodated: That is, when a man is, as they say, accommodated: or when a man is—being—whereby—he may be thought to be accommodated; which is an excellent thing."

(Written in 1868 as a letter suggesting, half-humorously, half-seriously, new means for mathematical research.)

DEAR SENIOR CENSOR:

In a desultory conversation on a point connected with the dinner at our high table, you incidentally remarked to me that lobster-sauce, "though a necessary adjunct to turbot, was not entirely wholesome."

It is entirely unwholesome. I never ask for it without reluctance: I never take a second spoonful without a feeling of apprehension on the subject of possible nightmare. *This naturally brings me to the subject of Mathematics,* and of the accommodation provided by the University for carrying on the calculations necessary in that important branch of Science.

As Members of Convocation are called upon (whether personally, or, as is less exasperating, by letter) to consider the offer of the Clarendon Trustees, as well as every other subject of human, or inhuman, interest, capable of consideration, it has occurred to me to suggest for your consideration how desirable roofed buildings are for carrying on mathematical calculations: in fact, the variable character of the weather in Oxford renders it highly inexpedient to attempt much occupation, of a sedentary nature, in the open air.

Again, it is often impossible for students to carry on accurate mathematical calculations in close contiguity to one another, owing to their mutual interference, and a tendency to general conversation: consequently these processes require different rooms in which irrepressible conversationalists, who are found to occur in every branch of Society, might be carefully and permanently fixed.

It may be sufficient for the present to enumerate the following requisites: others might be added as funds permitted.

A. A very large room for calculating Greatest Common Measure. To this a small one might be attached for Least Common Multiple: this, however, might be dispensed with.

B. A piece of open ground for keeping Roots and practising their extraction: it would be advisable to keep Square Roots by themselves, as their corners are apt to damage others.

C. A room for reducing Fractions to their Lowest Terms. This should be provided with a cellar for keeping the Lowest Terms when found, which might also be available to the general body of undergraduates, for the purpose of "keeping Terms".

D. A large room, which might be darkened, and fitted up with a magic lantern, for the purpose of exhibiting Circulating Decimals in the act of circulation. This might also contain cupboards, fitted with glass-doors, for keeping the various Scales of Notation.

E. A narrow strip of ground, railed off and carefully leveled, for investigating the properties of Asymptotes, and testing practically whether Parallel Lines meet or not: for this purpose it should reach, to use the expressive language of Euclid, "ever so far".

This last process, of "continually producing the Lines", may require centuries or more: but such a period, though long in the life of an individual, is as nothing in the life of the University.

As Photography is now very much employed in re-

cording human expressions, and might possibly be adapted to Algebraical Expressions, a small photographic room would be desirable, both for general use and for representing the various phenomena of Gravity, Disturbance of Equilibrium, Resolution, etc., which affect the features during severe mathematical operations.

May I trust that you will give your immediate attention to this most important subject?

Believe me,
Sincerely yours,
MATHEMATICUS

February 6, 1868.

THE NEW METHOD OF EVALUATION

AS APPLIED TO Π

"LITTLE JACK HORNER
SAT IN A CORNER
EATING HIS CHRISTMAS PIE."

THE problem of evaluating π which has engaged the attention of mathematicians from the earliest ages, had, down to our own time, been considered as purely arithmetical. It was reserved for this generation to make the discovery that it is in reality a dynamical problem: and the true value of π which appeared an *ignis fatuus* to our forefathers, has been at last obtained under pressure.

The following are the main data of the problem:

Let U = the University, G = Greek, and P = Professor. Then GP = Greek Professor; let this be reduced to its lowest terms, and call the result J.

Also let W = the work done, T = the Times, p = the

Dodgson's satire was quite specifically targetted: the discussion of lobster sauce as an unwholesome adjunct to turbot, would appear to be a mocking allusion to the notion of laboratory accommodation as a necessary, if, in Dodgson's view, unwholesome adjunct to the study of experimental physics. His next comments clearly satirize Clifton's requirements for carefully divided accommodation to preclude such interference between contiguous experiments that would diminish the accuracy of experiments carried out; Dodgson's specification of absurdly specialized functions for each of these rooms is particularly pointed. The final comments upon the photographic room derisively hint at Clifton's overstatement of the importance of the subject, cataloguing a few varieties of physical phenomena that were patently not amenable to recording by photographic means.

Overall though, we can interpret the early Lewis Carroll here as making a somewhat pungent commentary on what contemporaries might have considered the most provocative or even radical features of Clifton's arguments in the context of the wide variety and large number of schemes that Convocation were obliged to consider at the time [see third paragraph]. Certainly Dodgson's satirical rendition of Clifton's paper seems to have done little damage to the Professor of Natural Philosophy's campaign for a physical laboratory, for work upon erecting the Clarendon began almost immediately after the February vote of Convocation.

Whilst work began on the foundations of the Clarendon, Clifton continued his experimental teaching in the suite of rooms temporarily allocated to him in the museum. Of this teaching we have a considerable amount of detailed information from the two Government commissions of inquiry that interviewed him in the two and a half year period over which the Clarendon was built. Firstly in May 1868 Clifton was summoned as a witness before the Parliamentary Select Committee on Scientific Instruction who interviewed him primarily to determine his views upon the importance of scientific education for the industrial classes. Later in July 1870, he was interviewed by the Royal Commission on Scientific Instruction and the Advancement of Science whose enquiries focussed upon the research facilities at Clifton's disposal. In the course of these interviews Clifton supplied the Committee with some incidental information about the operation of his temporary laboratory in the Museum, and how the demands of his experimental teaching necessarily precluded the possibility of much research.

5): Clifton's teaching in the pre-Clarendon laboratory.

First of all, Clifton explained how his teaching methods at Oxford were a direct extension of those he had developed at Owen's College earlier in the decade, describing the two courses of experimental physics in the Museum as follows:

> [Firstly there are] the simple lectures, which are very much the same as the Owen's College Lectures...[the number of students in the] lecture class has gradually increased in the two years that I have been at Oxford, and the last number of attendants was 40 to 50;[11]....
>
> [Secondly there is] a thoroughly practical course. The students in that class do experiments for themselves. Each man has a certain amount of work given him to do; apparatus is placed before him, and he has to use it. He is instructed in the use of that apparatus, and [then] has to use it for himself. This class is necessarily small......I have, at present eight students. I have been obliged to

[11] i.e. about as many as were attending in his rebuilt lecture-theatre in Manchester in 1865.

decline six applicants in consequence of not having room for them[12]......Our arrangements for it are very deficient; but the new building....is designed expressly to enable the students to join that class in larger numbers.

[Clifton,1868b,2699]

Clifton explained that great competition existed for places in his classes because as he argued in "The Offer of the Clarendon Trustees" there was then a high national demand for science teachers and indeed most of his students professed to have "teaching for their object" also attending the laboratory classes in chemistry to qualify themselves for a career as a science master [Clifton,1868b,q2704]. As his teaching of experimental physics became established in the university over the next two years, his laboratory clientele developed something of a broader character[13], as he informed the Devonshire Commission in 1870:

> Some come [to the laboratory] simply because they think that a modern liberal education is not complete without some knowledge of natural science; there are a good many of these. They do not generally spend much time in the laboratory, in fact they have not time to do so. If they study any other subject very seriously, they cannot spend much time in the laboratory. Those who work longest in the laboratory are apparently intending either to go into the medical profession, thinking physics a fundamental part of their training; or they are studying with a view of going to the bar, where it is thought some knowledge of science might be useful; or to engineering, or some branch of engineering were science may be useful.....

Nevertheless, the vast majority of Clifton's students were still concerned to qualify themselves as science masters:

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[12] At the time of his interview for example he had already received four requests from students to reserve places in Clifton's practical class of May 1869 [Clifton,1868b,q2708].

[13] Clifton had projected this in 1868, in his comment to Select Committee "the attendance [of] science teaching at Oxford is very decidedly on the increase. There has been a very greatly increased attendance for every term that I have been there" [Clifton,1868b,q2705].

.....But by far the larger part are intending to become teachers, and, judging by what they do afterwards the great majority become teachers. Almost every pupil that I have had who has really seriously done good work, and of whom I have thought very highly, has become a teacher of the subject either in the university or in the schools.

[Clifton,1870,q3005]

The practical instruction which Clifton gave to the eight students that he could fit into his small pre-Clarendon laboratory was essentially a training in the techniques of making precise measurements with a wide range of instruments. These "quantitative determinations" as he euphemistically referred to them, first of all took the "form rather of physical manipulation, and the accurate use of instruments; afterwards, researches or portions of researches, which have been [previously] performed are repeated" [Clifton,1870,q2999]. In referring to "researches" Clifton meant standard experiments of well-known results: his obituarist in Nature explained that his laboratory instruction consisted "almost entirely of repetitions of known experiments carried out with as much accuracy as possible....every student received a sound grounding in accurate experimental work [Nature,107,19][14].

As Clifton later explained to the Oxford Commission of 1877, the rationale of students repeating these standard experiments to such a great level of accuracy was intimately related to their training as teachers. In order for these students to have the authority to teach physics, they had to know the *level of precision* with which the currently accepted

[14] Another remarked similarly that his students "were all given a thorough grounding in experimental methods and were invariably taught to aim at the highest attainable accuracy in their work"[Monthly Notices of the Royal Astronomical Society, Obituary,82,248].

theories of physics had been tested in order to displace their rivals. They wished, therefore:

> to go into the evidence upon which the generally received statements and theories of science depend, and in order for them to become fully qualified teachers of physics, this evidence is vital to present. Rough apparatus would merely give a very general and usually very imperfect idea of the evidence. It is necessary in order to obtain the evidence complete in our present state of knowledge that students should be able to repeat the detailed experiments which the different shades of opinion have held and then supported or opposed to get to our present state of knowledge. It follows therefore that we must have for such students really good apparatus.
>
> [Clifton,1877,q439]

Since Clifton acquired or constructed the best apparatus he could in order for his students to carry out such high accuracy confirmations of contemporary theories, he went to great lengths in planning the Clarendon laboratory to ensure that it would be the ideal environment for his students' accurate measurement experiments. He consequently employed every artifice at his disposal to ensure that such measurement operations were carried out in a laboratory domain isolated from disturbing influences active both inside the laboratory and also beyond the laboratory walls. He thus not only partitioned the laboratory to prevent "mutual interference" between experimenters engaged in different types of experiment, he also contrived to avoid the use of any metal in the superstructure of the building to prevent stray induced currents from adversely affecting measurements of the earth's magnetic field [Birkenhead,1961,90].

Thus as the Clarendon approached completion in March 1870 Clifton was consequently deeply concerned at the threat posed by the prospect of nearby heavy traffic to the elaborately contrived physical isolation of his laboratory's experimental environment. As Clifton wrote to Stokes in great alarm:

> ...about 50 feet from the new Physical Laboratory is a road which is the property of the University...it is now proposed to throw this road open and...I fear there may be a considerable through traffic, which will seriously in my opinion, interfere with the use of our delicate instruments. I have consequently protested against having the road thrown open to heavy traffic, but in many quarters I cannot get believed as to the injury and hindrance which may be caused by shaking the instruments. Will you kindly bode me up by expressing an opinion on the subject as strongly as you feel able and allowing me to use your letter...to convince the more sceptical.
>
> [Clifton to Stokes, 12/03/1870, UL Cambridge MSS AD 7656 C714]

Since no further controversy on the subject has been recorded, it may be inferred that the matter was rapidly settled in Clifton's favour.

Another strategem used by Clifton to maximise the quality of the measurement work characteristic of his laboratory instruction was to restrict entry to only those students who had a high minimum quotient of prior knowledge of mathematics and physics: "the students in my laboratory are almost all mathematicians. I insist upon considerable previous knowledge, both of mathematics and of science, before I allow them to handle the apparatus. It would naturally be a costly class to keep up with unprepared pupils"[Clifton,1868b,2704]. In this comment we see the first indications of Clifton's protective attitude towards his delicate apparatus and also of an apparent elitism in his teaching methods which contrasts greatly with that of his Scottish colleagues. Although Clifton was criticized for these traits by his successors and also by some of his obituarists, the origins of this behaviour are quite comprehensible when considered in the context of his student clientele and the material resources at his disposal.

As Clifton complained to the Select Committee in 1868, his students at Oxford, like those he had taught at Owen s College were frequently as ill-prepared in arithmetic due to the low teaching standards prevalent in schools: a "very great many" of students at both of these institutions coming up "shockingly ill-prepared". Hence he regarded improvements in the mathematical teaching in schools as "essential" for "anything like accuracy in science" [Clifton, 1868b, q2619&2745]. Without a sufficient conception of mathematical and scientific exactitude his students could not comprehend the high accuracy of measurement for which Clifton had designed his apparatus, nor the consequent care with which they thus had to be handled.

To avoid the possible whig interpretation that Clifton's demands for mathematical expertise were unnecessary in their idosyncratic extremes, it is important to note the support he received for this policy both from colleagues at the Museum and from his counterparts at laboratories in other institutions. As Henry Acland, the Oxford Professor of Anatomy, argued in 1870: "supposing a person to come to Oxford to work in Professor Clifton's laboratory of physics, it is quite clear that the thing which he most wants is intelligent training in mathematics. It is a mere waste of time for him to have to work in what is called a practical laboratory, if he comes up to Professor Clifton untrained in general literary culture, and without any taste or mathematical power whatsoever" [Acland,1870,q2964]. Such views on this subject were not dissimilar to those of Clifton's undergraduate associate and contemporary at King's College, London William Grylls Adams who, for the same reasons, only allowed students in their final year to undertake laboratory work [Ch.5].

Nevertheless, whilst Clifton was therefore quite orthodox in excluding junior students and non-mathematicians from his laboratory, he was exceptional in the quantity of labour that he invested in the construction of his laboratory equipment, a feat that was financed by three annual grants of £150, a block grant of £1000 from the University Chest in addition to the annual income of the Leigh fund which now amounted to £94 [Clifton 1870,q2993-2996][15]. One obituarist has pointed out that much of the apparatus "was designed and redesigned by him until perfection or something approaching it, was reached, and so much loving care had been spent on an instrument that it needed to be kept jealously under lock and key, taken out from time to time to be dusted and cleaned, possibly used in a lecture, but never entrusted to the careless handling of a student of Physics" [Gunther, 1921,vii]. Another declared that equipping the Clarendon with apparatus was a "labour of love to Clifton, who was a born instrument maker, and reiterated Gunther's point that "much of the apparatus had been of his own designing, with the result sometimes that when an instrument had been brought to perfection it had become too sacred to be entrusted to the common herd" [Nature,107,19].

Clifton used this finely-wrought apparatus in his lecture demonstrations, and according to his obituarist in Nature he was no less zealous in his preparations for these than he had been in constructing the equipment: "Clifton was an excellent and inspiring lecturer, and spent an enormous amount of time in designing and fitting up apparatus for lecture purposes, so that his lectures were often more of the nature of laboratory demonstrations"[Nature,107,19]. An obituarist writing for the Royal Astronomical Society commented in similar vein that "Clifton spared no pains in the preparation of the experiments for illustrating his lectures"

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[15] Clifton himself estimated that, including the value of the apparatus that he had inherited with the professorship in 1865, the contents of the Physical Cabinet were worth at least £3,500 [Clifton,1870,q2996].

[Monthly Notices RAS,82,248]. Thus Clifton, like G.C.Foster [Ch.4] and Stokes [see above & Ch.5], regarded the lecture theatre as an extension of the laboratory, his private laboratory work being consumed entirely in preparing for the lectures and the lecture demonstration consequently carrying the authority of a laboratory discovery.

Given that after lecturing Clifton devoted the rest of his day to supervising the work of his laboratory students, since until the Clarendon opened in October 1870 he had no demonstrators to assist him with class teaching, it is important to ask when he actually found the *time* to construct and prepare his experimental equipment. The answer to this question lies in the various accounts given of Clifton's somewhat unorthodox habits of night-time working, commencing between dinner and midnight and continuing until between 5.30am or 8am - presumably these sources differ because they were based upon reports from different years of Clifton's career [Gunther.,1921,ix;Nature,107,19]. With this work as well as his day-time teaching it is thus quite easy to understand why Clifton was not able to carry out original researches of his own in the late 1860's prior to the opening of the Clarendon. Clifton's inability to carry out research was, in fact, the central subject of his interview with the Royal Commission on Scientific Instruction and the Advancement of Science in July 1870, a few months prior to the opening of the Clarendon in October 1870.

6): Research and the students of the pre-Clarendon Laboratory

To alleviate the prohibitive magnitude of his teaching load Clifton trained a number of his students to become laboratory demonstrators, most of these being college fellows who in several cases later became eminent physicists. For example, Richard Abbay who graduated in 1867 with a BA First Class from the School of Mathematics and Physics spent a year in Clifton's laboratory from 1867-68, was appointed as Lecturer and Demonstrator in Physics at King's College, London in July 1868. Abbay was appointed to work under Professor William Grylls Adams, who had just negotiated the creation of a physics laboratory in the College [Ch.5], the young Oxonian being chosen because (as Abbay put it) he was "almost the only candidate who had had any experience with scientific apparatus". Indeed in considering his application, the College reported approvingly of Abbay's skills: "he brings evidence of high ability, as well as of extensive theoretical and practical knowledge of the subjects which he will have to teach and has gone through a complete practical course in a physical laboratory. [Minutes of KCL Council,1868-9, K, 192 &255; Ch.5].

A contemporary of Abbay's in Clifton's laboratory, was Arnold W. Reinold who had a spectacularly successful career at Oxford winning scholarships and Firsts in mathematics throughout his undergraduate career. After obtaining a First in Mathematics and Physics in 1866, for which he was given a Fellowship at Merton, Reinold spent a year in Clifton's laboratory and subsequently won a First in experimental physics from the School of Natural Science. After this Reinold was elected Senior Student and first Lee's Reader in Physics at Christchurch in 1869, subsequently being appointed by Clifton as the first laboratory demonstrator at the Clarendon, a post he occupied for one year after it was initiated in 1870 .[16] Reinold was succeeded in the demonstratorship by Arthur Rucker, who was an equally distinguished student of mathematics and subsequently Fellow and lecturer in Physics at Brasenose College after following Reinold through Clifton's course in 1869-1870 [Shaw,1921,276; Thorpe,1915,290].

Prior to the appointment of Reinold as demonstrator in the Clarendon, Clifton had evidently become somewhat overwrought in his teaching duties, for when the Devonshire Commissioners questioned him closely on the amount of time he found for research, an issue which they had been specifically appointed to investigate, Clifton responded:

> Absolutely none. I may mention that during the term in which I have had the laboratory class, in the rough way that it has been possble.....I have rarely managed to do the work which I considered necessary in less than eight hours laboratory work a day, and I was too exhausted at the end of the day to devote myself to research. And even during the terms in which I have merely given lectures, these required so much preparation that I found very little time for anything more than answering the questions of students who called upon me.
>
> [Clifton,1870,q3048]

Clifton naturally took the opportunity to agree with the Commissioners that professoral physicists *should* have sufficient time to do some original research [Clifton,1868,q3049], and argued that a staff of assistant professors to share his teaching load would enable him to achieve this goal. He articulated a scheme in which both he and his

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[16] In 1873 Reinold was appointed to the new chair of Physics at the Royal Naval College, Greenwich whereupon he formed a physical laboratory out of part of an old hospital. Arthur Rucker, his later contemporary at Oxford, was similarly appointed as the first Professor of Physics at the Yorkshire College of Science (Leeds) in 1874, and he too created a physical laboratory, this time from a disused Bankruptcy Court that had been burnt out in its subsequent manifestation as a cookery school. Rucker collaborated with Reinold at Greenwich on research into the properties of liquid films [Shaw,1921,276;Thorpe,1915,290].

projected staff would form indvidual specialisms within physics, just as Clifton was already inclined to do in the Stokesian field of optics, and they would pursue their specialisms in both their teaching and their research since physics, as Clifton argued, was a subject expanding so rapidly that it was now "too large a subject for one man to attempt to know" [Clifton,1870,q3050]. Yet although he felt it to be important for young physicists to have the necessary resources with which to carry out research[17], he was nonetheless unequivocal that this activity should not compromise the practical teaching that was the primary function of the laboratory:

> I think also that *any man who may not be engaged in teaching*, but who may wish to undertake a research, should be encouraged to do so; he should have the means placed at his disposal for carrying out any research which seemed likely...to be worth doing. Every help should be given to a young man in the way of supplying expensive apparatus, or rather that of allowing him to use the University cabinet, *provided always that he does not interfere with the teaching*. Th[e] new physical laboratory is cut up into small rooms, so that - during the vacations, for instance - a large number of men might severally have each a room, and *carry on researches there when it was not wanted for teaching purposes*.
>
> [Clifton,1870,3054 - emphasis added]

Thus while Clifton agreed with the commissioners on the importance of original research at Oxford, he was "afraid" that not much in the University was done in that direction, mitigating his position by arguing that his problems were applicable to *all* the scientific professoriate and

[17] In particular he argued that the fellowship fund at the centre of the contemporary "Endowment of Research" controversy should be used to "promote knowledge" if only the "vested interests" of the colleges could be overcome [Clifton,1868,q3032] - see [Engel,1983, chapter 3] for the background to this controversy.

not just himself:

> I suppose that the professors are officers appointed to increase our knowledge, as well as to hand down the knowledge that we have, but their time is so completely taken up with teaching now that it is almost impossible for them to do anything more than teach, and in fact not do that as thoroughly as they could wish. I think that one main duty of a university is to promote scientific research, and another is to supply the country with the very best teachers, but to perform both these duties well a considerable staff of scientific teachers is required, and the number of teachers now employed is so small that one must be neglected; to attempt both would be to fail in both. The demand for teachers is now considerable, and is increasing, so that to maintain the supply we have to spend nearly our whole time in teaching.
>
> [Clifton,1868b,3023]

Other Oxford Professors interviewed by the Commissioners made very similar comments which make plain that it was not just Clifton's corner of the Museum that lacked a certain "spirit of research". Henry Smith, the Savilian Professor of Geometry, for example commented that "as yet there is not enough of such a spirit in the atmosphere of the place", and in so saying confirmed that Clifton and his colleague in the Chair of Physiology, George Rolleston had too many Professorial duties to find time for research [Smith HJS,1870,3483-3484]. It is important to note this universal feature of Oxford science as a counter to the critical claims of physicists and others [Poulton,1911,71;Birkenhead,1961,90] that the Clarendon lacked an atmosphere conducive to research under Clifton's directorship.

Despite Clifton's complaints about the institutional constraints placed upon his research activities, his general mood when interviewed by the Devonshire Commission however was one of optimism about the prospects of experimental physics at Oxford once he was able to use the well-equipped new Clarendon laboratory, and call upon the assistance of his demonstrator Reinold. In the next sections we shall see how far Clifton's expectations of the Clarendon's success were borne out in succeeding decades.

7): Experimental Physics in the Clarendon Laboratory 1870-1877

The Clarendon was opened in the last week of October 1870, an event which inspired considerable hyperbole in the scientific press. On December 29th, for example, an article in Nature described the Clarendon "as the most perfect physical laboratory in the world" [Nature,3,171]. Indeed it is important to note that the Clarendon was considered to be such a perfect model of its type that the battery of other laboratories erected soon afterwards were implicitly or explicitly modelled on their Oxford sibling: as Clifton's former pupil Arthur Rucker described the Clarendon to Section A of the B.A.A.S. in 1894: "it has served as a type. Clerk Maxwell visited it while planning the Cavendish Laboratory, and traces of it can be found in several of our university colleges", Indeed in draughting his design of the Cavendish, Maxwell worked from plans of the Clarendon given to him by Clifton in the early 1870's[18] [B.A.A.S Report, 1894, (Part2) ,1].

In relation to Cambridge, it is interesting to note further that Clifton's Professorial career as a lecturer and teacher was conspicuously more successful than that of Clerk Maxwell in the early-mid 1870's, Clifton attracting regular audiences of between 40-50 students whilst Maxwell's often numbered as low as two [Fleming,1934,64]. With regard to

[18] Maxwell's copy of the Clarendon's plans is actually the most detailed extant view of the laboratory, and it can be found in the Maxwell archive at the UL, Cambridge.

the mutual relation of research at Oxford and Cambridge it is highly significant to note that, when the Cavendish was opened in 1874 in the quite unprecedented form of an offical laboratory of *research* , Clifton was approached by Stokes to advise upon which of his workers in the Clarendon would be suitable as Cavendish research fellows. Clearly Stokes would not thus have made such enquiries unless the Clarendon had acquired a reputation for training first-rate physicists under Clifton's guidance and indeed Clifton was able to suggest six possible candidates for research posts at the Cavendish [Clifton to Stokes,11/03/1876, UL Cambridge AD 7656 C.716]. From this it could readily be inferred that in the early years of the Cavendish, its Oxford counterpart lay in a position of greater relative prestige and popularity.

Ironically, however, the magnitude of teaching at the Clarendon had risen with the ever-increasing demand for the training of science teachers, such that even with a laboratory staff of greater than one man Clifton found that it was still very difficult to carry out much research. As he wrote to Stokes of his current and "most excellent" demonstrator, W.N. Hocker:

> His work with me has been so heavy that he has had no opportunity of doing original work, but I think it is not for the power or will. He has taken entire charge of the teaching [in] the department of weighing, measuring and heat, and has helped me greatly in optics. I never have occasion to interfere in his department as I always find everything going well when I do look in....I feel sure that Hocker has originality from the care with which he gets over difficulties in the laboratory work.
>
> [Clifton to Stokes,11/03/1876, UL Cambridge AD 7656 C716]

Whilst the Clarendon thus gained recognition outside its walls, it continued to receive the general internal support of the University as a centre of teaching in the precise techniques of physical measurement, for when Clifton appealed to the Vice-Chancellor to grant £1000 for "elaborate measuring instruments" in February 1877 he met very little opposition. Included in his list of required measurement apparatus were balances, diffraction benches, elctrometers, magnetometers, a theodolite, a torsion balance etc., and Clifton emphasized that these were now needed in multiple quantities since "the increasing number of students and the requirements of practical examinations render it necessary to have more than one instrument for the same purpose". The Convocation gave Clifton a clear indication of confidence in his work by voting 48 to 7 in favour of this move [Clippings fron University Gazette, Bodleian 05/02/1877, 22].

With the equipment that Clifton thereby acquired he was able at last to carry out a scheme of research into the electrical behaviour of metals in batteries with the assistance of his two "very efficient" laboratory demonstrators. His paper was intended as the start of a research programme involving precise measurements of the "difference of potential produced by contact with different substances" and in May 1877 he sent it for publication in the Proceedings of the Royal Society as an incomplete preliminary investigation. Although he stated his hope that "at some future time to be able to communicate a more complete investigation with reliable quantitative determinations" [Clifton,1878,299], this wish was never fulfilled. As he told the next investigative Government Commission in October of the same year, although he had been able to use the results of his research in his lectures, he was sure that the research "had seriously interfered" with his attentions to the laboratory students and hence would "hardly venture to undertake such work again" [Clifton,1877, q440].

