Scientific naturalists and the government of the Royal Society 1850-1900

Thesis

Scientific Naturalists and the government of the Royal Society 1850-1900

by

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Abstract

The everyday life of the Royal Society in the second half of the nineteenth century is a largely unworked field within the history of Victorian science. As the principal forum for English science, the Royal Society was a crucial context for the working out of the major changes in science over the period. The Society made its own singular responses to the developing needs of science for funds to support increasingly expensive researches, and for a more efficient means of publication for the growing number of active workers. These aspects are dealt with at length in the first section.

The image of science which was held to by some of its leading practitioners and organisers is very significant in tracing the developing tensions within Victorian science. This led to a widespread sensitivity to any commercial or political involvements on the part of prominent men of science, which might have seemed to compromise their disinterestedness. An area which is very revealing of many characteristic modes of thought entertained by Victorian men of science, is the evaluation of scientific performance. Enshrined in the refereeing procedures of the Royal Society, this process provides many insights into the contemporary meaning of the issues of the day.

For a long period following 1870 the government of the Royal Society was in the hand of the group of scientific naturalists who surrounded Thomas Huxley. Their personal ambitions and energetic support of the cause of scientific naturalism contributed to an extremely vigourous phase of the Royal Society's history. A detailed coverage is provided of the spectacular rise and surprisingly rapid decline of the power and influence of this group in this focal point of Victorian science.

* * *

List of Abbreviations of Names of Sources

ASN	Marie Boas Hall, All Scientists Now, Cambridge University Press, 1984.
LL JDH	Leonard Huxley (ed.), Life and Letters of Sir Joseph Dalton Hooker, John Murray, London, 1918.
LL THH	Leonard Huxley (ed.) Life and Letters of Thomas Henry Huxley, Macmillan, London, 1908.
TRS	Henry Lyons, The Royal Society 1660-1940, Cambridge, 1944.
Phil. Trans. (P.T.)	Philosophical Transactions of the Royal Society of London.
Proc. R.S.	Proceedings of the Royal Society of London.
RFC	The Raven Frankland Collection of the papers of Sir Edward Frankland.
Huxley Papers (ICL)	Collection housed at Imperial College London. Catalogue by Warren Dawson.
Kelvin Papers (CUL)	Cambridge University Library, Add. MS 7342.
Stokes Papers (CUL)	Cambridge University library, Add. MS 7656.

INTRODUCTION

The main objective of the research which forms the basis of this work has been to provide a detailed consideration of the way in which certain leading Fellows of the Royal Society took a central part in its organisation and conduct during the second half of the nineteenth century. Publicists of the Royal Society have commonly assumed that basic divisions of interest, and the personal and institutional tensions to which these inevitably gave rise, were effectively removed by the reform of the Statutes in 1847. Much primary evidence suggests that this was far from being the case. Recent consideration of the social relations of Victorian science has dwealt largely on the operation of informal networks of influence. The present work represents an attempt to investigate the nature and extent of these networks in the context of the central forum of British science. A major difficulty placed in the way of this investigation proved to be similar to that facing many studies of institutional behaviour: the understandable concern for its public image enacted by the institution in question. Frequently the august nature of the Royal Society's perceived position within British scientific life led many of its leading Fellows to realise their interests in its affairs clandestinely whilst maintaining an outward appearance of the severest rectitude. During the second half of the nineteenth century, the interest groups whose conflicts had so profound an influence on the conduct of science in this country were united in their concern to maintain the magisterial dignity of its public image. This meant that the tensions referred to above were not usually resolved in the relatively exposed arena formed by the Society's apartments at Burlington House. As a result of this it is necessary to trace out the actions and intentions of several central figures in other scientific contexts than the Royal Society itself.

The place of Thomas Huxley in Victorian science has often been described elsewhere. The present work attempts to ascertain more about the place of Huxley and his more active supporters within the Royal Society - recently termed the "Upper House" of British science. Running alongside the involvement of Huxley was that of George Stokes. Contemporarily a less newsworthy focus of affairs than Huxley, in the popular sense, Stokes nevertheless wielded great scientific influence. He occupied the office of Secretary of the Royal Society for 31 years. Of the part played by Stokes, much remains to be established.

There is evidence to suggest that the rapid rise and subsequent failure of the enterprise undertaken by the "Huxleyites" involved some individuals and events not hitherto recognised by historians of the period. In recent years a good deal has been written about scientific naturalism, the world picture that Huxley and his followers did so much to promote. The manner which this promotion took place within the Royal Society is examined in detail in the second part of this work. The first part consists of several detailed studies of the institutional setting of the Royal Society. These consider the development of routine procedures bound up in the everday institutional life of the Society at Burlington House.

* * *

CHAPTER ONE

PUBLICATION

The relative isolation of British science following the climax of Newton's dispute with Leibnitz in 1715, which divided British and Continental mathematicians into mutually hostile camps, produced a legacy of unquestioned insularity that dogged the outlook of British instruments of scientific communication far into the next century.[1] An efficient and reliable method of maintaining scientific communication between the European countries began to develop as the newly forming scientific societies of the send half of the seventeenth century began to set up journals to replace personal correspondence.[2] In spite of the aura of heroic pioneering which is commonly lent to these early stirrings, it seems that the most impelling motive of work was an increasingly urgent desire to codify a uniform means of securing personal and national priority for particular scientific performances.

Deliberate secrecy with its attendant anagrams, mystical messages, and sealed packets broke down in the face of what Merton referred to as "motivated public disclosure". There was an increasing awareness of the need for promptness and reliability in the reporting of individual scientific performances.[3] The development of the Royal Society's publication policy throughout the nineteenth century consists of little more than that body's consistent inability to achieve either of those two aims. That it was conventional to expect a useful availability of scientific intelligence in printed form by the end of the seventeenth century is documented by the following observation of John Flamsteed:

> "From this time [1669] I began to have accounts sent me, of all the mathematical books that were published either at home or abroad."[4]

The consequences of the situation alluded to above, which estranged British and Continental mathematicians until the beginning of the second quarter of the nineteenth century, are hard to divine from British accounts of the period. A difficulty arises because in considering the inception and rise of the scientific journal it quickly becomes clear that the English experience within the ambit of the Royal Society has been taken to be naturally prototypical (and thereby self-justifying).[5] In such a way we learn of the "invention" of the scientific paper by Henry Oldenburg who endeavoured to allay Robert Boyle's keen dread of "philosophical robbery" by asking him to send his new book into the Royal Society one section at a time, as they were completed.[6] It is helpful in understanding the problems created by the Royal Society's nineteenth century publication policies, to remember the extent of the insularity engendered by the ructions of years gone by.[7] As will be seen in a later chapter, reverberations of what has been termed "The Great Sulk" were enacted late in the nineteenth century in a rather spectacular fashion.

In 1874 Alphonse de Candolle wrote to the Royal Society lamenting the poverty of scientific communication between France and the British Empire and stressing how much better the situation was between France and North America due to the busy offices of the Smithsonian Institution.[8] As the level of European scientific activity grew quite rapidly towards the end of the noneteenth century so the difficulties of English men of science increased. (Even prior to the burgeoning of scientific activity they seem to have been surprisingly ignorant of continental work.) The traumas of forestalled discoveries and priority disputes which were the natural outcome, seem to have been instrumental in galvanizing the Royal Society to take up Joseph Henry's suggestion of a Catalogue of Scientific Papers. British scientists' difficulties regarding foreigh languages and access to foreign scientific literature were not viewed sympathetically by their continental colleagues. This is perhaps

understandable in the light of the rapid increase in the amount of published scientific information at this time. The definitely non-cosmopolitan outlook of the British was likely to have increasingly troublesome consequences. The oceanologist John Murray wrote in 1923:

> "It has become impossible for any man to keep pace with the progress of any important branch of science. It looks as if the scientific, like other revolutions, meant to devour its own children . . . as if the man of science of the future were condemned to diminish into a narrow specialist as time goes on."9

The shortcomings of the channels of scientific communication from time to time received publicity from the occasional cause célèbre which in part they brought about. Such a case reached the public's attention when the Plumian Professor of Astronomy at Cambridge, James Challis, spoke out in the aftermath of the furore surrounding the disputed discovery of Neptune. He stated that he had been unable to read Le Verrier's papers at the Cambridge Philosophical Society. As well as a lack of access to foreign journals was the tardy and irregular publication of the Royal Society's Philosophical Transactions and its Proceedings which first appeared in 1832. During the first half of the century the unreformed Society maintained a generally lackadaisical and inward-looking disposition justified, to that minority of Fellows who took any interest in science, by a complacent reliance on the power of past glories. William Sharpey, Secretary of the Royal Society, wrote to his fellow Secretary of the Royal Society George Stokes on the 10th of August 1866 to tell him that the next number of the Proceedings would be out in November, while the present one for August "is just breaking the shell."[10] With the pace of scientific life considerably accelerated by the above date, a time lapse of three months for the quickest possible publication of new work by the Royal Society made many authors a prey to profound

misgivings in the matter of securing their priority claims. At this time the convention held that the first rate work submitted to the R.S. was to be the stuff of the Philosophical Transactions while the more routine work would naturally seek a place in the Proceedings. This situation tended to produce further anguish in scienfic authors because in order to claim the heightened prestige of inclusion in the P.T., they had to tolerate a much greater delay and risk the deposition of their papers in the Society's archives by referees who by their active participation in the same area of work were often well-known rivals of the hapless author. The awareness of grave defects in the R.S.'s publication procedures was widespread among active scientific authors throughout the second half of the nineteenth century, and it becomes quite clear that the problems were not overcome despite repeated adjustments of the procedures. In the early 1850's William Allen Miller, who later served as Treasurer to the R.S., suggested to the Philosophical Club the weekly or monthly publication of a Compte Rendu of proceedings from the various scientific societies headquartered in London. Although he was supported by the meeting, Miller's scheme was lost amid inter-societal jealousies and insufficiency of means.[11] The growth of commercially grounded journals which were independent of the scientific societies and published much more frequently was a predictable outcome of their torpid reluctance to effectively adapt to change. A rapidly quickened circulation of scientific information was achieved by the Chemical News (1859), Nature (1869) and the reinvigorated Philosophical Magazine (1798). These frequently published journals quickly assumed one of the main functions of the Philosophical Club itself. The Club went into a gradual decline as the century progressed. In April 1892 Joseph Hooker gave his opinion of its shrinking rôle to Thomas Huxley:

> "The backbone of the Phil. was the grip it had of the R.S. and the thoroughly scientific character of its gatherings. As also the novelty of the communications of scientific interest, all which latter you now get 3 weeks before the meeting in "Nature"! Then too we all knew one and other: all changed now . . .."[12]

The Philosophical Transactions were started by Oldenburg in 1665, partly as an attempt to put his large personal correspondence on a business footing and partly to lend the then limited respectability of the R.S. to the international aspects of his Secretaryship which had lately led his political masters to place him in the tower.[13] By the mid-nineteenth century the chief danger to the editor seems to have been from the chagrin of disgruntled authors of papers delayed for months or years in their publication. The biologist J. G. Buchanan sent a paper to the R.S. from Hong Kong whilst there with the "Challenger" in January 1873. On his return to this country during June of 1874 he was told by Secretary of the Royal Society George Stokes that his paper was still "under consideration".[14] Wheatstone, Grove and Sharpey strove to promote the regular and more frequent appearance of the Society's Proceedings (1832) as the main Royal Society channel through which priority could be properly claimed by the rapid publication of the main points of a discovery. Their scheme came into continuous conflict with the traditional view of the Phil. Trans. as the proper and exclusive preserve of all the most important science presented or communicated to the Society. It seems apparent that the inability of the successive councils to resolve this conflict was the chief cause of the continued complaints about the Society's publications throughout the second half of the century. Writing to Stokes in 1872, at a time when modifications to publication policy were once again under consideration, the Astronomer Royal George Airy reiterated the twenty-year-old notion of making the Proceedings more regular and prompt in the manner of the Académie's Comptes Rendus with the

capacity to present recent scientific news and notice of forthcoming events such as "the great aurora of February 4th." Airy did not approve of Nature but saw the necessity of a weekly publication which was not interrupted by the Society's holidays "and not strictly confined to the presentations of its meetings."[15] In a situation in which the Councils felt constrained to maintain the R.S. monopoly over the distribution of legitimacy and prestige in all British scientific affairs, they encountered their difficulties in trying to strike a balance between the various functions which they perceived the R.S.'s publications as fulfilling. Firstly it was seen as a necessity that Fellows should be able to secure their priority by prompt publication. The second consideration, which proved to be crucial, was the widespread commitment to maintain the Phil. Trans. as a cumulative monument to the glory of British scientific truths. This role turned out to be quite incompatible with the other major prerequisites for an efficient channel of communication in a scientific world which was both growing and quickening its pace between 1850 and 1900. The obvious solution was to develop the Proceedings and leave the Phil. Trans. as the more slowly acting repository of the detail which inevitably had to be left out of preliminary publication in the Proceedings. Finally some weight must be given to what the R.S. frequently saw as the threat from the plethora of specialist scientific societies. It seems possible that the R.S. proved ultimately to lack the resolve to dull the supreme lustre of publication in the Phil. Trans. Influential Fellows felt that the compromise of its pre-eminence would be an inevitable result of any major modernization. The concomitant increase in the importance of the journals of the special scientific societies came to be seen as undesirable by successive R.S. Councils. These vested interests, held by the established custodians of the

R.S.'s scientific and social prestige, tended to make the Society act conservatively. At the meeting of the Philosophical Club on the 23rd February 1854 William Grove, the physicist and lawyer who had taken a large part in the reform of the R.S. in 1847, gave a clear account of the problem. He sought to avoid the contradictions of the well-meant determination to reform the publications issuing so erratically from the Royal Society's rooms in Somerset House. Grove lent his weight to those who wished for the rapid publication of preliminary notices in an improved Proceedings while the detailed substance of important new work would be reserved for the Phil. Trans. It was Grove's further blithe hope that the Phil. Trans. would publish all the crucial new work in all branches of science.[15*] At the next Club meeting on March 23rd the first number of the new form of the Proceedings was exhibited to the members. Despite the intentions of the small group who wielded power within the R.S., the disparate nature of their aims served to prolong the difficulties which dogged the Society's publications. Despite the fact that the Proceedings was supposed to publicise the "rushes" of Philosophical Transactions' papers, those sent in for the former were not usually refereed whilst papers intended by their authors for the latter (signalled by the submission of an accompanying abstract) were invariably refereed, frequently with great rigour. As a consequence of this and the maintenance of other practical and symbolic distinctions of status between the two publications, the intended dovetailing of their functions was to remain a forlorn hope throughout the nineteenth century. On the 25th May 1872, nearly 20 years after the Philosophical Club's "solution", Sharpey addressed a letter to his fellow Secretary of the R.S. George Stokes repeating as if afresh the same basic questions:

> "The Proceedings have undergone so large a degree of development within the last dozen years that it might be well to consider whether there should not be some standing committee to refer to . . . the chief points for reform being:
>
> a) The matter of the communication whether suitable or not.
> b) Whether it should be put with the Proceedings or referred for Transactions.
> c) If for Proceedings the extent to be printed.
> d) The question of illustration figures."16

Five years later the conflict between speed of publication and the maintenance of traditional dignity had gone no further toward resolution evidenced in the words of J. A. Brown to Stokes:

> "The Transactions are reserved for Swans. Some people think all their geese, swans. I am afraid it is a common failing. I think this is a swan, but my desire to present it to the world soon induces me to abandon the more honourable position which its swanship merits!"17

Authors realized that the additional prestige and renown dealt out to those of them who would risk the delays and the possible voracity of partisan referees was a worthy prize. But even to those willing to aim for the Phil. Trans., the full capacity of the R.S. for unhurriedness frequently came as a surprise. In July 1875 the zoologist Ray Lankester wrote to Walter White, Assistant Secretary of the Society, remarking that German practitioners were presently publishing the substance of his paper that had been in the Secretaries' hands for over eighteen months.[18] Lankester's hopes of minimizing the inroads the Germans had made into his priority claims, were based on a rapid distribution of the separate copies of his paper which he implored White to send to him as soon as possible. One hundred free separate copies were due to the author of each paper printed in Phil. Trans. Significantly, the author of a Proceedings' paper received none. The delay between the reading of a paper and its publication by the Académie in Paris in 1776 had been 2-6 years, and even in the context of a much more sedate pace of scientific life this prompted the

establishment of a new journal by J. B. F. Rozier.[19] In this country competition with the long-established, chartered scientific societies did not emerge for another hundred years. When it did occur, the R.S. seems not to have regarded the independent and commercially based scientific weeklies as nearly such a threat as the journals of the specialist scientific societies. As these increased in numbers and the extent of their coverage of various fields, a fear developed within the R.S. that it might be facing imminent redundancy as a general forum for British science. Indeed it was becoming increasingly difficult to unify the divergent interests of its ever more specialised Fellows. It would be wrong to assume that all the special societies were in any sense crouched in readiness to spring at this oppurtunity.

The Linnean Society began the reading of a voluminous memoir by Francis Buchanan in 1821. The readings continued annually until the completion of the paper after the author's death in 1852. On these thirty-one occasions, fourteen saw the presentation of no other paper. The Linnean was not revitalised until 1881, when the informal manoevrings of Sir Joseph Hooker and George Busk achieved the election of John Lubbock as President.[20] The more realistic rivalry of the Royal Astronomical Society with the R.S. will be described in a later part of this work. Despite the examples offered of the moribund Linnean Society and the ambitious R.A.S. (which was progressively frustrated in its rivalry with the Royal Society by the hugely escalating costs of contemporary astronomical work), there were bodies sufficiently moneyed and well set up to offer an effective alternative to the R.S. Fox and Weisz describe such a body in the shape of the Zoological Society of London. Possessing an elegant London club as its West End headquarters, a substantial income from its zoological garden in Regents Park and practical aids to the activities of its

members not available at Burlington House, the Zoological Society was in a position to claim control over the discipline for which it catered. The above authors go on to suggest that specialist scientific societies did not develop to the same extent in France because of the efficiency of "the heavily publicised and exceedingly prompt Comptes rendus, "in contrast with which", the Royal Society's publications were slender, certainly not superior to the Transactions of the Zoological Society or a number of other private publications."[21]

In the event the Royal Society did not lose its predominance in British science and consequently the leading exponents of specialist fields remained beholden to it as the arbitrator of legitimacy and monopolist distributor of scientific fame and position. The increase of the Government Grant to a total of £5,000 in 1876 served further to entrench the R.S.'s pre-eminent position. For a number of specialist societies the production of a journal amounted to little more than the inclination to procure a library by exchanging it with the publications of numerous other societies. The soliciting of such exchanges was by no means confined within national boundaries nor yet to exchanges between bodies of even roughly comparable standing. In February 1862 the R.S. received a request for an exchange of publications from the committee of the Society of the Trannsylvanian Museum. The committee expressed a wish to be able to "contribute somethings on its parts [sic] . . . in the progress of science". This was a wish that the Council of R.S. did not feel justified in gratifying.[22]

At the same time as stating that the R.S. kept its superior standing in British science throughout the period under consideration (1850-1900), it should also be noted that it failed to gain a reputation as a publishing medium amongst European men of science. Between 1862 and 1900 only 51 papers by foreign authors were published by the R.S.; of these 14 were printed in the Phil. Trans. The country

whose scientific men produced the largest single contribution to the overall total proved to be Germany with eighteen, while the French and Italians accounted for five and two respectively.[23] Quite apart from the attractiveness to foreign scientists of their native channels of publicism, the chronic problems of delay and the related confusion of function as between the Phil. Trans. and the Proceedings must have formed a deterrent to overseas authors who might otherwise have favoured the R.S. with their productions. Corresponding with this lack of Continental interest in the R.S. as a means of publication was a profound ignorance of European science on the part of many British practitioners. Their insular attitude naturally produced a lack of linguistic versatility.[24] The result was considerable bitter wrangling over priority. German academics in particular were felt to be almost unreasonably knowledgeable. The German biologist Emil Du Bois Reymond voiced a strong reaction to this attitude in his address to the Berlin Academy in 1878:

> "(According to the British) . . . The German investigator knows all that is going on in science, or at least has someone by him who does. If a German comes on a new idea, he can at once see, or be told, whether another has it or not, and in the latter case he can print the idea, and so secure the priority: the poor Britons on the other hand make the most splendid discoveries in the world without ever guessing that they have struck on anything new - like the Bourgeois Gentilhomme, they speak prose without knowing it - and let the priority slip them. The wily Germans! who instead of contenting themselves like other innocent folks with their mother tongue, sneak into foreign languages to spy out the discoveries that are being made."25

Concerning the movement of scientific information from England to the rest of the Scientific world, we find the situation no more thriving than the picture of the reverse flow, just presented. There seems to have been a widespread feeling on the Continent that significant British work lay concealed in obscure local journals. The taken-

for-granted authority of the R.S. publications which was held to by the majority of the active (mostly London-based) Fellows was a writ which did not run in Continental Europe. One commentator stated in 1893 that R.S. publications were no more accessible in Europe than those of the Royal Society of Edinburgh, and that most foreign students never saw anything other than abstracts of important British papers.[26] The same writer recommended as the only feasible corrective, the lavish distribution of private copies of papers, by the author himself. Some thirty years earlier the biologist Lional Beale declared a similar lack of confidence in the R.S.'s Journals. Requesting a number of copies of his recent paper on nerve fibres to be printed at his own expense, Beale described his own idea of how best to achieve Continental publicity: "one's views are sooner discussed in Germany in works that go through the booksellers than by giving away private copies."[27] Whichever channel was preferred for the distribution of private copies, the fact remains that the British Journals were unable to meet the needs of British men of science throughout the full term covered by the present study. They failed both in the matter of promptness of publication and its accessibility for foreign workers in science. In 1893, Michael Foster, then twelve years into his twenty-two year term as Secretary of the R.S. wrote to a number of British physiologists remarking that they made less use of the Royal Society's Catalogue of Scientific Papers than Virchow's Berichte Hofmann and Schwalbe or the index of the Physiologisches Centralblatt.[28] The well-documented dominance of German scientific publication had considerably increased in the forty years since 1852 when Professors Hofmann, W. A. Miller and Thomas Graham requested funds from the R.S. to continue the then three-year-old custom of translating Liebig and Kopp's "German Annual Report on the Progress of Chemistry".[29] Consistent dependence upon

the distribution of private copies of important papers by prominent British men of science throughout the period 1850-1900 bespeaks an enforced return to the method of personal correspondence which the advent of the journal is generally supposed to have transcended. By the end of the century German interest in the R.S.'s plan to organise the compilation of an International Catalogue of Scientific Papers was understandably low. Publicly it appeared that Burlington House had assumed a position of leadership that German science had no practical need to acknowledge. The International Catalogue scarcely ever managed to keep pace with the rapid growth of scientific literature and in the end was never completed. The sheer scale of scientific publication began to take on a somewhat alarming aspect in the last quarter of the nineteenth century. Internationalism was hailed as the solution of problems of cost and scale, but in several cases it served to promote fresh difficulties. Arthur Schüster stated that if the celebrated "carte du ciel" had ever been completed it would have yielded a pile of paper thirty feet high.[30] Much to the consternation of the Treasury and those of the R.S. Councils lacking biological sympathies a series of fifty volumes was harvested from the voyage of the "Challenger". When the four-year expedition returned in 1876 the initial assault on the accumulated material broke the health of leader Charles Wyville Thomson. Following his death, the more robust (and ambitious) John Murray saw the completion of the report in 1895.