In that year his teaching had been at its busiest, with the laboratory accepting its maximum of 16 students and even with his two demonstrators to assist him he was now spending a minimum of five and a half hours per day with his general class and with his most advanced students alone he sometimes spent this much time, often extending this personal tuition to a period of several days. The laboratory was this full because the national demand for teachers of physics was, as Clifton put it, "greatly in excess of supply" and more students than ever were drawn to seek a laboratory training to qualify as science teachers - particularly students who had been sent to Oxford by physics masters who were former pupils of his [Clifton,1877,q443]. Apart from straining his teaching resources to the maximum whilst he had been attempting to carry out research, Clifton found that the level of this demand had another deleterious consequence upon the prospects for research at the Clarendon:

> As soon as the best students have passed the university examinations, and often before they have taken the University examinations....they are bought off from continuing their studies by the larger schools....if they can say that they have had a years training in the laboratory, very frequently the certificate of having passed the University examinations in physics is not required. At the present moment I have lost two of my best pupils, neither of them having been through the schools, both of them being taken away to fill appointments as school teachers. It follows that the men who are most fitted to help in research...are taken away before they can be of any use...
>
> [Clifton,1877,q439-440]

In these circumstances, Clifton's reputation as a trainer of the best physics teachers actually worked to his disadvantage in another sense too, for his laboratory demonstrators were also attracted to the high salaries offered for teaching in the wealthy public schools:

> It has frequently happened lately that I have been consulted about appointments for teachers which have been vacant, and I have been quite unable to find any pupil of mine who was not already engaged. During the present period I am unable to find a pupil disengaged who is qualified to take the [vacant] office of 2nd demonstrator in our laboratory and the funds at my disposal are not sufficient to enable me to induce a man to leave schoolwork.
>
> [Clifton,1877,440]

Such was the ironic result of Clifton's success as a trainer of physics teachers.

Without a second demonstrator and with the ever-increasing content of contemporary physics brought about by the continuing growth of the subject, Clifton's strategy to maintain the high standards of his teaching was to restrict the scope of his laboratory teaching. Whilst Clifton taught optics he assigned the duties of instruction in weighing and measuring, and heat to his sole remaining demonstrator, effectively abandoning the subjects of electro-magnetism and acoustics [Clifton,1877, q447]. Whilst in his lectures he still maintained a fairly broad coverage of the subject, he considered this narrowing of his laboratory curriculum vital in order for it to be possible to pursue the "minuter details" of each subject, for as he agreed with Lord Selborne, the chairman of the University Commission, the progress of physics chiefly depended upon the minutiae of precision physics:

> "at the present time especially the progress of physics seems to me to depend on the progress of methods of exact measurement"
>
> [Clifton,1877,q451]

Hence even if he were not able to carry out research himself, through his teaching of laboratory techniques of exact measurement, Clifton thus considered that he trained his students in the requisite research skills for them to be able to contribute to the advance of physics.

8): <u>The outcome of the 1877 Commission and the decline of the Clarendon</u>

After hearing such evidence from Clifton and the rest of the Oxford Professoriate, the Commissioners framed recommendations in May 1878 that were sympathetic to Clifton's position, acknowledging research as a legitimate function of the University yet at the same time subordinating it to the Professorial duties of teaching [Engel,1983,189]. To assist Clifton in carrying out teaching and research in this order of priority they followed the course taken by the Hebdomadal Council in 1876 by recommending the foundation of the Wykeham Chair of Physics by New College with two associated demonstratorships to share the burden of running the Clarendon [<u>Nature</u>,<u>18</u>,24; Clifton to V-C, Bodleian b139(89)]. Since there was considerable optimism in the 1870's that the income of the university would increase throughout the following decades, both from agricultural improvements and college tenancies, it was assumed by all concerned that the financial requirements of the science chairs would easily be met [Engel,1983,201].

However, as A.J.Engel has documented, the agricultural depression that descended upon Britain in the years immediately after the Oxford Commission to all intents and purposes nullified the projected increase in the financial resources of the University which had been earmarked for these reforms. The Wykeham Chair of Experimental Physics was not founded until 1900, when John Townsend was appointed with the specific duty of providing lectures and laboratory instruction in electricity and magnetism. This must have been a particularly bitter blow to Clifton since this Chair was not only founded 20 years later than had been originally intended, but the University was granting a young outsider the facilities to teach a subject which they had earlier denied Clifton. As he had explained to the 1877 Commissioners, he had been obliged to drop electricity and magnetism from his laboratory curriculum to teach other subjects properly. Yet when he had applied to the University in 1887 for £4,800 to build a new electrical laboratory to meet the demands of students to receive up-to-date training in the burgeoning subjects of electromagnetism and electrical engineering, Convocation voted against it, agreeing only to pay for a new porter's lodge for the Clarendon [Clifton to V-C, Bodleian b139(89); University Gazette, 7/06/1887].

Janet Howarth has suggested that this was almost certainly the result of what she calls "the politics of numbers": a week before the vote in Convocation an article in the Oxford Magazine, asked "What do the Professor and Two Demonstrators do?" pointing out that the Clarendon had only produced three graduates in the previous year [Oxford Magazine,June 1877,249]. From Clifton's comments to the 1877 Commission about the frequency with which his students were "bought off" by public schools before even taking the examinations in the Natural Science School this figure of three graduates was an unfair reflection of his student intake: Howarth cites the figure of 20-25 students attending Clifton's class prior to graduation in 1886 which, amongst the 18 courses in the Oxford Museum, ranked 5th most popular between the extremes of 78 students for the Professor of Geology's lectures and 5-6 attending the lectures of Bartholomew Price, the Sedleian Professor of Natural Philosophy [Howarth, 1987,370]

This misrepresentation of his efficiency and popularity as a teacher must have been all the more galling for Clifton when in 1892 Convocation granted £7250 for an anatomy laboratory after the newly-appointed lecturer Arthur Thomson complained of the overcrowding of students in his makeshift anatomical shed [Howarth,1987,343-344]. It is not surprising therefore that when Townsend arrived to take up the Wykeham Chair in 1900, Clifton did not welcome him with open arms, nor offer the newcomer a room in the Clarendon in which to work; thus Townsend worked in laboratory space borrowed from his colleagues the Professors of Astronomy and Physiology until a new electrical laboratory was built for him and opened in 1910 [Biographical Memoirs of Fellows of the Royal Society, 3, 259-60].

In the meantime the teaching at the Clarendon continued almost exactly as it had done since 1870, because Clifton's facilities were not significantly augmented by grants from the University chest after the early 1880's. Thus we see why Lindemann found the Clarendon so different to the Cavendish upon his succession to Clifton's Chair in 1919: in contrast to a burgeoning research laboratory with an extensive personnel hierarchy, a dozen undergraduates were being taught by a single University demonstrator, with a minimal water supply, gas lighting, no mains electricity and an annual University grant of £2000 that was the merest fraction of the income available to the Cavendish and Clifton's colleague J.A. Townsend [Birkenhead,1961,89-90].

Conclusion

In blaming Clifton for the "moribund" status of Oxford physics in comparison to major British laboratories such as the Cavendish, Lindemann was oblivious to the exertions of Clifton during the preceding fifty years to build up the Clarendon as a national centre to providing the most comprehensive training to aspiring physics teachers. Using the skills in teaching and institutional politics he had acquired in the inhospitable accommodation of Owens College in the early 1860's, Clifton established the Clarendon as a prototype of the purpose built teaching laboratory of precision measurement in Europe. He also fastidiously safeguarded its functional specialization to the point where it could sustain the extremely taxing standards of accuracy in measurement that Clifton demanded of his own experiments and those of his students.

He was perhaps just unfortunate however in creating a course of teaching whose intensive and comprehensive character was most appreciated by the headmasters of public schools. These figures succeeded in depriving him of both the research workers that he had clearly envisaged since the earliest plans of the Clarendon, and also of the output of laboratory graduates needed to impress Convocation into furnishing the Clarendon with extended accommodation and staff, in the impecunious years of the agricultural depression. Thus Clifton, cherished by his students as a great trainer of physics teachers was not able to bequeath to posterity much direct indication of the enormous activity that made the Clarendon flourish and even outrank the early Cavendish laboratory in the heyday of Oxford physics that occurred in the 1870's.

Future studies of Clifton should thus focus attention upon the less visible inheritance of experimental physics that he created: the large network of his former pupils inculcating the methods of precision physics throughout British public schools; the research traditions and teaching carried out by former students of his who followed his example in creating physical laboratories of their own upon their appointment to chairs of physics: Arnold Reinold, Arthur Rucker and Sir Lazarus Fletcher; and finally the ways in which the Clarendon served as the architectonic model for physical laboratories throughout the 1870's to 1890's.

CHAPTER 7

Balfour Stewart, exact meteorology and the physical laboratory at Owens College, Manchester

An obervational science like meteorology or terrestrial magnetism is placed in some respects at a disadvantage when compared with the more experimental branches of physical enquiry. It is often difficult to obtain a good and readable account of what has been done. The reason of this is, that those who are personally engrossed with the science have to deal with such large masses of figures and precise measurements that they are frequently unable to spare the time necessary to give a good historical account of their favourite research. Those again who are the historians of science find it a very formidable task to bring themselves *en rapport* with all that has been done in such a science as terrestrial magnetism...

Balfour Stewart: 1875 review of Humphrey Lloyd's A Treatise on Magnetism, General and Terrestrial, in [Nature,11,221]

Introduction

Of all the educational establishments discussed in the period of this thesis, Owens College, Manchester was not only one of the most recently founded but also the one which underwent the greatest institutional growth both in overall teaching and in its facilities for natural philosophy. In this chapter we will thus locate the creation of the Owens physics laboratory in the context of civic and industrial support for the "extension" of the Manchester college during its general state of development in the 1860's. We will focus particularly upon the changes in the institutional status of physics effected by three consecutive occupants of the natural philosophy chair: Robert Bellamy Clifton, William Jack and Balfour Stewart from 1860 to 1887.

Discussion will first centre upon the actual creation of the chair of natural philosophy in 1860. The controversy surrounding the creation of the chair will be analysed in terms of the disciplinary interests opposed to the institutional autonomy of natural philosophy from mathematics .

Second, reference will be made to the way in which Clifton and Jack exploited the issues of the College's confined accommodation in Quay Street and also its location in the industrial setting of Manchester as rhetorical devices to procure funds for a physical laboratory in the college "extension" scheme of the 1860's.

Third, we will discuss the division of the chair upon Jack's departure in 1870 in terms of the College's recognition of the importance of research in the professorial prerogative. From the point of view of the College, the politics of the eventual appointment of Balfour Stewart to the senior Chair will be documented in terms of the financial expediencies of the Owens extension scheme.

Fourth, with regard to Stewart's career as a student of Forbes at Edinburgh and then Superintendent of the Kew Laboratory, we will see how he cultivated an expertise in precision observatory measurement which when applied by him to the nascent "exact science" of meteorology led to two terminal disputes with the Kew establishment in 1869-1870.

Finally, after Stewart's acrimonious departure from Kew to Manchester in 1870 we will see how he applied the three-fold "philosophy of measurement" developed at Kew to his laboratory training and research at Owens. Important themes in his work at the Owens laboratory will be the relation of his research-oriented student laboratory training to that of his Scottish colleagues Tait and Thomson, his divergence from the orthodox view of "closure" in the 1870's and also the ubiquitous recurrence of his interests as a physical meteorologist.

1): <u>The Creation of the Natural Philosophy Chair at Owens College.</u>

The Chair of Natural Philosophy was created at Owens College in 1860 against vehement opposition from the Professor of Mathematics, Archibald Sandeman, to the loss of his academic territory. As an analysis of the strategy used by the Owens professoriate to disarm Sandeman's objections, this section will constitute a case study of how the curricular independence of natural philosophy was negotiated against vested institutional interests during this period of its national disciplinary ascendancy.

In October 1859 the trustees of Owens College, Manchester received news that the University of London was shortly to establish a BSc degree in science. The old University of London B.A. had been an established goal for a large proportion of Owens students since the College's foundation; hence the College's syllabus had been specifically tailored to the requirements of that degree. The new regulations for both University matriculation and for successful completion of both the BA and the new BSc, however, required a considerably greater quantity of natural philosophy than the college had hitherto been able to provide. The trustees thus raised the question of whether it was necessary to create a separate Professorship of Natural Philosophy to provide the requisite increase in physics tuition, or whether the duties of the Mathematics and Chemistry professors could be extended to cover the extra teaching. Principal Greenwood was thus requested to consult the professoriate on the matter and advise the trustees of the outcome at their subsequent meeting on November 24th [Owens College, Minutes of Trustee's Proceedings[1], 27/10/1859, 287].

The Professor of Chemistry, H.E.Roscoe, told Greenwood that a special course of instruction provided by a professor of natural philosophy would be "desirable.....as subsidiary to the study of the higher branches of chemistry". Evidently, however, the Professor of Mathematics, Archibald Sandeman, had different views of the matter for when a motion calling for the appointment of a new Chair of Natural Philosophy was put to the professoriate on the 23rd January 1860, Sandeman's was the sole dissenting vote - the grounds for his dissent are discussed below. A college meeting was thus called on the 20th February both to formalize the scope and function of the new professorship and also to "remove the objections entertained by the Professor of Mathematics" [OMTP, 26/1/1860, 296-7]

At this meeting Sandeman's colleagues were adamant in their

[1] Henceforth "OMTP".

resolution that Owens had long felt "inconvenience" at the lack of a college course in experimental physics, particularly in the Department of Chemistry, and that without such a course the college was unable to provide tuition to meet the new University regulations. Sandemans' view of the institutional precedence of mathematics over physics were acknowledged in their resolution that "it is an indispensable condition to the usefulness of such a professorship that none but a sound mathematician be appointed to fill it". Nonetheless Sandeman argued that the "value of natural philosophy" taught to students by experimental demonstrations as an "instrument of intellectual training" was entirely dependent upon their prior mathematical training: "all experiment must be preceded by considerable mathematical attainment" [OMTP, 20/2/1860, 301].

To legitimate their contrary view that natural philosophy could be taught independently from pure mathematics by a competent mathematician, Greenwood summoned independent testimony from the "eminent" University mathematicians, Professors De Morgan and Stokes, "two gentlemen than whom it would be difficult to name any more competent to advise on the present question". In a letter to Greenwood of February 11th 1860 De Morgan argued, albeit somewhat fragmentarily, that:

> A course of experimental physics *may* be as rigorous in its way as a course of mathematics...I hold that a demonstrative course of experimental physics is in itself desirable. It shows what *demonstration* is out[side] of mathematics...Method is to be taught and quantity. When a student has been on fundamental points under a sound instructor, he *can read experiment* with profit.
>
> [De Morgan to Greenwood, 11/2/1860, in OMTP, 20/2/1860]

Writing to Roscoe on the 17th February in a similar vein, Stokes tellingly revealed a conviction of his own University's weakness in this respect and hinted that his own experimentally illustrated lectures on mathematical physics were themselves insufficient to do justice to the study of physics as an autonomous discipline:

> I entertain a very strong opinion as to the great value of a course of lectures, mainly experimental, on natural philosophy. I think it is a great defect in our system here [Cambridge] that our students have so little opportunity for attending or encouragement to attend lectures of this kind. The study of Natural Philosophy for its own sake and not merely as a field for the exercise of mathematics is, I think too much neglected among us....I think that in any establishment for higher teaching there is ample room for a chair of Natural Philosophy distinct from one of Mathematics.
>
> [Stokes to Roscoe, 17/2/1860, in OMTP,20/2/1860,302-303][2]

Sandeman's reply to such arguments from his peer group was a traditional defence of the pedagogical priority of mathematics over what he considered to be a secondary discipline:

> the value of [Natural Philosophy] is entirely dependent upon the order in which it is taken; that all experiment must be preceded by considerable mathematical attainment; and none of our students (or very few) either in the present or in former sessions, have possessed sufficient mathematics to make it expedient to invite them to enter on the independent study of the sister science.
>
> [OMTP, 20/2/1860, 301]

The vexed Greenwood denied, however, that an independent course of physics would necessarily be unscientific without a comprehensive prior training in mathematics; he and his colleagues collectively "believed that a Strictly Experimental and Descriptive course [of physics] so long as it is this and nothing more if conducted by a man of science and an able mathematician....would be free from all danger of misleading students as to what was and what was not science". Greenwood reinforced his argument

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[2] Incompletely cited in [Kargon,1977,174].

by citing precedents *for an effective* division of labour between chairs of mathematics and natural philosophy at University and King's Colleges London, Queen's College Birmingham, the University of Glasgow and the University of Edinburgh [OMTP 1860, 20/2/1860 301].

Since Sandeman maintained his position of obdurate dissent, Greenwood commissioned a fuller articulation of his objections to present to the College trustees on the 29th March 1860; Sandeman thus wrote a lengthy essay entitled "Touching the bearings of Natural Philosophy to education generally and more especially to the course of education at Owens College". In this paper he declared that "a mind.....enlightened by mathematical training can alone breathe life into the dead facts and experiments of Natural Philosophy...Hence teaching Natural Philosophy without a firm mathematical framework is likely to stuff pupil's heads with false notions." Rather than attempt to refute Sandeman's arguments, Greenwood's only response to this was to reiterate the necessity of providing a course of physics suitable as preparation for the University of London examinations as a matter of financial necessity in the college's survival. He argued that it was impossible for Sandeman to cover the wide range of subjects that the new regulations now rendered examinable under the heading of Mechanical and Natural Philosophy viz. statics, dynamics, hydrostatics, pneumatics, optics, acoustics, astronomy as well as teaching separately an entire course of pure mathematics. Furthermore Greenwood argued that it would be impossible for Roscoe and Sandeman to *share* the task of teaching the requisite amount of physics since the academic duties of both men were already extremely arduous[OMTP,29/3/1860,310-315].

Having thus presented his report of the College's internal conflict, Greenwood put it to the trustees that 1) the proposed Professorship was vital to the College's future success; 2) that the trustees should appoint a "sound mathematician" to the chair who could conduct 3) both "a short course on Experimental and Descriptive physics and a full course on Natural Philosophy treated mathematically"[3]. Faced, however, with the opposition of Professor Sandeman as an authority of central importance in this issue, the trustees were loathe to decide upon the controversial appointment at that February meeting and repeatedly postponed their final decision upon the matter until the 14th of June. At this meeting the trustees finally accepted Greenwood's lengthy report on the subject, a report placing heavy emphasis upon the need for such a professorship to meet the educational demands of a large student clientele entering for the University of London examinations, and agreed to advertise for a candidate for the Professorship at a salary of £200 plus fees.

On July 20th the committee of appointment reported that from 20 applications for the post, they had reduced the number eligible to two Cambridge Fellows: E.B. Stone of Queens College and Robert Bellamy Clifton of St Johns College. In the process they eliminated the applicant who had been both Senior Wrangler and First Smith's Prizeman in Clifton's year: J.M. Wilson [Gunther,1921]. After interviews and conferrals Clifton was appointed on the 31st of July to take up the post on the 29th of September[4].

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[3] For this latter course a fixed minimum of mathematical knowledge was to be prescribed to obviate Sandeman's major complaints.

[4] For an account of how Clifton followed Stokes' desideratum in establishing the study of Natural Philosophy "for its own sake and not merely as a field for the exercise of mathematics" transcending also Roscoe's requirement that it be merely "a subsidiary to the study of the higher branches of chemistry" see the account of Clifton's professorial career at Owens from 1860-1866 in chapter 6.

2): William Jack and the "extension" of Owens College 1866-1870.

Clifton resigned the Owens Chair in November 1865 in order to take up the prestigious Professorship of Experimental Philosophy at Oxford the following year and as a matter of etiquette the Owens Committee consulted him upon the appointment of his successor. By March 1866 the short list of candidates had been narrowed to two former students of William Thomson: J.D. Everett and William Jack. The latter had followed Thomson through St Peter's College Cambridge, graduating fourth Wrangler and First Smith's Prizeman in 1860 and thence proceeding to a College Fellowship. Although Jack spent the intervening years as a Royal Inspector of Schools in Scotland, his appointment to the Owens Chair of Natural Philosophy in 1866 was almost certainly closely connected with the common pedigree that he shared with Clifton as a Wrangler, First Smith's Prizeman and Cambridge Fellow [Gibson,1924,540].

In his contextual study of Science in Victorian Manchester Robert Kargon claims that "armed with hindsight, it is neither ungenerous nor unfair to view Jack's appointment at Owens as a stop-gap" [Kargon, 1977, 182]. Kargon is manifestly not armed, however, with a surfeit of historiographical sensitivity in making this dismissive judgement, for in making this judgement he evidently makes the fallacious inference that Jack's career at Owens was inconsequential merely from the *paucity* of extant primary sources indicative of Jack's activities at Manchester. In fact, a source heavily used by Kargon, Alderman Joseph Thompson's "Minutes of the Trustee's Meetings for Educational Purposes", reveals something more substantial of Jack's role at Owens College; upon his departure in 1870 the Owens Trustees minuted that "Mr Jack's unusual administrative talents have greatly assisted the progress of the College during the last four years" [QMTMEP 30/5/1870,53]. This section will therefore analyze Jack's role in promoting the interests of natural philosophy in the "Extension" scheme which was at the heart of Owens' "progress" during Jack's tenure. We will consider in particular how he took up Clifton's mantle in "extending" the institutional position of natural philosophy to the extent that the initiation of a physical laboratory became an integral part of the College's "Extension" scheme.

At the start of his tenure in 1865, Clifton had negotiated a private laboratory and lecture theatre out of the very limited institutional accommodation afforded by Cobden's old house in Quay Street [Ch6]. By early 1865, however, the local demand for instruction - especially scientific instruction - at Owens had increased to the extent that its Professoriate formed a committee to appeal for new buildings. The following enrollment figures were cited by them in support of this claim to a college meeting on 23rd February (starting at the point of the college's general nadir in 1857-58):

Session	Day Classes	Evening Classes	Total
1857-58	34	59	93
1858-59	40	107	147
1859-60	57	77	134
1860-61	69	102	171
1861-62	88	235	323
1862-63	108	287	395
1863-64	110	312	422
1864-65	127	312	439

Most emphatic in their demands for larger classrooms, "museums" and facilities for practical teaching were the Professor of Natural History, W.C. Williamson, and of Natural Philosophy, R.B. Clifton, whose classrooms were "crowded to a degree beyond the well-understood requirements of health and comfort". Clifton, for example, complained that the ventilation in his class-room was so poor that "when...I have to use a galvanic battery in the lecture, the presence of a small quantity of the vapour arising from it has compelled students to leave the class room fainting" [OMTP, 23/2/1865]. Professors Clifton and Williamson thus presented the Committee with plans of a two-storey building accommodating the classrooms, museums and *laboratories* etc which they sought for their subject and the College began negotiations for sharing facilities with other civic bodies such as the Manchester Natural History and Geological Societies [OMTP, 23/2/1865; Thompson,1886,257-286].

A year later in March 1866 the Professoriate published a statement reiterating their concern to expand their teaching beyond the confines of Quay St, entitled Owens College: Constitution, Progress and Current State of the College [Greenwood et al,1866]. In this pamphlet they argued for a scheme of extended accommodation to turn Owens into the leading institution of scientific education in the country:

> ...It would probably be found that in no other institution of the kind in the kingdom are so many persons under instruction in so confined a space...[and] it is not too much to affirm that the utmost measure of success attainable with the present buildings has [already] been reached...It is plain that there should be found in the North of England some central School of Civil Engineering, Surveying, Architecture, and Mining, and that Manchester is its natural seat. Owens College contains the nucleus of a very efficient school of these branches of Art and Science, and such a school attached to the College, would it is believed be warmly welcomed not only by Manchester, but by populous and busy manufacturing towns around. If Owens College cannot hope to rival the ancient Universities in the study of Literature and Mathematics, there is no reason why it should not aspire to be the leading School of Applied and Experimental Science in the country. It is not premature to state that steps are being taken to enable Owens College to make some of the extensions proposed; but there is no room for them whatever in the present buildings.
>
> [Greenwood et al, 1866]

Although Clifton still officially occupied the Chair of Natural Philosophy, William Jack was a signatory to this pamphlet and when the programme of institutional expansion contained in it was recapitulated at a meeting of the trustees in December 1866, Jack acted collaboratively with Roscoe in framing the recommendations of the Professoriate. First, they recommended the foundation of new Chairs to represent neglected "branches of Scientific and Professional Study": 1) Civil and Mechanical Engineering as had been advocated by Clifton in 1863[5]; 2) Astronomy and Meteorology with observatory because of the special "interest taken in the study of astronomy in this district" and 3) Applied Geology and Mining "in view of the importance of the mining industry in the district of South Yorkshire and Lancashire". Roscoe and Jack also argued that to effect the desired goal of turning Owens into a national centre of *experimental* science it would be "requisite to set aside a considerable sum for the extension and maintenance of the Chemical, Physical and Natural History

[5] In April 1863 Clifton presented the trustees with a report explicitly arguing for the foundation of a chair of engineering to meet a local demand amongst manufacturers for instruction in practical mechanics to their apprentices. Demonstrating his political and financial acumen, he argued that "situated as Owens College is in a great centre of Engineering Science it would be desirable that some effort should be made to supply such practical instruction, thereby meeting a want much felt by local engineers, and at the same time drawing local sympathy towards the College...The want of such an engineering course is believed to be so deeply felt by the principal local engineers that they would probably be found quite ready to offer [financial] assistance to further any scheme which the trustees should propose to adopt..." [OMTMEP,4,113-118]. Whilst the trustees made no minuted comment upon this plan at the time, a meeting of the city's leading engineers such as Whitworth and Fairbairn on December 11th 1866 put the principles of Clifton's plan into operation by launching a campaign to raise funds for the mooted Professorship and its necessary pedagogical adjuncts [Thompson,1886,295]. An account of how the Chair of Civil and Mechanical Engineering evolved after Clifton's departure can be found in [Kargon,1977,182-190].

Departments" [OMIMEP 14/12/1866,8,13]

After Greenwood read the further details of Roscoe's and Jack's report to the trustees, the revived New Buildings Committee prevailed upon the trustees to organize a meeting with all the local industrialists and civic dignitairies to raise the £100,000 requisite for the "extension" of Owens College; Thompson gives an exhaustive account of the events which followed the subsequent august gathering in Manchester's Town Hall on the 1st of February 1867. This movement for the "extension" of Owens took great force from the controversial outcome of the 1867 Paris Exhibition: indeed a deputation of Owens trustees was sent to the conference on technical education organized by the Society of Arts in January 1868 to discuss means of improving the training in experimental science received by the manufacturing population of the country.

As discussed in chapter 1 this was the conference at which it was generally agreed that laboratory training, especially in physics, should form part of the training for a variety of science-related professions. It is significant therefore that in the conference debates eminent governmental figures such as Lyon Playfair and Bernard Samuelson repeatedly referred to Owens as the only institution outside of London which could offer a training in experimental science to the manufacturing population. Heartened by such recognition of Owens educational role, a committee was formed to lobby Parliament for funding to match that given to British Universities in the immediately preceding years, for example the £120,000 that had been granted to the University of Glasgow for its reconstruction and removal in 1866 [Thompson,1886,325 &331].

From March 1868 to May 1869 this committee despatched three substantial delegations of Mancunian dignitaries, Lancashire and Yorkshire

M.P.s and (academic) men of science to Whitehall and Downing Street in which Jack, Roscoe, Greenwood were ubiquitous figures[6] [Thompson,1886, 326-27,328-29,333-34]. Nevertheless, both Disraeli and then Gladstone as Prime Ministers in turn refused Governmental assistance, the latter making a relatively explicit refusal on the grounds that Owens did not have the status of a University nor was it situated in a capital city; this only led the Manchester lobby to redouble their claims that Owens was in fact virtually a scientific university in a city that was effectively the capital of Northern England [Thompson,1886,334-339].