The various reports of specially appointed R.S. committees have been ennumerated elsewhere rendering unnecessary detailed consideration of them.[31] These reports were prompted partly by government requests for scientific information on the basis of unpaid consultancy (eg. Colour Vision Committee Report 1892). Events of general scientific interest sometimes were marked by a new R.S. Committee, such as the

explosion of Krakatoa. A committee occasionally served as a battle-ground for theoretical schism as in the case of the Evolution Committee. The latter ill-starred enterprise broke up in 1901 because of the deep mutal antagonism of the two theoretical schools of thought which constituted its membership. No report was ever submitted. The first and third examples of special R.S. publications will be dealt with in more detail in other chapters. The second in the shape of the Krakatoa Report well illustrates how the ad hoc and frequently unco-ordinated policies of the Society sometimes came to grief. The Krakatoa Committee was appointed in 1883 and produced its report five years later. Containing the novelty of coloured illustrations, the finished product was expensive for the R.S. which issued no free list for its distribution to other scientific societies as had been done in the case of earlier special reports. The officers viewed disconsolately the sluggish sale of the volume which in any case had a German rival - usually a decisive blow to R.S. hopes of pre-eminence in any domain of its activities during the late nineteenth century. Herbert Rix, the Assistant Secretary since the retirement of his eccentric prede-cessor Walter White in 1885, showed great resource in trying to procure a free copy of the rival report out of the hands of its architect Professor Kiessling in order that the R.S. officers could study the opposition.[32] During the eighties the Society became rather short of money which inevitably meant that blunders in the area of publica-tion would be more keenly felt. The production of the zoologist von Lendenfeld's monograph on the horny sponges in 1888 provides such an instance. The author was a nephew of Arthur Cayley the Cambridge Professor of Mathematics. The ensuing charges of plagiarism, innacur-acy and injustice prompted Michael Foster to exclaim in writing to Thomas Huxley: "How I loathe this v. L., it was a black day when we

put the R.S. hand to his beastly plough."[33] Although von Lendenfeld's draft was curtailed the final publication was a failure which nonetheless reached parts of the scientific world when the R.S. began to send copies out to unsuspecting scientific institutions which the Society deemed unworthy of receiving the Phil. Trans. or Proceedings, following applications for the exchange of journals. In 1895 recipients of this telling prize included the Legislative Library of Victoria, British Columbia and the Hull Public Library.[34]

Producing the Royal Societies' Journals

At the end of May 1895 Rix wrote to Michael Foster to explain that the pressure on the R.S.'s publishing resources had not been brought about by an absolute increase in the number of papers submitted to the Society, but rather by an increase in the number considered appropriate for publication. The following figures were included in the letter:

Year	Papers Submitted	Papers Actually Published[35]		
		Phil. Trans.	Proceedings	Total
1884	100	29	20	49
1885	93	37	17	54
1886	113	20	16	36
1887	129	34	17	51
1888	115	38	14	52
1889	123	23	15	38
1890	106	18	41	59
1891	102	23	27	50
1892	117	28	23	51
1893	95	32	17	49
1894	138	42	19	61

Rix might have added that the ever increasing need for promptness in publication was suffering even more frustrations from the new practice of refereeing papers for the Proceedings as well as those for the Phil. Trans. Delays were prolonged even further by the general adoption of illustration as a normal part of scientific papers. Matters

were not made easier when the Phil. Trans. was divided into series A and B in 1894. Arthur Cayley took the trouble to inform Stokes that he had received his copy of the first number of the Phil. Trans. in the new form and found them "fearfully clumsy". Bearing in mind that Cayley's somewhat cloistered Cambridge outlook had prompted him to look forward to a rather unbalanced division of the Phil. Trans. into three series consisting of mathematics, physics, and biology, his critical view of the new format is understandable.[36] Eight hundred copies each of series A and B were printed where one thousand of the old combined version had been produced. At the same time five hundred and seventy copies of the Proceedings were being posted to English addresses with a further three hundred and fifty going overseas. The total number of copies of the Proceedings required from the printers in that year was sixteen hundred so it can readily be seen that the Proceedings had become the leading instrument of the R.S.'s advertisement overseas. Deliberate steps had been taken in the wake of the earlier attempted reform of the status and public image of the Proceedings, to define an equality of esteem between publication in its pages and those of the Phil. Trans. How typical then of the repeated confusion and failure of the Society's officers to improve the siutation in that Foster should write in July of the following year:

> "[We] have determined to restrict the future publication in the Phil. Trans. to papers of great merit."37

Even allowing papers to be printed in extenso in the Proceedings (the usual limit of twelve pages had been confused by the possibility of printing excellent work at greater length by authors in a hurry, as a part of the 1870's reforms) had failed to break down their reputation as the dustbin of the Phil. Trans. The chief editor of the Proceedings, the Senior Secretary Michael Foster, admitted the situation in 1895 in the course of arbitrating two conflicting referees' reports:

> "I quite agree as to the undesirability of using the Proc. to publish papers not good enough for the Phil. Trans. but as long as we have Proc. in our present form we shall from time to time be driven to this and I trust we are near the end of a bad system."[38]

The retention of the relative alacrity of publication in the Proceedings would obviously have become impossible if the papers were all refereed as they were for Phil. Trans. In this situation speed of publication and equality of esteem as between the Phil. Trans. and Proceedings were mutually incompatible objectives. The officers seem to have failed to recognise the fact that Herbert Rix was the initial recipient of the papers at Burlington House. He displayed a staunch disregard for any scientific content or merit which the papers might, or might not possess. Rix's criteria of suitability for publication were strictly as follows:

i) The time of the month when a paper came in.

ii) Whether long or short.

iii) Whether they have many tables, plates or cuts.

iv) How many others had been received.

v) Whether the author was sufficiently eminent to disturb the equanimity of Rix himself, should a complaint be made.[39]

The illustration of papers became increasingly usual as the second half of the century wore on and inevitably the cost of illustrations became a burden for the Society and a criterion in the publication of papers. Just one of the plates for Julius Plücker's Phil. Trans. paper of 1864 cost £48-6s-0d. This Stokes described as "a bit high" in a letter to Edward Sabine, then President. Biological authors tended to produce the more voluminous and amply illustrated papers during the period here under consideration. Late January 1887 saw Rix lamenting a dearth of papers yet recalling that Professor W. K. Parker "had one yesterday nine inches thick".[40] The palaeontologist Professor H. G.

Seeley single-handedly kept a part of the R.S. and one of its regular engravers Miss Gertrude Woodward busy for years. It seems however that the quality of illustrations in R.S. publications remained relatively low throughout most of the period. Secretary Sharpey remarked to his newly appointed Junior Secretary George Stokes in 1855 that whilst their copperplates were all that could be wished, the lithographs were poor in comparison with other publications. Thirty years on Michael Foster writing to Huxley noted that through his influence, the Cambridge Scientific Instrument Company was being favoured with the R.S. illustration work. The firm was run by Charles Darwin's fifth son Horace.

> "Dew is putting his long back into lithography and turning out some admirable work. I think that it is time that the plates in the Phil. Trans. ceased to be the laughing stock of the scientific public."41

Ten years later some leading biological Fellows agitated for the "B" series of the recently divided Phil. Trans. to be published in quarto to further facilitate illustration. The rise in the costs of publication for the R.S. were considerable and formed an unlooked for addition to the Society's outgoings on what was already an expensive operation. In 1863, the total cost of producing the Phil. Trans. and Proceedings had been over £3,000. The selling price of the Proceedings remained set at one guinea per volume from 1856. From then until 1888 the price of the Phil. Trans. fluctuated when the price was set at £1-6s-0d for Series A and £1-19s-0d for Series B. In writing of the new prices, Rix noted the harm which price fluctuation had caused, and that the new prices offered good value for money "for such bulky volumes". Series A was to be 526 pages with 26 plates while its companion was to be 400 pages with 30 plates.[42]

Conclusion

The Royal Society failed to deal adequately with the problems posed by the rapidly increasing specialism in science during the latter part of the nineteenth century. This failure is clearly thrown into relief by the development of its publication policy. Dissatisfactions continually came to the surface despite the modifications made to procedures in 1854, 1872, and 1896 when Sectional Committees were reintroduced. The continual confusion of several possible interpretations of the function and status of the two journals, engendered by the growth of specialist scientific societies with their own journals, occurred because the pace of change in the scientific world at large outran the effects of the R.S.'s procedural modifications. These problems continued to trouble British men of science largely because the R.S. successfully retained its pre-eminent place as the clearing-house for the distribution of legitimacy, renown, employment and financial resources to the scientific community. In that situation a scientific man might well get himself published efficiently in a specialised journal yet doubt the beneficial effect which publication might have on his career. As a result the most promising and ambitious group of practitioners were driven to seek the reassertion of the status of the widely vilified edifice of R.S. publication channels. In Europe it seems that the academies retained their prestige whilst passing on to specialised scientific societies and private journals the function of rapid original publication. The Berlin Academy published its main journal only twice yearly and yet German scientific publishing as a whole dominated, both in terms of its coverage and the level of prestige attatching thereto. The Royal Society belatedly made a number of concessions to the intrusive demands of modernity but these were small and always piecemeal. Archived papers were

conventionally copied for possible publication elsewhere by the beginning of the century's last quarter. At about the same time the requirement that R.S. papers be wholely original and exclusive to that body began to be informally waived by the Officers on a quite regular basis. By 1891 it had become so common for abstracts of the papers forthcoming in Proceedings to appear in Nature that Rix asked Michael Foster for a formal judgement on the issue. Foster was well aware of the outcome of a ruling against this practice, which continued unabated. It has constantly to be borne in mind that whereas Rix's problems were those of overwork and unwieldy office procedures, the issue for an unknown author was often the crucial one of his entire scientific and professional future. This is made evident in the communication between Rix and Foster of 1891, alluded to above:

> "Have had a lot of correspondence and telegraphing and consulting over a similar case on the physical side and the man in agonies all the time with an editor hanging over him."43

Attempts at "fine-tuning" the fundamentally unworkable publication procedures of the R.S. continued until, and indeed beyond the end of the century. Following the reintroduction of the Sectional Committees in 1896, as an attempt to institutionalize the division of scientific labour by subject area within the context of the R.S., it was discovered the process of evaluating the papers was not markedly accelerated. The device known as "Standing Order 43" was incorporated which allowed the summary judgements of the Sectional Committee Chairman and Secretaries to be sufficient to set publication procedures in motion. The effect of this was to undo the accumulated reforms and return to the situation of old in which individuals conferred with the Senior Secretary in the meetings of Council when it sat as the Committee of Papers.

The perennial openness of this and other problems of the R.S. led, not unnaturally, to some pretty radical speculation on the subject of its future rôle in the arena of British science. Some of these ideas were forthcoming from persons who might well be thought to have been pillars of its status quo. William Thistleton-Dyer, son-in-law of Sir Joseph Hooker and Director of Kew told Thomas Huxley that he saw no future for the R.S. as a publishing body. Dyer wanted to see the Society devoted much more to its position as the representative of British Science to government and the world at large. The same letter from Dyer to Huxley of 1893 contained a reference to Foster's vision of the R.S. as a sort of central bureau with the special societies affiliated to it. This pooling of the R.S.'s prestige with other bodies filled Dyer with horror. Huxley himself apparently wished to see the R.S. publishing papers "dealing with principles . . . detailed investigations [to] go to the special societies."[44] Sir Norman Lockyer also responded to the atmosphere of trepidation with a suggestion for reordering the scientific societies. The fact was noted by Hooker in a letter to Huxley written during December of the same year. As it turned out, the mood of impermanence was itself more perishable than the Royal Society although the serious difficulties inherent in its mode of publication were not overcome. The physicist Hertha Ayrton was able to state in 1909 that:

> "The R.S. Proceedings with regard to papers need a thorough reform. Everyone knows this, and yet, like 'everyone's business' at all times it does not get done. Of course I can do nothing because I am a mere outsider, being a woman!"45

* * *

Notes

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36. Stokes' Papers (CUL), C 267, C 278.

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38. Royal Society, RR 12, no. 175.

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CHAPTER TWO

THE GOVERNMENT GRANT

The enduring theme which runs through the history of the R.S. Government Grant is the sense of unease experienced by its administrators. Unlike the situation in France, where Hahn tells us that from the very outset the managers of French science sought to define real scientific work as being outside the domain of the dilettante and the amateur, British men of science were not attuned to the notion of receiving money for personal subsistence whilst doing basic research.[1] Many aspired to academic appointments and many sold their expertise commercially, as I have described in Chapter Three. Despite the familiarities of many Fellows with the ways of mammon, the conventional view of private research tended to stress its proprietory, exclusive nature. This view appears to have held sway among the prominent Fellows who witnessed the early operation of the original Government Grant of £1000 made at the suggestion of Lord John Russell in 1849. Of the leading Royal Society men who were chary of official funds from the outset not all were traditionalists.

The Schlagentweit Affair

William Whewell, one of the appointees to the first Government Grant Committee, told Roderick Murchison of his misgivings about the projected new position of the Society as a distributive agent for Treasury funds:

> "Some persons, I find, doubt whether the old practice of applying the screw of opinion in the scientific world to Government on each special occasion was not better than this perennial stream of bounty . . . . I am not quite sure that I like the responsibility of handling, or directing the handling of parliament money."2

Whewell was concerned that some of the experiences of himself and others in the management of BAAS-funded research would not be repeated in the forms of "waste, caprice, partiality, and jobbing". Lord Rosse, PRS at the Grant's inception, based similar forebodings on the widespread

personal jobbery and bureaucratic formalism which he saw as the consequence of similar schemes of governmental patronage of various continental academies.

Sensitivity to all the possible pitfalls was focussed acutely a few years after the inauguration of the Grant by the scandal which followed the support given to the brothers Schlagentweit for their Asian expedition by the East India Company and the Treasury. The brothers' expedition was supported by Alexander von Humboldt and much taken up with that lynchpin of his kosmographical research programme: terrestrial magnetism. The East India Company, which had never allowed Humboldt access to India for fear of an exposé of Company treatment of its inhabitants similar to his depiction of slavery in South America, approached the Royal Society for advice and thus secured the eager involvement of the arch-Humboldtians of English science, Roderick Murchison and Edward Sabine. After the completion of the Germans' expeditioning in 1858 (it had proved fatal for Adolphe Schlagentweit) the very lengthy preparation of their account of it and that account's varying quality began to prompt questioning in English scientific circles. The first part of it contributed to the R.S. was referred by two ardent Humboldtians, John Herschel and Balfour Stewart, who recommended its printing in the Philosophical Transactions in 1863. The second part presented in the following year was recommended for the R.S. Archives by Herschel and John Tyndall. A year later part three was archived, even though it had been communicated to the Society by Sabine, then the President.[3] These rejections represented a radical reappraisal of the worth of the Schlagentweits' endeavours which had not been publicly questioned until their first complete volume appeared in late 1861. After an uncomplimentary review of it appeared in the Athenaeum, Joseph Hooker went to Burlington House to check up on the date of his appointment to the Committee which recommended the Schlagentweit's course of observations in India. The Assistant Secretary,

Walter White noted in his journal that following Sabine and Murchison's explanations, offered in response to the Athenaeum review, Hooker was inclined to blame the Indian authorities for passing over such men as the Stracheys and Thomson in favour of the less capable Germans. White's report of Hooker's stated opinion continues:

> "That the S's appointment was a flagrant job but at whose instigation he cannot tell. That Colonel Sykes told him that while the brothers were in India and when their work was but half done that they had spent £20,000. That they wanted him to describe their plants - that he offered to do it as a public duty and to enlarge his knowledge of botany, though not especially for their book . . .. That they got a good swag out of the sum allotted to them and bought a barony and an estate near Munich. That Thomson spent all his pay in making collections and then was refused leave to publish the results - that while the S's were encouraged, the collections made by Wallich, Thomson, and Stracheys were rotting in the vaults of the India House; waggon loads, which cost £40,000 in collecting."[4]

Mention of these matters is noticeably absent from the pages of the R.S. Council Minutes. Five years later, Sharpey, in his capacity as Secretary of the Royal Society, wrote to Viscount Cranborne offering the restrained approval of the R.S. President and Council of the Schlagentweits' magnetical and astronomical observations, disowning the rest, and noting that "the mode of publication of the work is considered unnecessarily expensive".[5] The archiving of their third R.S. paper communicated by P.R.S. Sabine the year previously stands as the damning evidence of the brothers' fall from grace, and the extent to which the indirect patronage of the R.S. could go awry.

R.S. Reluctance to Administer the Government Grant

If the Schlagentweit affair furnished graphic evidence of what could go wrong when the R.S.'s reputation was tied to its sanctioning of the expenditure of public money on scientific projects, then the administration of the grant in its early years reflects the conservatism engendered by this climate of ideas. The concentration of the

few grants made within a small group of well-established London-based men of science had occurred from the very outset, being reinforced by the effect of incidents such as that detailed above. Between 1860 and 1864 94.9% of the grants went to Fellows, the great majority of whom lived in London.[6] The escalating costs of research, particularly instrumentation, forced many workers to apply where their natural inclination might have been to work in the tradition of the gentleman amateur. Money became a widely prohibitive barrier to private research in the physical sciences initially, with astronomy perhaps the most expensive of all.[7] The much-vaunted "Young Guard" of British science which has often been exemplified in the group of scientific naturalists who made up the X club, were no more enthusiastic about the Grant than surviving representatives of the noble tradition such as Lord Rosse and Whewell.[8] Following the Government's (Devonshire Commission prompted) offer of a further £4,000 to the R.S. in 1876, Huxley expressed his view of the situation to his fellow Secretary George Stokes:

> "I don't know what your feeling may be about the administration of £4,000 - but I look on it as about the gravest and most troublesome business that the Royal Society has yet undertaken."9

Huxley's intimate friend Joseph Hooker was President of the Society at the time. He shared Huxley's view and expressed it in a letter to Darwin late in 1878:

> "Between ourselves I think there will be a wretched outcome of the Government Fund (the £4,000 per annum). I am sure that if I had the uncontrolled selection of persons to grant it to, and was free to use my authority over them, I could have got ten times more done with the money. I shirked the subject with my address."10

The men of power and influence within the Royal Society found themselves in something of a cleft stick with regard to the Government Grant. It seems clear that their foreboding of unhappy outcomes of taking on the responsibility of the initial Grant and the addition of 1876, were at both times outweighed by a jealous concern for the

dominant position of the R.S. as the chief representative of British science to the world at large and to the Government. The Society's officers clearly did not want final authority in scientific matters as central as the evaluation of the validity of basic research projects to pass to some such body as the BAAS, the Department of Science and Art or, heaven forbid, the Treasury itself. Once the R.S. had set out on a career of protecting itself with this mixture of self-importance and a sort of corporate noblesse oblige, its senior servants developed an exaggerated sensitivity to misinterpretation of its position and practices. As a result, great care was taken to publicise the fact that the R.S. was acting only in a trustee capacity and was not itself a beneficiary of the Government Grant. Willian Spottiswoode was understandably careful to return any unexpended part of the year's Grant during his periods as Treasurer and President. The leaders of the R.S. were, throughout the period, intent on maintaining the Society's freedom from Government control which was seen as an insidious accompaniment of financial dependence. It quite frequently happened that the Government response to R.S. requests for funds for special projects came in the form of a reproof of such special pleading and the recommendation that the R.S. use part of the annual £4,000 for its purposes. This happened even before the additional sum had been made available in 1876, such as on the occasion when Sir Henry Holland took a letter from the President (Sabine) in 1864 to Palmerston requesting financial help with the production of the Catalogue of Scientific Papers. Palmerston asked Sir Henry three times if Sabine's mention of 167,000 titles was not a mistake which had augmented a true figure of 16,700. Palmerston asked why the Government Grant was not going to be applied to this purpose:

> "and being told that we viewed the grant as applicable rather to the aid of persons engaged in discoveries in science said that he was told that there were very few discoveries or researches just now, and that it was otherwise a legitimate application."11