Meanwhile in Manchester itself, subscriptions to the new buildings fund had raised £76,859 by October 29th 1868 [Thompson,1886,341] and, buoyed up by financial optimism and increasing student enrollment[7], the Professoriate submitted relatively ambitious plans for the accommodation they required to a College meeting on November 4th. For the Department of Natural Philosophy, Jack requested two lecture rooms, a *physical laboratory* and additional apparatus rooms, and doubtless the propriety of Jack's claims was supported by the contemporary developments at King's College London and the University of Edinburgh, where physical laboratories had been opened in the previous month [Chs.2 & 4] [OMTMEP[8]

[6] Joule was also elected to represent their claims on the first two of these occasions [Thompson,1886,327&329]

[7] Although there had been another lull from 1865-67 during the Manchester "cotton crisis" precipitated by the American Civil War, the salient figures show an accelerating growth:

Year	Day Classes	Evening Classes	Total
1866-67	113	280	393
1867-68	173	324	497
1868-69	210	473	683

[Thompson,1886,355]

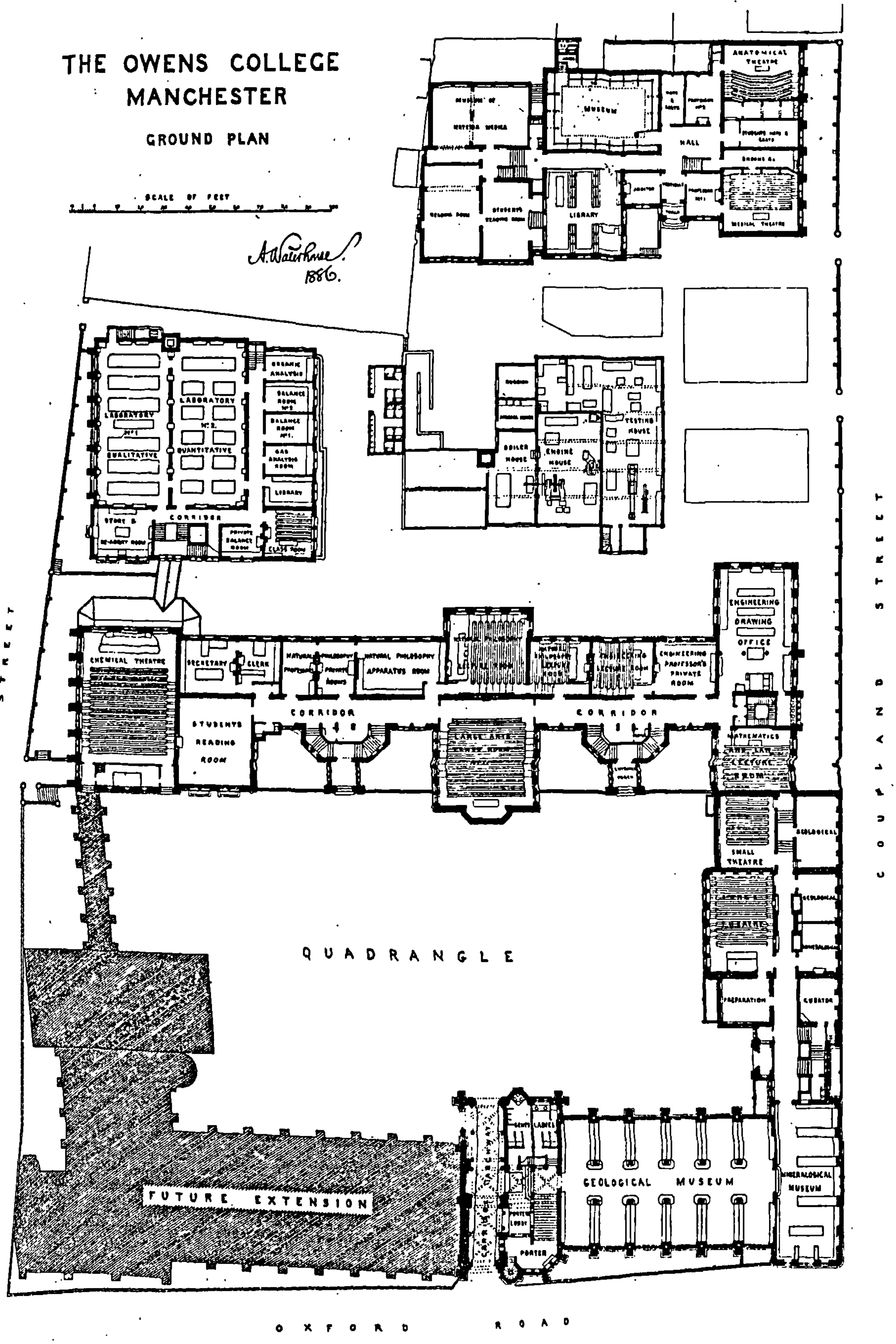
[8] Kargon's abbreviation for Owens Minutes of the Trustee's Meetings for Eductional Purposs [Kargon,1977].

4/11/1868]. However, in the three alternative architectural plans drawn up in early 1869 Jack noted with dismay that none of them provided for a physical laboratory except as a possible future extension of the new plans. Hence at a College meeting on the "extension" buildings revisions were proposed according to Jack's wishes, as embodied in the following minute:

> The plans are [now] arranged to allow of the commencement of a Physical Laboratory. The maintenance of a Physical Laboratory proper will cost £300 pa. In one of the largest and most famous German Universities (Heidelberg) the Physical Laboratory is attended by 14 students. These laboratories are of the utmost educational importance....
>
> [OMIMEP, 9/3/1869]

Plans incorporating a suite of small physical laboratories were thus incorporated in the final architectural plans agreed on December 3rd 1869 - illustrated in [Thompson,1886,351] - but the campaign for building funds continued as we can see from the proceedings of another conference organized by the Society of Arts in December 1869. This conference on "Science Colleges" was held in Manchester and although it had specific reference to the extension of Owens as a local college, it topically addressed also the wider development of the nation's system of elementary education to prepare the working population for the training that Owens proposed to supply. At this conference Jack presented the principal motion that:

> ...in the opinion of this meeting the best interests of the country demand the establishment of a complete system of primary instruction, the extension of a system of science classes under a responsible department of the government, on a definite plan, and especially the establishment of science colleges in the principal industrial centres of the United Kingdom; that such colleges ought to be established and maintained partly by local efforts and partly by liberal assistance from the State, and that existing institutions such as Owens College, ought to be made available for this purpose.
>
> [Journal of the Society of Arts, 24/12/1869,118]


THE OWENS COLLEGE
MANCHESTER
GROUND PLAN
SCALE OF FEET
A. Waterhouse
1886.
MUSEUM
HALL
ANATOMICAL THEATRE
MEDICAL THEATRE
LIBRARY
MUSEUM OF MATERIA MEDICA
READING ROOM
STUDENTS READING ROOM
BOILER HOUSE
ENGINE HOUSE
TESTING HOUSE
ORGANIC ANALYSIS
BALANCE ROOM No 2
BALANCE ROOM No 1
GAS ANALYSIS ROOM
LIBRARY
LABORATORY No 1 QUALITATIVE
LABORATORY No 2 QUANTITATIVE
CORRIDOR
STORE & RE-AGENT ROOM
PRIVATE BALANCE ROOM
CLASS ROOM
CHEMICAL THEATRE
SECRETARY
CLERK
NATURAL PHILOSOPHY APPARATUS ROOM
ENGINEERING PROFESSOR'S PRIVATE ROOM
ENGINEERING DRAWING OFFICE
CORRIDOR
CORRIDOR
STUDENTS READING ROOM
MATHEMATICS AND LAW LECTURE ROOM
SMALL THEATRE
PREPARATION
CURATOR
QUADRANGLE
FUTURE EXTENSION
GEOLOGICAL MUSEUM
MINERALOGICAL MUSEUM
GENTS
LADIES
LODGE
PORTER
BURLINGTON STREET
COUPLAND STREET
OXFORD ROAD

Public debates such as this were instrumental in providing the requisite publicity for raising the remaining funds needed for completion of the building: by the summer of 1870 the extension fund had reached just over £100,000 and plans were thus made to commence construction at the new premises in Oxford St in the early autumn [Thompson,1886,390-92]. Before the foundation stone of the new college was laid on September 23rd 1870, however, Jack resigned. The *manner in which* his chair was divided and his successors equipped with a physical laboratory in Quay St will be the subject of the next section.

3): The "extension" of natural philosophy at Owens in 1870.

In a letter written sent by J.P. Joule, a Mancunian gentleman of science intimately acquainted with the workings of Owens College, to Sir William Thomson in early April 1870 we learn something of the circumstances of Jack's departure. Joking at Jack's treachery in leaving science for business as editor of a prestigious Scottish newspaper, Joule wrote: "Jack is leaving Owens and is going to help on the Glasgow Herald. Is it not a sort of apostasy? I am sorry for he was popular among the students and likely to do well for the college" [Joule to Thomson 6/4/1870, J289 ULC ADD 7342]. In a second letter of April 11th we learn that although Jack had not yet made his resignation public, Joule was already busy in "promoting the claims" of Thomson's nephew, James Thomson Bottomley, for the Chair of Natural Philosophy - Joule declaring that Bottomley "would be quite the man for Owens" [Joule to Thomson 11/4/1870, ULC ADD 7342 J290].

Although Joule spoke here in terms of a single appointment, the status of the Chair came under review after Jack's formal resignation on the 25th of April: on the 6th of May the Owens Trustees appointed a committee to select Jack's successor in collaboration with the existing "extension" committee [OMTP 6/5/1870,5,22]. The primary manifestation of this collaboration was a discussion of whether in "extending" the college as a whole it was appropriate 1) to open an experimental physics teaching laboratory in the interim period before the opening of the new College in Oxford Road and 2) to "extend" the teaching of natural philosophy to two separate professorial chairs. After consultation with Jack, Roscoe and Greenwood about the "present position of the Department, and of the steps which are desirable to increase its efficiency", the Natural Philosophy Professorship Committee reported to the trustees on the 18th of May that:

> 1) The trustees should take immediate action to advertise for candidates for the Chair, and that they should in their advertisement express their hope that full provision will shortly be made for the establishment of a Physical Laboratory;
>
> 2) The Committee have given their attention to the question of the future enlargement and reorganization of the Natural Philosophy Department and have come to the conclusion that is is essential to provide a Physical Laboratory and adequate apparatus and assistance, and that it is desirable, if possible, to appoint a second Professor of Natural Philosophy.
>
> [OMTMEP 18/5/1870,4,60-61]

At a College Committee meeting held on the following day it is clear that these proposals to draw upon the extension fund to finance a laboratory and second professorship for the Department of Natural Philosophy had been accepted by the Extension Committee. Nevertheless the plurality of the Chairs was evidently still an open issue, for the Natural Philosophy Committee were instructed to advertise a single Professorship only [OMTP, 19/5/1870].

However, even before the advertisement was issued, Roscoe had already approached the Director of the Kew Observatory, Balfour Stewart, to submit an application for this Chair as we surmise from Stewart's reply to Roscoe dated 20th May: "Many thanks for your letter. I will get together my application testimonials...I suppose the advertisement will mention when the election is to take place" [Stewart to Roscoe 20/5/1870, RSC Archive]. The fact that Roscoe actively solicited an application for the chair from Stewart, even before the post had been publicly advertised is highly significant.[9] As we will see below, Stewart's candidacy was very probably sought because professorial deployment of his broad expertise not only in physics, but also in meteorology and astronomy would enable the College to effect economies as regards the teaching of the latter two subjects, a separate chair for which had been mooted in the extension plan discussed above. It will be argued that this economy helped alleviate the strain imposed on the college finances by the creation of two salaried chairs in physics out of the extant chair of natural philosophy.

From the evidence of William Thomson to the Devonshire Commission in 1870, we know that both the trustees and Thomson's friend Joule had consulted him upon this division of the Owens chair in about May of that year. Thomson argued in favour of the division on the grounds that two professors of natural philosophy would share the heavy workload of teaching *and* also find time to carry out original research:

2674 *Chairman of the Commission (to Thomson):*

> I believe your advice has been that those professors should not be permitted to lecture to such an extent as to make it impossible for them to devote a good deal of their time to experimental research? -

[9] This is a point overlooked by Kargon is his account of this episode [Kargon,1977,215-16].

Thomson:

> Yes, I have urged that very strongly upon the trustees, and they are thoroughly convinced of the rightness of that advice, and I myself have been fortified in a letter which I have received from Dr. Joule. Dr Joule maintains that a professor cannot enter with spirit into investigations, and cannot take the position proper for a professor in such an institution as Owens College, unless he is largely occupied in original research, and from information which Dr. Joule gave me I perceive that it was impossible for the Professor of Natural Philosophy, with the duties hitherto laid upon him, to give much time or energy to original research[10].
>
> [Thomson,1870,q2674]

By arguing thus Thomson evidently hoped to bring to the Owens laboratory the form of institutionally recognized research which he had cultivated at Glasgow for the past two decades [Ch.2]. However, as Thomson pointed out with regard to the financial resources available for this disciplinary "extension": "there has been very great difficulty, in consequence of the insufficiency of the funds, to establish two professorships on such a scale and with such incomes to the professors as the trustees would desire" [Thomson,1870, q2675].

Notwithstanding this problem we know that, by 29th May 1870, the College and trustees had finally agreed to separate the natural philosophy in two, and had made a definite proposal as regards the division of professorial labour between the prospective chair holders. This much we can ascertain from a letter Roscoe had written to Balfour Stewart on this date warning him - very significantly - that in his interview for the professorship the trustees would ask him "how the Chair could be worked", specifically with regard to his views upon the "methodological" division of labour between the two chairs tentatively proposed by the trustees as

[10] The only recorded research undertaken by Jack during his tenure was an investigation of the galvanometer read at a meeting of the Manchester Literary and Philosophical Society during 1867 [Jack,1867].

"Applied Mathematics" *vis-a-vis* "Experimental Physics". In his reply of 6th June 1870 Stewart rejected this division in favour of one which could accommodate in one chair the inter-related fields of expertise which he had cultivated at Kew during the preceding decade [see next section].[11] Stewart's suggestions were as follows:

> I begin by supposing there are to be two chairs. I do not think the division of the subject into applied mathematics and Experimental Physics would be a good one, for Natural Philosophy without experiment is merely a mathematical exercise whilst experiment without mathematics will neither sufficiently discipline the mind nor sufficiently extend our knowledge in a subject like physics.

Instead he argued that an appropriate division would be:

> Course A - mechanics (including statics and dynamics) and the forces and properties of matter.
>
> Course B - the energies of nature and their laws of transmutation, cosmical physics including a sketch of astronomy, meteorology and terrestrial magnetism.
>
> [Stewart to Roscoe 6/6/1870, RSC Archive]

With reference to one of the three new chairs mooted in Jack and Roscoe's extension plan of December 1866: Stewart added significantly: "of course if a Chair of Astronomy and Meteorology is founded this will relieve the Professor of certain branches" and also "I think the division is sufficiently elastic to admit of any specialty of either man being taken advantage of". It is clear that the trustees did "take advantage of" Stewart's Kew-based expertise in teaching astronomy and meteorology, for

[11] We can contrast this with Kargon's view that Stewart was merely a "widely ranging scientist" with such widely "varied interests" that it was "not surprising" that he was "reluctant" to accept the proposed division of the chair. Kargon evidently misses the *biographical* and *institutional* significance of Stewart's preference for professorial chairs divided according to specialism rather that methodology [Kargon,1977,215].

in eventually appointing Stewart to one of the Natural Philosophy Professorships they obviated the need for such an additional chair, and thereby created the financial means of providing two separate professorial salaries.

By contrast the two other chairs proposed in the 1866 paper [OMTMEP 14/12/1866,8,13] were established within five years of being mooted: Civil and Mechanical Engineering in 1868 and Mineralogy in 1871 [Thompson,1886,628-629]. The strategic economy of thus employing Stewart both as a physicist and as meteorologist/astronomer was clearly sought by Roscoe in his ingratiating letters soliciting and guiding Stewart's application for the new Chair.

In the light of the College's tactical deployment of Stewart's meteorological and astronomical expertise from Kew Observatory to Owens College, it is important to note Stewart's perspective on this institutional continuity. As Superintendent of Kew his intention was to incorporate the methods of observatory physics into the teaching and research practices of the laboratory that the trustees were to furnish for the newly appointed Professors. Stewart thus revealed his plans for being "Superintendent" of the laboratory in his letter of 6th June 1870 to Roscoe:

> ...Then as to the Physical laboratory that I should be nominally superintendent of it, with virtual freedom to my colleague to do what he wished.
>
> The Physical laboratory would not only be used in experimental illustrations of certain laws enunciated in the lectures but I think that *some observational and also some experimental research ought always to be going on in order that the more advanced students should be brought into contact with nature.*[12] Then they ought to be taught the use of the various instruments and set to devise and work

[12] Emphasis added - see chapter 1.

> out experiments. They ought also to be taught the philosophy of experiment:
>
> 1. To pay attention to and evaluate all sources of error giving due weight to each and dismissing those that ought to be disregarded (thus it is a very common mistake to give inordinate importance to some utterly useless refinement).
>
> 2. To pay strict attention to residual phenomena as indicating something new[13].
>
> 3. To reach the legitimate conclusion from an experiment no more and no less.
>
> But this is a subject on which one could write without limit...
>
> [Stewart to Roscoe 6/6/1870, RSC Archive]

This philosophy of experiment was a subject upon which Stewart could write "without limit" because it had been a major preoccupation of his during the eleven years he had just spent as Superintendent of the Kew Observatory. The biographical and institutional studies which follow of Stewart's activities at Edinburgh under J.D. Forbes, in the B.A.A.S. measurement committees and at the Kew observatory in standardizing and utilizing measurement equipment will be used to account for the three aspects of his "philosophy of experiment" described above. After showing how this "philosophy of experiment" derived from these early researches we will demonstrate that it lay at the foundation of his laboratory practice as Owens professor of experimental physics between 1870 and 1887.

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[13] Kargon's deciphering of Stewart's almost illegible script here is: "to pay strict attention to [natural] phenomena as... something new" [Kargon,1977,216].

4): Balfour Stewart's apprenticeship with J.D. Forbes at Edinburgh.

Balfour Stewart was born in Edinburgh in 1828 and acquired his earliest scientific education through his uncle, Dr Cloaston, who as minister of Stanwick was a well known naturalist and meteorologist [Proc.Phys.Soc.,9.9]. At the age of 13 he briefly attended the University of St Andrews before moving on to the University of Edinburgh where he studied in the Natural Philosophy class of James D. Forbes during the session 1845-46 [Mon.Not.Roy.Astro.Soc,48,166]. Of Forbes' lecture class Stewart later recalled that "the truth was conveyed to his hearers in the best possible words...he was not content with merely apprehending a truth, but he viewed it in all possible lights, and finally selected one as the best point of view from which to paint it to his class" [Stewart,1871, 391-92].

Leaving Edinburgh at eighteen to be apprenticed into the maritime business, he became active in Forbes' subject again on a trip to Australia in the mid 1850's, and published two papers whilst in Australia: "On the adaptation of the eye to the nature of the rays which emanate from bodies" and "On the influence of gravity on the physical condition of the Moon's surface" [Tait,1887a,289; Roy.Astro.Soc obituary,106; Stewart,1855a&1855b]. To pursue his interests in natural philosophy Stewart returned to Britain and from 1855 to 1856 he worked as Assistant to John Welsh at the Kew Observatory on a thermometric instrument for recording extremes of temperature change. Welsh saw a use for this in the Observatory's meteorological surveys and commissioned the B.A.A.S. Kew Committee to authorize construction and undertake ratification of the instrument. Thus from 1856 Stewart's expertise in meteorological measurement was integrated into the every day operations of the observatory [Scott,1885,57; Stewart, 1856].

Moving from Kew back to Edinburgh, Stewart returned to the University to study mathematics under Kelland and natural philosophy under Forbes, being assistant to the latter from 1856 to 1859. As assistant he lectured in mechanics and mathematical physics, took over Forbes' own more general lectures during his periods of illness and assisted him with his experimental investigations [Stewart,1871,392; Tait,1887a,289]. This work directed Stewart's attentions to some of the Professor's specialist subjects viz. radiant heat, meteorology and terrestrial magnetism and also gave him the experimental expertise to carry out his own researches in precisely these fields during the subsequent three decades[14]. For example, Stewart's work on Prevost's "Law of Exchanges" in radiant heat between 1857 and 1861 was an extension of earlier work done by Forbes and Melloni, and his researches were acknowledged by contemporaries as establishing the equality of radiative emissivity and absorbence for all materials [Stewart,1858;1859c;1859d].

The Royal Society in particular acknowledged this work by making him a Fellow in 1862 and awarded Stewart the Rumford Medal in 1868; historians of theoretical physics - in their all too infrequent references to Stewart - have ironically followed suit in focussing attention *only* upon this aspect of his work e.g. Siegel in his biographical article for the D.S.B.. By contrast Stewart tellingly emphasised the *experimental* aspect of his work on radiant heat in later discussing his debt to Forbes:

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[14] For a discussion of Forbes' researches in these and other subjects e.g. glacier dynamics see the "scientific biography" written by another student of Forbes in [Tait,1871,457-577].

> [Forbes] very generously gave me many hints, and allowed me to use not only his own apparatus, but to make use of his valuable specimens of rock-salt. Had it not been for these facilities, I should not have succeeded in the investigation to any extent...I know that I owe any success which I have attained in a very great measure to those habits of thought which a man like Forbes was so well qualified to communicate.
>
> [Stewart,1871,392]

These "habits of thought" which Stewart attributed to Forbes in his experimental work matched the kaleidoscopic yet scrupulously selective methods of his lecture technique described above, remarking that: Forbes "was not satisfied in his own researches with viewing a thing in one light, but he insisted on verifying his conclusions by corroborating evidence derived by regarding the subject from a different light" [Stewart,1871,392]. The significance of this otherwise banal hagiographical commentary upon his former mentor is that Stewart's own superficially diverse experimental researches were similarly unified in being different approaches to a subject of great importance to Stewart viz cosmical physics. In his pursuit of the correlation between the dynamics of terrestrial meteorology and magnetism and the observable behaviour of the sun, we find the inter-relation between Stewart's work from 1855-1859 on constructing atmospheric thermometers, measurements of the earth's magnetic field and radiant heat [Stewart,1856;1858,1859a;1859b;1859c, 1859d]. Stewart's symbiotic research into these subjects will be analysed through a study of his activities in *observatory measurement* as Superintendent of Kew Observatory from 1859 to 1870.

5): Observatory measurement and cosmical physics at Kew.

Subsequent to his short stay at Kew in the mid-1850's, Stewart cultivated a reputation as a meteorologist through his work on the B.A.A.S. "Magnetic Survey of Scotland" between 1857 and 1858. In this survey he assisted the superintendent of Kew, John Welsh, in carrying out measurements of magnetic dip, declination and field strength in observation stations throughout Scotland [Stewart,1859a]. After Welsh died Stewart was appointed to replace him as Superintendent of the Kew Observatory in July 1859 and the Kew Committee explained of their appointment: "from the experience he obtained under the direction of Mr Welsh, [Balfour Stewart] is peculiarly fitted for the office" [B.A.A.S. Report,1859]. From July 1859 Stewart thus took up the manifold duties of terrestrial and solar *measurement* that were attached to the post, and into these duties he brought the skills in measurement that he had acquired under Forbes[16].

The functioning of Kew Observatory as a centre of measurement under the auspices of the B.A.A.S. date from 1842 when Lieut.-Colonel Edward Sabine negotiated its transference from the Crown to a Committee of the B.A.A.S. in order to act as a metropolitan geophysical centre for his global "Magnetic Crusade" [Cawood,1979,514]. This Kew committee included members of the "magnetic lobby" such as Sabine, Sir John Herschel and John Gassiot as well as Charles Wheatstone, and their prerogative was to oversee the meteorological and magnetic observations at Kew. In addition they were to supervise the Observatory's operation as a depository for the

[16] Not long after his arrival at Kew, his Edinburgh heritage of natural philosophy was reinforced through his appointment as Additional Examiner in mathematics at the University of Edinburgh in 1861, when he formed a long-term alliance with Forbes' successor P.G.Tait [Tait,1887,290].

B.A.A.S. manuscripts and apparatus and hence also its role as the British Association's research and standardization laboratory particularly for the Verification Department set up in 1850 using equipment obtained from Regnault to provide a national service in calibrating and graduating thermometers [Scott,1885,53-55].

The work done by Sabine and the "magnetic lobby" at Kew and at the other Observatories in their Empire was essentially a global "natural history" of terrestrial magnetism and meteorology and also of solar astronomy. From their vast "Humboldtian" collection of measurements of the earth's magnetic field, colonial weather patterns and solar activity Sabine *et al.* arrived at two "inductive" generalizations. First of all there was a temporal correlation between the sunspot cycle and the periodicity of terrestrial magnetic storms, and secondly that the secular variation in the earth's magnetic field could be analysed into both intra-terrestrial and extraterrestrial components [Cawood,1979,493&516; Sabine, 1851;1852]. Such was the received view of "cosmical physics" underpinning the symbiotic meteorological and magnetical work as well as the instrument ratification undertaken at Kew Observatory when Balfour Stewart worked there as an Assistant in 1855-56 and then as Superintendent from 1859-1871 [Scott,1885,57].

Stewart's tasks upon becoming Superintendent at Kew were thus to supervise the operation of the late Welsh's self-recording magnetographs, to complete Welsh's Magnetic Survey of Scotland, to assist Warren de la Rue in his photoheliographic recordings and to verify meteorological apparatus (barometers, thermometers and hydrometers) for the Admiralty, the Board of Trade, and Opticians [B.A.A.S. Report,1859,xli-xliii; 1860, xxxi-xxxiii]. However, whilst Sabine's Woolwich militia and later

Stewart's own observatory staff were employed in reducing and tabulating the measurements of magnetic dip, declination and force recorded by the magnetograph, Stewart occupied himself outside of his routine observational and administrative work with researches on the "cosmical connections" between terrestrial meteorology, geomagnetism and sun-spot activity following the programme laid down by Sabine in 1856 [B.A.A.S. Report,1856,xxx]. And throughout the measurement work Stewart undertook for both his routine Observatory duties and his cosmical researches during the 1860's we find him constantly applying the "philosophy of experiment" enunciated above in the 1870 letter to Roscoe regarding his role as prospective "superintendent" of the Owens Laboratory.

Stewart's first dictum of experimental practice was "to pay attention to and evaluate all sources of error giving due weight to each and dismissing those that ought to be disregarded". In his 1859 report on the Scottish Magnetic Survey we find Stewart undertaking fastidious analyses of the errors in his measurements and placing them in a hierarchy of significance before introducing any necessary "correctional" factors [Stewart,1859a,168-169,184,187-189]. We find a similar analysis in his work from 1867 onwards as Secretary to the Meteorological Committee of the Board of Trade, for example in his "Account of certain Experiments, on Aneroid Barometers, made at Kew Observatory, at the expense of the Metorological Committee"[Stewart,1868]. This is a detailed study of "the circumstances which may be supposed to affect the workings of aneroid barometers" in which he analyses the possiblities of error in their measurements due to time, temperature and sudden variations of pressure. After a cursory examination of the apparently negligible effects of the first two, his paper subsequently addresses the effects of sudden pressure change which he concludes can deleteriously effect the barometer's working to the extent that it becomes a differential rather than an absolute instrument [Stewart,1868, 472-3&480].

A definitive example of Stewart's analyses of error hierarchies in measurements made with barographs, thermographs and anemographs at Kew can be found in his comprehensive 1869 report to the B.A.A.S. of "A description of the means adopted by the Meteorological Committee for ensuring accuracy in the numerical values obtained from their self-recording instruments" [B.A.A.S. Report,1869, l-lxxv]. It was for work of this sort that Stewart received a reputation for fastidious exactitude in standardizing equipment and making standard measurements. As one obituarist wrote:

> Every species of inquiry which had to be carried out at Kew - whether it consisted in the testing of thermometers, sextants, pendulums, aneroids, or dipping needles, the recording of atmospheric electricity, the determination of the freezing point of mercury, or the melting point of paraffin, or the careful studies of the peculiarities of the air-thermometer received the benfit of his valuable suggestion and was carried out with his scrupulous accuracy.
>
> [Proc.Phys.Soc,9,10]

Stewart's second experimental dictum was "to pay strict attention to residual phenomena as indicating something new", meaning that the unexplained "residual" regularities observable in secular measurements of geomagnetism, solar-sunspot rotations etc were a research resource for discovering new causal relations. This hunt for "residual phenomena" he applied particularly to his studies of sun-spot activity in collaboration with Loewy, and de la Rue, both in the varying *magnitude* of sun-spots and in the oscillating *period* of their solar rotation.

Stewart et al, measured the size of sunspots in recording their movement over the solar disc with de la Rue's photoheliograph and found that in their complex perturbations of size there were regular minima nearest the planets Mercury and Venus. From this they drew evidence for their cosmical theory (after Sabine) that there was a geomagnetic interaction between the sun and its planetary satellites [Stewart, 1870b; Stewart, de la Rue & Loewy, 1865a; 1865b; 1866; 1869; 1870]. Similarly, in analysing the variation in sunspot periodicity between 1780 and 1870, Stewart found that the curve of solar activity could be represented by superposed oscillations of ten and a half, twelve and sixteen years. This corresponded to periods of planetary conjunctions from which Stewart drew further conclusions about the nature of cosmic interactions between the planets and the sun [Stewart,1871][16].

In relating this search for "residual phenomena" to Stewart's third philosophical dictum "to reach the legitimate conclusion from an experiment, no more and no less" we can informatively cite this obituarist's view of his attempts to legitimate his conclusions from perturbation analysis:

> Balfour Stewart paid attention especially to the shorter periods which have been detected in solar activity. The problem, in the absence of a well-defined theory, is one of very great difficulty, for it is almost impossible to decide how far accidental regularities may mislead, and how far irregularities may hide effects, which are admittedly small. Balfour Stewart was always cautious in his statements, and tried to verify his results by repeating his reductions whenever he had an opportunity of attaining fresh material.
>
> [Mon.Not.Roy.Astro.Soc.,48,167]

[16] To comprehend Stewart's application of this third experimental dictum would require a fuller discussion of his research protocol than is appropriate here. However, see below for a discussion of some of his *abortive* researches at Owens.

Stewart's attempts to frame a "well-defined theory" for his work on correlations between solar activity, geomagnetic activity and terrestrial meteorology at Kew brought him to a cosmical view in which sunspot variations were responsible for geomagnetic disturbances and magnetic storms, and these geomagnetic disturbance in turn were causally related to patterns of change in global wind patterns. This theory formed the substance of both his introductory lecture at Owens College in October 1870, "Recent Developments in Cosmical Physics" [Stewart,1870a], and also of his address as President of Section A of the B.A.A.S. in 1875 [Stewart, 1875]. However, his attempts to make a causal and "exact science" out of this cosmical meteorology through his observational measurements at Kew brought him into conflict with the residual core of the incumbent "magnetic lobby" amongst the Kew Committee and the Royal Society. Sabine and Gassiot, in contrast to Stewart's approach, considered their prerogative at Kew as one of a gentlemanly "natural history" of meteorological observations, collecting, reducing and tabulating vast quantities of observatory measurement but eschewing theoretical speculations about the correlations thereby obtained.

6): <u>exact science vs natural history in Kew meteorology.</u>

The divergence between Stewart's campaign to forge meteorology into an "exact science" and the Kew Committee's concern only with the natural historian's "collection" of observational measurements was manifested in two controversies in the late 1860's. These disputes were over duties and methods in the reduction of meteorological and geomagnetic data and led to their mutual estrangement in 1869 and eventually precipitated Stewart's departure to Owens in the following year.

As Secretary to the Meteorological Committee from 1867 onwards Stewart supervised and tabulated the measurements of 1) air temperature, 2) elasticity of the aqueous vapour, 3) barometric pressure, 4) pressure of dry air and 5) humidity made at Kew and these results were published by Sabine in the Proceedings of the Royal Society, of which Sabine was then President [e.g. Sabine;1868;1869]. In his 1869 paper it is clear that Sabine saw this meteorological work very much as a form of natural history, as may be discerned in his prefatory explanation of the paper's content: "in meteorology and climatology much instruction may often be derived from tracing the modifying influence of diversities of situation". Following this he goes on to make entirely qualitative comparisons between these specimen results of climactic conditions and those collected in "diverse situations" at meteorological stations at the same latitude as Kew in both Russia and Asia [Sabine,1869,3-7].

By contrast Stewart, as a natural philosopher, believed that these collections of wide-ranging and precise measurements should instead be used to forge meteorology into a branch of exact physical science. Thus in 1869 he explicitly advocated a sixth meteorological measurement of "the hygrometric quality of the air", i.e. the flow of water vapour through the atmosphere, in order that the dynamics of precipitation could properly be analysed. He thus considered that measuring this parameter along with the other five mentioned above would render the collective reduced observations at Kew sufficiently comprehensive for a quantitative study of atmospheric dynamics to be made [Stewart,1869a,43-45]. Schuster tells us however that Gassiot, the chairman of the Kew Meteorological Committee told Stewart that he was "much opposed" to the latter's scheme of reduction and that there was "not much chance of it being adopted"

[Schuster,1932,208].

Hence when Stewart decided to publicize details of his new scheme to the British Association at Exeter in the summer of 1869, the venue of publication was not the official Report of the B.A.A.S Kew Committee but instead in the Proceedings of Section A (Mathematics and Physics). In this document "Remarks on Meteorological Reductions, with especial Reference to the Element of Vapour" he laid out his claims to be founding a physical science of meteorology through his scheme of measuring vapour transference, explicitly diverging from the purely "climatic" interests of the "naturalists":

> In the first place meteorological reductions may be pursued with the immediate object of acquiring information as to the climate of a place; or secondly, they may be pursued with the immediate object of extending our knowledge of meteorology, regarded as a physical science...the amount of vapour present in the air is without doubt a very important part of the climate...[but] regarding meteorology...as a physical science, it is one of our objects to ascertain the distibution and laws of motion of the dry and wet components of our atmosphere; and it cannot be denied that we are in very great ignorance of these laws.
>
> [Stewart,1869,43]

Some of Stewart's colleagues at the B.A.A.S. were extremely sympathetic to his scheme, e.g. the doyen of measurement physicists Sir William Thomson who declared approvingly in early 1870:

> Dr Stewart proposes to establish a *cordon* of meteorological stations, and to arrange a reduction of observations taken at them, so as to keep, as far as possible, an exact account of the quantity of water vapour entering and leaving the space over the surrounded district. This appears to me a most valuable proposal, which, if well carried out, must have a very important influence, tending to raise meteorology from its present empirical condition to the rank of a science.
>
> [Thomson,1870,306][17]

Nevertheless Gassiot *et al* at Kew viewed Stewart's public criticisms of their meteorological policies with little favour; according to Schuster, "Stewart was naturally distressed by the manner in which his advice was set aside, no scientific grounds being given. Fearing that the anxieties of his office might endanger his health he resigned his secretaryship [of the Metereological Committee]" [Schuster,1932,209]. This he did on the 8th October 1869, only about a month after reading his paper on this subject to the B.A.A.S. meeting, and his resignation was to take effect on the 31st of March 1870 [B.A.A.S. Report,1870,xlix].

Nevertheless, before his resignation was effected Stewart took the opportunity of venting his spleen in the newly-launched Nature during November 1869. In his article "Physical Meteorology - its present position", he alluded bitterly to his battles with the Kew committee in speaking of the "scientific worker" as having to "work with the one hand and fight with the other" and more explicitly declared "of course we all

[17] In his polemical evidence on this subject to the Devonshire Commission in 1872, Stewart cites letters from both Thomson and Airy dated October 1869 in support of his scheme of "hygrometric" monitoring; Airy, the Astronomer Royal at Greenwich, remarked to Stewart of his scheme "I do hope that by going on thus you may make meteorology into a science of causation, and raise it from its present contemptible state" [Airy to Stewart 7/10/1869, cited in Stewart,1872, q11354]

know that there has been a deplorable lack of co-operation among observers, as well as of system in making their observations" [Stewart, 1869b,102]. Acrimoniously referring again to his dispute with Gassiot et al, over the propriety of his vapour measurements in the Kew system of meteorological reductions, Stewart argued: "if we want to obtain physiological results we must reduce our observations with especial reference to physiology[18], while if physical results be desired, they must be reduced with especial reference to physical laws" [Stewart,1869b,b103].

Stewart's ill feeling on this subject continued some time after his departure from Kew and is palpable in his 1876 article for Nature "Meteorological Research" [Stewart,1876]. Here he writes sardonically of the "natural historical" phase in the development of meteorology, hitherto prevalent in "Royal" bodies such as Kew and the Royal Society, in terms of:

> ...a period when our whole duty to meteorology was considered to be fulfilled by attaching observers of the barometer and thermometer to Royal Societies and Astronomical Institutions. These produced results, which were reduced after a mechanical and strictly statistical method, and then put aside in a drawer...the last mentioned method might have been pursued to the end of the world without leading to anything like a true science of meteorology. To take an extreme case, it would have been just as useful to tabulate the number of leaves that fall in autumn, or a number of swallows observable in a day of summer...
>
> [Stewart,1876,389]

[18] "Physiology" and "natural history" were interchangeable terms in this period. For example in the late early 1870's Huxley's Chair at the Royal School of Mines was referred to variously as "Physiology" and "Natural History" in his interviews with Select Committees and the Devonshire Commission.

This is Stewart's most emphatic denigration of traditional meteorology a' la Sabine as an anachronistic practice of mere natural historical collection[19]. Stewart's dark reference to the collected results of meteorological observations being "put aside in a drawer" was explicated further in arguing for what he considered to be the major desideratum in framing meteorology as an exact science:

> ...individual observations ought to be thrown open to men of science in general, who should be encouraged and aided to utilize them to the greatest possible extent...with men of science having the greatest possible access to the observations and generously aided in their enquiries. It is only by this means that the edifice of a true science of meteorology can ever be erected, and then only stone by stone on the foundation of accurate observation.
>
> [Stewart,1876,389]

The specific incident alluded to here by Stewart in these passages was Sir Edward Sabine's disingenuous "hoarding" of Kew geomagnetic observations, Stewart's protests against which led him to another major altercation with the Kew establishment in 1869-70. In 1867, after Sabine had completed and published his reduction and tabulation of geomagnetic work at Kew up to December 1864, he arranged to transfer all such work from his personal headquarters in Woolwich back to Stewart's staff at Kew [B.A.A.S. Report,1867,lvi]. This added greatly to the work load of Stewart who had in the same year been appointed Secretary to the Meteorological Committee of the Board of Trade but was allowed only to use the surplus funds from each financial year to pay extra staff to assist

---

[19] This imputation of ossification he reinforces in alluding to the control of British meteorological research by gentlemanly amateurs: "a..system controlled by a committee [viz the Kew Committee] consisting of eight unpaid members of the Royal Society, all of whom are eminent in science, although not all eminent in meteorology..." [Stewart,1876,389].

him in these reductions. Two years later in June 1869, Stewart was grudgingly given extra funding in order to carry out the reduction of the magnetograph records from 1863 to 1870 in order to have the work done "with as much rapidity as is consistent with accuracy" [Stewart to Gassiot 4/3/1870, in B.A.A.S. Report,1870,liv-lv].

However, in pursuit of causal correlations for his quantitative researches on cosmical physics Stewart proposed that "a more intimate comparison between solar and magnetic records be made". To this end the Kew Committee agreed that he be "requested to prepare such a comparison for *one magnetic component* , for a whole period of solar disturbance" and was instructed to focus his attentions primarily to the "phenomena of the disturbances from from 1863 to 1870" [Stewart to Gassiot 4/3/1870, in B.A.A.S. Report, 1870,liv-lv]. These stipulations were an implicit rejection of Stewart's desire for such comparison to be made for all three components viz magnetic dip, declination and force, and to be made over a longer period starting in 1857. In his (slightly confused) rendition of this controversy, Arthur Schuster cites an extant letter from Stewart to Sabine in which it is clear that Stewart had been waiting, prior to 1865, for Sabine to pass on the Kew magnetograph observations for 1857-1862. Sabine however kept these magnetographic records in his private collection at Woolwich, and refused to pass them on to Stewart [Schuster,1932,211].

Matters came to a head when, about 1869, Stewart spotted what he considered to be an error in Sabine's minuted geomagnetic reductions but as Stewart later informed Schuster "[Sabine] informed me there was no mistake and added in answer to a question that he, *on his own responsiblility* , had authorized the preparations of those results at the central [Woolwich] Office as had not been authorized by the Committee".

Schuster thus remarks that "Sabine admitted having cooked the minutes" [Schuster,1932,208]. Stewart's protests and threats to resign the Superintendency elicited from Sabine a memorandum on March 1st 1870 in which he dismissed Stewart's scheme for new reductions of the magnetographs between 1858 and 1862, explaining that he had published all appropriate information on these records in his Royal Society paper of 1863 [Sabine,1863]. Sabine tersely added that it was now Stewart's duty to continue this reduction work for the years *subsequent* to those for which Sabine had completed his reductions, according to a resolution at the Kew Committee meeting in June 1869 meeting cited above [Sabine to Gassiot, 1/3/1870, in B.A.A.S. Report,1870,l-lii].

Given that his attempts to reform the Kew systems of meteorological and geomagnetic reduction had thus been rejected by the conservative forces of the Kew establishment, Stewart's bid to establish Kew as a centre for studying cosmical physics and meteorology as inter-related exact sciences became untenable. While Stewart's reluctant and simultaneous resignations from the secretaryship of the Meteorological Committee and from the Superintendency are thus entirely comprehensible, it is pertinent to note that even before his resignation had been formally accepted Sabine had sent a letter to a Colonel Smythe in Bombay to offer him Stewart's recently vacated positions [Schuster,1932,209-210] .

We can surmise then that Stewart would have been greatly relieved when on May 20th 1870 he received a letter from H.E. Roscoe inviting him to apply for the Owens' Chair of Natural Philosophy [Stewart to Roscoe 20/5/1870, RSC Archive]. This was an opportunity for Stewart to escape the inimical political environment of Kew, to find sympathetic scientific company for establishing meteorology and cosmical physics as exact sciences and to have a "corps" of laboratory students available to assist him in his meterological and magnetic reductions. Morever, here was an opportunity for Stewart to bring to the Owens laboratory his "philosophy of experiment" which, as described in his letter to Roscoe of the 2nd June 1870, derived directly from his extensive expertise in observatory measurement throughout the 1860's.

7): Stewart's "philosophy of experiment" in the Owens laboratory 1870-1887

By July 1870, the Owens Trustees and Extension Committee had agreed to follow the recommendations of William Thomson and Dr Joule in dividing the chair of Natural Philosophy into senior and junior Professorships. After holding interviews on the 7th of July they appointed Balfour Stewart to the senior post and Directorship of the Physical Laboratory, and J.T. Bottomley to the junior post [OMTMEP 7/7/1870,5, 31-32]. On the 21st July Bottomley declined the trustees' offer in order to work as Thomson's assistant in the newly-built laboratory at the plushly reconstructed University of Glasgow [Ch.2]. Thus it was somewhat ironic that, on the same day as news arrived of Bottomley's enticement to the palatial facilities of Gilmorehill [Ch.2], Stewart met the Owens College Committee to "explain the nature of the accommodation which would be required for the physical laboratory" of which he was to take charge [OMTMEP,5: 21/7/1870,36; 22/9/1870,44].

The laboratory suite he was shortly to receive were "the rooms hitherto used for the Natural History Department[20]" and these were restructured and fitted up according to Stewart's suggestions. He was readily given a grant of £100 for new apparatus and had a laboratory and lecture assistant, Frances Kingdon, appointed on 22nd of September. Yet as Schuster recalled of the state of Stewart's Quay St rooms: "the equipment of the laboratory, when Balfour Stewart took charge of it, consisted of the barest outfit for lecture-room illustrations, and the most urgent needs had to be supplied by the instrument makers before any start could be made" [Schuster,1932,53]. Thus on the very day after the laboratory's opening on the 19th October, we find in the minutes of the College Committee that Stewart urgently sought equipment sufficient for his students to carry out the measurement exercises described below. [OMIMEP,5: 21/7/1870,36; 22/9/1870,44; 20/10/1870,47].

Nonetheless, Stewart had a vision of laboratory physics on a somewhat grander scale than he was at first able to put into effect. His inaugural lecture given at Owens in early October entitled "The Recent Developments of Cosmical Physics" is an informative document, giving a survey of his post-Kew outlook on the interconnections between meteorology and solar physics [Stewart,1870b]. However, it also illustrates his views of the role of a student research laboratory in the local Mancunian context of technical education. Explicitly aligning himself with the pedagogical faction favouring education through "scientific principles" rather than "scientific facts", he articulated his view of the prospective Owens laboratory as a prerequisite of such an education. Here he speaks with the

[20]The operations of the Natural History Department had been transferred to the premises of the Manchester Natural History Society in 1867, apparently to accommodate just this form of laboratory expansion [Thompson,1886,279-281].

characteristic contemporary rhetoric of the laboratory as the venue for students to have direct personal "contact" with nature [Ch.1] and thus to discover for themselves what William Thomson called "new truth" [Ch.2]:

> ...it is...eminently desirable that the student should have an intimate, ready, and comprehensive knowledge of scientific principles...Now to bring this about the lecture room is not enough, but it must be supplemented by the laboratory, whether chemical or physical, in which the student desiring proficiency may be broght into intimate contact with nature herself.
>
> In the laboratory he may see with his own eyes, and handle with his own hands, as well as hear with his own ears.
>
> If he be determined to cross-question nature after a fashion of his own, he will have the opportunity of doing so. If he thinks he has found out a new truth, he will have the opportunity of making good his conjecture.....The laboratory is a place where a certain class of speculations may be brought at once to the test of experience...
>
> [Stewart,1870b,6-7]

Integrated into this somewhat Thomsonian conception of a student laboratory as a place for students to make their own researches, Stewart also emphasized the liberal education that would be received by employing the observatory practices embodied in the "philosophy of experiment" which he had articulated to Roscoe in June:

> But, besides its use in supplementing class instruction, an experimental laboratory has an important, though indirect, influence upon the training of the mind [for] above all things the student is taught caution in his deductions [Dictum 3].
>
> Another lesson which the laboratory student may learn, is "not to despise the day of small things". An unexpected result always means something [Dictum 2], it may mean some experimental error which the student is thus taught to avoid [Dictum 1], but it may also mean some new truth.
>
> [Stewart,1870b,7]

Most tellingly with regard to his second dictum on "residual phenomena" in laboratory measurement experiments as a "research resource", Stewart allegorically related an Eastern legend of a fisherman who caught a tiny vase in his net which turned out to house a mighty genii:

> Now the experimental philosopher who in his laboratory discovers a new truth resembles this fisherman. The whole history of science is full of instances where the greatest results have flowed from the most trivial experiments.
>
> [Stewart,1870b,7]

We will shortly consider a few of the experiments which Stewart undertook to discover "new truths" from residual effects in "trivial experiments".

It is important to note for the moment that although Stewart projected, after the manner of fellow Scot William Thomson, that the research of "new truth" would be a major component of his student laboratory, when his laboratory opened on he 19th October, a few days after this inaugural lecture [QMPTEP 20/10/1870,5,47] we can discern a still stronger similarity between Stewart's laboratory training and that of his Edinburgh friend and collaborator P.G.Tait. As we saw in chapter 3, Tait's laboratory differed from Thomson's only in the respect that Edinburgh students went through a more *systematic* course of training in measurement techniques than the somewhat haphazard inculcation received by students in the Glasgow laboratory. The systematic course of measurement training was advertised in the Owens Calendar of 1870 as follows:

Practical Course in Physics

> The physical laboratory will be open for practical instruction in physics daily throughout the Session... The following will form some of the subjects which will be practically taught in this course:-
>
> 1) Accurate determinations of the mass and comparative density of bodies, by means of the Balance and other instruments.
>
> 2) Measurement of the volume of bodies.

3) Determination of the rates of expansion of bodies by heat.

4) Methods of measuring temperature, and determining specific and latent heat.

5) Illustration of the laws relating to the radiation and absorption of heat [i.e. Stewart's researches of 1857-1862], with the application of those laws to Spectrum Analysis.

6) Means of determining the intensity of light, and the index of refraction of bodies; also the focal length of lenses.

7) Measurements connected with Electricity, suich as those of quantity of Electricity, of Electro-Motive force, and of the Electrical Resistance of bodies.

[Owens College Calendar, 1870, 39-40]

Although there is no extant correspondence between Stewart and Tait discussing the establishment of the Owens laboratory, from circumstantial evidence it appears very likely that Stewart drew upon Tait's expertise as a laboratory teacher in planning his experimental course of instruction. For example, in the same month that his laboratory was opened (i.e. October 1870) Stewart published his Lessons in Elementary Physics and in the preface to this text we find the acknowlegement that "the various branches of the subject have been so arranged that the student may perceive the connexion[21] between them. For many particulars of this arrangement I am indebted to my friend Professor Tait" [Stewart, 1870b, v]. In addition, we know that from about 1865 Tait collaborated with Stewart in his research on "cosmical physics", one particular long-term collaboration being a study of a viscous interplanetary "aether" through

[21] The perceivable inter-connexion which Tait advised him upon was none other than the principle of energy, and Stewart's elementary text embodies simplified versions of the laws of energy behaviour that were equally much at the heart of Tait and Thomson's 1867 Treatise on Natural Philosophy [Thomson & Tait, 1867].

laboratory measurements of frictional effects upon a disc rotating in vacuo -see below [Stewart&Tait,1865,1866,1869]. Since Tait was thus Stewart's primary collaborator in laboratory research prior to the foundation of the Owens laboratory, it is not too conjectural to infer that similarities in the operations of the Manchester and Edinburgh laboratories were not coincidental but rather the result of mutual discussion and a common context of laboratory research.

Something more tangible of the laboratory alliance between Tait and Stewart can be discerned in Tait's somewhat incestuous review of Stewart's Lessons in Elementary Physics in December 1870; this review appeared shortly after Stewart had been severely injured in a train crash on November 26th, engaged upon a journey which rather ironically consisted of a trip back to the hitherto hostile Kew Observatory [Nature,3,92; Physical Society Proceedings(obituary),9,11]. Tait wrote at the end of his glowing review:

> It is peculiarly sad that Prof. Stewart should have been temporarily disabled just when he was getting into working order his Physical Laboratory in Manchester: *no one is better fitted for such work than he is;* let us hope that he may soon be in a position to resume the direction of it, and to teach beginners by means of his excellent manual [viz. Lessons..]

It was nine months, however, before Stewart could be moved from the location of the accident at Harrow, and so what laboratory work took place in that session was under the supervision of his newly-appointed junior colleague T.H. Core [Proc.Phys.Soc,9,11].

When Stewart returned to Owens in the autumn of 1871 his first major act was to attempt to recreate something of his work at Kew in requesting £200 from the College to erect a magnetic observatory in the back garden of his home, there being insufficient room at the Quay St site for such an edifice [OMIMEP,7,20/9/1871,19-20]. As a student newly arrived in the Owens laboratory in the session 1871-72, the young Arthur Schuster observed that despite the sudden aging and physical disability caused by his near-fatal accident: "Stewart was indefatigable in his work. While the days were spent in the laboratory, he pursued his statistical investigations on magnetic and solar phenomena in the evenings" [Schuster, 1932,213]. Arriving at Owens in the mid 1870's, J.J. Thomson recollected that Stewart eased his investigative workload by incorporating the assistance into the reduction of his observational measurements on solar physics and terrestrial magnetism, in a manner typical of the Scottish "democratic tradition" discussed in chapters 2 and 3:

> Balfour Stewart was enthusiastic about research, and succeeded in imparting the same spirit to some of his pupils. I remember, shortly after I began to work in the laboratory, he was talking to me about sun spots, and said that he had made a large number of observations which he thought might throw some light on the connection between them and terrestrial phenomena, but that he had no time to reduce them. I ventured to say that if I could be of any help I should be glad to do what I could, and he gave me a number of observations to reduce. Though the work I did was purely arithmetical, I liked doing it and enjoyed the feeling that I was taking part in some real science.
>
> [Thomson .,1936,19-20]

From the comments of Schuster and J.J.Thomson it is evident that his formal lecturing took a secondary position to his laboratory teaching and research; as Schuster compared the two roles "he was not a good lecturer and had difficulty on keeping order in the lecture-room - perhaps it would be more correct to say that he did not take the trouble to keep order, being too sympathetic with youthful exuberance. In the laboratory [however] he was an inspiring teacher...[Schuster,1932,206]. Schuster explained that Stewart was an inspiring teacher "because he was one of the few who did not discourage attempts [by students] to discover new facts [Schuster,1911,21]. This impression of relative liberality in Stewart's teaching is confirmed by the recollections of J.J. Thomson of his work in the new three-room suite of Owens laboratories opened at Oxford Road in 1873:

> The new laboratory...was, I believe, the largest outside London at the time...[but] the classes in it were not very largely attended, and the work of each student was not so rigidly prescribed...we were allowed considerable latitude in the choice of experiments. We set up the apparatus for ourselves and spent as much time as we pleased in investigating any point of interest that turned up in the course of our work.
>
> [Thomson,1936,19]

From these comments we can discern, especially from Schuster's remarks, that Stewart was one of "the few" teachers of experimental physics who, at the time, was not exclusively committed to inculcating his students into the laboratory practices of "closure" viz. the regimented pursuit of the last decimal place in well known standard measurements [Ch.1]. This is a clear reflection of Stewart's own commitment to progressive research in the relatively new science of exact meteorology: although he cultivated the advancement of his subject through the conventional practices of precision measurement, his pursuit of hidden correlations between solar and terrestrial behaviour placed him in a divergent position to the orthodox wisdom of experimentalists that the content of physics had been almost completed by the measurement researches of the 1850's to 1860's [Ch.1].

Stewart's implicit denial of "closure" is discernible particularly in his attempts, following the second dictum of his "philosophy of experiment", to find new phemomena from the detection of "residual effects" not only in meteorological and astronomical observations but also in "trivial" laboratory experiments. As Schuster described this facet of Stewarts' work in the Owens laboratory "he was always busy trying to open out new fields of enquiry", and specifically cited Stewart's experiments on aetherial friction with a rapidly rotating disc in vacuo, carried out "in a room partly used for laboratory instruction" [Schuster,1911,21]. In these experiments the professor sought to detect a small rise in temperature of the disc caused by the frictional action of the viscous aether upon it: he believed that a positive result from this experiment combined with his earlier work upon thermal radiation would enable him to establish conditions under which the Second Law of Thermodynamics might break down [Stewart,1873,32-34; Schuster,1911,21-22].