The treatment by sucessive governments of the Government Grant as more or less an annual budget for the Society became no less galling for its regular repetition. Michael Foster came up against the problem thirty years after the Catalogue incident when the Government became reluctant to cover the expenses of the R.S. work on the problem of the tsetse fly and its activities as a carrier of disease which were proving detrimental to colonial interests. In this case the Colonial Office seemed to be regarding the Government Grant as a form of lump sum payment to the Society in return for performing its offices as scientific consultant to the Government. Considerable outgoings were involved including the maintenance of Major Bruce whose secondment from military duties R.S. representations had secured in order that he perform the necessary fieldwork in Africa. Foster noted that "meanwhile, what we at the Royal Society are doing, we are paying for ourselves".[12] To a considerable extent these attitudes could be seen as having been deliberately struck to express the official parsimony gladly enforced by the inflexible disciple of retrenchment, Lingen. At the end of 1885 the Treasury was attempting to convince the R.S. that it should pay a part of the cost of the recent eclipse expedition out of the Grant. The same year saw Lingen's departure and the more tractable Reginald Welby established in his place. The easing of immediate tensions did not alter the Government's basic attitude however. Two years after the coming of Welby the R.S. Treasurer John Evans complained afresh to Stokes: "It would almost seem as if the Treasury held the view that the R.S. ought to dictate to the Government Grant Committee the direction and amount in which the Grant is to be expended."[13]

Organising the Government's Money for Research

Despite Lingen's keen disapproval of Norman Lockyer's Secretaryship of the Devonshire Commission whilst still being employed within the War Office, the Royal Society found an additional £4,000 at its disposal in 1876 following the Commission's Report.[14] Lingen objected to Lockyer's profligacy and empire-building alike and attempted to block his transfer from the War Office to the Department of Science and Art in 1875. Lingen's negative attitude reflected both his own outlook and a recent renewal of the spirit of strict economy within the Treasury. According to Macleod, the total amount of Government expenditure on science apart from education was £321,000 in 1875. If this figure is augmented by expenditure on scientific and technical work under the aegis of the Army and Navy the total becomes £347,000 or 2.7% of the total civil estimates for 1875-6.[15] The initial Government offer of replacing the old Grant of £1000 with a new one of £5,000 to be administered by the Science and Art Department was seen by the R.S. Council as a usurpation of the Society's prerogative. Despite the misgivings felt by many members of the Council regarding the provision of grants for personal support under the new scheme, the R.S.'s insistence on retaining overall control was made clear. Hooker, as P.R.S., wrote with a suggestion which, following its adoption, left the old Grant unchanged and the additional £4,000 called the Government Fund to be administered as a separate entity and to come from the vote for the Science and Art Department. This was distributed by separate sub-committees dealing with the various subject areas. By the late 1880's the degree of specialism wrought by actual scientific practice was such that the original category for mathematics/physics/astronomy and its two fellows dealing with biology and chemistry were no longer adequate. The R.S. Committee appointed to consider

the question in 1888 produced a design for seven grant sub-committees each of which was to have eight members who were to be elected every four years. Two members were to retire annually and no member was to succeed himself. This was the first step taken by the R.S. to inhibit the time-honoured domination of its Council and Committees by small groups of self-electing oligarchs who effectively controlled all the Society's business. This tradition was first publicly remarked upon by William Spottiswoode in the course of his Presidential Address in 1881.[16] It is natural that the elaboration of administrative procedures should frequently be seen as progress and yet this was not the case with the creation of Boards A to G in 1888. Herbert Rix wrote to Sir Douglas Galton on this issue in 1895:

> "Down to 1889 the task was comparatively simple but after that date it has been rather a troublesome task in consequence of the very complicated system of boards under which we now suffer."17

The recognition of the prototype of modern practice in a particular historical context should not in any circumstances be taken prima facie as evidence of its beneficent influence or absolute worth. Such a tendency invests the only extensive accounts of the R.S.'s administration during the nineteenth century in a manner wholly detrimental to their utility.[18] The new Grant boards performed the same anachronistic function as the reintroduction of Sectional Committees for the consideration of papers in 1896: that of promoting the unity of the Society by differentiating its internal structure in pursuit of a more elaborate division of labour.

Personal Grants

Easily the most contentious issue brought up by the coming of the additional annual £4,000 in 1876 was that of personal grants. In 1854 the President, Lord Wrottesley, wrote to the Treasury to ask the reason for that body's withholding of the annual grant of £1000. In doing this he particularly stressed that none of the £5,000 so far received since the grant's inception in 1849 had been spent on

personal maintenance for men of science. Such emphasis cannot be interpreted as mere punctiliousness. What was at issue in this incident of 1854 remained unresolved for years afterwards and is bound up with what is no less than the question of the basic nature of the scientific enterprise and the competition for ultimate legitimacy between its severally defined versions. Only in this context may the long-running "problem" of personal grants as a part of the R.S. Government Grant be fruitfully approached. Wrottesley, with his aristocratic assumptions and sufficient private means to enable him to maintain astronomical observatories at both Blackheath and Wrottesley, might be expected to have deprecated any way of doing science but by noble devotion of the isolated amateur. The odd thing is that many of the group which has been identified as representing the cause of scientific naturalism, were almost as eager to preserve the uniquely British type of independent gentleman amateur as they were to ameliorate their own salaried, professional condition. In his evidence to the Devonshire Commission Edward Frankland stated:

> "Men of this class [i.e. scientific amateurs eminent in their respective fields] are really peculiar to England, for I have never known any such instance in Germany or in France, of men altogether disconnected with teaching, taking up research in the way it is done in England. I think that for such men the establishment of national institutions . . . would be peculiarly useful."19

Huxley and Hooker looked askance at personal grants in particular and the additional £4,000 per annum of 1876 in general. William Flower objected to personal grants which encouraged "cottage research" in principle in so far as they could be seen to identify science as an activity peculiarly suitable for the impecunious and the feckless. Regarding the new Government Fund application form produced in 1876 Huxley suggested to Stokes that it, "had better have a little less

outdoor relief look than our worthy Assistant Secretary has given it."[20] There seems to have been a widespread expectation that the Grant would follow the Civil List in becoming a paupers' roll rather than an efficient means of recognising real scientific merit. In Frankland's statement quoted above he aimed for the availability of facilities and equipment for the scientific amateur rather than his personal economic maintenance. It is apparent that a widespread feeling at the time held that the heroic potential of the private scientific worker would be tainted or even lost by his becoming the Grant's creature. William Spottiswoode, who as an amateur mathematician and electrical experimenter with the financial security of the family publishing firm behind him had little to lose by questioning the soundness of personal grants, suggested that the R.S. should reflect before assuming moral responsibility for "interrupting the business" of young men's lives "merely for the sake of science".[21] A strong impetus was doubtless lent to the critique of personal grants by the staunchly proprietorial view which was taken of intellectual products such as the original scientific ideas. The energetic legalism forming the monumental background assumption of that outlook, could not be other than discomposed by the seemingly wanton encouragement of future scientific heroes to become kept men. The extent of the welfare rôle enjoined by the provision of personal grants varied according to individual recipient's immediate circumstances. An extreme case was that of a Dr. Collins of Dublin who in February 1875 was about to receive an R.S. Grant when the issue was forestalled by his going mad and being put in an asylum. His proxy wrote to Stokes requesting permission to anticipate the decision of the Scientific Relief Fund and dispense the £50 Government Grant to Mrs. Collins at £10 per quarter. She intended to take in lodgers for her livelihood while the rent was paid by the R.S. Grant which would

also buy furniture.[22] The personal grant for individuals to conduct research privately on their own premises was a peculiarly British development intended to democratize the highly traditional ideal of the gentleman amateur. This circumstance goes some way towards explaining why disapproval of personal grants reached high into the ranks of the professional "Young Guard" of British science. (Though by 1876 the powerful coterie of scientific naturalists who have commonly been held to be the most potent force in late-Victorian scientific England had become a decidedly "Middle-Aged Guard".)[23] In 1877, X club member Thomas Hirst recorded his own disapproval of R.S. personal grants but pointed out the commitment to them of three powerful representatives of the next generation:

> "At the club I met M. Foster, Thistleton-Dyer, and Moseley. They had just come from a Government Grant Committee meeting. In reply to questions concerning the utility of these grants, I stated that some years ago I had asked to be withdrawn from this Committee, because I saw that the application for personal remuneration (that is to say for time spent on investigations, irrespective of money disbursed) were having a demoralizing effect on the younger applicants. Without attempting to defend the practice, I could see that the three gentlemen to whom I was speaking were prepared to overlook the abuse which was inseparable from such applications, and against which I had protested. Sir Henry Roscoe, who had joined our group, evidently sympathized with me. He preferred the British Association practice."24

The latter consisted of the provision of instruments and other material facilities which formally reverted to the grant-giving body following the research, although usually the recipient was allowed the continued use of them afterwards. This method of sponsorship was also that used by the R.S. for the original £1,000 annual sum. Some applications made on this basis were still very large by the standard of the day. Norman Lockyer, who ranks as the second largest recipient of R.S. research funds of the period, put in a successful application for £500

for a 30 in. reflector to be ground by A. A. Common whose observatory at Westgate-on-Sea in Sussex prompted Lockyer to build a house there with a canvas and wood structure to house the new reflector which was used in the cleaner air for photographic work on spectra.[25] The sum Lockyer obtained was additionally intended to cover the salary of an assistant. His modern biographer graphically depicts the way in which the ambitious Lockyer acquired enemies in the course of his career. It is small wonder that this arch-beneficiary of R.S. funds should be accused of their misappropriation. Quite apart from his personal receipts, Lockyer managed to mobilize the R.S. and Royal Astronomical Society to send out large eclipse expeditions in which he had a crucial vested interest in physical and astrophysical terms. Lockyer received £2,000 in total from the Government Grant and Government Fund, a circumstance which taken in conjunction with his predeliction for controversy in terms of fierce personal acrimony, not unnaturally prompted accusations of impropriety. Captain W. Noble is reported as leading something of a clamour to this effect when he suggested that Lockyer had reared a large family on what he had put aside from a series of R.S. Grants.[26] As a matter of interest Lockyer's biographer states that he obtained a small sum of money for the Westgate-on-Sea project of 1888 from the Government Grant Committee which was specifically for the erection of the canvas and wood structure to house the new 30 inch reflector containing Common's mirror. At the same time Lockyer built a substantial house for his own family and the observers.[27] On the 18th of September during the same year William Huggins wrote to Stokes, who by then had terminated his 31-year Secretaryship for the R.S. Presidency, noting Lockyer's Grant of £500 for the mirror by Common and an assistant's salary and asserting that he wanted a similar mirror for his own work and could obtain one for £130.[28] Cases like Lockyer's,

whether they were fairly proven cases of abuse or not, provided tangible ammunition for the many trenchant opponents of the endowment of research movement who tended to see the entire Devonshire Commission as the "mere tools and catspaws of a needy and designing confederacy."[29]

Frankland wrote that the relatively small sums of £100 and less allotted by Board C had proved to be the most likely to yield satisfactory results. On the basis of judging the outcome of grants of this size he stated that "as many as 29 have yielded very satisfactory results" out of a total of 54.[30] His conclusion was that of a total sum granted to chemistry of £2917 between 1882 and 1887, £1307 was known to have given good value and that all that amount had been given in the sums of £100 or less which facilitated private research. When the original Government Fund's tripartite system of grant distribution was set up in 1876 each of the three boards was provisionally given £1300 to distribute annually. The R.S. could not be said to be following out any clear perceiveable trend in making its leading beneficiary Mr. William Kitchen Parker, who received £3,150 from Board B between 1876 and 1888. Parker filled the office of Hunterian Professor of Comparative Anatomy from 1873 to 1890, during which time he specialized in producing for the R.S. papers of such stupendous prolixity and far-ranging illustration that his name became an adjectival bye-word for these dubious attributes. The zoological paper which was nine inches thick has been alluded to in the earlier section on R.S. publication. Parker was deeply committed to his orthodox Christian views which he held in common with the Secretary of the R.S. George Stokes. As he remarked to Stokes in a letter of July 1887 during the latter's Presidency:

> "Whatever becomes of the theory of Evolution 'the foundation of God standeth still', and to you and to me the fate of any scientific theory is a small thing, compared with the truth of the 'Everlasting Gospel' of our own Lord and Saviour."31

Parker's views suggest the important place of religion in the disposition of rival camps within British science and the R.S. in particular which forms the subject of a later chapter. The extraordinariness of the treatment meted out to W. K. Parker might be explained in terms of his forming the focus of uncritical support from the Christian party within the R.S.'s ruling group, which beheld so much of the excellent work on the biological side being channelled in Darwinian directions. Parker himself toiled on unabashed. On the 1st July 1889 Rix mentioned Parker's plans to Michael Foster:

> "Professor Parker wishes me to say that he is not at all sorry that the grant from the donation fund is a smallish one, because in 1891 he will want 'to open his beak again'."32

After William Parker's death Rix wrote to his son Jeffrey Parker, who for a long time worked at South Kensington as one of Huxley's demonstrators, inquiring after details of the grants which his father had received. The year was 1893, when the detailed, anonymous attacks on the R.S. made for a number of years in the pages of The Times had started up in earnest. It seems to emerge that the support for Parker's special position had passed and an effort was wanted to conceal the facts of his case:

> "Dr. Michael Foster wished me to write and explain that it might be as well not to make too much of this personal grant and especially of its having been awarded annually for so many years. Some objection was raised latterly to this and other annual grants, and there was considerable discussion of the whole matter which ended in the Government Grant regulations being altered."33

Throughout the nineteenth century the direct patronage of men of science through governments or academies suffered by its incompatibility with the enduring ideal-type provided by the romantic image of the gentleman amateur. This predeliction was an important part of the outlook of many of the "new men" who are held up as pioneers of professionalism as it was of the authentic survivors of the age

of aristocratic dilettanteism. The latter still made up over half of the Society in 1860. The "string of brilliant names" who formed the visible part of British science continued to research independently establishing little in the way of formally institutionalised schools of thought. The familiar roll of honour runs through Hutton, Faraday, Joule, Maxwell, Crookes, Darwin and Rutherford.[34] Berman asserts a relationship between these men's careers and "the aristocratic model of science as an avocation". Berman specifies two important features of this model as financial independence and freedom from pressure towards conformity with the opinions of any formal network of fellow practitioners. Clearly, all the eminent men mentioned above do not meet both criteria. Also this standpoint seems unable to give a full account of the emergence of radical new scientific ideas within tightly organised professional academic institutions on the continent. Berman's notion that the superabundant "Field Marshals" of British science were "devoid of the hunger for personal recognition" is plainly open to question. The most cursory view of the careers of men such as Davy, Herschel, Kelvin, Crookes, Lyell, and Huxley reveals rather a thorough devotion to the accumulation of personal renown. Nonetheless, a modified form of Berman's implications appears to be relevant to the long-running "problem" of the Government Grant and especially that of personal grants. Contemporary doubts about the grants are partly traceable to the continuing vitality of the ideal picture of the financially independent scientific amateur. The contradictions inherent in both holding to that world view and attending to the practicalities of rendering science more open and democratic are well-illustrated in the person of Joseph Hooker. His opposition to personal grants has been noted earlier, yet one of his main innovations as President (1873-78) was the reduction of the cost of membership so

that it could not act as a barrier to worthy but impecunious candidates. The essentially romantic vision of the scientific enterprise held by Hooker and several of his rather less diplomatic compatriots from the camp of scientific naturalism is made clear in the two following quotations which express his view of the scientific traveller. The first occurs in a letter to Huxley when their careers were just opening out in 1854:

> "a man who (like you) works out a point of science during the difficulties and discouragement of a voyage has in my opinion an equal claim at least than a man who works the same in his easy chair; even though the latter works it better."35

Twenty years later in a letter to Darwin he laments:

> "I think Humboldt is underrated nowadays [as was Sabine]. Well, these were our gods my friend and I still worship at their shrines a little."36

In 1881 Hooker still regarded Humboldt as the greatest scientific traveller, despite his headlong fall from grace in the scientific world at large. The strong strand of traditional thought in the world view of the "new professionals" of British science has usually been set aside by those seeking the genesis of modern trends.

The Distribution of the Grant

The levelling function which might well be seen in the provision of personal grants after 1876 was not effective in diminishing the imprint left on Victorian science by economic and social class distinction. A contemporary recipient of a grant, George Gore, estimates that for every £100 of grant received £1,000 was spent by the recipient on his own account. His experience does not sit comfortably with that of Hugh Breen, who wrote to Stokes in 1869 reporting that he could not live on the £50 grant. The profoundly literal Secretary R.S. wrote back to say that the situation presented no difficulty because Breen was not supposed to.[37] An influential figure who shared

with many others the inability to appreciate that rectitude was a luxury in which only the financially secure could indulge, was George Airy. In November 1878 he wrote to Stokes pointing out that he had made a mistake in setting out the equations for applying Delaunay's Numerical Lunar Theory to Gravitational Astronomy, as suggested by the Board of Visitors to Greenwich. Airy's response was to reverse the familiar direction of the flow of patronage for science by paying for the repetition of the work himself.[38] When he accepted the Presidency in 1871 the Astronomer Royal offended many Fellows by stipulating that all his Presidential expenses would have to be covered by the Society. Even though Airy was the first President who possessed no private wealth the same attitudes were mobilized in this situation as were so often prevalent in connection with the Government Grant. Lingen predictably looked with approval on Airy's unwillingness to distribute Government money which in 1871 he expressed as a request for greater openness of both access and operation of the Government Grant Committee.[39]

In the above communication the President gave as his reason for desiring greater openness, the wide opportunities for favouritism in the act of distributing trust funds. Scrutiny of actual distribution of the Grant over the years of its operation reveals the truth of Macleod's contention that it was given "by the few, to the few". Over the whole period 2,316 projects were endorsed with funds in the name of 938 men to a total of £179,000.[40] The highly exclusive nature of this distribution was established early as can be seen from the fact that during the 1860's 90% of the grants were to Fellows, nearly all of whom lived in or near London. The privileged group of leading practitioners who benefited most from the Government Grant Committee inevitably had considerable overlap with its actual membership - which,

as has already been pointed out, showed remarkable constancy over a great many years. The necessity for a Committee member to virtuously resign if he had made an application of his own was removed some years later. There is considerable evidence of informal dealing between the Society's officers and putative recipients from the group of leading practitioners in the early years of the Government Grant. A clear instance is provided by the approach of Sabine as Treasurer R.S. to William Thomson in April 1851, before Thomson had been elected a Fellow:

> "It has been suggested that the effect of pressure on the solidifying point of bodies might be an important subject for experiments for which public funds might be with great propriety be allotted. You have been at work at this in the case of water . . . ."[41]

Three days later on April 25th Sabine wrote to say that it had been arranged that Thomson's Cambridge tutor and colleague William Hopkins would do the work if approval for the payment of his equipment expenses could be got successfully through the Grant Committee. Sabine went on to explicitly state the terms on which such a thorough working out of the Matthew Effect was to take place:[42]

> The members of the Grant Committee appear to be generally opposed to the apportionment of any part of the public money to subjects in regard of which they cannot entrust the expenditure to persons of known competency and character."[43]

A fortnight later Thomson wrote to Stokes assuring him that Sabine had told him that Stokes' application for £200 for work on fluid friction, "is pretty sure to get it". In passing, Thomson related how the Treasurer of the Society had also shown him the list of that year's successful candidates on which appeared both their names.[44] Once a system of allocation of funds becomes subject to informal dealing which is only later stamped with the imprint of legitimate practice, its usefulness for those lacking the personal acquaintance of the ruling group and working outside that group's theoretical

orthodoxy is exhausted. The biological Secretary R.S. William Sharpey informed Stokes in 1858 that he and Bence-Jones felt so much in accord with F. W. Pavy's work on the liver that it would be well to present him with a retrospective grant of £100 for it. Sharpey added: "I will write to Pavy recommending him to apply if he has not already done so."[45]

The terrific Metropolitan bias consistently reflected in the apportionment of the Grant is predictable enough. Provincial applicants could seek no redress if unfairly turned down while there appears to have been scant opposition to Galton's dictum that provincial mediocrity is the functional obverse of London excellence.[46] This is reflected in the indomitable attachment to the capital formed by the leading scientific naturalists of the X club and many others. Between 1850 and 1876 Scotland received twenty-four grants but of these, eighteen went to Thomson and Joule. Of this George Chrystal wrote to the R.S. in December 1903 following his "sudden ejection from Cambridge" and removal from the Government Grant Committee. He said that: "As a stimulant of research in Scotland the Government Grant, as I daresay you know, has proved a failure. It could hardly do otherwise, as I pointed out many years ago."[47] As late as 1914 it appears that the situation had not eased. In that year Devereux Marshall wrote that research did not pay and that a man must possess private means or fall into the closely circumscribed Government Grant Committee range of approval:

> "This is certainly wrong and must have done an incalculable amount of mischief in this country."[48]

For a man who was "right" of course the R.S. had much to give. One month after the inaugural meeting of the X club in 1864, the Anniversary dinner of the R.S. took place at Willis' rooms after which the seventy-odd diners heard the recipient of the Rumford Medal, John

Tyndall, thank the Society for "supplying him with everything but brain." All within a short time-span he had received money from the Grant, made two of his papers into Bakerian Lectures and been presented with a Rumford Medal.[49]

Macleod noted that Rosse's early review of the Grant stressed that mundane fact-collecting could only be sustained by such a means where "much labour and little fame is involved." In this way a bipartite prestige class system was established de facto, which enabled the R.S. power holders to bolster the normal science tradition in which they maintained so strong a vested interest by voting grants for one and other and one and other's acolytes, whilst necessarily suppressing any radical theoretical challenge by informally blacklisting suspicious applicants. This process would have a high degree of effectiveness because the R.S. power-holders' writ ran also in the few other grant-giving institutions. In this way unknown young men of science would, if they were honoured with a grant at all, be contained within the anodyne domain of data collection enacted in the uncritical manner of the natural historian so beloved of Humboldtian science. The system of grant allocation within the Royal Society was conducted under the formal terms of bureaucratic impartiality. In its actual day to day running these solemn proprieties seem to have been by-passed. Grant allocation became almost wholly the outcome of the informal and far from disinterested personal knowledge of a small number of powerful individuals within the Society. The hopeful ploy adopted by many obscure applicants was to send promising but unconsummated papers to the Society's apartments. The chances of these fledgeling scientific performances attracting the offer of a grant were very slim if the scientific work involved was not carefully calculated to appeal to the special sensibilities on one or more members on the

Society's leading group. To stand the best chance of success very thorough canvassing of interested insiders was essential. This would frequently take the form of attempts on the part of unknown young men to interest influential Fellows in acting as the communicator of their papers.

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CHAPTER THREE

"RICH ENGINEERS, CHEMICAL TRADERS, AND EXPERTS": FELLOWS OF THE ROYAL SOCIETY AND COMMERCE

Towards the end of the year 1877 a letter arrived at the Royal Society's apartments in Burlington House which drew attention to the doings of a gentleman trading as "William Thomson F.R.S." from a cellar beneath the Royal Institution of Manchester. The situation of this charlatan who was operating as a commercial chemist with both a better title and address than he could legitimately claim was as clearly deplorable to his virtuous contemporaries as it is to modern judgement.[1] A more difficult question concerns the extent of involvement in trade and commercially based activity on the part of the elected, legitimately styled Fellows and how this was interpreted by the historical actors of the time. The formulation of this question is naturally prompted by the well-known concern of a number of historians of science with the extent and significance of professionalization as a trend in the history of Victorian science. Just over ten years ago Morris Berman suggested that the second half of the nineteenth century did not promote so much a withering away of the ideal-type of gentleman amateur as a new impulse of aspiration to that persona on the part of some leading members of the new middle class of industrial entrepreneurs.[2] Berman stated that the maintenance of political power and control of economic resources on the part of the aristocratic, landed interests was not seriously altered until well into the present century. This, he asserted, would sufficiently have bolstered the hegemony of the cultural ideal of the gentleman amateur. Although the professionals and experts gained ascendancy in the end, replacing aristocracy with meritocracy and patronage with competition, the writer concludes that the eagerness of the Victorian professionalizer was effectively restrained until after the First World War.[3] After

that time the "conservative cult of expertise" upon which professionalism is based is presumed to have held the stage. To these questions the various aspects of the following evidence bear varying degrees of pertinence. The trade involvements of the Royal Society's Fellowship have received scant attention by publicists within the field of the history of science, therefore it seems that evidence touching on Fellows' contacts with and attitudes to commercial activity may be useful.

The Royal Society was wont to take some pains to stress the remoteness of its objectives from technological pursuits and the education of the public. There was therefore an undeclared contradiction between its public image as the supreme forum for the disinterested promotion of pure science for its own sake, and its similarly time-honoured rôle as the leading scientific consultant to the government on technical issues. It requires no exhaustive reiteration of the specific subjects involved in these consultations with Whitehall to establish that a railway accident at St. Neots and colour vision testing for military recruits were not projects born of the search for knowledge for its own sake.[4] Other features of the unfolding tension between technical trouble-shooting and unaffiliated "natural knowledge" seeking will become evident later in this chapter. The quotation which stands as its title is taken from a quite commonly quoted letter from Thomas Huxley to his intimate friend Joseph Hooker in 1889:

> "The only science to which Bramwell has contributed so far as I know is the science of self-advancement and of that he is a master. When you and I were youngsters we thought it the great thing to put and end to aristocratic flunkeyism which reigned in the R.S. - the danger now is that of seven devils worse than the first in the shape of rich engineers and chemical traders and "experts" (who have sold their souls for a good price) and who find it helps them to appear to the public as if they were men of science . . . ."[5]

The villain provoking Huxley's tirade was Sir Frederick Joseph Bramwell who in many ways typifies the late nineteenth century growth in the commercial demand for respectable technical expertise. Bramwell was born in London the son of a banker in 1818. He trained as a mechanical engineer and after setting up on his own in 1853 developed a large engineering consultancy and appeared a good deal as a scientific witness particularly in the boom area of water supply.[6] His elder brother Lord Bramwell, who was a judge, was reputed to set four divisions in the classes or liars: "liars, d----d liars, expert witnesses, and my brother Fred". Bramwell displayed disarming candour in explaining why his bills were so much larger after his election to the R.S. in 1873 and knighthood eight years later. He would recount how it was necessary to draw the attention of clients to the sense in which the letters F.R.S. could be taken as meaning "fees raised since", with the later admonition to his customers that they would be required to pay more for Knightwork.[7] Bramwell was made a baronet in 1889, the year following his somewhat controversial Presidency of the British Association for the Advancement of Science. Thomas Hirst the mathematician and X club member questioned the propriety of Bramwell including in his Presidential Address at the Bath meeting, a reference to the then live issue of building a channel tunnel.[8] Bramwell further affronted two other X club members Hooker and John Tyndall by introducing a final publicity seeking flourish to his Presidential year. He gave a banquet at the Goldsmith's Hall for William Flower, who was President-elect for 1889. Hooker wrote to Hirst enclosing a communication from Tyndall to himself which included the lines:

> "I knew it, my dear Joseph, I knew that you would refuse to taste, touch, or handle, the unclean thing. . . . Take you care my good friend that this blatant humbug does not one day become President of the Royal Society. The rule and governance thereof are in honest but flabby hands.

> I do not think I can continue to think of Flower as I have hitherto thought of him."9

In his reply to Tyndall, Hooker stressed his fears of science suffering a lowering of its position in public and government estimation. The writer accurately noted that the most damaging aspect of this manifestation of "the 'loud' position that science is assuming under the patronage of wealth", was the support of it by Flower, whose full scientific bonafides went unquestioned.[10] The X club was not quite unified on this matter as will be seen below. Tyndall himself had conceived a particular loathing for Bramwell since it had become apparent that the eminent expert had, as Honourary Secretary of the Royal Institution, schemed to assist James Dewar in displacing Tyndall from his long-held position there. What is clear is that men such as Bramwell were elected to the Royal Society in substantial numbers. This trend will be examined in a later section.

Commercial Activities of Some Prominent F.R.S.'s

During the latter phase of the century the powerholders within the R.S. were inclined to lofty ideals of public service and the nobility of pure scientific research untrammelled by considerations of direct material gain. Contrary to the view of Berman outlined earlier, it seems to have been the case that the eager "professionalizers", who included a number of important scientific naturalists, took jealous exception to the blending of science and commerce whereas those in the gentleman amateur class were not in the least bit embarrassed by it. In the course of his last Presidential Address to the R.S. on the 30th November 1854 Lord Rosse included a eulogium of Charles Babbage's calculating machine which excited the disapproval of Assistant Secretary Walter White. White was a committed supporter of the 1847 reforms and their architects who disapproved of the "Baconian" notion of materially useful science except for its special place

within the rôle of the R.S. as the government's scientific and technical consultant.[11] The somewhat surprising approval from a leading aristocratic amateur of the mobilization of scientific activity for practical purposes and gain, will be examined further in the conclusion to this chapter. The year 1893 saw the production of a code of conduct regarding "paid opinions" by a special committee of the R.S. The assumption that appears to have been made where this subject has been considered, is that the honour of the Fellowship was put to lucrative commercial use only by a minority of peripheral figures whose perfidy could well be policed from the focus of power in the Society.[12] After all, the title F.R.S. had conferred prestige and dignity upon its holders for over 150 years (it may not have been considered an unmitigated boon prior to that time) and would scarcely be omitted from a man's self-description merely because it succeeded in its intention. The difficulty arose out of the fact that it had always been supposed that the normal class background of Fellows would ensure their social distance from the vulgarities of trade.

The body of "traders" who increasingly attained the Fellowship after 1860 typically emerged by two main routes. Successful industrialists and their progeny whose products necessitated a relatively advanced degree of technical knowledge gained the interested support of the Fellows they mixed with commercially and socially. Moving in the other direction, as it were, were men who first came to prominence by establishing a reputation in pure science then having long been working Fellows of the Society, endeavoured to profit from various practical implications of their work. Neither of these two common courses represents an approach to the ideal of an authentically Baconian way of doing science in which disinterested rearch and practical requirements would be blended for the common good. Following the

passing of the Metropolitan Gas Act in 1868 jobs were to be had for prominent F.R.S.'s as Gas Referees whose occasional duty it was to monitor the product. The list of men who secured these posts reveals a very catholic spread of backgrounds and dispositions. It includes John Tyndall, William Pole, and A.V. Harcourt. A. W. Williamson was replaced as Chief Gas Examiner by Lord Rayleigh in 1901 when Williamson's eyesight was failing. The duties were light, the post well paid and Rayleigh kept it on until his death.[13] This sort of post was quite scarce and inevitably the gas refereeships and the kindred positions were monopolised by a small group of eminent multiple post holders. After all, it was the aura (and usually reality) of unquestionable authority which such employers sought above all else. Remuneration for the sort of position represented by the gas refereeships could be said to exemplify an intermediate position between the achievement of salaried academic status through an original prominence in pure science and achievement of official scientific recognition for the accumulation of wealth by means of technical facility and industry in a commercial context. Sir William Armstrong provides an instance of a leading industrialist whose material success was marked by admission to the R.S. in 1846, the year prior to the reforms which were intended to make the scientific enterprise its dominant concern. Armstrong was born of a Newcastle-on-Tyne corn merchant in 1810. Having initially trained as a solicitor he was drawn towards things mechanical. In the eyes of contemporaries, Armstrong's fame rested on his work in hydraulics and his invention of the breech-loading Armstrong gun. Few R.S. doors were closed to industrial celebrities when Armstrong was in full prominence and in 1867 he was elected to the R.S.'s Philosophical Club. Armstrong was made a baron in 1887.[14] Ten years later the much honoured man's firm merged with that of Sir Joseph Whitworth.

The often remarked upon estrangement of science and industry during the later nineteenth century is not fully borne out by the career of Whitworth.[15] Whilst at Spezia on the Riviera in March 1884 following a chance meeting with Thomas Hirst the great industrialist invited him to become a co-director of the vast Manchester based manufacturing empire. Whitworth told him that the firm already had Dr. John Hopkinson (F.R.S. 1878), who was Professor of Electrical Engineering at Kings College London, as a director. Hirst wrote in his journal:

> "His notion is that I might make his works and productions better known on the Continent."

Although remuneration was offered in respect of little work, Hirst predictably made no move in the matter.[16] A figure who was very prominent at this time who cannot readily be ascribed to either the group of industrialists who subsequently acquired the formal lustre of scientific connections, or those originally eminent in science who than sought profits, is Sir Frederick Abel. Trained as one of Hofmann's original students, he became the War Department's chemist in charge of explosives for thirty-four years from 1854. Abel became F.R.S. in 1860, won the Society's Royal Medal in 1887 for the invention of gun cotton and was a rather unpopular President of the B.A.A.S. in 1890. He was elected to the R.S. Philosophical Club in 1869 and after further honours was made a baronet in 1893. Able joined the G.C.V.O. one year before his death in 1902.[17] Abel's research group worked at the Woolwich Arsenal. A recent writer on this subject has suggested that the group's rivalry with Nobel was not always conducted on strictly ethical lines.[18] Necessarily the use of scientific knowledge came early to the offices of the military so it is not surprising that Abel's biography offers a prototype of the career applied scientist in the Government service. He worked on the development of cordite with James Dewar and despite inadvertently breaking the windows in

Downing Street with a quantity of the new gun cotton, Abel was later appointed Director of the Imperial Institute. Substantial rewards for technical innovations were sometimes distributed by Whitehall to men for whose applied science was not a systematic career either as an official functionary or as an entrepreneur on their own account. Sir William Snow Harris abandoned the practice of medicine in Plymouth to experiment with electricity. His work produced a new marine lightning conductor which, although it encountered official opposition at first, was accepted by the Navy in 1841 when the inventor was granted a £300 annuity. Six years later Harris was knighted and given a lump sum of £5,000. In 1860 he was made a scientific advisor to the Government.[19]

A careful scrutiny of the available sources reveals a wonderful lack of predictability about the connections between men's original scientific interests in a thing, and the eventual means by which they turned it to profit. John Murray, who went with the "Challenger" expedition and took over from Charles Wyville Thomson the processing for publication of the results, eventually became wealthy by his exploitation of the Christmas Island phosphates. By 1914 the British Government had recouped in taxes from this business a sum equivalent to the whole cost of the original expedition and its fifty volume report.[20] Where the link between science and profit became direct and immediate, secrecy followed as an inevitable consequence. Early in 1854 Charles Wheatstone entered into an agreement with the United Kingdom Telegraph Company whereby he, acting as the company's "scientific referee" would receive £700 per annum for the three year period during which their underground pipe was to be laid along the route of the London-Edinburgh turnpike road. Wheatstone did not want to make a present of his new telegraph machine to the Company and so

forebore to publish anything at all about it.[21] Ten years later Wheatstone was able to command a far more expansive financial agreement with the telegraph company for London. He invested £4,000 and received shares to the value of £10,000, with a claim on more shares to the value of £17,000. The inventor was also to receive five pounds for each pair of instruments sold at £25-30 per pair. In 1864 roughly ten pairs were being sold each week. It is not surprising that Wheatstone's appearances at King's College London, where he was Professor of Mathematics, became rarer and rarer.[22] Success was not inevitable when academic scientists attempted to prosper personally from applications of their science - frequently it occurred that the technically minded opportunist would get ahead of eminent academic authorities in developing a potentially profitable product. The well known cases of Walther Nernst in electric lighting and Marconi in wireless telegraphy were re-enacted many times at less publicly visible levels. The case of John Perry, the Irish physicist turned electrical engineer, prompted the following comment from J. Brown of Belfast to Perry's old Cambridge mentor Joseph Larmor in November 1895:

> "I saw Perry in London very full of his tram. It is a bold idea but I should say expensive and inefficient compared with overhead wires."23

The Boom in Water

By far the most common way for men of science to earn money for applying their technical expertise and scientific knowledge outside the confines of an academic post was to become involved in the burgeoning field of water analysis and its attendant litigation. By 1868 the prominent chemist Edward Frankland was devoting a great deal of his time to water analysis, particularly in connection with the River Commission. Writing to his student H. E. Armstrong who was visiting Professor Kolbe at his laboratory in 1868, Frankland refers

only to the water question, with inquiries about the Leipzig water and its previous state of contamination by sewage.[24] The traditional discharge of sewage into the Thames with its attendant summer stinks prompted the Metropolitan Board of Works to ask Hofmann and Frankland in 1859 to research the best method of deodorizing the water. After a considerable amount of work, also involving W. A. Miller (soon to be elected Treasurer of the R.S.), they recommended the use of Dale's muriate of iron which was a concentrated solution of ferric chloride intended to make the foul part of the sewage into a mud. A controversy developed between the official reporters and Odling and Letheby who stressed the need for dredging the existing mud and the treatment of the water with calcium chloride, which produced neither an additional muddy deposit nor the arsenic which Frankland's opponents declared to be a result of the use of Dale's muriate of iron.[25] The concern with the purity of water turned out to be no fleeting fad. It began with the technical investigation of spa waters and was bolstered by both miasma and germ theories of disease, which laid comparable stress on the importance of the purity of water as a basic requirement of public health. Nineteenth century epidemics systematically reinforced the degree of salience lent to the issue throughout the second half of the nineteenth century. Some time after his first appointment by the London authorities, Frankland was able to charge 100 guineas for a three-day inspection of a sewage farm. The fee included the report but analyses of individual samples were charged for additionally at 5 guineas each, rail and hotel expenses also had to be covered by the client.[26] In 1891 the London County Council applied to the R.S. for help which was given in the form of the Water Research Committee. That body's task was to research the possibilities of improving methods of detection of water-borne pathogens and developing more

effective methods of water treatment. The two principal workers appointed were Professor Marshall Ward and Frankland's son Percy. The inquiry was paid for jointly by the L.C.C. and the Government Grant of the R.S. Marshall Ward and Percy Frankland were both awarded an honorarium of £100 with £75 provided to pay for an assistant. The still unresolved confusion over the relationship between scientific work and payment is well illustrated by two incidents which took place in connection with the activities of the Water Committee's principal researchers. In March 1895 Professor Oliver, Chairman of Board B of the Government Grant Committee of the R.S., wrote to Burlington House to ascertain whether Ward and Frankland's honoraria were to be used for "personal expenses" which was a veiled form of the question of whether they could be seen as wages.[27] Contemporary attitudes to the place of "personal grants" in the distribution of the R.S. Government Grant were decidedly mixed and distinctly held. The socially sanctioned ideal of the British scientific man had not yet shifted from the isolated amateur hero to the corporately diminished academic expert, whose salary was seen as a taint on his scientific accomplishments. Percy Frankland himself surprised Rix by requesting fifty free copies of the Water Committee Report. At the time it was unclear as to whether co-operative research for which remuneration was received could legitimately be counted as one of the means by which personal renown was to be accumulated.[28] At the time of the Water Committee work Percy Frankland was Professor of Chemistry at University College, Dundee. Prior to this he had been working alongside his father who was the Professor of Chemistry at the Normal School of Science at South Kensington. There was a minor furore over the suggested carrying out of "private" analysis work on water samples in the school's laboratory which culminated in 1884 in Edward Frankland's largely

successful defence of his conduct.[29] His son's physical separation became also a theoretical and emotional estrangement from the father, whose conception of water pollution remained strictly chemical while Percy's shifted to a view of water pollution as a bacteriological process.[30] The movement of a large number of scientific men's interests into the area of water analysis inevitably prompted demarcation disputes with the previously established authorities within the fields into which the new water experts had strayed.

An early supporter of the bacteriological approach to the problem of polluted water supply was the Sheffield amateur Henry Clifton Sorby. Primarily known for his pioneering work in petrology and crystallography, Sorby worked from the secure basis of a private income from the proceeds of his family's engineering business. His involvement in a lengthy programme of investigations on behalf of the Metropolitan Board of Works during the period 1882-3, dealing with London's main drains and the state of the Thames, would not have been economically motivated. In a letter to George Stokes of 1883 Sorby stated that he had daily been able to achieve what Dr. Lionel Beale the biologist had told the Royal Commission on the Thames water was impossible - viz. "The detection of 1/1000 th part of a grain of the detritus of human faeces per gallon of water." Sorby declared that: "Chemistry is left behind - it can't distinguish between living and dead matter - between human faeces and living animalcules."[31]

It is probable that the moneyed amateur was attracted to water analysis simply by virtue of the topicality which it acquired due to the attention paid to it by so many of his distinguished London acquaintances. Such attention was very widespread and included such names as John Evans, future Treasurer of the Society, Crum Brown, William Spottiswoode (P.R.S. 1878-1883), William Crookes (P.R.S. 1913-15)

and Henry Roscoe who sided with the younger Frankland against his father on the question of the rôle of bacteriology in water analysis during the 1890's. The infamously foul state of the Thames combined with the periodic deaths of eminent persons (the Prince Consort in 1861 of an illness acquired from bad palace drains, the prince of Wales serious illness ten years later from a similar cause, and the death from typhoid fever of the President of the Royal Society in 1883) and the climactic cholera outbreak of 1866 all served to keep up the momentum of the proliferating field of scientific water studies. The problems of pollution and disease nowhere permitted of a ready solution and so, notwithstanding the intended milestones of the Royal Sanitary Commission of 1869 and the Public Health Act of 1875, the field remained vital and open as an attractive focus of remunerated scientific work up to and beyond the turn of the century.

A material corollary of the rising tide of "paid opinions" about the condition of the nation's drinking water was perceived in the beckoning commercial potential of sewage itself. The oddly disinterested water work of Henry Sorby suggests a possible incongruence between a practitioner's activities and the simple ways in which such activities are often typified by historians of science. Another unusual scientific career of note, one which included numerous attempts to turn a profit from the scientific enterprise, was led by William Crookes. Where most of the consulting water experts limited their involvement to analysis and water deoderization planning for a clientèle of local and national governments and private persons, Crookes did not scruple to become fully involved in an entrepreneurial rôle. As a director of the Native Guano Company he travelled frequently between Leeds, Glasgow, Manchester and Paris. The aim of the company was to extract

fertilizer and clean water from the London sewage. Although technical problems defeated this intention, Crookes received £200 per year until he ceased to be a director in 1880.[32] For a number of years he was also partly taken up with the matter of disposal of the Paris sewage and made frequent visits there on that account. Crookes was particularly anxious to win election to the Acadèmie des Sciences in Paris. In pursuit of this goal he communicated with the Comte de Moniel, asking him to present a note to the Acadèmie on Crookes current sensation, the radiometer. Although the Comte was able to arrange the reading of Crookes' note in February 1878, the ambitious Englishman was not elected until nearly 30 years later. The Acadèmie had long performed the same rôle of scientific consultant to government as the R.S. had done in this country. Indeed Acadèmicians were salaried (and uniformed) servants of their political masters.[33] Despite these circumstances it appears that the ethos of pure science conducted disinterestedly (this usually implies access to private means) formed the ideal French way of doing science. This parallel with the British outlook is surprising in the light of the great institutional differences.