Returning from Germany to act as honorary demonstrator in 1873, Arthur Schuster found Stewart undertaking a similar search for "residual effects" in gravitational phenomena, attempting to detect a minute variation of gravity either due to chemical combination or mechanical screening [Schuster,1932,20-21]. He thus engaged the assistance of the undergraduate J.J.Thomson to make high precision measurements of net weight changes in chemical reactions; in his autobiography Thomson relates in detail how his zealous pursuit of such measurements nearly led him to being permanently blinded [Thomson,1936,20-21]. Both these projects, as well as Stewart's attempts to find interference effects in perpendicular electric currents, produced null results. Yet although Schuster as his successor in the Manchester chair later criticized him for using insufficiently refined and anachronistic equipment, he remarked that as an assistant both he and the laboratory students had "benefited from [Stewart's] alertness and freshness of mind" in observing these otherwise abortive experiments [Schuster,1911,22; 1932,213].

During the 1870's, Stewart was able to research as intensively as this in the Owen's laboratory because he was able to delegate much of the practical teaching in systematic measurement techniques to his junior colleague T.H. Core, to Kingdon, his demonstrator and to Schuster who from 1873 was an unpaid "honorary" demonstrator [Schuster,1911,20]. After Schuster left for the Cavendish laboratory in 1876, Stewart appointed another of his early students who had been third Wrangler in Cambridge that year to be demonstrator: John Henry Poynting. Whilst teaching at Owens between 1876 and 1878 Poynting carried out measurement experiments of the more orthodox "standard" variety that Stewart in attempting a laboratory determination of the mean density of the earth [Poynting,1878]. However, when Poynting resigned in 1878 to take up a Fellowship at Cambridge, at the same time transferring his terrestrial measurements to the Cavendish [<u>Proc.Roy.Soc.</u>,<u>92</u>,ii], Stewart was unable to appoint a replacement. As a result of the ever increasing size of his laboratory clientele[22] Stewart thus was obliged to take a more active role in the laboratories practical teaching.

In 1879 he and Core devised a more elaborate scheme of measurement training, but which for lack of teaching assistance they held in abeyance until the following year [<u>OECMPC</u>[23] 7/3/1879,359]. Although still without a

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[22]

Session	No. of laboratory students
1871-72	8
1875-76	13
1880-81	27
1885-86	62

[University of Manchester,1906,1]

[23] <u>Owens Extension College Minutes of Proceedings of Council</u>.

demonstrator in 1880, perhaps buoyed up the elevation of Owens to become a central component of the Victoria University in that year, the two professors decided to initiate the new course anyway, and with successful results [Owens College Minutes of Senate 16/6/1880]. Full details of this new scheme appear in the Calendar for 1880-81 and whilst being in part an elaboration of the 1870 syllabus, it also explicitly introduces students to the measurement procedures which had been his specialism at Kew during the 1860's:

> ...13) Practice with the standard and with the working barometers, and wet and dry bulb thermometers, and with Regnault's Hygrometer.
>
> 14) Practice at the Magnetic Observatory and with the transit theodolite...
>
> [Owens College Calendar, 1880-81, 56-57]

In the following year Stewart was at last able to appoint a new demonstrator, W.W. Haldane Gee to assist him in teaching this expanded course of measurement [University of Manchester, 1906, 133-34]. With Gee's assistance, Stewart developed his course - with its ubiquitous traces of meteorological practices - into the subject of a laboratory manual viz. Practical Physics [Gee & Stewart, 1885]. A brief analysis of this collaborative text will serve to illustrate how Stewart ultimately adopted the characteristic didactic practices of "closure" physics, i.e. measurement of standard physical quantities, whilst still inculcating his observatory-derived 1st dictum of his "philosophy of experiment" into the training of students in reduction and error analysis.

In the preface of Practical Physics, Stewart and Gee explain that the book "took its origin in the felt necessity for systematizing the work of our physical laboratory...[and]...learning from various quarters the desirability of a simple yet systematic treatise on physical instruments, we were at length induced to undertake the task ourselves [Gee & Stewart, 1885,v]. After a detailed *apologia* for experimental training in the physics laboratory *vis-a-vis* the chemistry laboratory, the authors went on to document a comprehensive course of lessons in the measurement of length, angle, mass, area, volume, density, elasticity, pressure and gravitation [Gee & Stewart,1885,vi-vii & 1-258]. Significantly they acknowledged the assistance of their laboratory students in working out their examples and methods and for checking the numerical accuracy of their results, very much as Thomson and Tait had acknowledged the work of their own students in publishing research [Gee & Stewart,1885,viii].

Most revealing of their pedagogical ideology of experimental physics, however was the appendix "On the selection, conduct, and discussion of operations suitable for the physical laboratory", in which Stewart and Gee argue specifically that whilst qualitative discussion of physical relations was appropriate to the lecture-room, the work of the laboratory was necessarily *quantitative* in character [Gee & Stewart,1885,259-277]. Thus they argued, for example, that a student familiar with the laws of energy from lectures should make the kind of measurement experiments described in their text "to impress upon him the reality of these laws in a very forcible manner" [Gee & Stewart,1885,259]. From this view of the *cognitive* importance of laboratory measurement they laid out a definitive late nineteenth century view of a training in "closure" physics of exact measurement in terms of the addition of extra decimal places to the accurate determination of known physical constants:

> [A student] ought first to know experimentally the instruments most frequently used in physical research, as well as the proper method of using them. And after that, when more advanced, he may employ these instruments or invent others with the view of increasing our knowledge, more especially in the direction of *completing tables of of physical constants...*
>
> [Gee & Stewart,1885,261]

After this expression of commitment to the orthodox view of experimental measurement physics as a vehicle for "completing" the last unresolved numerical details of the discipline, Stewart and Gee give a detailed exposition of the practices at the heart of his 1st dictum of the "philosophy of experiment". In devoting an appendix to the methodology of analysing and avoiding errors, and also to the appropriate means of accurately reducing the results of systematic observations, Stewart communicated something of his unique expertise in observatory measurement to the generation of physics students who used Practical Physics as a standard laboratory manual in the 1880's and 1890's.

Stewart died in 1887 without achieving his professed intention of further incorporating this ideology and practice of observatory measurement into pedagogical texts on specific branches of experimental physics [Gee & Stewart,1885,preface]. Nevertheless, Stewart's other long-term goal of establishing meteorology as an exact branch of physics was achieved by his former pupil, assistant, colleague and finally successor to the Owens chair of experimental physics: Arthur Schuster. In 1892, at Schuster's request, the Victoria University of Manchester founded a separate lectureship in meteorology with the charge of the University observatory within the department of physics. To this post they appointed G.C. Simpson, the physical meteorologist who wrote Professor Schuster's obituary for the Royal Society in 1934, thus epitomising the intimate relation of experimental physics to the pursuit of exact meteorology which had existed at Owens since Stewart's appointment to the Manchester chair in 1870 [University of Manchester,1906,31-33; Simpson,1934].

Conclusion

The physical laboratory at Owens College, was borne of a period of great institutional turbulence and expansion in the Mancunian industrial context of the 1860's. The chair of natural philosophy was established in 1860 as a financial expedient to support the college's bid for a regular clientele of University of London external students, despite the opposition of the incumbent mathematician to the disruption of his traditional academic prerogative. In the cramped confines of the College's Quay Street accommodation the first two professors of natural philosophy, Clifton and Jack, campaigned for a teaching laboratory as part of a wider appeal for the "extension" of science instruction at Owens, an appeal which took great force and raised considerable funding in the context of the much debated outcome of the Paris Exhibition in 1867.

Although a new Owens site with purpose-built laboratories was already under construction at the time of Jack's departure, the College offered a new laboratory to his joint successors in the Quay Street buildings. In soliciting an application for one of the two now extant chairs from Balfour Stewart, then Superintendent of Kew Observatory, Henry Roscoe made a bid to incorporate professorial expertise in both natural philosophy and meteorology into one appointment. This successful move effected both major economies in the Owens extension scheme, and also rescued Stewart from an acrimonious dispute with the conservative establishment of Kew and the Royal Society over his pursuit of meteorology as an "exact science".

In his role as laboratory teacher and researcher at Owens from 1870 to 1887, Stewart inculcated his students in the skills of observatory measurement which he had acquired at Kew in the previous decade, engaging his students in his researches in the typically "democratic" manner of Scottish natural philosophers. In the Owens laboratory, Stewart was particularly concerned to promulgate his three-fold "philosophy of experiment" which we have interpreted as an expression of his commitment to the advancement of meteorology as an exact science through careful measurement and error analysis. Stewart believed that major new laws of cosmical physics were to be uncovered from application of this "philosophy" to laboratory practice, and his research and teaching consequently reflected a divergence from the orthodoxy of "closure" adhered to by most contemporary experimentalists [Ch.1].

Although his later teaching of highly standardized measurement practices indicate that he moved to a more conventional laboratory pedagogy in the 1880's, his pursuit of meteorology as an exact science relating terrestrial physics to solar activity was ubiquitous throughout his professorial career at Owens. We can thus appropriately conclude with Stewart's view of the role of the measurement laboratory in the advancement of cosmical physics:

> Our object [in cosmical physics] being to detect not merely the chemical constituents, but likewise the temperature, the pressure, and the velocity, and perhaps mass of the solar currents, it is essential to know by means of laboratory experiments how the various influences affect the spectra of terrestrial elements...the same individual who observes the sun should also experiment in the laboratory.
>
> [Balfour Stewart to Lieut.Col.Strange 29/5/1872 in Devonshire Commission 3rd Report,1873,Appendix VII 29]

CHAPTER 8

Frederick Guthrie, the physical laboratory at the Royal School of Mines and the formation of the Physical Society

There was a time when Guthrie lived a curious life; he would not leave his laboratory, even at night. He had a hammock rigged up, and used to live in the laboratory...

Oliver Lodge: 1924 speech to Jubilee meeting of the Physical Society [Physical Society,1924,39].

Introduction

The last in this series of case-studies will be a study of two related developments in metropolitan institutions of experimental physics that were intimately associated with the career of Dr Frederick Guthrie. The creation of a physical laboratory for Guthrie's use at the Royal School of Mines in 1872 and Guthrie's own formation of the Physical Society in 1873 will be treated as closely connected phenomena since the meetings of this Society took place in the RSM physics laboratory, and the running of the Society was very closely linked with the chair of physics at the RSM throughout the last decades of the nineteenth century. Indeed, the President of the Society at Guthrie's death in 1886, Balfour Stewart, spoke of the "very great advantage" to the Society which had derived from this close connection during Guthrie's lifetime [Stewart,1887,7].

These twin subjects form an appropriate common endpoint to the preceding case-studies for two similarly related reasons. First of all, the founding of Guthrie's laboratory, along with similar experimental facilities for chemistry and biology, to form a Government centre for teacher-training in South Kensington symbolised the national recognition of experimental skills as prerequisite to the practice of school science teaching. Secondly, the coexistent Physical Society, of which almost all British laboratory physicists had become members by the time of Guthrie's death in 1886, functioned as a common forum for Foster, Guthrie, Clifton, Adams et al, to discuss the experimental methods and apparatus which they employed in giving laboratory instruction to their predominant clientele of trainee physics teachers.

However, neither the Physical Society nor Guthrie's physical laboratory were created without considerable and extended controversy. Indeed, the problems Guthrie encountered will be portrayed in this chapter as characteristic, respectively, of the contemporary internal and external institutional opposition to the ascendance of academic experimental physics. We will explore in some detail the tensions within the community of academic physicists which led both to the foundation of the Physical Society amongst laboratory experimentalists and to the Society's alienation from the Cambridge mathematical analysts [Ch.1]

In the first section of this chapter, the genesis of Guthrie's physics laboratory at the Royal School of Mines will be placed in the context of the institutional conflict between the School's professorial "mining" faction and the ascendant "scientific" faction between 1868 and 1872. Having documented Guthrie's acquisition of laboratory facilities we will then discuss his early career as an experimental chemist and relate his subsequent development as a highly idiosyncratic physicist to his long period of isolation in Mauritius. We will analyse the manifest heterodoxy of his scientific practices, as perceived by contemporaries, with regard to i) the character of his teaching at the RSM, ii) the critical reaction to his textbooks and iii) his problematic relations with the [redacted] Royal Society, and its Secretary: George Gabriel Stokes.

To establish, first of all, the institutional context of these controversies we will consider the evolution of the School of Mines up to the year of its removal to the laboratory complex in South Kensington.

1): Physics at the School of Mines 1851-1872

Although a number of detailed documentary histories of the School of Mines are extant from the 1890's onwards [Chambers,1896; Reeks M., 1920; [redacted], none of these accounts explicitly document the genesis of Guthrie's physical laboratory or T.H. Huxley's biological laboratories. This section will thus consider the movement of biology and physics from their position as elementary subjects in the School's first-year curriculum to become quasi-autonomous laboratory-based specialisms at the end of the first two decades of the Schools' existence.

In 1849 the Government extended the Geological Survey, under the direction of Sir Henry de la Beche, to become a geological school after it had received appeals from the mineral industry to supply a scientific training for the nation's mining engineers. The staff of the Survey were employed as the professoriate of the Government School of Mines in Jermyn St, the resident geologist being De la Beche, the metallurgist Percy, the mineralogist Smyth, the chemist Playfair, the naturalist Forbes and Robert Hunt, the Keeper of Mining Records, was the practitioner of experimental mechanics. From 1851 the School of Mines therefore existed in symbiosis with the Geological Survey and the Museum of Practical Geology under the Directorship of De la Beche, and his successor of 1855, Roderick Murchison [Chambers,1896,x-xii; RSM Propectus,1871,2].

The curriculum of the Royal School of Mines was originally construed to provide professional qualifications for mining engineers, geologists and metallurgists rather than a comprehensive general or liberal education in science [Chambers,1896,xv]]. Over the next four years the Board of Trade widened the vocational basis of the School to become the "Metropolitan School of Science applied to Mining and the Arts" in 1853. To this end the Royal College of Chemistry under the jurisdication of August Hofmann, was affiliated to the School; G.G. Stokes was appointed to the newly-created lectureship of physics in 1854: concurrently with his (unremunerative) post as Lucasian Professor of Mathematics at Cambridge. In the same year, the dynamic T.H. Huxley took up the Natural History chair, and thereby arrived the leading figure of the "scientific faction" of the School's Professoriate [Chambers,1896,xv-xxiii; Nature, 26,233].

Nevertheless, "the mining and metallurgical division" of the School regained its precedence over the scientific "general division" in 1855 when Roderick Murchison was appointed to the Directorship after the death of de la Beche [Chambers,1896,xv-xxiii]. When the responsibility for the School was transferred from the Board of Trade to the Department of Science and Art in 1856 Murchison was given a free rein in structuring the educational role of the school [Chambers,1896,xxiv-xxv; Reeks M.,1920, 96-97]. The fate dealt by Murchison to the Department's 1853 plans for a centre of metropolitan science was thus later related by his adversary Huxley as follows:

> ...this course of development was more or less nipped in the bud. The instruction in Jermyn Street narrowed instead of widening; the general and technical division were generally abolished, and the institution restricted itself as far as it could, to being a school of mining and metallurgy, pure and simple....The change of policy was signalised in the year 1857 by another change of name; the institution was then called "the Government School of Mines...until in 1863 the tile was..[dignified] ..to the "Royal School of Mines".
>
> [cited in Nature,26,233]

As Margaret Reeks points out, the first change of title was due to a personal request by Murchison to the Lord President of the Council, Lord Salisbury, that the School's function and title should revert to their original form[1] [Reeks M.,1920,98]. And subsequently Murchison's strictly mining-oriented regime was thus sufficiently effective to win Royal assent for the "narrowing" of the School's function so deplored by Huxley. This

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[1] In his retrospect Huxley incorrectly cites the year as being 1859: Huxley himself had gone before a Select Committee in the year before this to explain that the change of title in 1857 had been "thought expedient, partly to avoid the appearance of having drifted away from the original object of the School, and partly to remove any grounds of jealousy on the part of other scientific bodies" [Select Comittee,1858,q7972]

assent followed from Murchison's newly-won support from the Committee of Council on Education for his adherence to the interests of the mining community, as expressed in their report to the House of Lords in 1862:

> The aim to be kept always in view should be to make the school as directly useful as possible to the mining interest...the instruction should have especial reference to mining, and should be of a technical character such as is not available elsewhere, embracing those branches only of general science which are applicable to mining, and touching on Chemistry, General Mechanics, Physics and Natural History, only as far as is required for mining purposes...this elementary instruction must be regarded as an arrangement of a temporary nature, to cease as soon as the standard of attainment on entrance can be raised sufficiently high...[indeed] if there were no Mining School it is doubtful if general instruction in science should be undertaken by the Government.[10th Report of the Department of Science and Art,1863,189] see also [Reeks M.,1920,101]

Given that the Government considered the teaching of Huxley on biology, Tyndall on physics and Frankland on chemistry could be so readily dispensed with, the scientific professoriate were hardly in a position to demand improvements in their teaching facilities during the mid-1860's. However, in 1867, in the aftermath of the Paris Exhibition (ch.1) the Department of Science and Art formulated plans for a Central College of Science to meet the emerging demand for an industrially applicable scientific education, and in particular to provide a formalized training for the growing body of school science teachers [see Ch.1]. In these plans the Department's Secretary Henry Cole, and the "Official Inspector of Science" Captain Donnelly envisaged an expansion of the RSM as the means of creating this College: despite the severe constraints that Murchison had placed upon the general teaching of physics, chemistry and biology there, Donnelly concluded that "[by] far the larger proportion" of the instruction given in it by Huxley, Tyndall et al was nonetheless "general, and would [thus] form a course applicable to any industry"

[Select Comittee,1868, Appendix 11,448].

Donnelly and Cole's specific suggestion was that the RSM be amalgamated with the only other existing Government-run institution of scientific training, viz the Royal School of Naval Architecture, for which a large new building was already under construction in South Kensington. As it was, the Jermyn St premises of the RSM were exhaustively occupied with very little hope of structural extension to accommodate the RSNA or further laboratories for the RSM Professoriate [Reeks T.,1869,q1264-1265]. However, the issue of accommodation for the proposed Government Science College was one of many educational issues debated in the 1868 Select Committee on "instruction in theoretical and applied science to the industrial classes" and the Royal Commission on Scientific Instruction and the Advancement of Science formed in 1870. In the context of these governmental investigations, two major issues were raised in relation to the use of the new premises at South Kensington: the importance of *practical* instruction for science teachers in training with the Department of Science and Art [[redacted] Ch.1] and the need for increased laboratory space expressed by the scientific professoriate of the RSM.

The latter issue was addressed with some vehemence by T.H. Huxley in his interview with the Select Committee, now that the continued existence of his department was assured by the basic principles of Cole and Donnelly's plan:

> ...I find it a very great impediment that I neither have any [teaching assistants], nor have I anything corresponding to the laboratory of the chemist. I cannot teach my own branch of science properly, because I have nothing answering to a dissecting room, or biological laboratory. The young men should have studied the facts for themselves at all events, to a certain extent; but there is not even a room provided for such purposes.
> [Huxley,1868,q7958]

Similarly in 1870 Huxley complained that there was

> an entire want at the School of Mines, as it now exists, of teaching several of the subjects practically. For example I am set there to teach natural history without the means of showing a single dissection. I am in the position of a chemist who is set to teach chemistry without a laboratory..[thus]..I cannot teach in the proper[a] sense of the word.
>
> [Huxley,1870,q296-297].

In a later reminiscence Huxley was a little more specific about his colleague Guthrie in relating that "the same want must have been felt in the teaching of physics" [cited in Nature,26,233-234].

Although Guthrie himself did not give any evidence to either the Select Committee, we do know from the testimony of the School's Registrar, Trenham Reeks, that in 1870 there was indeed felt to be a "want" of practical instruction in physics and other subjects. Reeks stated that the chemistry and metallurgy laboratories at the RSM were consistently oversubscribed: "the place is altogether full", and "certainly" a *general* wish had been expressed that further accommodation be provided [Reeks T., 1870, q400-402]. Even Roderick Murchison acknowledged that laboratories were desirable for both Huxley and Guthrie to commence practical teaching [Murchison,1870,q2472 - see below].

Whilst there was a general consensus expressed by the RSM professoriate to the Devonshire Commissioners on the importance of Huxley and Guthrie being granted laboratory space for their teaching, there was great dissent over Donnelly and Cole's proposal to achieve this goal by moving the entire School to the South Kensington site. Trenham Reeks

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[a] In 1882 Huxley declared in similar vein: "For eighteen years I did my duty as well as I could towards that institution [the RSM], lecturing about natural history, and I am sorry to say, all the time, with the more or less definite consciousness, that I was an involuntary imposter and that it was not possible for me to teach in any genuine fashion, because I had no room in which practical instruction could be given" [cited in Nature,26,233-234].

explained that the choice presented was essentially between "two evils", the one being the disadvantageous *separation* involved in removing the School from the Geological Museum and Survey housed in Jermyn St, and the other evil being the "impossibility of enlarging and giving the professors their proper accommodation" if they remained at the Jermyn St site [Reeks, 1868,404]. Being in favour of the move, Reeks was inclined to play down the level of opposition to it blandly reporting that two of the School's "mining faction" closely associated with the Survey and Museum viz. Smyth, the mineralogist, and Ramsay the geologist, would be "inconvenienced" if they had to lecture away from the Jermyn St base [Reeks,1870, q406 & 421].

However, Reeks' comment that he had heard no "serious objection" to the proposed move to South Kensington [Reeks,1870,422] was clearly disingenuous, as is apparent from the later testimony of Sir Roderick Murchison - a man to whom Reeks acted as personal Secretary. Murchison gave the Devonshire Commission the following belligerent exposition of the "mining faction's" views on the constitution of the School of Mines:

> I wish now to place on record my protest against the scheme for breaking up the Royal School of Mines...[In Donnelly and Cole's interviews with the 1868 Select Committee] it was...too evident that the great change to be made in the Royal School of Mines had been pre-arranged by certain authorities without any reference to myself....If my own views were to be carried out, I should like to see two houses in Jermyn St rented or bought, wherein to establish sufficient laboratories for those professors who require more space for their teaching and illustration; viz those of natural history and physics"
>
> [Murchison,1870,q2472][3].

[3] It is extremely important to note here that Huxley was himself a member of the interviewing Devonshire Commission: in these circumstances it would obviously have been difficult for Murchison or any other interviewee to deny the propriety of Huxley's claim for a biological laboratory !

In advocating a laboratory annexe in Jermyn St in preference to the South Kensington option, Murchison also strategically played down the need for Frankland's and Percy's respective laboratories to be extended, thereby attempting to remove this issue from the armoury of the South Kensington protagonists: "I know from Dr Percy, who had so successfully taught [metallurgy] in Jermyn St, that a small amount of additional accommodation will perfectly satisfy him [Murchison,1870,2472]. Of Frankland's laboratory at the Royal College of Chemistry Murchison similarly claimed that "not one inch more space was required for anything connected with our mining establishment" [Murchison,1870,2477]. This was clearly a *strategic* argument against the move to South Kensington because his views here completely contradict those he articulated in his memorandum to the Lords of the Committee of Council on Education during February 1869. This correspondence[4] on the condition of Percy's and Frankland's working environments closed with the telling words: "the inadequacy of accommodation in these laboratories is, therefore, deeply to be regretted, amd demands their Lordship's serious attention" [16th Report of Science & Art Department,1869, Appendix A 24-26].

To pre-empt the Commissioner's attack on this inconsistency in his case, Murchison finally argued from his position as Director of the Geological Survey that the School of Mines, as an institution dedicated to assisting the national industries of mining and agriculture, would gain no benefit from amalgamation with another college of more general scientific

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[4] This memorandum entitled "Inadequacy of Accommodation in Laboratories" contained letters from both Percy and Frankland explaining how the lack of laboratory space prevented them from both meeting the student demand for a practical training in their subjects, and also from keeping abreast of the most recent researches in their respective branches of chemistry [16th Report of Science & Art Department, 1869, Appendix A 24-26].

education. "The Royal School of Mines," he declared, "is, in short, a public institution, which will always serve its legitimate purpose if not commingled with other public teachings, which have nothing in common with it" [Murchison,1870,q2473].

The Devonshire Commissioners were nonetheless unmoved by Murchison's appeal to the integrity of the School's original function, for in their first Report of March 1871 they expressed their conviction that "there is no necessary connexion between the direction of the Geological Survey of Great Britain and Ireland and the government of the Royal School of Mines" [Commission,1871,1(n2)]. Thus in recommending the foundation of physical and biological laboratories, and the expansion of the chemistry laboratories, the Commissioners argued that these new facilities should be accommodated *separately* from the Survey by moving the RSM to South Kensington so as to form the basis of a new science school [Devonshire Comission 1st Report, 1871, 1-2].

Six weeks after the Commissioner's Report was published, the "mining faction" of Ramsay, Percy and Smyth, with the unequivocal support of Murchison, sent a formal letter of protest at these recommendations to the Science and Art Department on 22nd April 1871. A sub-Committee of the Commissoners considered their objections, precisely those cited by Murchison, and summarily dismissed them [Chambers,1896,xxxv]. Yet despite such displays of full support by the Commissioners for the laboratory endeavours of Huxley, Guthrie and Frankland and for the DSA's general plan for a Science College at South Kensington, the First Devonshire Report was not immediately implemented - the Commissioners having no legislative power to support their recommendations. Moreover, the mutually opposed views of the "mining" and "scientific" factions within the School of Mines professoriate probably inhibited any decisive Government action.

Upon Murchison's death in October of 1871, however, some resolution of the situation appeared possible: firstly because the most vociferous objections to the South Kensington plans were silenced; and secondly because the joint Directorship of Mining School and Survey held by Murchison was thenceforth discontinued. To effect a convenient fission of of the two roles the DSA appointed Ramsay to the Directorship of the Survey, and authorized the School to be managed by its Council of Professors - following the 10th recommendation of the First Report [Cole to RSM Council, 22/5/1872 in RSM Minutes of Council, 22/5/1872].

Yet even though the Survey and School were now institutionally autonomous, a supplementary Report issued by the Commissioners on February 28th giving detailed recommendations on utilizing the South Kensington buildings as an amalagamated School of Mines and teacher-training centre, the DSA still made no moves to enact the Commission's Reports to effect the physical separation of the two bodies. Only in the summer of 1872 was the Department galvanized into action by the exasperated complaints of the science Professors at the cramped and ill-appointed accommodation in which they were obliged to work; as Huxley later described the mood he shared with Frankland and Guthrie in July 1872:

> By that time some of us had got extremely tired of it, and I was one of those who were so tired, my chemical colleague was another, my colleague the Professor of Physics was a third, and we got up a little sort of pronunciamento to say that we really could not go on teaching in that way any longer; [and] that at South Kensington there was a large building which was standing perfectly empty[5], and might we be allowed to do our business in a more efficient way by being transferred to this empty building?
>
> [cited in Nature,26,234]

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[5] It would appear however that Huxley and Guthrie had used the building to give practical teaching to DSA trainee science teachers in the summer vacation of 1871 [Ch.1] [Devonshire Commission 1st Report, 1871, xix(51); Nature,4,361-2]

Faced with such a "pronunciamento" on 8th of July 1872, the "mining faction" of Smyth, Ramsay and Percy abandoned their opposition to splitting the School between two sites and unanimously voted with the rest of the RSM Professoriate for the following resolution:

> The Council of the Royal School of Mines regret that notwithstanding their repeated representations sufficient accommodation has not yet been afforded for efficiently conducting the classes in the building in Jermyn St, where large collections essential to the School of Mines already exist. But, as there appears to be no prospect of obtaining an extension of premises, the Council are of opinion that it would be advisable to transfer the instruction in Chemistry, Physics and Natural History to the new buildings in South Kensington where it is understood that accommodation may be obtained.
>
> [RSM Minutes of Council,8/7/1872]

Now despite Huxley's retrospective account above, this building was not *quite* empty, for Huxley and Guthrie had used it to give practical teaching to DSA trainee science teachers in the summer vacation of the previous year viz. 1871 [Ch.1].And indeed, by May 1872, part of the South Kensington building had almost been completely furnished as a laboratory-based centre for training science teachers by the DSA [19th Report of the Department of Science and Art,1872,xi(14)]. The proposal resolved by the RSM Council was received by the DSA just as the summer teacher-training courses were underway, and consequently the reaction of the DSA was to amalgamate the laboratory facilities for these courses with the biology, physics and chemistry teaching of the RSM. In so doing, the DSA found a single solution to the twin problems of i) creating a metropolitan centre to provide certificated training for science teachers, and ii) of finding laboratory accommodation for the disaffected RSM Professors. Huxley and Guthrie were at last given their own laboratories in which to teach.