In late nineteenth century France and Britain the paucity of senior academic posts necessitated a broad view being taken of the commercial "wild oats" sown by young men of science. Because it came to be a conventional assumption that commercial involvement did not necessarily mean the violation of a man's intellectual integrity, so the way gradually became clear for the knighted William Crookes, with his checkered history as a trader, to assume the Presidency of the Royal Society in 1913. Apart from his forays into sewage and fishmeal processing, Crookes was advantageously involved in the Alizarin and Anthracene Company and with James Dewar ran the Water Inspection

Laboratory in Colville Road which saw to the inspection of the Metropolitan water supply.[34] This rich source yielded Crookes £400 per annum from the date of their joint appointment in 1892. This amount nearly matched the income Crookes received from the publication of the Chemical News. During 1880, when he moved to 7 Kensington Park Gardens and installed what was among the earliest attempts at domestic electric lighting, Crookes was keenly alive to the possibility of making a fortune by producing an efficient and durable incandescent bulb. Of the outcome his biographer remarked: "In this as in other ventures he fell short of complete success."[35] Later schemes included running his own radium mine, the manufacture of artificial diamonds, and a commercially viable solution to the problems of the wheat grower. Crookes' relationship with Dewar followed a typically Victorian course by finding its way to the courts over the discovery by his son Henry of a way of obtaining metals in a stable colloidal state. Dewar felt his rights had been infringed and Henry Crookes' bankruptcy followed. The younger Crookes worked at the Water Supply Laboratory and when he prepared to market his colloidal metals as "Crookes' Collosols" his father objected, remarking in a letter to Silvanus Thompson of the 7th July 1913 that: "It would seriously damage my scientific position."[36] Having decided not to sue his son in court (one senses that the decision was made by a narrow margin) Crookes was duly accused of being the patentee of a quack medicine when he came under consideration as a possible P.R.S. and thereby unworthy of high office. The crucial point is that neither this nor his full commercial life prevented his election as P.R.S. by the Council in 1913. Perhaps it is not remarkable that Crookes found his way into so many commercial schemes. He never filled an academic post and his staple living came from publication and consultancy. The heavy teaching load which

attatched to many of the newly created academic posts may have precluded their incumbents from deriving much financial benefit from the expanded domain of the consultant and the expert witness. The same restriction seriously curtailed the extent to which many of the ablest young men could involve themselves in the doings of the Royal Society. Edward Frankland, as has been noted earlier, was accused during the late 1870's and early 1880's of seriously compromising the interests of the chemistry department of the Normal School of Science by failing to maintain regular hours at his post, running a nearby private laboratory whilst using the school's facilities and students in order to do private work for profit. In answering the charges in the course of a long letter to Thomas Huxley, Frankland describes how professional consultancy work had become a large demand on the time of a leading group of chemistry professors which he lists as: the later Professors Brande and Graham of the Royal Mint, Allen Miller of Kings College, John Stenhouse of St. Bartholomew's Hospital, Swaine Taylor of Guy's Hospital, Gregory of Edinburgh and Anderson of Glasgow.[37]

The Emerging Rôle of the Expert

Because of the nascent condition of academic science in this country the "Young Guard" of Victorian science found employment opportunities very scarce within their chosen fields. Tyndall and Huxley both contemplated emigration in order to secure academic posts before they were respectively "saved" by the Royal Institution and the Government School of Mines. A host of lesser lights were obliged to go abroad, usually within the Empire, in order to pursue a life of science. This trend is well evidenced in the correspondence of the R.S. which contains an ever increasing number of requests for deferral of formal admission into the Society from newly elected men who did not expect to be back in England again for some time. In these circumstances

of early struggle and hardship it is not surprising that the successful few developed an economic sense which was keenly attuned to the possibilities of financial betterment. In Frankland's letter of explanation referred to above, he stated that he had largely given up the "professional" work of the expert witness and consultant which for the year 1883, brought him in fees only £73-10s for 19 hours' work. Thirty-five years earlier the income potential of this field was outlined by A. W. Hofmann, Frankland's predecessor at the Royal College of Chemistry from 1845-1865, who in a letter of 1853 to Liebig remarked that he could make somewhere between eight and nine thousand pounds per year as a legal expert witness.[38] Although the busy Hofmann did not actually devote himself full time to the rôle of expert witness, he did have a number of other economic irons in the fire. The position of non-resident assayer to the Mint was created under John Herschel's Mastership in 1861. Hofmann followed the joint incumbency of W. Allen Miller and Thomas Graham and upon his return to Germany, Hofmann was in his turn replaced by Dr. John Stenhouse. To the latter's intense chagrin the post with its associated £600 per annum was abolished by the Chancellor of the Exchequer in 1871.[39] Between 1854 and 1856 Hofmann worked with Graham and Theophilus Redwood, a pharmacist, on a form of ethanol which could be used industrially because it would not be liable for excise duty.

The complaints which one frequently reads in the personal records of the time about the gradual giving up of original investigations due to pressure of work upon academic scientists, are not heard in connection with the time demands of expert witness work. William Grove, one of the most active promoters of the R.S. reforms of 1847 who served as Secretary for the first year of R.S. business under the new statutes, had to be set aside from consideration for the

Presidency in October 1872 because it was thought he would not be able to spare the time.[40] As well as being a man of science, "Shady" Grove (as he was latterly known for his sombre valetudinarianism) was very busy as a barrister specialising in patent cases. Walter White reported hearing in 1848 that Grove had not thus far lost such a case. White and his successors in the post of Assistant Secretary to the R.S. were troubled by the growth of scientific expertise as a commodity in so far as they were made responsible for investigating cases of bogus F.R.S.'s tempted into the field to share in the marketability of the hallowed suffix. Traditionally it had been the medical profession which looked to election into the Society as an obvious route to material betterment.

Ten years prior to the inaugural meeting of the X club the R.S. had awarded its 1854 Rumford Medal to a Dr. Neil Arnott for the manufacture of a new kind of fire-grate. Between that date and the end of the century the cult of technical expertise accredited by Fellowship of the Royal Society as guarantor of the participant's standing in the higher realm of pure science grew apace. Even before the middle of the century the leading figures of British science were much sought after for their professional opinions. Grove was already busy in this area, as was Michael Faraday.[41] At the beginning of this chapter a distinction was drawn between Fellows whose election was founded on an original involvement in pure science and who subsequently became involved in manufacturing, providing technical opinions or testimony, and those other Fellows who came much later in life to election into the Society as a potent form of recognition of the practical success of their technological and entrepreneurial activities. A large number of successful candidates of the later years of the century readily fit the latter category. Perusal of the Society's Certificate Book

reveals the frequent operation of a caucus of "rich engineers and traders" who proposed candidates of their own type. By providing the signatures of its own members this caucus was able to raise the issue of electing men without a background in original research. The personal influence of caucus members was frequently able to secure the addition of prestigious manes of eminent men of science to the certificates of reputable entrepreneurs and achieve their election. The Royal Society Candidates Book for 1874-79 refers to Henry Bessemer as the "discoverer of the Bessemer process for making steel." He was proposed by C. W. Siemens, the well-known electrical engineering magnate. Bessemer's certificate was signed by Frederic Abel, the co-inventor of cordite, Sir Frederick Bramwell, the consultant and expert witness, Isaac Lowthian Bell, the Scottish Ironmaster and M.P., Dr. John Percy of the Royal School of Mines, John Hawkshaw, the wealthy and renowned civil engineer, Lyon Playfair, the distinguished promoter and administrator of science and William Lassell whose wealth provided a large telescope for the Royal Society.[42] The first five names in the foregoing list were very reular activists in the entrepreneur-promoting group within the fellowship. The Certificate Books covering the last quarter of the nineteenth century contain numerous evidences of support for non-specific candidates being provided by fellows whose forcefully declared position on the admission of non-scientists appear to have been quite at odds with such actions. A striking example of this is to be found in the case of Robert Giffen (afterwards K.C.B.). Described on his certificate as "Assistant Secretary to the Commercial Department of the Board of Trade. Author of 'Stock Exchange securities: an essay on general causes of fluctuation in their price'." Giffen was proposed by G. J. Goschen, the Chancellor of the Exchequer in Lord Salisbury's late Ministry. Giffen was elected to the R.S. in 1892.

His certificate was signed by W. E. Gladstone, then Prime Minister, Lord Derby, Joseph Hooker, Thomas Huxley, Norman Lockyer, John Lubbock, Lyon Playfair, Douglas Galton, and Frederick Bramwell. The above signed on the basis of their personal knowledge.[42]

To a significant extent the body of "commercial gents" within the Fellowship formed a source of lucrative employment for a group of Fellows whose credentials were founded in pure science.[43] Many of these were leading Fellows with a wide span of personal influence and access to the Council. Despite the intentions of the reforms of 1847 non-scientific men continued to be elected to the R.S. in large and increasing numbers. This trend prompted both Huxley's willingness to stand for election to the Presidency in 1883, and the written expression of his fears to Hooker in 1889 referred to in reference (5) earlier in this chapter. It has been stated in a recent study already referred to that:

> "Eminence in applied science was therefore recognised as the equal of eminence in pure science."[44]

This view overstates the virtue of the Society's role as technical consultant to the Government in order to document a mistaken view of the criteria of eligibility for election. The simple fact is that power and influence in society at large were quite capable of ensuring the compromise of the Royal Society's avowed devotion to pure science.

The continued pre-eminence of credentials in the domain of pure science as the source of scientific legitimacy par excellence is unmistakably apparent in connection with an incident accompanying the award of the R.S. medals in 1870, W. Hallowes Miller, who served as Foreign Secretary to the Society from 1856-1873, was proposed for a Royal Medal in 1870 by Airy, the Astronomer Royal for work on the restoration of the standard weights. Miller was incensed at being put up for a medal for mere applied science:

> "the restoration of the standard of weights, being paid for by the State is withdrawn from the class of labours suitable for reward at the hands of the Royal Society and mineralogy and mathematical crystallography which have occupied the larger portion of the rest of my times are not subjects for which I suppose he entertains any very great respect."[45]

This diminution of his scientific standing really rankled with Miller, to such an extent that after his death his widow wrote to Stokes stating her relief that her husband's obituary in Nature had contained no reference to his work under the auspices of the International Metric Commission. The notice had instead: "[exalted] his crystallographic labours in the way he would have liked best himself."[46] Edward Frankland, whose involvement in original investigations was supplanted by the second career he developed in the field of water supply, had his knighthood announced in June 1897. Lyon Playfair wrote to him regretting that the newspapers had attributed the honour to Frankland's work on water analysis:

> "When given justly because by your splendid researches you have made yourself head and shoulders bigger than your master and all other British chemists . . . ."[47]

Even those who did well out of applied science did not seek to redefine its standing within the scheme of values promoted by the R.S. Frankland himself, in submitting his written proposal of W. H. Perkin for one

of the 1876 Royal Medals questioned the propriety of citing patented discoveries as grounds for medal claims. He restricted his view of Perkin's claims to purely scientific work.[48] The colourful range of honours bestowed upon William Thomson, who combined mathematical physics with the design and manufacture of patented domestic water taps, elicited from R.S. "insiders" responses consistent with those recorded above. This usually took the form of exasperation with the lack of discrimination made by the wider society and its newspaper men between the noble pursuit of disinterested original investigations, and the rather sordid process of turning nature's eternal verities into ready money. The structure of the employment market within science meant that, of necessity, men imbued with a solemn sense of the primacy of the former pursuit would have taken part in the latter one. The tacit convention which licenced such dual rôles is brought into sharp focus by examining occasions when public ignorance of it seemed to menace the clarity of the distinction which it upholds. William Thomson's commercial career has been well-documented in the various biographical treatments of him. It remains somewhat anachronistic to the extent that his family circumstances provided him with that functional corollary of the career of disinterested original research: financial independence. Thomson could well have lived out in full the career of Berman's hegemonic ideal type of heroic gentleman amateur devoted to science "because it is there" from the secure material haven of private means.

It has often been illustrated how official recognition in society at large can be more easily won by tangible, practical achievements. The laboratory which W. Thomson had established at Glasgow in 1850 was not backed financially by the University until the effect of the publicity surrounding the success of the Atlantic cable and Thomson's

part in it in 1866 had altered the outlook of its ruling group. Prior to that date the laboratory had had to manage with what he could spare from his own purse and R.S. grants obtained in his own name. It is hard to escape the conviction that "the inspired schoolboy", as he was termed by Thistleton-Dyer, was never fully aware of any important distinction between disinterested research and applied science conducted for profit.[49] For years the rôle of student in the Glasgow laboratory was inextricably bound up with work for the firm of White which Thomson ran in conjunction with it.[50] Thomson's influential position within the world of pure science and the authority of his 1866 knighthood conferred for "scientific, technical and entrepreneurial contributions", proved potent enough to silence polite R.S. misgivings about the propriety of disbursing money from the Government Grant to such a set up. Thomson's frequent absences from lectures whilst on consultancy business were covered by his son-in-law J. T. Bottomley. The granting of a peerage in 1892 prompted further expressions of disapproval from notable Fellows of the R.S., who evidently found it awkward to cope with a unique case in which the profit-making applied scientist was also the country's leading physicist. Thomas Hirst noted in his journal that Thomson's peerage would have been a new departure in granting high civil honours to scientists were it not that it had been obtained by the direct intervention of Arthur Balfour,

> "and his extreme laudation of Sir William on the occasion of his recent rectorial visit to Glasgow. But in truth Sir William's Telegraph, Marine Compass, and such work, helped more than his purely scientific investigations to obtain for him his Peerage as well as his knighthood."51

The taint of trade on the scientific escutcheon could be far more peremptorily managed where Fellows did not have to deal with anomalies as unmanning as the career of Thomson alias Kelvin. James Glaisher, who retired in 1874 as Superintendent of the magnetical and

meteorological department at Greenwich, wrote in the same year to Stokes describing how he and the Council of the Photographic Society had resigned as a means of protest against the election there of a "shopkeepers Council".[52] In this instance the protest succeeded in rendering the body ungovernable.

Electrical Engineering and Science

The rapid growth of electrical engineering during the last quarter of the nineteenth century brought about a close comingling of the concerns of advanced basic research and those of a rapidly developing new industry. Following the inclination of the B.A.A.S. Committee on Electrical Standards, Maxwell incorporated work on them as a part of his brief at the newly opened Cavendish Laboratory, where Rayleigh, his successor, made them the major preoccupation. Most of the leading mathematical physicists of the day were involved on the work on electrical standards. However, these men did not markedly seek a means of distancing themselves from the implication of devotion to a merely applied science in the manner adopted by W. H. Miller in connection with his work on the standard measures of weight. This would appear to be because the determination of electrical standards was being attempted for the first time, and so the fundamental theoretical issues of electrical science were centrally involved in what at first glance might be thought to be a process of merely technical codification. This somewhat unusual parallel course of fundamental research, and the demands of the electrical engineering industry showed clearly the durability of the R.S.'s formal commitment to pure science. When William Thomson learned of the rejection of one of Latimer Clark's papers on the absolute measurement of the electromotive force, at the beginning of 1873, he wrote to Stokes to tell him that if he had known there might be any doubt about the paper he would have had it

communicated by someone else in the expectation that he himself would have been named as one of the referees. He continued:

> "It would I think be a great discouragement to practical men and an injury to science if when they do something so exceedingly good as this in the way of raising the scientific character of their profession, the Royal Society should throw cold water on their efforts."53

Papers from "practical men" were rarely printed by the R.S. This continued through the 1880's when a majority of the Society's leading physicists were tied up in one way or other with the work on electrical standards. Several significant and well-publicised feuds took place at this time as "practical" men possessing technical experience jock-eyed with theoretically motivated pure scientists for final authority in the still new field. Oliver Lodge, who was not without material ambition in the matter of applied electrical science, went through a quite stormy passage of public disagreements with W. H. Preece over this issue during the late 1880's. Preece was the President of the Society of Telegraph Engineers and Secretary to the Post Office. Lodge maintained that the engineers and Preece in particular did not understand or appreciate the significance of the true nature of the electrical discharges which produce lightning. Lodge's experiments told him that lightning was caused by high frequency AC discharges which necessitated self-induction to be taken account of in the construction of lightning conductors. The climax of the battle with Preece was staged by the B.A.A.S. organisers in early September 1888.[54] The Welshman was William Thomson's oldest friend in the electrical engineering profession and was elected a Fellow of the R.S. in 1881. Such was his power and influence that he was able to suppress the mathematical papers of Oliver Heaviside in the Journal of the Institution of Electrical Engineers (of which more in a later chapter dealing particularly with such matters). With his retirement from the Post Office, Preece set up as a consulting

engineer with a Mr. Cardew at number 8, Queen Anne's Gate in 1899. In the same year he was elected to the Athenaeum.[55] A bitter twist in the unfolding of later events for Lodge, was Preece's energetic promotion of Marconi who was a rival of Lodge in the competition to make an efficient communication system out of the new Herzian waves.[56] William Thomson did not, despite his numerous successful involvements in the commercial application of science, form a very impressive estimate of the industrial potentialities of electrical engineering. In 1882 Alan Campbell Swinton, a young engineering apprentice under Sir William Armstrong, visited Thomson in his Glasgow laboratory. Thomson had just had electric light installed in his home and Campbell-Swinton suggested that electrical engineering might "have a great future and be a useful business to take up". The great man demurred, saying that "it would never be more than a plumber's job". J. J. Thomson related to Swinton how when Kelvin was asked to take a share in the recently formed Marconi Company, he stipulated that the capital should not be more than £100,000 "because no wireless telegraph company can ever want any more money than that".[57] Writing nearly a decade ago J. Heilbron suggested that by the turn of the century the various British academic departments of physics were given over to the production of electro-technologists who were intentionally imbued with a sense of the primacy of international industrial competition rather than a devotion to pure science. He noted that by 1900 18,000 persons were engaged in physics courses in universities and higher schools, of whom less than one per cent went on to do graduate work.[58] It strikes one as being rather odd that only twelve years earlier the young C. T. R. Wilson, having recently qualified by scholarship to go to work as a graduate student at the Cavendish laboratory, should have serious misgivings about the employment prospects of a freshly trained Cambridge physicist:

> "I could not imagine what career I was fitting myself for, as there were remarkably few openings for trained physicists. . . . The prospects of gaining admission into an electrical engineering works seemed rather remote."59

The Royal Society frequently welcomed the new electrical gadgetry to its own apartments, especially on the nights of the soirées. At the same time the Society could remain aloof in the face of claims to its recognition on the part of candidates of a purely electro-technical background. Preece had to wait for an election, until he could be more or less slipped in without taking precedence over a physical scientist of unmistakeable merit.[60] Herbert Rix referred to the Institution of Electrical Engineers' Journal as "a white elephant" in a letter sent from Burlington House in 1890. This seems to reflect a pervading attitude which became conventional within the Society at the time when the "official" status of electrical work was rather ill-defined because of its rapid pace of development. At this time the joint growth of academic electrical science and electro-technical profit-making was at its spectacular height. Six years on, Rix was asked to mobilise some assistance for S. P. Thompson who was going to demonstrate the "new photography" at a meeting. The Assistant Secretary wrote back to him saying that all the arrangements would have to be made between Thompson and the instrument maker from whom he would have to hire his cells. Rix assured Thompson that: "no one here knows anything of electrical science . . . ."[61]
The frequently used dichotomies between pure and applied science, professionals and amateurs, along with the motivational distinctions between seeking mere subsistence, large profits or strictly personal and national honour do not appear to have a great deal of explanatory power when applied to actual cases. The ideal of the gentleman amateur suggested by Berman, which was inclined to produce as its tangible

outcome, the series of isolated scientific heroes characteristic of nineteenth century British science, seems to have been held to more by the salaried young leaders of the cause of scientific naturalism than the noble or moneyed heirs to the aristocratic tradition. Taken as a whole the Cambridge mathematical physicists seem to have been far more at ease with applied science than the young professionals of more modest class backgrounds. Even if this tendency is ignored, the propensity of prominent scientific men to prosper from either the "cult of expertise" or directly from the market-place for the products of applied science appears to vary irrespective of the conventional dichotomies. The unifying element of social behaviour which most forcibly seems to cut across the familiar binary oppositions of the historian of science is the fierce Victorian litigiousness which claimed stupendous personal property rights in all intellectual productions, regardless of whether they generated scientific renown and enhanced social status or simply turned a profit. The phenomenon of multiple discovery was viewed with the gravest suspicion, as much by a worker in pure science like William Crookes during his contested hunt for the new element thallium, as by the multitude of aspiring and actual patentees striving for rapid fortunes. The fragility of the distinction between the two modes of behaviour is well-documented in Frank James' account of Crooke's treatment of the industrially irrelevant thallium as an eminently negotiable commodity in the market-place for recognition and renown on the purportedly disinterested upper plane of pure science.[62] The rhetoric used by contemporary biographers of leading Victorian scientists often unctuously implied that a discovery of some aspect of the inner working of elemental forces was just as much "given to the world" by means of the philanthropic genius of the particular discoverer as was a patented method

for the artificial manufacture of diamonds (another of Crookes' frustrated wealth-generating schemes).

One cannot readily avoid the conclusion that eminence in applied science was held, by convention amongst those with effective authority within the R.S., to be vastly inferior to eminence in pure science. However, it clearly became admissable to pursue parallel rôles without forfeiting status in the superordinate domain of pure science. Conflicts inevitably occurred between mutually contradictory expectations confronting the same man occupying two differently defined rôles at the same time. When this gap seemed to yawn too wide to sustain the credibility of the man concerned, remedial actions of concealment and redefinition of the meaning of his commercial productions might be attempted. In this way the more Baconian interpretation of some instance of applied science would be stressed at the expense of its vulgar commercial counterpart. After the death of Kelvin, who rather naively saw no harm in marketing his patent non-leaking water taps as the special creation of the current P.R.S., Archibald Geikie sent a word of warning to Joseph Larmor who was preparing Kelvin's obituary notice for the next number of Proceedings. Larmor took a rather different view of the situation, and was more at pains to avoid offending the votaries of technological progress than the pious preservers of the purity of original investigation. This was perhaps due to the special eminence of Kelvin, whose standing in the international field of mathematical physics passed unquestioned. On the 7th June 1908 Geikie wrote:

> "It strikes me that if you could condense into a paragraph or two mere ennumeration of the more notable of his inventions you would allay possible criticism. (I wouldn't include the patent water taps!)"63

As a representative of the Great Western Telegraph Company, Thomson as he then was, had contacted the Admiralty in 1872 in order to have a line of soundings made from the Azores to Bermuda as a part of the initial phase of the "Challenger" expedition. Since the success of the Atlantic cable five years previously, quickening Government interest had made it increasingly likely that funds would be found to despatch the ostensibly oceanographic expedition. In a letter to Stokes regarding the matter, Thomson appears quite unabashed at his declared commercial interest:

> "For the sake of the Great Western Telegraph Company, of which Professor Jenkin and I are engineers, I hope this may be done."64

The official R.S. view of the expedition was, and is today, that of an entirely noble venture conducted on behalf of the boundary-less Republic of Learning. So far as one can tell no record was made by the R.S. of this surprising extra-scientific character of the expedition.