As the DSA reported the following year:

> In accordance with th[e resolution of the RSM Council] we decided to remove the College of Chemistry from Oxford St, and the instruction in biology and physics from the Royal School of Mines in Jermyn St to the new buildings at South Kensington. The instruction in physics and biology hitherto consisted of lectures only. It is now supplemented by laboratory practice, and we have thought that the opportunities afforded might be of real use in training teachers.
>
> [20th Report of the DSA,1873,xi(12)]

In the first year that the South Kensington centre was operational eight teachers were selected to take the first full-time training courses in chemistry, physics and biology alongside the trainee geologists, metallurgists and mining engineers attached to the RSM [20th Report of the DSA,1873,xi(12)]. This swelled the numbers in attendance at the chemistry and physics classes as shown below:

Attendance at RSM lectures 1866-1879

	Chemistry	*Natural History*	*Physics*	
1866-67	39	5	28	
1867-68	31	9	36	
1868-69	30	11	14	Guthrie replaces Tyndall
1869-70	36	24	13	
1870-71	24	28	15	
1871-72	42	20	23	
1872-73	62	16	33	1st year at S.Kensington
1873-74	48	19	40	
1874-75	66	19	29	
1875-76	64	21	20	
1876-77	59	16	31	
1877-78	59	21	24	
1878-79	70	27	20	
1879-80	59	27	24	

[Compiled from annual figures in 13th-26th Reports of the DSA,1866-1879]

As the Departmental Report in 1874 read:

> ...since the removal of the departments of physiology, physics and chemistry to the new buildings in South Kensington and the consequent improvement in the quality of instruction provided, a great influx of students in experimental science had occurred....The new laboratories have been and now are overcrowded with earnest and diligent students....
>
> [21st Report of the DSA, 1874, 36]

In the first year of the new school's location at South Kensington, the courses in the physics laboratory were not compulsory for RSM students but on June 7th 1873 Guthrie won the support of the Professorial Council for his motion that a "practical knowledge of physics should be required of every candidate for an Associateship at the RSM" [RSM Report 20 (1873), 10; RSM Minutes of Council,7/6/1873]. The next annual intake of students into Guthrie's laboratory thus rose sharply so that in the session 1873-74 he was teaching almost twice the number that attended his classes in the year before the move to South Kensington [see table above].

Given the apparent popularity and success of his laboratory courses let us now consider the origins and character of Guthrie's expertise in experimental physics. The following biographical account will analyse his transference from experimental chemistry to physics and then focus attention upon the evolution of his highly individual views and practices in experimental physics which dated from his long period of academic isolation in Mauritius.

2) Biographical background to Guthrie's career as an experimentalist.

Frederick Guthrie, although of Scottish descent, was born the son of a Bayswater tailor in October 1833, receiving his earliest education from a private tutor, the chemist Henry Watts F.R.S..Guthrie entered the School attached to the secular institution of University College, London at the age of 12, moving on to the College itself in 1849. Guthrie spent three years at the College specializing between 1850 and 1852 in Chemistry under Alexander Williamson, but also studying Mathematics under de Morgan - a significant point for our later discussions. It was in Williamson's laboratory that Guthrie again encountered Henry Watts, this time as principal assistant to the Professor of Chemistry; here Guthrie also met H.E. Roscoe, a student of Williamson's one year above him [Roscoe,1906,25 & 104]. Watts was subsequently a life-long friend and colleague of Guthrie's who commissioned a number of articles from Guthrie and another of his former students at UCL, G.C. Foster, for his Dictionary of Chemistry [Ch.4] [Foster,1886,8; 1887a,10-11].

After graduating B.A. in 1852, Guthrie probably acted as a laboratory assistant to Williamson at UCL and would then have met G.C Foster who arrived at UCL as an undergraduate in 1852 [Ch 4]. In the next two years Guthrie and Foster thus cultivated an acquaintance that was later drawn upon to form a close Professorial alliance in the late 1860's to 1870's; however in the spring of 1854 Guthrie departed for the Continent to study under a number of eminent German chemists. First, he worked with Bunsen at Heidelburg and then moved on to research with Kolbe at Marburg where he took a PhD in 1855. As a result of his researches he published three papers on organic chemistry in German, English and French journals [Guthrie,1856a;1856b;1857a] before returning to England in 1856[6] [Foster, 1886, 8; 1887a,10-11].

Guthrie returned to England in 1856 to take up a post as laboratory assistant to Edward Frankland, then Professor of Chemistry at Owens College, Manchester and later Guthrie's colleague at the Royal School of Mines. When H.E. Roscoe replaced Frankland in 1857 he later wrote that he

[6] The assertion by Oliver Lodge in [Lodge,1919a,320] that Guthrie was a co-student of Foster in Germany is therefore misleading since Foster did not leave England for Germany until 1858: the humorous and reminiscent exchanges between Foster and Guthrie about Germany reported by Lodge probably stemmed instead from their respective sojourns with Bunsen in Heidelburg.

found "my old friend Frederic [sic] Guthrie installed as the sole assistant" [Roscoe,1906,104]. From Manchester, the young chemist moved to Edinburgh[7] in 1859 to assist Professor Lyon Playfair in his University chemistry laboratory. After a decade of acquiring laboratory expertise in chemical teaching and research Guthrie's took up an appointment which broadened his academic prerogative to include physics [Foster,1886,8; 1887a,10-11].

In 1861 Guthrie was appointed Professor of Chemistry and Physics at the Royal College in Mauritius, and for six years as servant of the British Empire "he devoted himself to endeavouring to introduce and establish on a durable basis scientific instruction in the colony" [Foster,1886,8]. As Guthrie himself later described, "we teachers were meant to make ourselves generally useful..[hence]..I was supposed to teach chemistry, physics and the natural sciences" [Guthrie,1886a,634]; yet while obliged to teach such a range of scientific subjects he found time to carry out researches. In his researches on "Drops" and "Bubbles" in 1864-65 [Guthrie,1864;1865], we can clearly see the continuity of his transition from practical chemistry to a highly empirical form of experimental physics. As Foster characterised this formative transition:

> These are excellent examples of purely experimental investigations, well-planned and carefully carried out. No doubt it was in his work as a practical chemist that the properties of Drops and Bubbles first attracted his attention; but it is characteristic of much of his subsequent work....that he saw matter for careful study and experiment in the most familiar things, instead of, like most of us, letting familiarity with the outward look of things blind him to how little *accurate* knowledge of them we possess. [Foster,1887a,11]

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[7] Guthrie's congenial contact with Peter Guthrie Tait, who was appointed Professor of Natural Philosophy at Edinburgh in the following year, is suggested by the fact that Tait later christened one of his sons *Frederick Guthrie* Tait.

Since Guthrie was thousands of miles from the mainstream of scientific activity in Europe and America, in "circumstances in almost all respects adverse to scientific work" [Fostér,1886,9], it was perhaps not surprising that the character of Guthrie's experimental work was at a comparable "methodological" remove from his contemporaries: a single extant letter to Henry Roscoe in June 1861 requesting information on European developments in chemistry is suggestive of Guthrie's general sense of isolation [Guthrie to Roscoe, 5/6/1861, RSC Archive]. Indeed, since much of the development of precision physics documented in chapter 1 took place whilst Guthrie was abroad in the 1860's, we can trace the origins of Guthrie's characteristically idiomatic scientific practices, palpable in much of his subsequent career, to this period of effective isolation in Mauritius.

Guthrie's heterodoxy is clear in two respects in the papers on hydrodynamics and pneumatics that date from this period:

i) his idiosyncratic choice of subject matter,

ii) his disengagement from any abstract mathematical analysis.

In both "On Drops" [Guthrie,1864] and his subsequent analogical paper "On Bubbles" [Guthrie,1865] we see Guthrie attempting to acquire "accurate knowledge" of such familiar things as drops and bubbles, and specifically of the factors affecting the *size* of moving liquid drops in gases and gas bubbles in liquids. Guthrie's investigations are highly individualistic insofar as no reference is made by the author to any previous theoretical or experimental work on these subjects and consequently uses working definitions and a terminology that are entirely of his own manufacture. His "clarification" of the terms "drop" [Guthrie,1864,444-445] "bubble" [Guthrie,1865,22] [redacted] are typical instances of his idiosyncratic definitions and his discussion of "liquid cohesion" is a classic example of his etymological innovation:

> ...We are forced to the conception of two distinct kinds of cohesion [in liquids] - *stubborn* and *persistent*. These may co-exist, but are not identical. The one is strong to assert, the other pertinacious to maintain. The four following substances may serve to illustrate the possession of these two cohesions in various quantity.
>
> Talc has little stubborn and little persistent cohesion.
>
> Glass has much stubborn and little persistent cohesion.
>
> Gold has little stubborn and much persistent cohesion.
>
> Iron has much stubborn and much persistent cohesion.
>
> [Guthrie,1864,469]

Characteristically there is no attempt by Guthrie to convert this analysis into mathematical form. Such is the case with his determination of how five separate factors influence the size of drops[Guthrie,1864,445] for although he makes measurements of the highest precision i.e. up to six significant figures, he presents his results in tabular and graphical form to arrive at purely qualitative "laws" of drop and bubble behaviour. For example his first law of drop size reads as follows:

> *Law 1.* - The drop-size depends upon the rate of dropping. Generally the quicker the succession of the drops, the greater is the drop; the slower the rate, the more strictly is this the case.
>
> [Guthrie,1864,482]

Guthrie thus eschewed any notion of either converting his results into an analytical format, or of subjecting them to theoretical analysis. To see how his contemporaries viewed this persistent characteristic of Guthrie's experimental work, it is highly significant that this issue is raised in two of his obituaries: W.J.H. in the Dictionary of National Biography recorded that Guthrie had "but slight respect for the work of mathematical as distinguished from experimental physics" [DNB: Guthrie]. Even as close a friend and colleague as G.C. Foster wrote *critically* of his heterodox practices in his Nature obituary:

> [Guthrie's] scientific knowledge...was, much more than most men's of his own getting, the result of his own observation and experiment. In others, also he valued even a small scrap of self-gotten knowledge more than a large store of second-hand erudition. In this respect he sometimes went to excess, and, though, not without mathematical knowledge, he was somewhat apt to underrate the scientific importance of the work of mathematical physicists in comparison with that of pure experimentalists...
>
> [Foster,1886,9]

The consequences of his low respect for mathematical practitioners [Ch.1], despite his early mathematical training under De Morgan [see above], will be illustrated in our later discussions of his difficult relations with Stokes and Maxwell.

Guthrie's final paper at Mauritius bears out Foster's judgements in the way that he employed his singular notions on the dynamic behaviour of liquids in "a speculation concerning the relation between the axial rotation of the earth, and the resistance, elasticity, and weight of the solar aether" [Guthrie,1866]. Guthrie's predeliction for the genre of qualitative verbal discourse contained in this paper, in preference to abstract mathematical theorization on his researches, will be a recurrent theme in the rest of this chapter, as will his penchant for "unusual" subjects of investigation.

Upon returning to London on leave in 1867 Guthrie encountered John Tyndall, a physicist not unlike him in his original pedigree as a chemist and in his preference for experimental subjects, and discussed with him his researches on the thermal resistance of liquids. In submitting his paper on the subject to Stokes, the Secretary of the Royal Society, Guthrie informed Stokes that "Professor Tyndall has read and agreed to communicate it to the Royal Society, and should he be referee would advise publication in the Philosophical Transactions" [Guthrie to Stokes, 15/10/1868, RS MC.8.255]. Tyndall subsequently communicated the paper to the Society in October 1868 where it was read on Jan 21 1869 and thence published in the Proceedings [Guthrie,1869a1]. Nonetheless,it is clear that Stokes and Guthrie's referees did not readily concur with Tyndall's rec mendation as we can surmise from Guthrie's letter to Stokes of July 1869 - asking Stokes either to publish his paper in the Transactions or to return it to him without delay, commenting tersely: "I am bound to have my observations made useful to the public with as little further delay as possible" [Guthrie to Stokes,23/7/1869 ULC AD 7656 G833].

One of the Royal Society's referees for this paper was very probably James Clerk Maxwell, as we can discern from the well-informed letter that he wrote to Thomson in November 1869:

> Mr Tatlock tells me you want to know something about the conductivity of liquids for heat. The last thing on the subject is "on the thermal resistance of liquids"..[Guthrie] states in his paper (I do not know if it is to be printed.. ..) previous results. His experimental methods seem very good. His chief defect is that he never seems to know what he is going to measure. He works at the Royal Institution and has been so Tyndallized that he describes the specific resistance of a liquid to be "the ratio" of the quantity of heat arrested by the liquid to that arrested by an equal thickness of water...
>
> [Maxwell to Thomson 16/11/1869, ULC ADD 7342 M107]

The rest of Maxwell's letter follows in the same dismissive vein, implying that Guthrie's confusion over the application of measurement to thermal conductivity resulted from his lack of a clear theoretical conception of what he was attempting to achieve in his experiments - a point which we shall return to in discussing Maxwell's later reactions to Guthrie's work.

Maxwell's description of Guthrie as having been "Tyndallized" is significant for it is clear that the Tyndall was Guthrie's predominant scientific associate upon his return to England, and the low opinion of

Tyndall's work held by the Scottish natural philosophers, Maxwell and Tait, is well known [Knott,1911]. As a highly popular metropolitan lecturer Tyndall was nonetheless a useful ally for Guthrie and in late 1868 was instrumental in electing Guthrie from his post at Clifton College Bristol to the post that Tyndall himself had vacated in June of that year at the Royal School of Mines. After resigning as Lecturer in Physics at the RSM he had offered to assist the School's Council in selecting his successor, and four months after Tyndall had recommended his paper to the Royal Society Guthrie was appointed to occupy the position that had now been elevated to the Chair of Physics on 23rd January 1869[8] [Guthrie to Stokes,15/10/1868, R.S. MC.8.255; RSM Council Minutes,20/6/1868 & 23/1/1869].

Although this institutional elevation of natural philosophy reflected the recognition increasingly given to physics as an autonomous academic discipline [Ch.1], Guthrie's teaching duty upon taking up this post was the delivery of forty annual lectures that were "supposed to be sufficient physics for the future geologists, miners and metallurgists" [Guthrie, 1886c,660]. However, in the summer of 1869 Guthrie also acquired the new role in the Department of Science and Art of training Government science teachers and, as discussed in chapter 1 and above, he worked in concert with Huxley and Frankland to introduce a system of laboratory instruction into the DSA programme. After establishing practical teaching at South Kensington in the summer courses of 1871, exactly a year later these courses were developed by Guthrie into full-time courses of practical work for students at the Royal School of Mines: the rationale and content that

[8] It is highly likely also that the RSM Professor of Chemistry, Edward Frankland would have canvassed for Guthrie's appointment from his first-hand knowledge of Guthrie as his laboratory assistant at Owens College in 1856 [see above].

Guthrie gave to these courses will be discussed in the next section.

3): Laboratory measurement and teacher training 1872-1886.

In Guthrie's first session at South Kensington, the curricular position of physics was expanded from 40 annual lectures to 62 in the following proportions:

I: Molecular Physics and Sound - 12 lectures.
II: Heat - 15 lectures.
III: Light - 15 lectures.
IV: Electricity, Frictional Electricity and Magnetism - 20 lectures.

[RSM Calendar,1872,22]

Although laboratory work was not compulsory for RSM students reading for their associateships until Guthrie's second year at South Kensington, the relation of laboratory work to this course of lectures on physics was permanently established in the first session of 1872-73. Each laboratory class was designed to illustrate the daily lecture that immediately preceded it [RSM Calendar,1872,22-23] and in this respect it followed the pattern set by Guthrie's summer classes for provincial science teachers which still ran at the end of every session. As an observer in Nature explained of these classes, the trainee teachers used their own instruments "in repeating the experiments seen in the morning lecture, or in making physical measurements when ever it is possible to do so [Nature, 12,206].

Guthrie's entirely conventional view of the central importance of precision measurement in physics education was given its most definitive statement in his 1886 retrospective lectures: "there is no physical science without exactness, and there is no exactness without measurement" [Guthrie,1886c,660]. This concern with precision measurement originated in from Guthrie's earlier work in exact quantitative chemical analyses - just as we saw in our biographical study of G.C. Foster [Ch.4] - and it was by direct extenSion of such methods from chemistry to physics that Guthrie legitimitated this practice:

> I have always...tried to persuade those of my friends who are engaged in teaching chemistry that they would do well to begin at once with quantitative methods and determinations in the laboratory...I believe, as I have always believed, that the making quantitatively of a few preparations, and the carrying out of a few quantitative analyses give more material for thought, more nourishment to the mind than, and are educationally of infinitely greater avail than, the guessing of chemical conundrums. I am speaking of course of laboratory or practical work. This quantitative element is still more essential in physics. There everything should be quantitative and exact.
>
> [Guthrie,1886c,660]

It was in reference to such arguments on the importance of measurement in education that Guthrie explained the curricular emphasis on electro-magnetism that led him to give more lectures upon that subject than any other: "Electricity, especially voltaic lends itself perhaps more abundantly to exact measurement in the elementary laboratory than the other branches [of physics], and it is on this account...that it occupies a rather prominent part" of the course at South Kensington [Guthrie,1886c, 662-63]. One afternoon's work, for example, was specified by the following set of instructions:

1. *Measure* relative resistances of different lengths of the same copper wire by Wheatstone's bridge.
2. Find lengths of copper wires by *measuring* their relative resistances, the length of oneof the wires being known.
3. *Ascertain* relation between resistance and weight.
4. *Ascertain* effect of temperature on resistance.....

...6. *Measure* the external resistance of your cell

7. *Compare* the electromotive motive force of your cell with that of a Grove's cell. [Nature,12,246]

The italicized terms here show the extent of Guthrie's emphasis upon practical quantitative determinations in electrical work, whether explicitly so in "measuring" or implicitly so in "ascertaining" or "comparing".

Of this pursuit of pedagogical exactitude, however, Guthrie made the very important qualification that in laboratory teaching "there are different degrees of exactness", specifically explaining for example that one would not expect a student's analyses to be "of the same degree of refinement as though he were determining the weight of an atomic element. Let his analyses be *sufficiently* exact to convince him of the faithfulness of nature and the trustworthiness of the statements of science" (emphasis added)[Guthrie,1886c,660]. This attitude towards the training of teachers can readily be contrasted with the conviction of Robert Clifton [Ch.6] that a schoolmaster should have studied physics to the highest levels of exactitude in order to have the *authority* to teach the subject as accurately ratified knowledge. The difference between Clifton's and Guthrie's training of physics teachers was of course essentially a social one: on the one hand Clifton personally nurtured a handful of mathematicians of the highest pedigree to become an elite group of science masters in the country's top public schools. On the other hand, Guthrie's role as an employee of the Department of Science of Art was to certificate dozens of provincial teachers with little or no tertiary education as fit to give practical instruction in Government schools, providing them with a laboratory training of the minimum expense and time.

To train large numbers of aspiring school teachers within these two constraints and yet, at the same time, also ensure a *sufficient* exactitude in his pupil's experimental work to convince them "of the trustworthiness of the statements of science" Guthrie employed a special system of laboratory instruction.

4): <u>Building apparatus and teacher training</u>

Contrary to the impressions of one former student at South Kensington (H.G.Wells - see later), the scheme for practical physics teaching that Guthrie implemented in his laboratory from 1872 onwards was developed *collaboratively* with two other metropolitan laboratory physics teachers William F.Barrett and G.Carey Foster, as well as Guthrie's own laboratory demonstrator W.J. Wilson [<u>Nature</u>,<u>12</u>,245]. While Foster's laboratory teaching needs no further discussion [Ch.4], it is important to note that the South Kensington building to which Guthrie made summer teaching forays during the early 1870's until taking permanent residence in 1872 was also the home of the Royal College of Naval Architecture at which Barrett was lecturer from 1869 to 1873 [Chambers,1896,xxxi]. Thus until Barrett took up the Chair of Physics at the Royal College of Science, Dublin in 1873, Guthrie would have engaged in a frequent exchange of views with him in the metropolitan laboratory complex at South Kensington.

As a physicist, Barrett was an experimentalist of a similar character to Guthrie, taking his scientific apprenticeship as assistant to Tyndall at the Royal Institution from 1863 to 1867 [Barrett,<u>D.N.B</u>]. Indeed he effectively modelled himself quite closely upon Tyndall's example: as Oliver Lodge later recorded in an obituary article: "as a popular lecturer and teacher in the experimental phenomena of physics Barrett was very successful." As regards his similarity to Guthrie, Lodge also intimated that Barrett shared the RSM Professor's predeliction for non-mathematical experimentation remarking that "[Barrett] never pretended to follow the recondite mathematical and dynamical investigations of [the nineteenth] century, typified by the great names of Stokes, Thomson and Tait" [Lodge, 1925,88].

Even after the tripartite grouping of Guthrie, Foster and Barrett was dissolved when the lattermost moved to Ireland, we find strong evidence of the continuing character of their solidarity as proponents of practical physics teaching in Barrett's 1875 review of Weinhold's "Introduction to Experimental Physics" (trans. Loewy). In reviewing Foster's preface to this work, Barrett wrote very supportively of the UCL Professor's pedagogical creed of accurate physical measurement [Ch.4], and commended Weinhold's book as an ideal textual adjunct to a pedagogical scheme of laboratory work by then fully employed in Guthrie's laboratory [Nature,12, 482].

Guthrie defined the problem which he and his "eminent colleagues" [Guthrie,1886c,660] faced in creating the scheme as one of having a class of pupils aged from sixteen to sixty, and numbering from six to sixty, all anxious to learn experimental physics but lacking experience of practical work and having (for schoolteachers on vacation) only four months in which to aquire the necessary expertise in Guthrie's laboratory. In his view this meant that a major part of the problem reduced to being one of providing suitable apparatus:

> Such apparatus as is used for lecture demonstrations is for the most part quite unsuitable for the laboratory. Again instruments of precision as supplied by the best makers are far too delicate and too expensive to be be placed in the hands of the elementary student. For their use would be required as many skilled assistants as there are students to be taught [cf Clifton's identical views in Ch.6]. On the other hand the apparatus sold in the shops for use in elementary instruction is for the most part trash...as a rule the prevailing sentiment appears to be inspired by the "bag of tricks" of the conjuror. The parts involving the principles are deliberately disguised...
>
> [Guthrie,1886c,660-661]

His solution to the lack of suitable ready-made equipment was thus to introduce the innovative practice of students constructing *their own* apparatus; as Foster later recorded:

> The course of work he devised for the "Certificated Science Teachers", who came under his instruction at South Kensington was an entirely new departure in the teaching of Physics. Physics laboratories for the instruction of students had then begun to appear, but in them men were taught for the most part to use ready-made instruments. Guthrie's plan was to teach his pupils not only how to use, but how to make the instruments for themselves.
>
> [Foster,1887,12-13]

This satisfied the important desideratum of a cheapness in their course of training entailing the students' expenditure only on raw materials and tools such as files, hammers, screwdrivers, pliers, nails etc [RSM Prospectus,1874,23]. An additional benefit of this scheme was that the student would aquire special practical skills in glass-blowing and metal-soldering that were necessarily required in the assembly of experimental apparatus; "...what human being should be without this [skill]?", asked Guthrie rhetorically against the view that he was merely training students to become skilled artisans [Guthrie,1886c,661]. One former student, H.G. Wells, un-nostalgically accused him of just such a diversion from proper scientific goals, sardonically remarking that Guthrie "swept aside the idea that physics is an experimental science and substituted [for it] a confused workshop training". Wells later related: "when I came into the physics laboratory I was given a blowpipe, a piece of glass tubing, a slab of wood which required planing, and some bits of paper and brass, and I was told I had to make a barometer. So instead of a student I became an amateur glass-worker and carpenter" [Wells,1934,213][9].

The reactions of Guthrie's immediate contemporaries in the scientific

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[9] Wells' views are discussed in more detail below.

community were on the whole much more favourable, as for example an observer from Nature enthusisatically recorded of his tour of the South Kensington laboratories during the 1875 summer teaching courses:

> ...we find the teachers constructing apparatus which, though simple and often rough, is well adapted for teaching purposes. The raw material is provided them, printed instructions are given to each one, and under the direction of Prof. Guthrie and the gentlemen associated with him, the most useful physical instruments are built up....
>
> ..The advantages of this plan are apparent. Students unaccustomed to manipulation find to their astonishment, when they begin, that all their fingers have turned to thumbs, and are amazed at their clumsiness and stupidity. Very soon, however, fingers begin to re-appear, and the first successful piece of apparatus that is made gives them a confidence in themselves which they have thought impossible to attain. The pleasure of having made an instrument is increased a hundred-fold when it is found that by their own handiwork they may verify some of the more important laws in physics; or make physical determinations, which before they would have considered it presumption to attempt.
>
> [Nature,12,206 & 245]

In Guthrie's system the accuracy to which students could effect their verification measurements was of such importance that their assesament constituted *half* of a student's examination marks. As the Professor explained: "The apparatus so made is frequently and carefully examined and credit given for it, and as the experimental results obtained by its use depend entirely on the trustworthiness of the apparatus so the student soon gets to be exact in manipulation and exact in his habits of thought" [Guthrie,1886c,661]. Indeed,the level of "exactness" which was possible in these students "manipulations" with such self-made apparatus is clear from his quantification of how close their measurements could come to "the truth":

> The instruments [that] the student of average skill can and does make under proper instruction with these means are far more accurate than any he is at all likely to be able to buy...his barometer is a far more exact instrument than one for which he would have to give several pounds; that his spectroscope will divide the sodium line; that his coils are true to the thousandth of the nominal [resistance] value; that he can determine the wavelength of light to within 1/1000 of the truth, the specific heat of a metal to 1/100, and the length of a sound wave to 1/200 of the truth.
>
> [Guthrie,1886c,661]

Evidently this accuracy was enough to fulfil Guthrie's desideratum that a student's measurements and analyses be "sufficiently exact to convince him of the faithfulness of nature and the trustworthiness of the statements of science" for as he concluded of each student's ultimate qualifications:

> He finds himself at the end of the course with a set of apparatus made by himself and fully tested by himself; and he finds himself in possession of verifications of many of the great generalizations of physics,which generalizations, having been simultaneously been acquired by the working of brain and hand, he is slow to forget.
>
> [Guthrie,1886c,662]

Despite such lofty ideals Guthrie did not make great demands of his students - it was "rare indeed that one fails through want of sufficient ability" [Guthrie,1886c,661] - as the case of H.G. Wells clearly demonstrates. Wells was a student of Guthrie's in 1885-86, a time when the Professor was unknowingly terminally ill and in the last year of his life. We are left in no doubt from Well's autobiography that, in his debilitated state, Guthrie failed to win any respect from Wells; Wells for example coldly remarked that Guthrie's throat cancer "greatly enhanced the leaden atmosphere of his teaching" [Wells,1934,210] - a condemnation fuelled by Wells' preceding and vastly more successful year in Huxley's laboratory on the top floor of the South Kensington building [Wells,1934,199-206].

Whilst Wells condemned the manner in which Guthrie's lecturing "maundered amongst ill-marshalled facts" - in response to which Wells acted as a source of "ironical applause and petty rowdiness" amongst the audience - he condemned more vehemently still Guthrie's laboratory class. "Instead of being used in real work on the science of physics, the time of the class was frittered away in the most irrelevant and stupid "practical work a dull imagination has ever contrived for the vexation of eager spirits". Wells thus took great relish in giving a detailed account of his "horror of that physics laboratory" and his attempts to "guy and contemn Guthrie's instruction in every possible way" [Wells,1934,212,215 & 217].

To give just one example of Well's hostile response to Guthrie's programme of measurements using self-constructed equipment, we can cite the occasion on which Wells was set to determine the frequency of a tuning fork by measuring the trace made on a blackened glass plate as it dropped against a brush attached to the fork. Wells first attempted to harangue the demonstrator Mitchell into giving a definite statement of how much atmospheric resistance "vitiated the precision of the experiment", and failing in that quest he made extensive atheistic objections to the use of a wooden cross in the initial suspense of the glass plate - "a needless aberration probably tainted by the theological preposessions of Professor Guthrie". Thus Wells developed a more "Deistic" version of the same experiment to "guy and contemn" poor Guthrie [Wells,1934,216-217].

Nevertheless, when he assembled his apparatus at the end for marking at the end of the year, apparatus "of such distinguished badness that it drew an admiring group of fellow students" and which also for its sheer singularity was preserved in a cupboard for several years", Wells still managed to attain a second class result for his work in experimental physics [Wells,1934,217&233]. Failure in Guthrie's physics practical class was thus "rare indeed"...

Well's biographical anecdotes are perhaps enlightening with regard to his youthful rebellion against science into the arms of literature and politics, and also to the institutional rigidity that entered Guthrie's teaching in his later years at South Kensington. Wells' hostility to Guthrie's teaching, however, certainly contrasts with the views of his earlier students at South Kensington for if class attendance figures can be taken as a reasonable measure of Professorial popularity, we can note that throughout the 1870's Guthrie's lectures vied with Huxley's for the second place in student's estimation after Frankland's course on chemistry [see above table from DSA Reports 13-26(1866-1879)].

We know also of three physicists who responded more favourably to Guthrie's teaching in the mid-early 1870's than did Wells: in 1872 Oliver Lodge worked in Frankland's laboratory and occasionally attended Guthrie's lectures on the ground floor of the South Kensington building; of these he recalled "most of what Guthrie said I knew before; but there was no harm in recapitulation. Indeed I enjoyed the whole winter...." At Guthrie's request Lodge lectured in physics at Bedford College, the University of London's institution for women's education, from 1876 onwards and carried out some research for Guthrie until Lodge left London for Liverpool in 1881 [Lodge,1931, 70-71 & 91-92]. A.J. Fleming met Lodge at South Kensington whilst working with both Frankland and Guthrie between 1872 and 1874 [Fleming,1934,28]; Guthrie invited Fleming to give the first paper at the Physical Society's opening meeting in 1874 and thereafter Fleming found the latter to be a "most valued friend" [Fleming,1934,28]. Thirdly, although Silvanus P. Thompson initially judged Guthrie to be a "ponderous Scotchman" upon his first encounter with him, he subsequently spent two years from 1874 to 1876 engaged in joint research with him, e.g. on electrostatic induction which again was reported at the meetings of Guthrie's Physical Society [[redacted]] [Thompson H.G. & J.S.,1920, 17-27]]

Nevertheless, if Guthrie's students were appreciative of his teaching methods it is clear that the Professor of Physics did encounter considerable *initial* hostility to such an innovative scheme of laboratory work that involved the student's own manual construction of apparatus. As Guthrie retrospectively commented in 1886 upon the initial response of industrialists, educationalists and civil servants to this scheme "Many told me it could not succeed, many that it ought not to succeed" [Guthrie,1886c,663] - unmistakeable evidence that views akin to those of Todhunter on the propriety of practical laboratory teaching [Ch.1] were not lacking in general currency. Nonetheless, we can conclude this section by noting the level of support and success that Guthrie enjoyed in his innovative course of practical teaching; although his UCL colleague, Carey Foster, had elsewhere criticized some of his research practices, his praise for Guthrie's teaching was unqualified:

> Considering how absolutely the value of the system of instruction in science that has been organized under the Science and Art Department depends upon the pupils being made familiar with actual facts and phenomena, and not merely, with descriptions or statements about them, it may be regarded as a matter of national importance that for the last fourteen or fifteen years every Science Teacher who has gone through the course of physics at South Kensington, has gone back to his classes not only with a stock of thoroughly serviceable instruments, but he has learnt to make them for himself at a merely nominal cost, and can, if he chooses, spread the art amongst his pupils, and thus make his teaching of a thoroughly genuine and practical kind.
>
> [Foster,1887a,12-13]

<u>Nature</u> too, well known for its unequivocal support of laboratory training methods, celebrated Guthrie's scheme as being the foundation of nationwide practical physics teaching in schools:

> With a wise liberality the Department [of Science and Art] permits each teacher to take home with him, without any charge at all, all the apparatus he has made: and one can easily imagine the pleasure with which these simple and useful instruments are afterwards looked upon and used by those who have made them. Nor is this all; the impulse to sound and practical science teaching is given, and at the same time the hands have been disciplined to useful skill, and the senses to accurate observation.
>
> [Nature, 12, 206]

Guthrie thus equipped a generation of school physics teachers with laboratory apparatus and expertise to give practical demonstrations of experimental physics to an unprecedentedly wide audience of pupils. For example, the senior science master at the Manchester Grammar School, John Angell, who had evidently undergone the training course at South Kensington in the early 1870's, incorporated the system and content of Guthrie and Barrett's course into his 1879 conspicuously non-mathematical textbook: Elements of Magnetism and Electricity, with practical instructions for the performance of experiments, and the construction of cheap apparatus [Angell, 1879]. Guthrie thus claimed in 1886 that widespread emulation of his laboratory methods in response to the economic demand for trained science teachers had meant that practical teaching methods similar to his own "were growing into use in many places" [Guthrie, 1886c, 663].

Having discussed the innovation and proliferation of Guthrie's practical teaching it is now approriate to consider the controversies that underlay the foundation of the metropolitan institution which linked Guthrie's teaching and research at the RSM to the wider community of laboratory-based physics teachers: the Physical Society. By assessing critical reaction to his textbooks and to the papers he submitted for publication to the Royal Society, we will see that Guthrie founded the Physical Society in order to create a public institution more receptive to his unorthodox practices and views than extant scientific establishments.

5): Heterodoxy and publication 1869-1876

Although Guthrie held the institutional status of a physicist from the time of his appointment to the RSM in January 1869, his textbooks and research covered a range of chemical subjects. Significantly Guthrie's obituary in the South Kensington *Science School's Journal*, remarked both that he "became especially active after his appointment" to the RSM and that "almost all his original scientific work has tended more or less directly to the solution of problems lying in that interesting region of physical chemistry, where so much still awaits satisfactory explanation" [*Science School's Journal*, 1, 3]. As G.C. Foster explained of Guthrie's interdisciplinary work: "the side from which he approached the study that the subjects that occupied him principally had relation to what is usually called in the text-books 'molecular physics'" [Foster, 1886, 9].

However, Guthrie's use of idiosyncratic terminology and avoidance of mathematical theory, which had evolved during his period of isolation in Mauritius, resulted in these books and papers receiving harsh criticism from some contemporaries. As we saw above, James Clerk Maxwell considered that Guthrie showed considerable "ignorance" over his use of measurements in the thermal conductivity of liquids, and one particularly uncharitable reviewer in *The Student and Intellectual Observer* dismissed Guthrie's 1869 manual entitled *Elements of Heat and of Non-Metallic Chemistry* on similar grounds [Guthrie, 1869b]. Citing numerous flaws in the content and style of the book the reviewer completely demolished Guthrie's credibility as a pedagogue by asserting that "Dr. Guthrie has undertaken the hopeless task of teaching modern chemistry without first learning it himself" [*Student and Intellectual Observer*, 2, 478-79].

Not all critics were quite so damning, however. For example, although *Nature* found an equal number of infelicities in Guthrie's 1876 textbook *Magnetism* and Electricity, this journal expressed its characteristic solidarity for laboratory physicists by giving the work a qualified recommendation: "it has undoubtedly a freshness and originality of treatment which, although apt to shock electricians in parts, yet places this book in striking contrast to some science class-books of mushroom growth". Guthrie's terminology was noted in particular for its "originality" e.g. christening "a source of voltaic electricity" as an "electrogen", and a sympathetic hearing was given to material in the book that drew upon his controversial research of 1873 into the peculiarities of electric discharges from heated conductors (see below) [Nature,13,261].

Nonetheless, the reviewer expressed disapprobation of Guthrie's divergence from the conventional use, recently established by the B.A.A.S. Conmmitee on Electrical Standards, of the "C.G.S." system of electrical units and argued that Guthrie's exposition of Ohm's law "will certainly bewilder the reader unnecessarily", altogether dismissing Guthrie's analogy with the resistance of water in pipes as being simply "erroneous" [Nature,13,264]. To explore the background to this less than universal respect that Guthrie received for such works, we should now consider some responses to his academic research and in particular the somewhat strained relations with G.G. Stokes who, as Secretary of the Royal Society, dealt with the papers that Guthrie submitted for publication in the Society's journals: the Proceedings for papers that were read at Burlington House, and the Philosphical Transactions for papers of selected merit and originality.

Earlier we noted that the first of Guthrie's physical papers to be published in the Royal Society's Proceedings, viz. "on the thermal resistance of liquids", met with some delay in the process of refereeing and final publication for the Philosophical Transactions. Although this paper was ultimately accepted in the lattermost journal, we can surmise from the year long delay between submission and publication, and also from Maxwell's informed views on the "incoherence" of the paper, that in its original form this paper did not meet the standards maintained by the Royal Society for its Transactions [Guthrie,1869a2]. This was clearly the case with two of subsequent papers which were rejected outright from publication in the Transactions, and the first of these originated in the period immediately succeeding Guthrie's appointment at the RSM in 1869.

For this paper Guthrie carried out a study of a phenomenon he called "Approach Caused by Vibration" in which he investigated the apparent attraction exerted by a vibrating tuning fork upon a piece of cardboard suspended near it. Guthrie made a series of experiments "having for their object the determination of the cause and conditions of the fundamental observed fact" and demonstrated that the cause of this attraction was neither the expansion of air-currents generated by the fork, nor was it a result of "Mr Faraday's air current", Guthrie determining that the phenomenon took place beyond the range of the latter. However after a discussion of somewhat miscellaneous experiments he concluded speculatively that "the effect of apparent attraction is due to atmospheric pressure, and that this pressure [sic] is due to undulatory dispersion."[10] This study he submitted to the Royal Society on August 26 1869 and after Stokes agreed to communicate the paper, it was read during the summer recess [Guthrie,1870,93-94].

Whilst Guthrie's friend and counterpart at UCL, G.C. Foster, retrospectively described this as being a "remarkable discovery"

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[10] In relation to Crooke's experiments on the radiometer, he also speculated that "the dispersion of the vibrations which constitute radiant heat may cause bodies to approach being pushed not pulled."

[Foster,1887,12], it is very interesting to note that Foster was somewhat critical of the non-theoretical and non-mathematical way that Guthrie presented his paper (cf. obituary cited above [Foster,1886,9]). Thus after being asked by Stokes to referee "On Approach Caused by Vibration" for possible submission to the Philosophical Transactions, Foster wrote back on 21st February 1870 to recommend that "On approach caused by vibration" should not be accepted for publication in the Royal Society's most prestigious journal. Foster argued that his speculations on "undulatory dispersion" offered "no clear explanation of the phenomena under examination," and neither did Guthrie make "any attempt to determine the magnitude of the force exerted, nor to ascertain the conditions which cause it to increase or diminish." [Foster to Stokes,21/02/1870, ULC ADD 7656 F252]. Guthrie's characteristic aversion to both theoretical discussion and mathematical treatment of his results were thus most evidently at variance with the protocol of the Philosophical Transactions.

As a consolation however Foster suggested phenomenon was "sufficiently curious" for an abstracted version of the paper to be published in the Proceedings and indeed Stokes followed this recommendation in early 1870 [Guthrie,1870]. Despite this rebuff, Guthrie nonetheless managed to have the full version of his paper published in the Philosophical Magazine, a journal which Guthrie was increasingly using as the publishing vehicle for his papers on physics: "A speculation concerning the relation between the axial rotation of the Earth, and the resistance, elasticity and weight of solar aether" [Guthrie,1866]; "On the thermal resistance of liquids" [Guthrie,1869]; "Note of experiments upon the conduction of heat by liquids" [Guthrie,1868a]; "On a new form of voltameter and voltastat" [Guthrie,1868b] and "Description of a new thermostat" [Guthrie,1868c] were all published primarily or exclusively in the Philosophical Magazine. These latter two indicate Guthrie's nascent research interest in the development of experimental apparatus - a subject specifically outside the scope of the Royal Society's publication, but of interest to a more experimentally oriented journal such as the Philosophical Magazine, as we shall see shortly.

Nonetheless, Guthrie's paper on "Approach Caused by Vibration" was certainly of considerable import to at least one member of the Royal Society Establishment: Sir William Thomson. Thomson read Guthrie's abstract shortly after it was published in autumn 1870 with "great interest" and immediately wrote to Guthrie to inform him that:

> The experiments you describe constitute very beautiful illustrations of the established theorem for fluid pressure in abstract hydrokinetics, with which I have been much occupied in mathematical investigations connected with vortex motion...No branch of abstract dynamics has had a greater charm for the mathematical worker than hydrokinetics; but it has not hitherto been made generally attractive by experimental illustrations. Such refined and beautiful experiments as those you describe...tend noteably to give this branch of dynamics quite a different place in popular estimation from that which it [hitherto] has held; but what is perhaps of even more importance, they help greatly to clear the ideas of those who have made it a subject of mathematical study.
>
> [Thomson to Guthrie,14/11/1870 in Proc.Roy.Soc.,41,423-25]

In Thomson's excitement at this laboratory demonstration of hydrokinetic vortices - one that had such significance for his theory of the atom [Smith and Wise,1989,ch.12] - he indulged Guthrie in a personalized *qualitative* explanation of how his hydrokinetic theorem accounted for the observed attraction, showing much more sympathy for Guthrie's non-mathematical leanings than either Foster or Stokes. Indeed such was Thomson's enthusiasm that he wrote three further letters to Guthrie on the subject during the following ten days and upon receipt of the first letter Guthrie immediately responded to Thomson's revelations by writing to Stokes to request publication of the letter in the <u>Proceedings</u> [Guthrie to Stokes, 17/11/1870, R.S.MC.8.255]. In the event all Thomson's letters were published in 1871 along with related papers by other authors in a special collection compiled by Guthrie[11] for the Royal Society [<u>Proc. Roy.Soc</u>,<u>41</u>,420-429]. To this incident we can perhaps trace the warmer relations that subsequently existed between Thomson and Guthrie than between the Cambridge professors and the author of "On Approach Caused by Vibration" - see below.

A few months after this flurry of interest in his work on "Approach Caused by Vibration", Guthrie was informed by Stokes on June 9th 1871 that he had been elected a Fellow of the Royal Society [Guthrie to Stokes 15/6/1871, R.S. MC.9.215], and from the temporal proximity of this election to this widely discussed experiment we can surmise that his Fellowship was bestowed specifically for his work on this subject. Nevertheless, although Guthrie thereby acquired recognition for his experimental work, he again encountered substantial criticism from Royal Society referees for the results of his subsequent investigation which he personally considered to be of great significance for electrical theory.

Early in 1873 he submitted a paper to the Royal Society entitled "A new relation between heat and electricity" in which he claimed to have discovered a special asymmetry in the behaviour of heated metals towards electric charges. In cooling from white heat to red heat he found a temperature range in which the behaviour of an iron ball depended *qualitatively* upon the sign of its electrification e.g. it discharged only if positivly electricfied, and not if negatively so. In refereeing this paper for the Royal Society however, Fleeming Jenkin and J.C. Maxwell

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[11] Guthrie's ubiquitous footnotes throughout this collection suggest that he took a major editorial role in this compilation.

took exception to Guthrie's conclusions that this experiment demanded a revision in the theory of electrical capacitance, Jenkin in particular finding little to say in favour of Guthrie's paper:

> I consider it unsuitable for publication [since it] contains numerous experiments all of which are clearly explicable on well-established principles. Perhaps the experiments on the difference between the discharge of positive and negative electricity..[and his]..analogous methods have a little interest but Professor Guthrie's reasoning and explanations are quite without value and I think a judicious friend should give him a hint to withdraw the paper or only retain so much of it as is purely experimental - a short abstract of this is all that should be published. There is nothing that I see more novel in the paper than phenomena as well known as the discharge of a conductor and a point held opposite it....
>
> [Jenkin to Stokes,27/5/1873, R.S. R.R.7.244]

Maxwell was almost as dismissive of Guthrie's conclusions as Jenkin had been although he was more sympathetic in assessing Guthrie's research as a "number of interesting experiments" which formed the basis of what could be a valuable paper if his investigations were continued: "It is to be hoped that the author intends more fully to work out the subject, as he has now acquired sufficient data to direct him in choosing the proper points for investigation." However, as a theoretician himself, Maxwell did not consider Guthrie's paper possessed much cognitive coherence: "the theoretical position from which the author starts and that to which he thinks his results lead him are nowhere very clearly stated..." Indeed Maxwell criticised Guthrie's position wherever his presuppositions were made explicit, attacking in particular Guthrie's attribution of "co-ercive" electrical force to objects rather than dielectric media and also his anachronistic language about different kinds of electricity which were "intelligible only on the crudest form of two fluid theory being a physical fact".

Nevertheless Maxwell was sufficiently charitable to argue overall that the essential flaw of the paper lay only in its claim to have found a *new* relation between heat and electricity:

> ...I do not agree with the title of this paper as a description of its contents. The experiments relate to the effects of heat upon air as altering its electrical properties...I cannot therefore recommend the present paperto be published in the Transactions, though I do think that the research, if successfully carried out might furnish matter for a very valuable communication.
>
> [Maxwell to Stokes, undated(1873), R.S. R.R.7.245]

Following the recommendations of Jenkin and Maxwell, Stokes again relegated this paper to publication as a brief abstract in the Proceedings, following the fate of "Approach Caused by Vibration" both in this respect and also in following receiving full attention in the pages of the Philosophical Magazine [Guthrie,1871a;1871b].

From the priviliged stance of the late twentieth century, we can see that Guthrie's conclusions were particularly ill-received, conspicuously so by Jenkin, because experimentalists did not believe that any new discoveries of the sort claimed by Guthrie still remained to be made [Ch.1]. A.J. Fleming, who was a student of Guthrie's in 1873, remarked in 1924 that "Guthrie's observations were not capable of interpretation at the time he made them...the effect of the loss of negative electricity from incandescent carbon in vacuo...was not... explained until the researches of Sir J.J. Thomson had made us acquainted with the electron and electron emission from incandescent bodies" [Physical Society,1924,20]. Only in the context of the "new physics" of the late 1890's, when the popular view of "closure" in physics was in decline, could posterity thus interpret Guthrie's 1873 experiments as the "discovery" of thermionic emission, and thereby vindicate his claim to have found "a new relation between heat and electricity".

Guthrie clearly was not a party to the predominant view of closure in physics which had taken root whilst he was absent from Europe in the 1860's: with the benefit of hindsight Fleming argued that Guthrie had "very considerable originality and ability in opening up new subjects for research and many of his observations and experiments proved to be the starting point for much new knowledge" [Physical Society,1924,17]. However, without the orthodox expertise in mathematical theory required by the protocol of the Transactions, Guthrie could not receive formal recognition for his innovations from the Royal Society and indeed the experimental abstract of this 1873 paper "on a new relation" was the last item that he ever submitted to the Royal Society for publication. Since this establishment of Restoration science was the sole extant vehicle for the communciation of papers on physics, Guthrie set about creating an institution to further what he considered to be the interests of experimental physicists: the Physical Society.

6): The Physical Society: experimental teachers *vs.* Cambridge analysts

In the summer of 1873, immediately after "A New Relation..." had been rejected by Stokes from the Philosophical Transactions, Guthrie drew up plans for a new specialist society that would specially cater for the kind of teaching and research which he ardently practiced in the physical laboratory at the Royal School of Mines but which commanded little credence amongst the Royal Society establishment[12]. As a student in his

[12] Russell Moseley's suggestion that the Physical Society was formed by Guthrie in sympathy with a B.A.A.S. Report in 1874 which promoted practical science teaching in schools is therefore not only chronologically inept but also misses an important feature of Guthrie's biographical context, and certainly fails give any account of why the Society's origins lay in the summer of 1873 [Moseley,1977,426].

laboratory at the time, the testimony of A.J. Fleming on Guthrie's motives for founding the Society is of particular note:

> [Guthrie] thought that outside of complete or carefully worked out investigations in physical problems which might hope to claim a place in the Proceedings or the Transactions of the Royal Society,...there might be a field for a Society which should encourage and publish accounts of physical researches of a less ambitious kind, and not refuse to accept descriptions or exhibitions of new experiments even if imperfectly explained or understood, provided they had interest and novelty for physicists.
>
> [Physical Society, 1924, 17]

This interpretation of Guthrie's motives is supported by the primary evidence available from the Society's first report in 1875: "the Royal Society readily receives important physical papers; but it is difficult to exhibit experiments or discuss them in detail, at Burlington House, and minor or unfinished papers are obviously unsuited for communication to this, the chief of the learned Societies" [Proc.Phys.Soc.,1,5].

George Carey Foster, who was Guthrie's major partner in founding the Society [Lodge,1919,a320], later described in precise detail the specialized activities of experimental teachers and researchers in physics that the RSM Professor sought to represent in this new society:

> ...by virtue of his official position he was one of those upon whom the duty of promoting the progress of physics in London chiefly lay; and in his opinion a Society such as he proposed...was likely to be one the most effectual means of attaining this end. So far the only Society in London that concerned itself with physics was the Royal Society; and Guthrie thought that without in any way interfering with the field of action of the Royal Society; there was ample room for a *society that took cognizance of smaller matters, points of technical detail, useful laboratory contrivances, experimental methods of illustrating physical principles, questions connected with methods of teaching,* and other things of much import to the advance of our science which would nevertheless be out of place before the Royal Society. [Of such things deemed inappropriate by the Royal Society] Guthrie felt also that in a Society devoted specially to Physics, *the actual exhibition of physical phenomena to the members might usefully be made an important part of the proceedings.*
>
> [Foster,1887,a9-10]

This was clearly a manifesto for a society of laboratory teachers, a society whose source of cognitive authority for the public dissemination of scientific knowledge lay in laboratory demonstration rather that the *ex cathedra* pronouncements of Burlington House. Indeed Guthrie himself acted as experimental demonstrator in the Society from 1874 to 1884 to ensure that all papers read were accompanied by an *experiment* illustrative of point under discussion, the Society meetings taking place very significantly in Guthrie's own lab■oratory at the RSM [Physical Society, 1924,18-19 & 22].

When Guthrie's South Kensington associate and co-founder W.F. Barrett went to canvass support for the proposed Society on these terms at the Bradford meeting of the B.A.A.S. in the summer of 1873, he received the most favourable reponses from laboratory practitioners and teachers in Sections A and B. Enquiring particularly among those associated with Burlington House he sent a note to Guthrie shortly afterwards declaring that:

> The following Fellows of the Royal Society will support a Physical Society: Professor Balfour Stewart, Dr Frankland, Dr [J.H.]Gladstone, Mr Merrifield, Professor Williamson, Dr W.L. Miller, Dr Tyndall, Mr Crookes, Professor Huxley, Mr Glaisher, Professor Carey Foster, Colonel Strange, and Dr.Hirst.
>
> To these names must be added the following fellows of the Chemical Society: Herbert McLeod, Guthrie, A.Fletcher and myself [Barrett].
>
> [Physical Society,1924,15]

Shortly afterwards Guthrie drew up a list of those whom he would approach to support the Society: Wheatstone, Graham, Miller, Stokes, Adams, Atkinson, Barrett, Dupre, Goodeve, Grove, Hirst, Herschel, Huggins, Lockyer, Mattheissen, Tomlinson and those whom he would probably ask: Andrews, Tait, Thomson, Joule, Clerk Maxwell, Fleming, Jenkin, Clifton, Roscoe, Kelland, Odling, Williamson and Woodward [Physical Society,1924, 15]. Foster reports that these were men whom Guthrie thought "from their known tastes or official position" were "likely to take an interest in the scheme" and in the late summer of 1873 sent out the following circular to all these men and other metropolitan practitioners of experimental science:

> <u>PHYSICAL SOCIETY</u>
>
> I wish to try to form a Society for Physical Research: for showing new physical facts and new means for showing old ones: for making better known new home and foreign physical discoveries, and for the better knowledge of one another of those given to physical work. You who care for the being of such a Society, and are willing to help in its making, are hereby asked to write to me to that purpose before the first of October next. Whereupon you will be asked to meet so as to talk over the means.
>
> Cited in [Foster,1887,9]

After strategic political moves to get the eminent and wealthy amateur Dr John Hall Gladstone, to be President of the new Society, a meeting to discuss the formation of the Society was organized by Guthrie, Foster and Adams on November 29th 1873 in Guthrie's own Physical Laboratory at South Kensington. As <u>Nature</u> reported of this meeting:

> The chair was taken by Dr. J.H. Gladstone F.R.S. Thirty-six gentlemen were present, including most of the physicists of London. It was resolved that the following gentlemen be requested to serve as an organising committee:- W.G. Adams, [Dr] E. Atkinson, W.Crookes, A.Dupre, G.C. Foster, [Dr]J.H. Gladstone, T.M. Goodeve,[Dr] F.Guthrie, O.Henrici, B.Loewy, Dr. Mills, A.W. Reinold, and H. Sprengel.[13] A letter was read from the Lords of the Committee of Council on Education, granting the use of the Physical Laboratory and apparatus at the Science Schools, South Kensington, for the purposes of the Society.
>
> [<u>Nature</u>,<u>9</u>,113]

This constituted a considerable display of solidarity amongst the

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[13] Goodeve was a colleague of Guthrie's, occupying the Chair of Mechanics at the RSM; Henrici was a colleague of Foster's, occupying the Chair of Mathematics at University College London; Reinold was a former student of Clifton's who was now Professor of Physics at the Royal Naval College in Greenwich.

metropolitan community of experimental physicists and their academic and amateur allies, and at this stage the predominance of academics is apparent. As S.P. Thompson, another of Guthrie's laboratory students at this time, recalled in his Presidential Address to the Society in 1901: "our Society was originated by teachers of physics...from its inception the Society has been actively supported by teachers of Physics in the Schools and Colleges of London, as well as by the Professors of Physics in the Universities and University Colleges of the United Kingdom, and by the Lecturers in Physics of the great Public Schools" [Thompson,H.G. & J.S., 1920,20]. Thus the foundation and evolution of the Physical Society was grounded in the ascendancy of experimental physics teaching in the 1860's to 1870's ubiquitous in this thesis.