* * *

Notes

1. Royal Society, MC 11, no. 108.

2. Morris Berman, "'Hegemony' and the amateur tradition in British Science", Journal of Social History 1975, 8, 30-50, ff. 38.

3. Ibid., p. 33.

4. Royal Society, Manuscript General Series Catalogue.

5. Huxley Papers (ICL), 2.340.

6. Archibald Geikie, Annals of the Philosophical Club, London, 1917, p. 79.

7. Alan A. Campbell Swinton, Autobiographical and Other Writings, Longmans Green and Company, London, 1930, pp. 60-62.

8. Hirst Journal, 2540.

9. Ibid., 2599.

10. A. S. Eve and C. H. Creasey, Life of John Tyndall, Macmillan, London, 1945, p. 261.

11. Walter White, The Journals of Walter White, Chapman and Hall, London, 1898, p. 112.

12. Marie Boas Hall, ASN, p. 125.

13. Robert John Strutt, Life of Lord Rayleigh, Arnold, London, 1924, p. 294.

14. Geikie, ~~ibid~~., p. 600.

15. Nature, 1900-01, 63, p. 135.

16. Hirst Journals, 2139.

17. Geikie, ~~ibid~~., p. 69.

18. W. H. Brock, "The Spectrum of Science Patronage", G. L'E. Turner (ed.), ~~ibid~~., p. 181.

19. Geikie, ~~ibid~~., p. 12.

20. Margaret Deacon, Scientists and the Sea 1650-1900, Academic Press, London, 1971, p. 388.

21. White's Journal, ~~ibid~~., pp. 108, 113.

22. Ibid., p. 187. Wheatstone left over £70,000 in his will.

23. Royal Society, Larmor Letter, La 603 (1), vol. A-B, no. 158.

24. Royal Society, MM 10, no. 92.

25. Brock, ~~ibid~~., pp. 182-183.

26. Royal Society, MM 10, no. 98.

27. Royal Society, NLB 7, no. 305.

28. Royal Society, NLB 6, no. 204.

29. O.U. microfilm of Frankland Papers (RFC).

30. C. A. Russell, "Percy Frankland: the iron gate of examination", Chemistry in Britian, 1977, 11, p. 425.

31. Stokes Papers (CUL), S 1194. On Sorby Norman Higham, A very scientific gentleman. The major achievements of Henry Clifton Sorby, Oxford, 1963.

32. E. E. F. D'Albe, The Life of Sir William Crookes, Fisher Unwin, London, 1923, p. 259.

33. Hahn, op. cit., pp. 304-307. In 1880, the Academy gave Crookes a prize of 3000 francs for the radiometer.

34. D'Albe, ~~ibid~~., p. 377.

35. Ibid., p. 302.

36. Ibid., p. 392.

37. O.U. microfilm of Frankland Papers (RFC).

38. Brock, ~~ibid~~., p. 187.

39. White's Journal, p. 233.

40. Stokes Papers (CUL), S 589.

41. Morris Berman, op.cit., p. 34.

42. Royal Society Certificate Book.

43. It tends to be assumed that the pure vs. applied science distinction is modern and that is then distortingly projected on to the nineteenth century situation. However, the distinction was clearly drawn by a number of prominent participants of the time. For example, in 1884 Helmholtz in a letter to his wife expressed his disapproval of Sir William Thomson's technological involvements concluding that: "On the whole however, I have an impression that Sir William might do better than apply his eminent sagacity to industrial undertakings", (Leo Königsberger, Herman von Helmholtz, translated by Frances A. Welby, Clarendon Press, Oxford, 1906, p. 349). Thomson's keenest supporter, P. G. Tait also disapproved of Thomson's many forays into the commercial applications of science. Tait saw such activities as tainting a scientific reputation (Romualdas Sviedrys, "The Rise of Physics Laboratories in Britain", Historical Studies in the Physical Sciences, 1976, 7, 405-437).

44. Hall, ~~ibid~~., p. 145.

45. Royal Society, Sabine Letters, Sa. 881

46. Stokes Papers (CUL), M 525.

47. O.U. microfilm of Frankland Papers (RFC).

48. Royal Society, Medal Claims 1873-1909 , 1887 pp. 6-7.

49 Huxley Papers (ICL), 27.232.

50. Sviedrys, ~~ibid~~., pp. 413-414.

51. Hirst Journals, 2854.

52. Stokes Papers (CUL), 9 180.

53. Stokes Papers (Cul), K 188.

54. W. P. Jolly, Sir Oliver Lodge, Constable, London, 1974, p. 83.

55. E. C. Baker, Sir William Henry Preece:Victorian Engineer Extraordinary, Hutchinson, London, 1976, p. 307.

56. A. G. Keller, The Infancy of Atomic Physics, p. 93.

57. Alan A. Campbell Swinton, Autobiographical and Other Writings, Longmans, Green and Co., London, 1930, p. 62.

58. J. Heilbron, "Fin-de-siecle physics", C. G. Bernhard, E. Crawford, P. Sörbom (eds.), Science Technology and Society in the Time of Alfred Nobel, Pergamon Press, Oxford, 1982, p. 63.

59. C. T. R. Wilson, "Reminiscences of my early years", Notes and Records of the Royal Society, 1959-60, 14, 163-173, ff. 165.

60. Preece wrote to Spottiswoode in April 1880: "I was sorry not to see my name among the honoured 15 the other day. But it is not in mortals to command success and we can but try and deserve it." (Royal Society, MC 12, no. 42).

61. Royal Society, NLB 12, no. 166.

62. Frank A. J. L. James, "Of 'Medals and Muddles' the context of the discovery of thallium: William Crookes' early spectro-chemical work", Notes and Records of the Royal Society, 1984, 39, no. 1, pp. 65-90.

63. Stokes Papers (CUL), L 106.

64. Stokes Papers (CUL), K 185.

CHAPTER FOUR

REFEREES AND PAPERS

The General Background

The mutually reinforcing elements of power and exclusivity were ever present within the fabric of the Royal Society. After the successes of the reforming party in 1847 the arena within which these forces defined rank and worth became formally scientific. Following from that, the specific procedures by means of which scientific performance was evaluated by its leading group of practitioners took on an increasingly crucial significance. The Royal Society's Assistant Secretary Herbert Rix noted that the first record of the referral of a paper by the Society relates to a paper by a Mr. Ludlow which in May 1780 was sent to Mr. Cavendish and then to Dr. Hutton. The next paper to be formally refereed was by the ailing Sir Humphry Davy whose work went to Faraday nearly fifty years later. Shortly afterwards the practice became common for papers appearing in the Phil. Trans. The Proceedings remained a more accessible mouthpiece for most of the Fellows for whom it continued to be of considerably less significance as a source of kudos than its venerable predecessor. In 1890 a prominent Cambridge mathematician told the reclusive (but brilliant) amateur Oliver Heaviside that "there was a sort of tradition that a Fellow of the Royal Society could print almost anything he liked in the Proceedings without being troubled by referees."[1] The ruling group which was attempting to sort out the publication problems of the Society during the century's last two decades did not assist themselves by ignoring this convention. From 1887 onwards the practice of placing a letter P beside the names of Fellows whose work had appeared in the Phil. Trans. was discontinued and all papers became, in principle, subject to the strictures of men described as "independent referees". Rix's verdict on this attempt to legislate an equality

of prestige for the two publications was a discreet: "We shall see."[2]

The official version of the nature of its basic enterprise which an organisation publicises is very often different from the informal, private disposition of its ruling group and leading practitioners. In the same way that politician's personal ambitions traditionally masquerade beneath the hallowed banner of the National Interest so the public image of the R.S. laid stress on the promotion of natural knowledge and its central rôle in the interplay of science and government. Rix's article on the R.S. for the popular magazine Leisure Hour of 1896 clearly shows up this concern to gloss over individual ambition with its sharp appetite for fame and material reward, in a grand posture of dutiful rectitude in the service of the State. Rix places the mediation with Government at the head of the list of R.S. functions with the presentation of individuals' scientific productions at its foot. By this means Rix assured the readership of Leisure Hour that the latter function was of less importance even than the Society's part in Greenwich Visitations.[3] The last decade of the century was a time of flux in the career of the R.S. marked by considerable soul-searching on the part of influential Fellows, public criticism in The Times following the Anniversaries and the production of a series of schemes for its radical modification. This will be given more detailed consideration in a subsequent chapter. Here it is sufficient to note the outline of a great divide between the Society's actual preoccupations and its earnestly depicted aura of corporate altruism. Ultimately, a scientific organisation claiming to contain the entire élite of a nation's scientific workers, lives out its pre-eminence in and through the internationally judged quality of the scientific performances of its individual members. The

semi-bureacratic function of mediation between the scientific establishment and Government cannot in itself legitimate that pre-eminence. Consequently, the way in which individual scientific performances were evaluated is of crucial importance for any detailed consideration of the inner workings of a body such as the R.S. Two decades ago Ziman made the simple but vital observation that a published paper is not a mere collection of facts and its author's opinions. He continues:

> "it bears the imprimatur of scientific authenticity, as given to it by the editor, and the referees he may have consulted. The referee is the lynchpin about which the whole business of Science is pivoted."4

The formal denial of this (which appears in the front of many scientific journals in the practical guise of an editorial disclaimer) might well be taken as a confirmation of the sociological significance of Ziman's above contention.

Thomas Huxley commented with disarming candour on his own self-seeking part in the interplay of public and private scientific purposes in connection with his first attempt at public speaking at the Ipswich B.A.A.S. meeting of 1851. He wrote that his efforts were:

> "not by any means to advance science, but to be 'advanced' myself - by getting the association as a body to recommend Government to publish my work."5

Refereeing was confined within a relatively small group of men appointed by the R.S. Secretary in charge of papers and (until the reintroduction of sectional committees in 1896) the Council sitting as the Committee of Papers. This tendency operated throughout the second half of the nineteenth century, although the group of most frequently used referees did increase somewhat in line with the growth of the scientific business of the Society. Between January 1859 and February

1862 a total of 262 papers were sent to referees. These were in the main intended for the Phil. Trans., as it was not until much later that refereeing was made mandatory for papers intended for the Proc. Of these 262 papers, 166 were refereed by a group of just 17 men. They were as follows:[6]

Referee	Number of Papers Refereed	Referee	Number of Papers Refereed
A. Cayley	17	T. H. Huxley	9
W. Spottiswoode	14	W. B. Carpenter	8
G. Stokes	13	W. H. Miller	8
A. W. Williamson	12	W. Thomson	7
W. A. Miller	11	G. Busk	7
B. Price	11	G. Boole	7
G. B. Airy	10	C. Wheatstone	7
J. Tyndall	10	J. Paget	6
E. Frankland	9		

Taken together with William Sharpey the other Secretary, Edward Sabine who was P.R.S., and (nominally) the residual members of the Council, this group was responsible for determining the form of scientific work in Britain at this time. Although 96 papers refereed during the period in question went to other Fellows, it appears from perusal of the R.S. Register of Papers that those of a seemingly controversial nature were almost invariably sent to a member of the central refereeing caucus. The approbation or otherwise of the R.S. for the work of aspiring young men was crucial in the cause of bringing their embryo careers to fruition. Rejection by the Society, if consistently applied to a man's papers, meant a public exclusion from the favour of what was far and away the most potent source of scientific legitimacy. Joseph Hooker alluded to the effect which this circumstance had on the young Charles Darwin when a paper of his on optical subjects was rejected:

> "[The paper] which, being rejected, disgusted him and led to his stifling his own early scientific tendencies and scoffing at those of others."7

Huxley, the recipient of Hooker's letter referred to above, had particular cause to be grateful to these R.S. patrons of science. They printed his papers in the Phil. Trans., secured Government money by their influence to pay for the work on his "Rattlesnake" material, elected him to the Fellowship at the very early age of 26 in a very strong year group of candidates and presented him with a Royal Medal for his paper on Medusae in 1852 having narrowly failed to procure for him the award for the preceding year. Of the medal Huxley said many years later that it had, "determined my career."[8] The year 1854 saw an end to Huxley's contemplation of emigration as the only means of pursuing a career in science when Sir Henry de la Beche offered him a job as palaeontologist and lecturer in Natural History at the School of Mines in Jermyn Street. This flourishing did not come about without a good deal of manoevring on the part of the man himself. Huxley's efforts to establish for himself a professional life in science are described further in a subsequent chapter.

"A Truly Scientific Society" (M. B. Hall)

Running through the few existing accounts of the R.S.'s function during the second half of the nineteenth century there is a dominating supposition that the reforms of 1847 marked a watershed in the scientific character and putatively the moral career of the Society. This convention identifies dilettantism with subterfuge and skulduggery in the unreformed Society counterposed to a scientifically derived rationalism as the driving force of the just and meritocratic Society created in 1847.[9] The simple assumption that the Society's procedures became open, fair, and devoid of ulterior motive as a direct consequence of scientific men coming to dominate the Council has seriously

disabled the attempts of the writers concerned to make any historical sense of the Society's doings. The notion of scientific rationality projecting itself from whatever position it might happen to occupy in the abstract domain of method, out to every department of a scientific man's mental economy and social behaviour was a popular one during the second half of the nineteenth century. This notion was keenly adhered to by that assortment of participants who have subsequently come to be known as scientific naturalists. Whilst that fact in itself is of significance, the adoption of the same assumption by modern historians of the period must inevitably obscure the situation which actually developed at Burlington House after the turn of the half century. The conventional axiom of the uniformity of the natural world can be taken to imply a unity of approach to the scientific study of it. When the train of objectivist assumptions is taken to its utmost and the behaviour and thought of each scientist is conceived of as an analogue of rational Nature, then the hubbub of fierce controversy and personal acrimony which marks Victorian science throughout its form and content becomes all but incomprehensible. Scientists occupying powerful positions within the R.S., whose continued potency and eminence has ever required that they bolster their vested interest by consistently reasserting the open rationality and "scientific" impartiality of the Society's practices, have not surprisingly been given to stress the historical nature of bias. In his printed evidence in connection with a case which is later (in this work) dealt with in detail, Arthur Lynch notes that jobbery and suppression although quite as prevalent in 1921 as they had been in 1889, were nevertheless held up at any particular time to be things of the past.[10] The embrace of this rather commodious anodyne is well-exemplified in the work of the R.S.'s most recent chronicler. In her treatment of the maverick

Manchester Fellow Henry Wilde's charges against the President Sir William Thomson in October 1891, Hall assures the reader that by 1891:

> "all papers were carefully refereed, so that it was most unlikely that Kelvin had anything to do with the rejection of Wilde's papers."11

"Careful refereeing" is a process which can be interpreted in a number of widely divergent ways. That it could and did take on a starkly malign significance for many young authors who were not yet established within the internal hierarchy of the incumbent scientific establishment, I shall be seeking shortly to establish. It has been reported that this particular type of "care" was so diligently exercised by one prominent Cambridge Fellow just following the turn of the century, that he felt constrained to telephone the Secretary R.S. every time he heard of the imminent presentation of a paper by a man whose views did not coincide with his own, requesting that the paper be sent to him immediately for refereeing.[12] Regarding the specific case of the P.R.S. and Henry Wilde, suffice it to note that the two men were well-known to be opponents in the theoretical field of electricity and magnetism. The Secretary R.S. in charge of papers at the time was Lord Rayleigh who shared with Thomson the background of Cambridge in general and its community of mathematical physicists in particular. Rayleigh sent Wilde's 1890 paper straight to Thomson for refereeing. He subsequently passed it on to Professor Perry and Arthur Rücker who were lesser lights but who had both gained from being within the future Baron of Largs' sphere of influence. One need not assert the contemporary viability of Wilde's electro-magnetic researches in order to identify a situation calling for further attention. The interlocking scheme of tacit conventions which appear to have regulated the refereeing of papers submitted to the Royal Society can only have provided the maximum free play for the consolidation of nearly inviolable orthodoxies

within the various sub-divisions of knowledge.[13] Having had his April 1890 paper archived, Wilde submitted another in November of the same year which was archived by Rücker (who succeeded Rayleigh as Secretary R.S. in 1896) and W. E. Ayrton, the inseparable friend and colleague of Perry. Ayrton's scientific career was of such a form as to render him another minor satellite in the firmament of Cambridge mathematical physics.[14]

It is axiomatic within the terms of the argument which I am presenting that the personal integrity of the individual participants who have found themselves the custodians of any particular normal science tradition at any particular time, is not the matter at issue. Individual sincerity, if it were suggested as the demarcation criterion between scientific bias and scientific justice, would necessarily become the only agent capable of vitiating the perfect, self-regulating version of scientific rationalism publicised as the inner workings of the post-1847 R.S. Therefore it is reasonable to see as unexceptional the righting of past wrongs by an individual fully taken up by the cause of enforcing the writ of the normal science tradition of his own day, within which his own reputation has been built and maintained. It will be amply clear from the foregoing that the well-known work of Thomas Kuhn provides the tenor and terms of this argument.[15] A number of obscure precursors of his overall position appear in the literature, to which might be added the name of Arthur Lynch. In the context which I have already mentioned above, he outlined the processes through which orthodoxy was maintained, specifically in connection with the R.S. itself.[16] The most well-known case of the R.S. making belated redress for the suppression of scientific work which subsequently assumed undeniable stature, took place by the agency of Rayleigh. One day, whilst looking through some of the records of the

R.S. he noticed a paper received on December 11th 1845 from J. J. Waterston entitled: "On the physics of media that are composed of free and perfectly elastic molecule in a state of motion". The paper had been read on the 5th March 1846, and subsequently archived by the Council sitting as the Committee of Papers. One of the referees in his report stated that:

> "The paper is nothing but nonsense unfit even for reading before the Society."17

Upon rediscovering the paper, Rayleigh's necessarily modern physical world view discerned a pathetically unheeded anticipation of James Clerk Maxwell's then well-established kinetic theory of gases. Having been refereed by Rayleigh himself and the current P.R.S. William Thomson, the paper was printed in the Phil. Trans. for 1891. In keeping with the trend of this discussion so far, I would in no sense conclude that Waterston's paper need necessarily have been the victim of incompetent or dishonest refereeing. It so happened that twenty-five years before Waterston submitted his ill-starred offering, John Herapath had sent in to the Society a paper which contained the first extensive working out of the calculations and applications of a kinetic theory of gases and was much later acknowledged as such by Maxwell. Stephen Brush has explained the rejection of Herapath's proffered addition to knowledge on the basis of its grandly theoretical scope which made scant appeal to his empirically minded British colleagues. The Society's doyen, Davy, was largely responsible for the non-appearance of Herapath's work according to Brush. Davy took the chair at the beginning of November of the same year, 1820.[18] Herapath withdrew the paper and eventually got it printed in Annals of Philosophy for 1821, although its impact on the practice of British science was minimal. He fetched up at length as the editor of a railway magazine.

There is a vast gulf lying between the interpretation of past scientific performances as part of a conflict of vested theoretical interests on the one hand, and the same developments within science viewed as a heroic procession of history-less rationalism on the other. The practical implications of this potential misalignment between image and substance touched Rayleigh himself on a curious occasion when his name had become deta ched from a manuscript which he had submitted to the Committee of the B.A.A.S. According to Barber:

> "The Committee turned it down as the work of one of those curious persons called paradoxers. However, when the authorship was discovered the paper was found to have merits after all."19

Once a paper had been placed (it was no accident) in the hands of a referee whose vested interest in the existing theories within the field concerned would dispose him to produce an unfavourable report, that fate of a paper from an unknown author was usually sealed. Writing in 1970 of recent times R. D. Wright provides a fairly accurate account of the range of possibilities used by the "wrecking" referee of the late nineteenth century as well. The three most common methods of discrediting a paper he cites as:

i) Setting up a "straw man" as though it represented the author's position and then showing it up as a fallacy.

ii) Suggesting that an author's ideas were confused.

iii) Accusing an author of unfamiliarity with previously published work, without specifying what this was.20

In a climate where these techniques were commonly practised, it is not surprising that authors frequently complained that referees stole ideas from them while their papers were scorned in the official reports. That a man's paper might be sent to a personally known rival was not likely to be a rare occurrence in view of the small number of active practitioners in each sub-department of Victorian scientific activity.

That this pattern of highly "interested" refereeing occurred so consistently in connection with papers sent to the R.S. requires a more detailed consideration of how referees were appointed, and by whom. The Secretary in charge of papers for 31 years from 1854 was George Stokes, whose narrow, guileless conduct in all worldly affairs was widely remarked upon by his contemporaries. His daughter related an incident which is very revealing of this vital aspect of Stokes's character, as part of a memoir of her father which appeared in the only major work covering Stokes's life and work produced by Joseph Larmor. Isabel Humphry, in evident admiration of her father's otherwordly social demeanour, describes how a visitor called one day at Burlington House to discuss the publication by the R.S. of a friend's work:

> "It was interesting to hear the different lines of argument, my father's very simple, quietly and calmly repeated opinion, that the work was not good enough, the visitor's varied arguments finally becoming rather heated. When the visitor, finding it useless had gone, I asked him if he had happened to notice that the man had left in a very bad temper."

Stokes then appeared to notice this as a complete novelty saying:

> "I thought that he seemed a little warm, but that he could not possibly be angry with me about what was purely a matter of business."21

Despite his standing as the publication shy understudy to Thomson and Maxwell at the head of British physics, it is difficult to escape the widespread contemporary notion of the Society's longest serving Secretary as an amiable blunderer in any matters other than those of pure physical science. Michael Foster (Secretary R.S. 1881-1903) wrote to Huxley in November 1886 when poor health had forced the latter's retirement as P.R.S., of the best official course to take in matter of marking the Queen's Jubilee in the following year. Of Stokes's administrative acumen and capacity to take an effective part

in the R.S.'s internal political machinations, Foster seems to have formed a remarkably small estimate:

> "I have mentioned it to Mumbo Jumbo - but that is not much use."22

At the time Huxley had been retired, and Stokes the P.R.S. for nearly a year. To have relied so heavily on the discretion of so artless a Secretary for papers over so long a period must have given a good deal of latitude for the exercise of personal prejudice by ill-chosen referees. This form of prejudice must then be added to the inbuilt scientific prejudice which the terms of my earlier argument seeks to define as the derogation of the ideal Mertonian precept of disinterestedness. Meadows had maintained that scientific prejudice had consistently outweighted personal prejudice as a criterion in the official evaluation of scientific performances.[23] An examination of some actual cases of R.S. refereeing seems to indicate that the system in actual operation does not suggest any means by which these two mutually interactive elements might be disentangled.