Nevertheless, there were several interests actively opposed to the formation of the Physical Society, most notably the Royal Society establishment itself. References to this opposition are indirect but unambiguous. For example, the chemist H.E. Armstrong remarked sympathetically to the Society's Jubilee meeting in 1924: "in early days, you were subject to Royal Society influences, which sought to keep you in the nursery" [Physical Society,1924,31]. As we can see from the first report of the Physical Society delivered by J.H. Gladstone in 1875, the organizing committee were extremely conscious of this delicate issue of diplomacy, since Gladstone publicly spoke with extreme deference to the traditional pre-eminence of the Royal Society in matters of scientific prestige [see above] [Proc.Phys.Soc.,1,5].

However, apart from the jealous politics of institutional science endured by Guthrie *et al.* in the early years, there were also individuals prominent within the Royal Society who opposed the ethos of this new body of experimentalists; as Fleming remarked in his autobiography: "some eminent philosophers doubted whether a fresh learned Society was required" although at the same time "many younger men welcomed the idea" [Fleming, 1934,29]. From the list of men approached by Guthrie in the summer of 1873 we can note that the Cambridge Professors Stokes and Maxwell never joined the Society. Guthrie approached Stokes again in 1885 but with no apparent response [Guthrie to Stokes 12/8/1885, ULC ADD 7656 G834]. We do know however the condescending reply that Adams, as a leading member of the co-ordinating committee, received from Maxwell in December 1873:

> I got Professor Guthrie's circular some time ago. I do not approve of the plan of a Physical Society as an instrument for the improvement of natural knowledge. If it is to publish Papers on physical subjects which would not find their place in the transactions of existing societies or in scientific journals, I think its progress towards dissolution will be very rapid. But if there is sufficient liveliness and leisure among persons interested in experiments to maintain a series of stated meetings to show experiments and talk about them, as the Ray Club do, then I wish them all joy...
>
> [Maxwell to Adams 12/1873, cited in Physical Society,1924,17]

From his experience of refereeing papers by Guthrie which "did not find their place in the transactions" of the Royal Society, Maxwell clearly regarded involvement with a society nurturing incomplete and unexplained experimental investigations as beneath his academic dignity. This rejection of Guthrie's ideology, grounded as it was in laboratory pedagogy and non-theoretical investigation, is all the more pointed in view of the fact that Maxwell was himself shortly due to become an academic laboratory physicist upon the opening of the Cavendish in the following year.

In interpreting Maxwell's disapprobation of the Physical Society's essential activities, Moseley has argued that the pedagogical and practical issues at the heart of the Society's operation were "unlikely to hold much attraction for the more senior academic physicist", thus explaining what he considered to be the "relative scarcity of eminent fellows" among the Society's early membership. And although William Thomson was an early member from 1875, Moseley asserts that he "communicated no papers to, and took no part in, the Society's active affairs" [Moseley,1977,427]. Moseley's evidence for this claim is untenable, however, for in both 1880 and 1881 Thomson acted as President of the Society and chaired a meeting on May 8th 1880 in which he communicated three papers: "On the Elimination of Air from Water", "On Steam Pressure Thermometers" and "On the Radiation of Water-Steam Pressure Thermometers"; in 1881 Thomson also accompanied W.G. Adams and G.C. Foster as the Society's delegates to the Electrical Exhibition in Paris [Proc.Phys.Soc.,3,4 & 4,6].

More questionable still is Moseley's claim that the Society was populated by "second-order" experimentalists who engaged in merely "elementary" activities [Moseley,1977,426-27]. In citing the 1898 Presidential Address of Shelford Bidwell with reference to the "lax regime" in early meetings that allowed the reading of papers that were "sometimes blemished by serious flaws", Moseley disingenuously omits Bidwell's comment that "the *demolition* of the authors added much to the interest and liveliness of the discussions" [Proc.Phys.Soc.,16,12]. Although eminent theoreticians were indeed absent from the Society's membership roll, Moseley's lack of contextual empathy leads him to overlook the fact that the most eminent experimentalists of the period all were deeply involved in the debates and administration of the Society. The table below shows how all the most eminent academic laboratory practitioners discussed in this thesis, with the exception of the Edinburgh-bound Tait, were Presidents of the Physical Society within Guthrie's lifetime:

Session	President
1874-75	J.H.Gladstone
1875-76	J.H.Gladstone
1876-77	G.C.Foster
1877-78	G.C.Foster
1878-79	W.G.Adams
1879-80	W.G.Adams
1880-81	W. Thomson
1881-82	W. Thomson
1882-83	R.B.Clifton
1883-84	R.B.Clifton
1884-85	F. Guthrie
1885-86	F. Guthrie
1886-87	B. Stewart

For an idea of the range and calibre of experimentalists which the Society attracted, a survey of its Proceedings up till 1880 shows that after its foundation in 1874 with Guthrie, Adams, Foster, Tyndall, Barrett, Gladstone, Cromwell Varley among its members, the Physical Society soon attracted professorial physicists, chemists, mathematicians as well as professional telegraphists to its ranks with much greater facility than might have been inferred from Moseley's caricature of the Society's mediocre membership:

Practitioner	Profession	Date of membership
Latimer Clark	telegrapher	21/03/1874
William Garnett	physicist(Cavendish)	18/04/1874
H.E. Armstrong	chemist (London)	18/04/1874
R.E.Baynes	physicist(Clarendon)	18/04/1874
Arthur Rucker	physicist(CLarendon)	18/04/1874
Balfour Stewart	physicist(Manchester)	02/05/1874
William Siemens	telegrapher	20/06/1874
J.D.Everett	physicist(Belfast)	13/03/1875
Alexander Kennedy	engineer (UCL)	13/03/1875
William Odling	chemist (Oxford)	10/04/1875
William Thomson	physicist(Glasgow)	12/06/1875
J.J.Sylvester	mathematician(UCL)	12/06/1875
R.B.Clifton	physicist(Clarendon)	27/11/1875
H.J.S.Smith	mathematician(Oxford)	27/11/1875

S.P.Thompson	physicist(RSM)	11/12/1875
Norman Lockyer	editor of Nature	03/02/1877
J.A.Fleming	physicist(London)	03/03/1877
Edward Frankland	chemist(RSM)	12/05/1877
William Jack	mathematician(Glasgow)	26/05/1877
Lord Rayleigh	physicist(Terling)	15/12/1877
W.E. Ayrton	physicist(London)	15/12/1877
W.M. Hicks	physicist(Sheffield)	16/10/1878
John Couch Adams	astronomer(Cambridge)	22/02/1879
George Liveing	chemist (Cambridge)	22/02/1879
Coutts Trotter	physicist(Cambridge)	22/02/1879
T.Archer Hirst	mathematician(UCL)	08/03/1879
R.T.Glazebrook	physicist(Cavendish)	13/12/1879
J.H.Poynting	physicist(Cavendish)	13/12/1879

[Proc.Phys.Soc.,1-5]

To conclude this chapter, we can make the poignant observation that two researchers at the Cavendish Laboratory in Cambridge joined the Physical Society a discreet five weeks after Maxwell's death and the day after Lord Rayleigh was appointed as Maxwell's successor to the Cambridge Professoriship of Experimental Physics on December 12th 1879 [Strutt,1924, 102].

Conclusion

Guthrie's career epitomized that of the sub-generation of laboratory physicists who changed their disciplinary allegiance from experimental chemistry during the 1860's. After a typical training in quantitative analysis under Williamson at UCL, and then working in chemistry laboratories in both Manchester and Edinburgh, Guthrie's work took a somewhat singular turn upon his appointment as a physicist and chemist to the remote island of Mauritius. In this isolated environment his penchant for unusual subjects of investigation and non-mathematical analysis became major idiosyncracies in his practice of physics.

Thus when he returned to England and was eventually appointed to the Chair of Physics at the embattled Royal School of Mines, Guthrie received negative reactions to both his research and laboratory teaching methods from the Royal Society establishment and conservative educationalists. Nevertheless, the level of support won by Guthrie for both his innovative course of laboratory training in apparatus construction and precision measurement for trainee teachers, and for his idiomatic laboratory research practices was considerable. His recognition by fellow experimentalists was sufficient, in fact, for him to establish the Physical Society, housed in his own pedagogical environment of the RSM physics laboratory in South Kensington during the mid-1870's as an alternative forum to the non-experimental meetings of the Royal Society.

The Physical Society rapidly developed into a metropolitan centre where university, college and school laboratory physics teachers debated and demonstrated issues germane to pedagogical experimental physics in the 1870's. However, its partisan membership reflected a tension between the experimental and mathematical traditions in the British community of physicists that was epitomized in the mutual disrespect of Frederick

Guthrie and James Clerk Maxwell. The following exchange between the two men in the pages of Nature will serve to illustrate the state of their relations in the year of Maxwell's death.

Maxwell wrote a particularly vicious review of Guthrie's 1878 volume Practical Physics, Molecular Physics, and Sound [Guthrie,1878] in which, after a sustained attack upon Guthrie's idiosyncratic arguments and terminology, he concluded with the barbed comment: "we have come to regard it as a decided merit that in this book on Molecular Physics we are not told anything about molecules" [Maxwell,1879,312]. A poem in Nature replying specifically to Maxwell's review in Broad Scotch dialect was very probably written by Guthrie himself for the RSM Professor was, like Maxwell, of Scottish descent. More tellingly still, it is signed "$d(m/n)^{1/2}$" with reference to a formula in Practical Physics that met with Maxwell's express disapproval, parodying at the same time Maxwell's own penchant for algebraic pen-names[14]. In the last two stanzas of this poem we see Guthrie's derision at Maxwell's *indolent management of the* Cavendish Laboratory:

[14] Maxwell = dp/dt, Thomson = T, Tait = T', and Tyndall = T''.

REMONSTRANCE TO A RESPECTED DADDIE ANNENT HIS LOSS OF TEMPER

Worry, through duties Academic
It might ha'e been
That made ye write your last polemic
Sae unco keen:

Or intellectual indigestion
O' mental meat,
Striving in vain to solve some question
Fro' "Maxwell's Heat".

Mayhap that mighty brain, in gliding
Fro' space tae space,
Met wi' anither, an collidin',
Not face tae face.

But rather crookedly, in fallin'
Wi' gentle list,
Gat what there is nae help fro' callin'
An ugly twist.

If 'twas your "demon" led ye blindly,
Ye should na thank him,
But gripe by the lug and kindly
But soundly spank him.

Sae stern but patronising daddie!
Don't ta'e't amiss,
If a puir castigated laddie
Observes just this:-

Ye've gat a brand new Lab'ratory
Wi'a'the gears,
Fro' which, the world is unco sorry,
'Maist naught appears.

A weel-bred dog, yoursel' must feel,
Should seldom bark.
Just put your fore paws tae the wheel,
An' do some Wark.

[Guthrie,1879,384]

CHAPTER 9

Conclusion: Academic experimental physics before and beyond the Cavendish Laboratory

...the pleasures of studying minute detail are fascinating and grow quickly to a point where accuracy becomes an end in itself instead of a means to an end...

Arthur Schuster: 1908 lecture on The Progress of Physics 1875-1908 [Schuster,1911,12].

From the evidence of the foregoing case studies, we can now fully address the questions raised at the beginning of this thesis:

1): <u>The genesis of measurement research</u>

The academic practice of precision measurement in physical research was first established in Britain by William Thomson and his "corps" of student volunteers in the thermodynamic, electrodynamic and telegraphic researches that he carried out from 1847 onwards in his series of laboratories at the University of Glasgow [Ch.2]. Through his work on the Atlantic Telegraph Cable, the practices of laboratory measurement were endowed with an economic cogency which led to the displacement of traditional "rule-of-thumb" practices in the telegraph industry. The subsequent demands of telegraphists for standardized electrical measurement led a B.A.A.S. committee consisting of such laboratory practitioners as Thomson, Maxwell, Stewart and Foster to develop uniformly accurate apparatus which physically embodied their collective expertise in precise thermodynamic and electrical measurement.

As the physical embodiment of B.A.A.S. expertise, this apparatus effectively communicated the tradition of precision measurement to the teaching laboratories in which it was employed e.g. those of Tait [Ch.3], and Adams [Ch.5]. Into this B.A.A.S. tradition of collective measurement work, the skills of Balfour Stewart as an exact meteorological observer at Kew [Ch.7], and George Carey Foster's chemical expertise in accurate quantitative analysis [Ch.4] were readily assimilated to cultivate a widely-based research directive in precise laboratory physics.

2): <u>Measurement in the progress of physics</u>

The disciplinary impact of measurement-based researches in thermodynamics and telegraphy done by Thomson, Maxwell *et al* was regarded by physicists as quite *revolutionary* in character. The new unification which the law of precisely quantified energy conservation gave to the hitherto disparate corpus of physical theory, and the public success which met the use of Thomson's exacting standards of telegraphic practice jointly gave great credence to the view that precision measurement was the generic vehicle of progress in experimental physics.

Differences of opinion existed as to whether the progress already achieved by measurement physics was so great that it had led to a *state of* imminent "closure" or whether the further application of precision measurement would reveal new areas of research. Among protagonists of the latter view were mathematical practitioners, e.g. Maxwell, who saw measurement as the interface with experimental physics *through which they* could progressively subjugate laboratory work to their own domain of abstract expertise; in addition, those who were attempting to forge new branches of exact physics, as was Balfour Stewart's aim in his controversial work on "scientific meteorology" at Kew, considered that great new physical correlations would be found by the systematic long-term application of precision measurement.

The general laboratory practices of academic physicists such as Foster, Adams and especially Clifton went, however, beyond the ideology of "closure" to the point where *they considered* perpetual improvement of accurate measurement techniques to be an experimental goal desirable as "an end in itself" rather than as a means of achieving the ends either of disciplinary completion in physics or of effecting major new discoveries.

3): The genesis of academic experimental physics

In the wake of such progress, the academic prerogative of teachers of physics was greatly expanded as whole new areas of thermodynamics and electromagnetism entered their disciplinary brief. Institutional acknowledgement of this progress resulted in higher status and a broader role being accorded to academic experimental physics as manifested in the creation of a new chair at Cambridge [1871]; the elevation of lectureships and readerships to professorships e.g. at Oxford in 1860 [Ch.6] and the Royal School of Mines in 1869 [Ch.8] or by the division of extant professorships to share the increased workload: UCL in 1865 [Ch.4] and Owens College, Manchester in 1870 [Ch.7].

Physicists appointed to these new chairs in the 1860's appealed for institutional resources to create teaching laboratories arguing that *experimental and exact* methods of instruction in physics were necessary to *match* the techniques by which recent progress had been achieved. To win financial support for such an innovative institutional venture as a physics teaching laboratory, they pointed to the recent emergence of audiences of telegraphists, science teachers and industrial trainees who were increasingly demanding a practical scientific education.

4): The demand for an education in laboratory measurement

By drawing attention to the twin economic benefits of accurately testing raw materials in cable manufacture, and of using precision equipment in signal transmission, William Thomson successfuly promoted the employment of laboratory measurement techniques in the telegraph industry against the traditional use of non-quantitative "rule-of-thumb" practices. As Thomsonian methods were assimilated into industrial practice, apprentice telegraphists became a major clientele for a laboratory training in electrical measurement.

Advocates of laboratory education followed Thomson in exploiting the undistinguished performance of British manufacturers at the 1867 Paris Exhibition in arguing that educating the industrial population in the skills of *practical* science in order to displace the non-scientific apprenticeship in the practices of "rule-of-thumb" was vital to secure the future prosperity of the nation. The effectiveness of this campaign resulted in engineering students seeking such a practical education for example, at Kings College, London, where they received a training in laboratory measurement from W.G. Adams, which they then applied to their professional practice in workshops throughout the Empire [Ch.5].

The post-1867 demand for scientific education was increasingly met by the expanding system of Department of Science and Art examinations through which prospective school-teachers, for example, generally acquired scientific qualifications. This growing demand was exploited by the Governmental examiners, Huxley, Frankland and Guthrie, who furthered their campaign for widespead practical teaching (the laboratory revolution) by recasting the examinations to require an *experimental* knowledge of sciences from DSA candidates. Within the DSA Guthrie *et al.* initiated a system of centralized practical teacher-training in which Guthrie not only acquired his own teaching laboratory, but also inculcated a generation of state schoolmasters in his specialized methods of experimental teaching. Prospective physics masters at *grammar or public schools similarly* sought their practical training in the laboratories of Foster, Adams or Clifton in response to campaigns by the B.A.A.S. for physics to be taught by experimental methods in courses of liberal education [Chs.4,5,6 & 8].

5): Teaching through measurement

All the academic physicists discussed in chapters 2 to 8 extended the contemporary practices of progressive measurement research into the pedagogical operation of their laboratories. Each endeavoured to achieve the most physically stable and structurally isolated environment to optimize the accuracy and "objectivity" of professorial and student measurement operations, engaging in complex institutional negotiations to achieve this goal [Ch.1]. The actual scheme of undergraduate measurement carried out in each laboratory was however dependent upon the nature of the institutional clientiele and to biographical traits of each professor.

Thomson, Tait and Stewart, all immersed in the "democratic" Scottish tradition of natural philosophy, typically allowed their students quite free reign in the laboratory and readily involved volunteers in the prosecution of professorial researches although Tait, and later also Stewart, *gave students a formal training in measurement techniques before* before allowing them to pursue laboratory investigations [Chs.2,3, & 7].

English physicists in London and Oxford viz. Foster, Adams and Clifton, were much more elitist in their laboratory teaching, generally allowing only final year students to work in the laboratory, and placing great emphasis upon a regimented training in the standard techniques of measurement. These courses extended over a period of an entire academic year, and after completing such a course only the most advanced students were allowed to carry out laboratory research with delicate professorial apparatus. The most conservative of these was Robert Clifton at Oxford, who insisted that laboratory students had a full training in university mathematics; who trained students in measurement techniques to the extremes of precision at which contemporary research was carried out; and who was notorious for jealously nurturing the delicacy of his apparatus but not applying it to any recognisable research activity [Chs.4,5 & 6].

Frederick Guthrie maintained the orthodoxy of pedagogical exact measurement, but beyond this was somewhat idiosyncratic in his execution of laboratory teaching and research. The scheme he employed for training prospective teachers and students at the Royal School of Mines to verify scientific laws with their own unsophisticated self-made apparatus was as controversial as the provocative researches that he unsucessfully submitted to the Royal Society [Ch.8].

Despite differences of local practice, however, all these physicists collectively promoted the *unique* educational value of laboratory precision measurement in terms of the unique "exact" habits of observation and "accurate" modes of thought which were cultivated by the study of experimental physics.

6): <u>The community of laboratory teachers</u>

Such common commitments were cultivated through separate co-operative partnerships which were maintained by correspondence and collaborative researches. Noteable pairings of laboratory practitioners were: Thomson and Tait, Tait and Stewart, Adams and Clifton, Foster and Guthrie. In addition to this network, from late 1873 onwards, the Physical Society of London represented the collective practices and interests of these physicists; by institutionalizing the laboratory experiment as the chief source of cognitive authority in its meetings and by focussing its discussions upon issues concerning laboratory apparatus and pedagogy.

However, although the majority of British experimentalists showed solidarity with this body by joining the Society in the 1870's, the

Cambridge Professor of Experimental Physics James Clerk Maxwell remained conspicuously aloof as an expression of his divergence from the standards and practices of his experimentalist contemporaries. A brief analysis of his divergence will be a suitable conclusion to this thesis since it will serve to throw the above discussion into relief as a characterization of the independent tradition of British academic experimental physics which existed "before and beyond the Cavendish Laboratory".

Epilogue: Maxwell, measurement and the Cavendish Laboratory

When James Clerk Maxwell was offered the newly created Cavendish Chair of Experimental Physics at Cambridge University in early 1871 he did not at first relish the prospect of adopting the mantle of the professorial experimentalist, evidently reluctant to risk a repetition of his problematic experience of teaching undergraduates at Kings College, London in the early 1860's [Ch.5]. In a letter to one Cambridge don in February of that year, he also revealed great reservations about undertaking practical teaching of the sort in which some of his contemporaries had far greater expertise:

> Though I have much interest in the proposed Chair of Experimental Physics, I had no intention of applying for it when I got your letter, and I have none now, unless I come to see that I can do some good by it.
>
> ...I am sorry Sir W. Thomson has declined to stand. He has practical experience in teaching experimental work, and his experimental corps have turned out very good work. I have no experience of this kind, and I have very little of the somewhat similar arrangements of a class of real practical chemistry...

[Maxwell to Blore, 15/2/1871, in Campbell and Garnett,1882,350]

Nevertheless, the offer made in October of the previous year by the Duke of Devonshire, 2nd Wrangler and First Smith's Prizeman of his year and now Chancellor of the University of Cambridge, had been to endow the construction and maintenance of a laboratory for *research,* Devonshire himself having heard appeals for the "endowment of research" as Chairman of the Royal Commission on Scientific Instruction and the Advancement of Science that summer e.g.[Thomson,1870b][Devonshire to Vice-Chancellor 10/10/1870, cited in FitzPatrick, Whetham *et al.,* 1910,4]. The Chancellor's aristocratic benificence effectively launched the nascent Cavendish above the vicissitudes of the political economy of undergraduate teaching, and thereby relieved Maxwell of his most potentially onerous duty.

With his research prerogative firmly established, Maxwell accepted the professorship and sought architectural models for his projected laboratory in the only two extant purpose-built British models: Thomson's Gilmorehill laboratory [Ch.2] and Clifton's Clarendon Laboratory [Ch.6], reformulating their designs as undergraduate teaching laboratories to accommodate his structural desiderata for original research [Campbell &Garnett, 1882,350-351]. After completion in 1874 graduates of the Mathematical Tripos "gradually drifted in" to take up researches for fellowship dissertations: although undergraduates were not officially disbarred from working in the Cavendish, tutors advised otherwise [Thomson,1936,102-03].

Indeed, some idea of the elitist conception which Maxwell early conceived for the professorial teaching of physics at the Cavendish can be seen in his comments to his wife: "I think there should be a gradation - popular lectures and rough experiments for the masses; real experiments for real students; and laborious [i.e. demanding] experiments for first rate men like [Coutts] Trotter, [Prof. James] Stuart and [Rt.Hon.]Strutt" [Maxwell to Mrs Maxwell, 20/3/1871, Campbell and Garnett,1882,381]. In placing a Professor and two college Fellows at the pinnacle of this hierarchy of participation in experimental physics, the aristocratic Maxwell distanced himself from the "democratic" tradition of Scottish natural philosophy teaching to which his compatriots Thomson, Tait and Stewart unequivocally belonged [Chs.2,3 & 7]. Indeed, such was Maxwell's disengagement from systematic teaching that he initiated no systematic work for the common Tripos student during his tenure although it is clear that there was a real demand for this: in 1880 one of the first acts of Maxwell's successor, Lord Rayleigh (paradoxically more of an aristocrat still), was to initiate courses of practical teaching for undergraduates similar to those documented in chapters 2-8 [FitzPatrick, Whetham *et al*, 1910,34-35,43-49; Strutt,1924,103; Thomson,1936,109].

Maxwell also eschewed the direct student co-operation in professorial researches cultivated by his Scottish colleagues: J.J.Thomson reported that "Maxwell himself did not do any continuous experimental research in Cambridge...the greater part of his time was spent in editing the works of Henry Cavendish, a pious work for a Cavendish Professor" [Thomson, 936,104]. Thus apart from his hourly conversational stroll around the laboratory, the nearest that Maxwell thus came to involving students in his work was his use of them as human galvanometers in repetitions of Henry Cavendish's original experiments upon the physiological measurement of electric currents [Schuster,*1911,29-30*]. And since his only original work in the Cambridge laboratory between 1874 and 1879 was the testing and editing of the Cavendish papers, we can understand the force of Guthrie's poetic criticism in 1879:

Ye've gat a brand new Lab'ratory
Wi'a'the gears,
Fro' which, the world is unco sorry,
'Maist *naught appears.*

A weel-bred dog, yoursel' must feel,
Should seldom bark.
Just put your fore paws tae the wheel,
An' *do some Wark.*
[Guthrie,1879,384]

Wranglers who did attend the Cavendish were, however, allowed free reign in their researches: Thomson reports that Maxwell declared "I never try to dissuade a man from trying a experiment. If he does not find out what he is looking for he might find something else", a view reflecting his emphatic denial of "closure" in the 1871 "Introductory Lecture" [Thomson, 1936,103]. Although he often recommended that his students carry out some remeasurement work with the B.A.A.S. Electrical Standards equipment which, as the physical apotheosis of the British tradition of electrical measurement had been symbolically deposited at the Cavendish, Maxwell distanced himself from the highly regimented training courses of his English colleagues, whose commitment to "measurement for its own sake" he had rejected in his "Inaugural Lecture" of 1871 [Ch.1] [Campbell &Garnett,1882,353; Fleming,1934,62-63]. Nevertheless, Maxwell shared the common commitment of laboratory-based experimentalists to cultivating expertise in exact measurement techniques, as J.J.Thomson reports of the brief training course through which Cavendish researchers were put:

> At first they were set to read scales and verniers, to measure times of vibrations, to use a [Thomson] reflecting galvanometer, to measure the resistance of a wire; after a short time spent in this way, *they were often* set to measure the horizontal component of the earth's magnetic force by a magnetometer of the Kew [i.e. Balfour Stewart's] pattern. It afforded practice not only in reading scales and measuring times but also in making adjustments, and the accuracy with which these had been done was indicated by the value obtained by the magnetic force.
> [Thomson,1936,102-103]

Maxwell's commitment to measurement practices was evident before both the opening of the Cavendish Laboratory in 1874 and his estrangement from the Physical Society during the previous year, as we can observe from his Treatise on Electricity and Magnetism, the work out of which later physicists such as Rayleigh and J.J. Thomson created the distinctive Cavendish tradition of electrical research [Sviedrys,1970;1976]. Contemporaries in the non-mathematical and purely experimentalist tradition of British physics had great difficulty in assimilating this radical work; as Fleming later commented of the Treatise: "its novel ideas and mathematical methods were proving difficult of digestion by those who had been reared on a diet of electrical facts as presented in such books as De la Rive's Large Treatise or Faraday's Experimental Researches" [Physical Society,1924,17]. To bridge the methodological gap between the laboratory based experimentalists, and the smaller coterie of mathematical practitioners to which he belonged, Maxwell wrote a sensitive preface to the Treatise in which he made a definitive appeal to the practice of measurement as the universal medium of physics in the 1870's:

> There are several treatises in which electricity and magnetism are described in a popular way. These however are not *what is wanted by those who have been brought face to face with the quantities to be measured, and whose minds do not rest satisfied with lecture-room experiments.*
>
> There is also a considerable mass of mathematical memoirs which are of great importance in electrical science, but... are for the most part beyond the comprehension of any but professed mathematicians.
>
> I have therefore thought that a treatise would be more useful which should have for its principal object to take up the whole subject in a methodical manner, and which also should *indicate how each part of the subject is to be brought within the reach of methods of verification by actual measurement.*
>
> [Maxwell,1873,viii]

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