Safe and Unsafe Papers

What will be considered in this section forms the obverse of Merton's often quoted but hardly documented "Matthew Effect".[24] The R.S. records for the late-1880's and early-1890's contain frequent requests from Herbert Rix to Michael Foster for the latter to mark the "safe" papers on a list so that they can be set up in type for a forthcoming ordinary meeting. In this way papers from men of established reputation were stamped with the official seal of approval before discussion at the meeting or formal refereeing had taken place. The unknown man's route to the pages of the Society's journals was far more steep and perilous, regardless of the apparent merit of his work vis à vis that of a well-known contributor. Even if a referee's dearest wish was to annihilate a paper by the issue of a swingeing

critique, the wish would usually be compromised where the paper's author could claim the allegiances which attach to eminence. It appears to be the case that authors who were a part of the leading group of practitioners frequently got to know by one means or another whom their papers had been sent to for judgement to be passed. This opened the possibility of reprisals being taken on a man who became known for too free an indulgence of his critical faculty. There again, the referees of a particular paper usually were aware of each other's identity from which inevitably developed a further point of access for the sirens of conformity.

An interesting example which vividly illuminates several of these tendencies is provided by the report written by John Tyndall on an 1855 paper of J. P. Joule. In the first instance it is more than a little surprising that Joule's paper should have been sent to a man who, a few years after this incident, was to achieve his first success in a lengthy campaign to secure for J. R. Mayer much of the renown which might otherwise have been due to Joule for priority in the matter of the first law of thermodynamics.[25] Tyndall's 1855 report reflects its author's determined attempt to diplomatically present his objections to a particular instance of Joule's science. Cagily, he hedged his bet by adding a paragraph of self-abasement which might spare him some of the active disapproval of powerful others who were Joule's supporters in the wider struggle for his international reputation. One such was William Thomson who was used to being appointed as his friend and collaborator's referee by Stokes. Tyndall concluded:

> "What I formerly stated has not been rendered quite negatory by recent alterations. Mr. Joule's eminence as an experimenter might be reasonably accepted as a sufficient guarantee that his communication is fit for the Philosophical Transactions though I confess my own opinion is that the reputation of Mr. Joule would not be increased by the publication of the paper. ... I say this with extreme reluctance simply because it is my unpleasant duty

> to say it, and I hope the council will bear in mind in forming their estimate of these remarks, that Mr. Joule is a far deeper labourer in the domain to which his paper refers than I am."26

Here the writer was attempting to thwart an established man of science who was more powerful than himself, who as I have said, had the collaborative support of William Thomson and thereby the force of Thomson's wider sphere of influence. This Tyndall sought to achieve in the cause of Mayer who had been kept in relative obscurity by the scientific world at large which did not care for the metaphysical style of his papers. These were seldom published or if they did appear, were met with indifference. Where a response did appear he was refused leave to reply, once by Liebig as editor of his Annalen. Mayer's misfortunes grew after 1849, he lost two children and his father, eventually suffering a mental illness which prompted him to throw himself from a second-floor window.[27] After a partial recovery he was committed to an asylum for two years. Mayer came to rest working in his vineyard and although lost to the cause of active scientific work he received a belated abundance of scientific honours. By dint of Tyndall's toils he received the Poncelet Prize, the Copley medal, and election to a number of foreign academies. Tyndall's skirmishings with Thomson and P. G. Tait over the division of the fame generated by the elaboration of the first law of thermodynamics rumbled on for many years. As time went on Tyndall consolidated his own position as a leading member of a powerful group within the R.S., so the need to stoop so as to limit the possible damage to himself as revealed in his 1855 referee report on Joule, gradually diminished.

Relatively unknown authors faced a quite different set of tacit conventions governing the treatment of their papers. They posed little or no foreseeable threat to the scientific careers of their referees through the mobilisation of networks of influence. The

little known or wholly obscure scientific author could only acquire such a status by having their own opinions embraced fully by the Society via its appointed referees. Where those opinions flew in the face of the conventionally acknowledged views of referees, views by means of which those referees' credentials as evaluators of new knowledge had been acquired and maintained, the outcome was usually (and understandably) the radical curtailment or outright rejection of the papers concerned. The pretext often used to explain the rejection of a paper from a relatively less well-known author was based on the notion of its being "too controversial". The outraged protests of such silenced contributors were rarely so lucid and penetrating as those of Alexander Ellis, a Fellow whose paper was excluded from the Proc. in 1873 seemingly because his views fell athwart those of the late George Boole, one of the Society's favourite mathematical sons. To no avail did Ellis point out that "doing science" must perforce reach beyond the respectful reiteration of received wisdom, however illustriously authored:

> "It is almost impossible to proceed in science without controverting former opinions. . . . When I recall the controversial papers of Professors Owen and Flower, which almost verged on personality and yet were printed, I am at a loss to understand how my very abstract work could be, as you say it 'appears' to be 'more controversial in character than suits the Proceedings'."28

The most cogent argument which Stokes could devise to explain this cavalier dismissal of Ellis' work was that if the paper were to be read at the R.S. or printed in the Proc. it would prevent some "defender of Boole's views" from being able to reply. This hardly accords with the common notion of the Proc. as a fairly open forum for Fellows' views which until December 1894 were not necessarily refereed. (As has been described in a previous chapter, following that date Rix was required to achieve parity of prestige between Phil. Trans. and Proc. and speed

up the regular production of the latter by introducing a long refereeing delay into the processing of each paper. He retired shortly afterwards due to the rapidly mounting pressure of work.) In the case of Ellis, it seems clear that the "defender of Boole's views" was the Secretary R.S. George Stokes himself. He strove to suppress controversy and succeeded except where the reputation of those involved empowered them to break by other means the silence which R.S. rejection was often intended to impose. As I shall describe in much greater detail later, Stokes participated in the scientific work of Fellows to a prodigous extent. He laboured tirelessly over editing papers, in the inspiration and direction of which he had himself (by correspondence) been the leading light. Stokes seemed to want the R.S. publications to contain only completed truths which could not be sullied by competitive gainsayings. What Ellis went on to say should scarcely have formed a novelty for the chief arbiter of scientific taste within its supreme national forum.

> "It is very common for papers which are read before societies to become the subject of comment in journals. It is one of the purposes of their being read before societies that they should be discussed before those societies. If Fellows are precluded from having papers read which might call for discussion, it appears to me that much of the use of scientific societies would be lost."29

The official avoidance of controversy did not of course reduce the Society's journals to the level of mere repositories of factual observations. Such Baconian or Humboldtian pipe-dreams received a good deal of pious lip-service but few young authors ambitious enough to thrust themselves forward into the R.S.'s ambit could so meekly still their voices. In a case such as that of the struggle between Owen and Flower mentioned by Ellis, the influence of both combatants was sufficiently great and nearly equal that the R.S. sponsored fiction of scientific equanimity was broken through entirely. For most cases, however,

and all those in which young workers necessarily approached Stokes in the situation of a patron-client relationship, the ex-Senior Wrangler and First Smith's Prizeman's magisterial gravitas compelled acquiesence in the matter of controversial opinions. Their deletion was commonly made the price of publication by the R.S. where a refusal to make such modifications would result in complete rejection. This naturally resulted in a host of emasculated papers being published without the opinions of the author or with a set of opinions provided by Stokes and lightly disguised as those of the author. Frequently when such men made further contributions obligingly free of "controversial opinions" or "speculation" as theoretical statements came to be known during the time of Edward Sabine (he served as Secretary from 1827-30, Foreign Secretary from 1845-50, Treasurer from 1850-61, and President from 1861-71), their work was refused as containing "insufficient novelty". The example of J. Bowerbank's travails with the management of the R.S. brings together in one illustration the complex workings of several of the informal procedures under discussion.

Keeping Controversy from View - The Career of Two "Unsafe" Papers

In 1862, J. Bowerbank, an amateur zoologist of orthodox religious persuasion, made a concerted attempt to overturn the dominant position of three leading scientific naturalists within the Ray Society. Bowerbank wished to see a proper investigation of the misappropriation of upwards of £1,000 of the Society's funds and various other aspects of its mismanagement. He named George Busk and Thomas Huxley along with Mr. Lubbock as:

> "the most energetic and active opponents to my exposure of these said affairs. . . . Now I ask whether in the midst of such an angry and exciting contest it was just towards me to select my most determined opponent as the referee to report on the "eligibility for publication" of the second part of my paper? Any other gentleman under such circumstances would have paused before he undertook the office, but no

> such scruples animated Professor Busk, the opportunity was too good to be lost, and ill-feeling generated in the Ray Society was promptly imported into the proceedings of the Committee of Papers of the Royal one."30

Bowerbank continues (in the course of a substantial document addressed to the President and Council of the R.S. in 1862) by describing how at an earlier meeting with Busk the latter told him: "I do not pretend to anything like the intimate knowledge of the Spongiadae that you possess." Bowerbank than forcefully protests that: "after this declaration his opinion on these subjects are to be made by a compulsory edict of the secret and irresponsible censors of the Royal Society."

The other referee was a Dr. Grant who met Busk and Bowerbank by chance at Burlington House on the 15th of January 1863. Grant is alleged to have declared emphatically that paper should retain the "Sarcode" section which Busk insisted should be omitted and that the author could pass this testimony to the Committee of Papers. Bowerbank deliberately kept hold of the revises of his paper so that the Committee could reconsider, staunchly maintaining that if the decision was not reversed he would pay from his own pocket for the printing of the paper as a facsimile of the Phil. Trans. layout containing the omitted section and full documentary coverage of the controversy, which would be sent to every library in Europe. Notwithstanding a niggling insistance that in twenty-three instances the author must change his use of "homologous" into "analogous" after the manner of Richard Owen, the Council saw fit to reverse its decision and print the paper. Looking at the wider background it begins to emerge that from the mid-fifties onwards that there was a growing and concerted effort on the part of the rising generation of scientific naturalists to foreclose upon the sort of descriptive, non-evolutionary natural history exemplified by Bowerbank. At the time of this controversy he had 36 published papers

to his credit, and the intention of the "Spongidae" paper had been to describe and name all of the organs of these creatures in order to frame a systematic nomenclature. These broader considerations are worthy of mention, partly to set the incident in its context, and partly to illustrate how the complex interplay of disparately engendered forms of personal and scientific prejudice were brought into focus in a specific case. There is seemingly quite substantial evidence in support of the contention made above touching on the efforts of the pro-Darwinian biologists to muzzle their opponents who held to the fixity of species.

In April 1858 George Busk and Thomas Huxley consigned to the R.S. archives a paper "On the Poison Apparatus in the Actinidae" by the renowned zoologist P. H. Gosse.[31] Gosse was a fiercely committed member of the Plymouth Brethren who had had many papers accepted by the R.S. for inclusion in the Phil. Trans. His work consisted of the exhaustive description and illustration of marine creatures after a fashion which had become traditional in the pre-Darwinian field of Natural History. It seems rather more than coincidental that Gosse's first rejection by the R.S. should occur at the hands of Busk and Huxley just four months after their victim's publication of a volume intended to overthrow contemporary geological doubts as to the status of the Book of Genesis, by substituting his own radical reinterpretation of the geological record.[32] The rejection of a purely technical paper so shortly afterwards by two avid scientific naturalists eager to prepare the ground for Darwin's coming revelations, smacks loudly of intent. The range of influences which informed the actions of author, Secretary, and referees in this case surely suggests a denial of the usefulness of the distinctions between altruism and selfish ambition, honesty and sincerity or competence and incompetence which are often drawn in discussions of official neglect of new knowledge.[33]

Merton's discussion of the "Matthew Effect" dwells upon the disproportionate enhancement of a once-accorded renown as if the concentration of fame and resources in a few exclusive hands was the neutral outcome of "certain psychological processes" creatively negotiating the absolute scarcity of rewards. The Nobel laureates he interviewed registered mostly a generous bemusedness at their own inordinate good fortune. There are no socially recognisable factors at work in the situation depicted by Merton. Using his method of analysis it is impossible to distinguish sociologically between interpretations of science as a random hierarchy, an open meritocracy, or a Machiavellian tyranny. This obscuring of the underlying processes involved in the course of a compendious overview of the subject undoubtedly lends succour to the more whiggish publicists of the R.S., who roundly maintain its complete devotion to the cause of open, rational meritocracy following the acts of high-minded self-redemption in 1847.[34] This view is expressed by the official Record of the Royal Society in the following words:

> "but this rising undercurrent finally and permanently lifted the Society towards that condition towards which it had laboured for two centuries. When the dust had settled after twenty years of protests, exposés, pamphleteering skirmishes, secret sessions by both sides and a good deal of rather vicious infighting, the Royal Society emerged as a genuine scientific body and left behind those practices and deficiencies which had so long made it vulnerable to charges of dilettanteism, private interest, nepotism, and snobbery."35

It might more perspicaciously have been remarked that henceforth the conduct of these practices within the R.S., became the exclusive preserve of men of science.

Proper and Improper Referees

Six years prior to Huxley's recommendation of Gosse's paper for the Society's archives he found himself, as a relative newcomer to the arena of public scientific performance, facing the rigours of the

reformed Society's refereeing system as the underdog. In this situation can be seen an intermediate case which lies roughly between, on the one hand the timorous newcomer making his earliest foray into the intimidating arena of the Royal, and on the other the seasoned insider with influential allies and a working knowledge of the tactics used in the intellectual struggle for existence. Huxley was elected to the R.S. in 1851 when he was 26. There were 38 candidates from whome ten (of the year's fifteen ordinary elections) had succeeded at the first suspension of their certificates. A disproportionate number of future members of the R.S.'s ruling group were elected with Huxley in the shapes of Stokes, William Thomson, James Paget, A. W. Hofmann and Robert FitzRoy. At the time of his election Huxley had had 14 papers published and in the same year narrowly missed the award of an R.S. Royal Medal for his Medusae memoir. The honour fell to Newport for his "Impregnation" because Huxley's work was thought "too small and short". At the time that this clatter of self-promotional enterprises was under way, Huxley was energetically seeking Government assistance to complete his "Rattlesnake" work and seeking London-based salaried employment in some scientific capacity. Writing at this time, he makes quite explicit the proportion of a promising acolyte's efforts which had to be expended on "making out" in the institutional and essentially social world of science:

> "Here in England the fighting and scratching to keep your place in the crowd exclude almost all other thoughts. When I last wrote I was but at the edge of the crush at the pit door of this great fool's theatre. Now I have worked my way into it and through it, and am, I hope, not far from the check-takers. I have learnt a good deal in my passage."36

The fact of Huxley's eventual breakthrough to a point of irreversible, sharply escalating eminence provides a firm retrospective rationale of his basic worth. However, it must be borne in mind that it is only the

famous whose early struggles are usually documented while those of the rest remain in the obscurity which they never left. This bare truism connects with what remains the most pertinent observation in Merton's disquisition referred to earlier. He points out that: "although eminent scientists may be more likely to make significant contributions, they are obviously not alone in making them. After all, scientists do not begin by being eminent."[37] The sustenance and growth of a scientific reputation thus works as a self-fulfilling prophecy as those men provided with the legitimacy of official approval monopolise the resources without which successful scientific performances are impossible. Frequently this is lost sight of and only the converse implication granted real credence. According to this, the unique intrinsic merit of any exceptionally able novice practitioner will inevitably win through in the end. This common outlook fits the notion of the preordination of an ultimately just distribution of scientific recognition and rewards to its logical sequel in the garb of the conveniently conservative ideology projected patiently by the scientific societies. This has usually taken the form of energetic reaffirmations of their rational, meritocratic basis "proved" by the "fact" that the few men who have risen to eminence within these prestige brokerages were the only men constitutionally equipped to do so, "proved" by the "fact" that they did so. The self-sustaining circularity of this rationale of an anything but rational status quo has rarely been questioned by the successful few to be squeezed through the "narrow gate" into the Fellowship of the R.S. Those with the exclusive stamp of authority are silenced by the simple expedient of being made the chief beneficiaries of that exclusivity. Those left out remain, by definition, negligible. The precepts of the view outlined above appear to be deeply embedded in the thinking of many historians of science despite the development

of apparently democratising sidelines such as prosopography. A study of rejected knowledge by someone versed in the internal technical nature of the science of the day would reveal more of this area of Victorian science than the present study of its social relations. In the context of this work, the main focus must remain restricted largely to a consideration of the early doings of men whose fundamental merit has been retrospectively put beyond question by eventual official endorsement. One such is Huxley who one year after speaking of "struggling and scratching to keep his place in the crowd", was faced with a specific threat to his prospects in the very small and exclusive world of professional British biologists:

> "You have no idea of the intrigues that go on in this blessed world of science. Science is. I fear no purer than any other region of human activity; though it should be. Merit alone is very little good; it must be backed by tact and knowledge of the world to do very much.
>
> For instance, I know that the paper I have just sent in is very original and of some importance and I am equally sure that if it is referred to my "particular" friend - that it will not be published. He won't be able to say a word against it, but he will pooh-pooh it to a dead certainty. You will ask with some wonderment, Why? Because for the last twenty years [Richard Owen] has been regarded as the great authority on these matters and has had no-one to tread on his heels, until at last, I think he has come to look on the Natural World as his special preserve and "no poachers allowed". So I must manoevre a little to get my poor memoir kept out of his hands . . .."[38]

The paper referred to was Huxley's "Mollusca" refereed by Edward Forbes and Thomas Bell and published in the Phil. Trans. Owen's repressive bent appears to have been based purely on his own robust estimate of his standing in the field of natural history. A few years later this became a central aspect of the struggle between the religious defenders of the fixity of species and the emergent group of scientific naturalists mobilised by the publication of Darwin's

evolutionary thesis. The significance of Owen's attitude to Huxley's "Mollusca" paper lies in the fact that it was quite natural and normal, being every bit as "scientific" as Huxley's behaviour in discharging the offices implied in being Mr. Gosse's "particular friend" six years later. The continuous projection into the past of the widespread practice of suppressive refereeing had stood firm through rapidly changing times as a rationale of the rejection of scientific work with viable contemporary claims on the attention of the incumbent scientific establishment. This version of the history of science commonly holds that such an occurrence as the dismissal of Waterston's 1845 paper is interpreted as having been merely a terrible mistake. Looked at carefully it turns out to be no more successful in adequately explaining that incident than it does in accounting for the general German indifference shown towards Max Planck's 1880 paper dealing with the first and second laws of thermodynamics - or, for the matter of that, Humphry Davy's sterling efforts to keep Michael Faraday out of the Royal Society.[39]

When the propriety of refereeing procedures is evaluated in the above manner, a different light is shed on numerous historical instances. In that remarkable year of elections to the R.S. Fellowship 1851, William Thomson wrote to his friend Stokes about a paper of the latter's which Thomson, in his capacity as editor of the Cambridge and Dublin Mathematical Journal was eager to publish. In passing, the editor requested Stokes to pass on to two further Cambridge referees, a paper by their mutual friend and colleague Professor Rankine, which Stokes had been refereeing. He added the striking phrase:

> "tell them that I shall direct them as to its reality!"[40]

This light-hearted recommendation nonetheless reveals the extent to which the official ratification of knowledge claims was a contingent

rather than an absolute process. This fact is further highlighted in the simple voting procedure of the R.S. Council sitting as the Committee of Papers following consideration of referees' reports. Archibald Geikie (Foreign Secretary R.S. 1889-1893, P.R.S. 1908-1913) was surprised when attending the Royal Society of Naples for his installation as its new foreign member to be asked to vote, by selecting either a black or a white ball, on the suitability for publication of a paper of which he knew nothing whatever. Geikie's reaction was not at the fact of voting on papers per se, but that its quite arbitrary nature could be flaunted so openly. He was also somewhat surprised to receive his 15 lire attendance money, shortly after his return home.[41] The point at issue here is that the more secrecy is practised in the process of the evaluation of new knowledge, the greater the scope for the prima facie plausibility of the grounds of its rejection. The staple pretext which has consistently served as the legitimation for the resistance of scientists to new scientific ideas is enshrined within the tractable Cartesian notion of "the consecration of Doubt". The records of the R.S. for the second half of the nineteenth century show frequent concern with the rules covering the secrecy of refereeing. As one might predict, experienced producers of scientific work would naturally be able to form a fairly reliable impression of who had refereed their work according to the recommendations made. At times when a full scale scientific debate was in progress the fate of particular papers would be taken as the official R.S. judgement on the issue which formed the basis of the debate. Not surprisingly at such a sensitive time, authors would tend to be very anxious to ascertain the identity of their referees, while the Society would be equally anxious to maintain the appearance of its lofty impartiality by increasing the degree of secrecy veiling its inner workings. At the very close of the century

the biometric-Mendelian debate had resolved itself into a bitter struggle. Ruthlessly the two sides strove to worst one another in the battle for the official endorsement of their respective theories implied by the Society's publication of the winning side's papers. The long-term R.S. abhorrence of controversy ensured that there would be unlikely to be any evenhanded publication of both sides of the question. The adjudication of the matter accordingly took place at the level of the Society's officers in conjunction with the sectional committee for zoology and the Council as a whole. The debate was entering its lively phase when Ray Lankester wrote to the Assistant Secretary Robert Harrison on the 24th of June 1898:

> "Dear Mr. Harrison,
>
> I am of opinion that, it would be advisable not to insert the referee's name in the minute book - in the case of the reference of the paper to Professor Weldon the motion would state 'a referee named to the Chairman' - instead of 'Professor Weldon'. I am of opinion that it would be desirable to erase now from the minute book all names of referees."42

Weldon's Mendelian outlook was endorsed by the R.S. which accordingly passed a resolution excluding biometrical papers on the grounds that they constituted an objectionable mixture of mathematics and biology. Because of Francis Galton's personal wealth the muzzled biometricians were able to set up their own journal in 1901. After five years Biometrika was still the only channel open to their writings and Pearson considered resigning from the R.S. Support certainly did exist for Pearson's theoretical position within the ruling group of the Society. This was clearly shown by the award to Francis Galton of the Darwin Medal for 1902. A number of leading figures in the forefront of decision-making at Burlington House were named by Weldon as his nominees for a projected Evolution Committee early in 1899. They were: Lankester, Foster, Dyer, Herdmann and Weldon. Pearson's 1901

paper had been given straight to his worst enemy in the bitter debate, William Bateson. The Society proved unable to enforce any semblance of solemn equanimity in the case of the Evolution Committee which broke up acrimoniously in early 1900.[43] Lankester's seemingly unremarkable suggestion of increasing the degree of secrecy surrounding refereeing procedures assumes a wholly new significance within this wider context. Pearson communicated his defiance of the Society's endorsement of the Mendelian view in a letter to the new Secretary R.S. Joseph Larmor, written on October 26th 1903. Pearson referred to the Committee of Papers resolution banning biometrical papers:

> "Dear Larmor,
>
> I told Weldon during the vacation that I had sent in a paper to the R.S. on Mendel's theory subject to the Secretary's determination of whether it fell under the resolution of the Committee of Papers or not. . . . I think it not a wise resolution, because I think that Biometry has got a future before it and the historian of science in the future will be inclined to smile at the wiseacres of the Council of the R.S. in 1903."44

In a situation such as that faced by Pearson between 1900 and 1906, wherein a particular faction proves to be of sufficient power to embody its theoretical predelictions in impersonal resolutions of the Committee of Papers, the injured party struggle in vain for redress. Even in cases in which the R.S. has been later revealed as having been wholely in error, restitution has rarely or never taken place in the lifetime of the victim. Quite apart from the wrongs thereby done to individuals, this has tended to produce a deeply complacent self-image on the part of the Society's leading Fellows, for whom the Phil. Trans. had to be preserved as the ultimate repository of finished truths. Where it became undeniably clear that the wrong side had been backed, down would fall the shutters of recognition ushering in a dark age of formal indifference to hitherto vital scientific questions. During

the 1850's and 1860's an important exchange took place between Emil Du Bois Reymond and Carlo Matteuci over the question of animal electricity. In the summer of 1850 a special Commission of the Academy in Paris repeated a number of the two contestants' experiments finally reporting in favour of Du Bois.[45] In England however, matters were differently arranged. Matteuci had been awarded the Copley Medal in 1844 and the group of his eminent friends contained the up-and-coming John Tyndall, his mentor at the Royal Institution Faraday, and Edward Sabine. A long series of Matteuci's papers was welcomed by the R.S. over a period of more than twenty years, appearing in both the Proc. and Phil. Trans. Du Bois Reymond eventually triumphed in terms of the international evaluation of the plausibility of his theories within the field of animal electricity, and the R.S. which abhorred unseemly controversy to the point of decreeing its non-existence, was left with an embarrassing public commitment to Matteuci matched by a consistent previous disregard for Du Bois Reymond. From almost the outset of their rivalry both men were proposed for the Foreign Membership of the Society nearly every year. This continued inconclusively for the next two decades. The remarkable exclusion of a Copley Medallist and major contributor to the Society's publications ended with Matteuci's death in 1868. Du Bois Reymond was elected to the Society's foreign list in 1877 after 25 years during which he was proposed annually. In the cases of both men, what happened to them was quite unprecedented.[46] In 1868, Richard Owen sent an emotional plea to Burlington House for the belated admission of Professor Matteuci, but to no avail.[47] It seems that an informal custom came into being during the second half of the nineteenth century whereby in certain cases foreign men of science whose election had been for many years blocked by opponents within the R.S. Council were admitted at the very end of their lives on the basis

of a sort of "short membership". Matteuci was unable to benefit from this development which has been greatly obscured by the background increase in the overall numbers of potential and actually proposed foreign men of science.

"Piling the Horrors"

The secrecy of the crucial function of the referee led to the occurrence of numerous ironies not fathomed at the time by all the participants themselves. Michael Foster informed Lord Rayleigh of a case in which an eminent physician experienced the curtailment of a paper he had written on the subject of a particular disease which was the special focus of his expertise. The man complained to Foster about the way that the referee had proceeded, and named another who would have been his own choice. Unbeknown to the eminent physician, it had actually been his named favourite who had insisted on the pruning.[48] The lively grapevine of supposition and rumour which inevitably attends a secret system of evaluation is clearly given fresh scope for conjecture when the directions for a paper's modification are subsumed under the rubric of a corporate body such as the R.S. Committee of Papers. It is in this context that the Society's starkly legislative stance (as opposed to its ostensibly evaluative position), in the process by which putative new knowledge is or is not rendered legitimate, comes unmistakably to the fore. During 1863, Richard Owen was irked to discover that his toothed fossil bird was to be renamed as "archaeopteryx" by the Committee of Papers. The same Committee sitting as the Council very nearly succeeded in awarding the Copley Medal to Charles Darwin in that year, although it was eventually received by the Reverend Adam Sedgwick. Darwin received the medal in 1864. The strong pro-Darwinian faction sitting in the Council of 1863 wished to prevent Owen from disguising what they regarded as the significance of

the fossil bird which combined teeth and feathers. They saw it as a telling link between birds and reptiles which bolstered the claims of Darwin's scheme. Owen had intended to name it the "Griphosaurus" or "Griphorus".[49] Joseph Hooker and Thomas Huxley were the two leading scientific naturalists within the ruling group of the late nineteenth century R.S. Not surprisingly, they became progressively more conservative as they grew older and rose to the leading positions of power. In the following extract from a letter to Huxley, Hooker betrays some vestiges of the timorous and fearful outlook of the author who is an outsider, and who therefore can be silenced at the merest whim or by the wickedest intent of his powerful, secret rivals:

> "You could not have answered T. better, I have long thought that the retention of rejected papers was a course that had its awkward side; it is so often regarded, however unreasonably, as "suppression" of the papers, which, added to rejection, piles the horrors. We must be unfettered in our power of rejection and we must keep the originals as our pièces justificatives, and I see no middle course but that of offering copies to be made at the author's expense."50

The built in capacity of the two referee system to perpetuate tacit collusion, explicit mutual assistance and the covert suppression of an individual's views by his rival was perfectly well recognised during the period which is under consideration here. Michael Foster declared as much in a postscript to a referee's report in 1894.[51] In places other than the R.S., the universal Victorian remedy of litigation became commonplace. Editors of journals who rejected papers were held to have infringed the intellectual property rights of authors. The recourse to legal remedies was at its height in May 1887 when Oliver Heaviside asked the editor of The Electrician to print a letter dealing with what he, Heaviside, saw as the colossal mendacity of that journal's referee who was blocking his paper. The editor C. H. W. Biggs was Heaviside's staunch sympathiser yet on the 30th of the month informed him that:

> "I would use your letter if I could, but it is dangerous in the present state of the law. . . . I may tell you that at present six of us have two libel suits each against us, or a round dozen altogether, and I venture to think that the cost even if we successfully defend ourselves will be considerable."52

Insider Dealing in Natural Knowledge

The groups of successful authors and communicators of papers were for the most part co-extensive and were established within the Fellowship of the R.S. Certain individuals proved able to steer a large number of papers by non-Fellows towards the pages of the Society's publications without having ever been prolific contributors of their own work. Michael Foster provides a telling example of this phenomenon. Drawing on his places within British physiology and the R.S. (both of which fell to him largely at Huxley's instigation) this Cambridge-based Secretary of the Society communicated 9 papers for the Phil. Trans. and 18 for the Proceedings in the period 1885-1893. During that time he contributed no scientific work of his own to the Society.[53] The communication of papers might be thought to compromise the consequences of Merton's Matthew Effect by providing an open route for novel ideas from obscure workers to reach the centre of the scientific stage. It requires no strenuous exercise of the imagination to see the point at which this trend of thought breaks down. Aspirant authors, in seeking a communicator for their papers were usually advised to try a Fellow known to be in accord with the views which they espoused. The prospective communicator then tends to act as a preliminary, external referee. The Matthew effect is not suspended by virtue of the communication system: it simply operates at one remove. The communication of E. B. Poulton's first paper to the Royal Society in 1888 by the then venerable arch-beneficiary of the Government Grant, William Kitchin Parker reflects the latter's keen appreciation of the informal

rules and ploys which must needs be observed in order to preserve new work from predation by the leaders of a dangerous and wary orthodoxy:

> "Dear Mr. Poulton, . . . what I want you to do is this - don't go and quietly pop it into Mr. Anybody's Journal but bring the whole paper before the R.S., it will be a kindness to me, you will find that I'm a useful tattler and I want to have an opportunity of letting loose mine opinions about Monotremes, Marsupids, and Placentals at THE SOCIETY. Schäfer may father it, I'll mother the paper and dandle it before the 'swells'. . . . That brings us two into the 'honourable mention' along with yourself in the 'blaze of glory' that will of necessity break forth when the fact is published."54

Poulton's paper sought to explain the entire lack of teeth in modern birds. Parker continued to produce voluminous scientific papers up to the end of his life. Two years prior to his communication of Poulton's paper, he had unexpectedly found himself at the centre of what Foster termed "The Parkerian Crisis". Parker's style of doing zoology was far out of fashion with the younger men who had come to wield influence in the Society by the late 1880's. The unusual step of appointing one of the leading "young brethren", Ray Lankester, to submit a third referee's report was taken. He recommended considerable curtailment and ommissions. Sir Richard Owen, another religiously orthodox, old style descriptive zoologist who had been pre-eminent in his day also felt the cold wind of change blowing as Foster described:

> "I found another paper by Sir Richard on Melocania down for yesterday!! The abstract read won't do much harm, but the publication of the paper offers another nettle."55

The image presented of two partially discredited authorities engaged in rearguard struggles with the spokesmen of a new and usurping research is a significant one. It begs the question of which sorts of defensive measures were commonly adopted by the R.S.'s dominant group of insiders whose channel of scientific communication the Society increasingly became during the last thirty years of the century. This group is considered in detail in a later chapter.

George Stokes' incumbency as Secretary of the R.S. covered a time-span of 31 years and was marked by a disproportionate increase in the amount of work associated with the job. When Stokes was about to relenquish it for the Presidency in 1884, Foster tried to smooth the way to securing Rayleigh as his successor by assuring Rayleigh's wife that much of the work-load could simply be cut out - this in spite of the overall growth of scientific activity over the past thirty years:

> "Correspondence etc. ought not and certainly will not in the future be as great as Stokes has made it. It has been painful to see how his energy has been wasted in this way."56

Perusal of his correspondence reveals that a large proportion of this additional work consisted of the patient study of papers not yet formally submitted, in the manner of a preliminary referee. Rather than seeking to document the myriad instances of Stokes acting in this manner it would be more pertinent to refer to the sole occasion on which he refused to actively participate in shaping both the form and content of a paper. The R.S. executive had been stalling the author Mr. Alfred Tribe for over two years and appeared to be rather at a loss for a means of politely making Tribe's work disappear. Stokes accurrately pointed out that his position did not require him to actually participate in the construction of the papers:

> "It is of course no part of my duty as Secretary of the Royal Society to revise or attempt to revise, papers that the authors send in, I have plenty of work of my own without that."57

Naturally authors would not seek to conceal the ardent Secretary for Papers involvement, as it would serve as a potent charm to blunt the enthusiasm of referees known to harbour a fondness for augmenting the Society's archives. Stokes was acting in this capacity as consulting participant co-author before becoming Thomas Bell's successor as Secretary in 1854; indeed it seems that he wielded sufficient influence

to guarantee a paper's fate even prior to his election to the Fellowship in 1851. In late July of 1850 Mr. H. Cox wrote to thank Stokes for "taking charge" of his paper:

> "A new writer has always a strong weight of prejudise against him when contending against established opinions and established names. Half a dozen words from you would silence this kind of opposition in the present case."58

In a host of cases, the two functions of informal preliminary appraisal and collaborative revision shaded imperceptibly into one and other. In the words of his daughter:

> "he remarked that he gave up an immense amount of time to the improvement of hopeful work but that he could not make bad work good."59

The R.S. definition of the nature of the scientific enterprise thereby devolved upon the person of its long serving secretary, to a quite extraordinary degree. This account will enter into the question of what Stokes held to be good and bad work as part of a later section. A significant illustration of Stokes' remarkable oracular place at the head of British science is provided by his part in the enduring patron-client relationship which existed between William Crookes and himself. Having once repaired the coolness which sprang up between them due to Stokes' firm evangelical aversion to Crookes' introduction of spiritualism into Burlington House in the early 1870's, the stage was set for an unparalleled series of virtually unacknowledged collaborations. The letters which passed between them over the period 1873 to 1900 reveal Stokes running the full gamut of involvements in Crookes' scientific work from the mere act of checking for mathematical errors progressing through the design of apparatus and experiments, to the position of wielding the final responsibility for the content of his theoretical statements. On July 22nd 1886 Crookes wrote to Stokes (by then P.R.S.) about the confusing spectroscopic maze of the rare earths:

> "Towards the end where I indulge in theory I hope you will not object to criticize and erase. From letters you sent me some time ago I think I shall have to omit some passages, but I hope to receive a further letter from you . . .."[60]

At several times during his chequered scientific career it becomes rather difficult to discern precisely what Crookes himself contributed to the scientific work that earned him such renown. His letters to Stokes dating from 1873-1878 present an intriguing picture of himself as the co-ordinator of the labours of his paid assistant (who as a sort of gentleman's mathematical gentleman had previously been in the employ of Augustus de Morgan), Stokes' willing efforts as reviser of the latter's work and supplier of experimental designs, and large sums of money made available from the Government Grant.[61] In the light of these facts it ceases to appear remarkable that Stokes sent all the papers submitted under the name of Crookes to his Cambridge confrère James Clerk Maxwell as first referee. Crookes experienced a quality of chagrin beyond the normal sadness of loss when Maxwell died prematurely in 1879.

> "The loss of Clerk Maxwell is indeed a heavy blow to science. I feel it particularly so as he had reported to the Council of the Royal Society on all my papers before they were printed and his reports were generally sent to me afterwards."[62]

Crookes' career as an "insider" at the R.S. (apart from his hastily retraced steps towards the spirit world) rested upon his complaisant indulgence in a community of interest and outlook with the Senior Secretary. Through the establishment of access to Stokes' crucial preoccupations as informal previewer and participating editor of the papers, Crookes made his position secure.

Another method of bolstering a new piece of work's chances of survival at the hands of an author's rival practitioners acting as referees, was to make the theoretical component of the threatened

paper a separate entity. If the worst came to the worst the offending views, being quickly detatchable, could be denied publication while the purportedly pre-theoretical facts or experiments went to the printers. The notion, widely held at the time, that nothing short of an immutable scientific truth was suitable for the journal which had publicised the majestic edicts of the illustrious Newton, created pitfalls for authors and R.S. officers alike. G. Johnson Stoney gave expression to his doubts as to the wisdom of this policy which caused the Phil. Trans. to become frequently a mere collection of uninterpreted experiments and untreated observational data. Writing from Dublin in August of 1867 he remarked:

> "If a paper's proceeding on what is deemed debatable ground is to be held as precluding it from the Transactions, this ought, one would think, in some way to be made known to persons at a distance."63

Stoney went on to note that controversial papers produced by persons of very large reputation such as J. F. W. Herschel and Brodie had nevertheless continued to appear in the pages of the Phil. Trans. No such deferential dispensation was available to the little-known Thomas Woods who submitted a paper late in 1856 which challenged Joule's view of the heat evolved in chemical combination. Only an account of Woods' experiments was passed as fit for publication. Although the wholesale emasculation of papers in this fashion was a common practice (after all Newton's were no longer forthcoming and the Phil. Trans. had to be filled with material of some sort) Woods appears to have been somewhat taken aback:

> "The paper must either be of no moment as to scientific interest or that the experiments are correct. Would it be possible for me to find out the real cause of its not having been thought worth publishing?"64

An author with any foreknowledge of this radical procedure was necessarily faced with a dilemma. Should he structure his work so as to

facilitate this abrupt censorship or ought he perhaps to enmesh the empirical aspect of his paper inextricably with his theoretical precepts? In the former case, any obscure author lacking powerful friends might expect an easier path to inclusion in the Society's publications, at the cost of forfeiting any possibility of winning the far more substantial reputational rewards flowing from the wide advertisement of his original views. Having settled upon a course of action in the above matter, the putative contributor to natural knowledge was then obliged to make another decision. The Assistant Secretary marked for refereeing and possible inclusion in the Phil. Trans. only those papers which arrived accompanied by an abstract. The remainder would be set up in type for the Proc. if marked as "safe" by one of the Secretaries. "Unsafe" Proc. papers would usually be returned to their authors. The new men who were aiming high thus had to risk appearing presumptuous in enclosing an abstract. To do so was to cross the rubicon of public self-estimation. The men who already mattered, as it were, tended to aggressively defend their place in the pantheon by the pre-emptive provision of an abstract while aspiring novices respectfully allowed their work to disappear. This invidious detail of R.S. publication procedure highlights the tangible institutional working of the Matthew Effect. The self-reinforcing dynamics of this effect were by no means restricted to the Royal Society. It occurred to G. F. Rodwell that the tactic of supplying an abstract became a two-edged sword in the hands of an unknown author who would more than likely be seen as revealing:

> "his own conceited belief that his paper was worthy of being printed in the 'Transactions.'."[65]

Rodwell was earning his living as a schoolmaster at Marlborough College at the time of this incident in 1882. He completed his papers by rising sometimes as early as 3 a.m., then working a normal school day.

He obtained the additional position of Assistant Examiner at South Kensington under John Tyndall. It was to Tyndall that he turned in order to "save" his papers from the Proceedings. He wrote to his mentor as follows:

> "I do not the least assert that the papers are good enough for the 'Transactions'; probably they are not, but I do venture to think that a man is put in a somewhat awkward position if his paper is not critically examined with a view to its insertion in the 'Transactions' before it is read before the Society and before it is set up in type, in full, for the Proceedings.
>
> P.S. If there is no doubt that the papers are worthy of the Transactions nothing to my mind would 'balance the honour' of having them in the Transactions but I understood that there was some doubt as to whether they might not, after some weeks be referred back to the 'Proceedings'."66

By contrast it is worth noting that Richard Owen, in spite of the decline of his influence in the face of a rising tide of scientific naturalism in the Royal Society had an unbroken series of 6 papers published in the Phil. Trans. between 1885 and 1893. This succeeded the "Parkerian Crisis" as a problem for the scientific naturalists.

The question of the timing of a paper's submission provided another telling factor in determining the career of any particular new piece of work. The decisions about what sort of exposure a paper would get were all made informally at the discretion of the Officers, usually the Secretaries. A paper might be given a prime position in the schedule of an ordinary Thursday meeting (the programme was published before hand in several daily newspapers) allowing time for discussion, or it might be read by the Secretary for Papers in abstract only. The least courtesy that could be paid was the reading of the title only. Confirmed "insiders" were quite accustomed to negotiate with the Officers in order to obtain the best possible forum for their scientific productions. In return, the Society felt it was able to maximise the

lustre of its meetings by these means. The degree of fastidiousness sometimes involved in the selection of papers for full exposure at ordinary meetings is revealed by an anxious inquiry which Rix directed to Foster in 1888. He wished to have W. B. Carpenter's paper brought forward: "You know we have only one paper."[67] This system did not succeed in bringing every meeting to life. A decade earlier Walter White remarked on the plethora of tedious detail being read out in the meetings.[68] The accumulation of papers which remained at the end of the session every June were largely those informally decreed to be of little account. "The slaughter of the innocents" as it was popularly known frequently saw the reading of up to thirty papers in one afternoon. Rix tried to convey the futility of attempting any full or proper presentation of new scientific work in these circumstances to Sir David Salomons in mid-June 1894:

> "Dear Sir David, I fear it will not be of much use for you to take any great trouble about experiments next Thursday. Unfortunately it is the last meeting of the session and the usual 'massacre' will take place. We shall probably have to read twenty papers in one afternoon."69

Where a paper could, fortuitously or by intent, be kept from the various pitfalls described above then it had to submit to the ordeal of discussion by a host of its "natural" detractors. From 1800-1845 discussion of the papers at ordinary meetings was not allowed at the Royal Society. The place of the Society in the Victorian world was considerably bettered in the reflected glory of technological innovations which were becoming a part of everyday life. This augmented the Society's own newsworthiness which was boosted considerably by the Darwin question in all its shades and tones. Collectively these factors appear to have produced a sharply whetted public appetite for scientific news, for which the R.S. was taken to be the fountainhead. The public visibility of the R.S. was increased by means of wider press

attention to ordinary meetings. It appears that reporters were "tipped-off" about meetings which held the promise of particular liveliness. This accelerated public interest was further acknowledged by Hooker's broadening of the scope of the soirées in the 1870's. Interestingly, despite this basic shift in the posture of the Society, the discussions of papers at ordinary meetings reintroduced in 1845 were not to be allowed publication in any form for the rest of the century. William Crookes received a sharp reproof for his reporting of these discussions in the Chemical News on one occasion in 1864. There seems to have been a broad consensus across the boundaries of factional loyalty on this issue. The intention was to preserve the appearance of magisterial unity as the public face of science. The vigorous squabbling between rival versions of scientific truth which conventionally took place was seen as undesirable by all parties close to the focus of power. This resulted in a general aversion to the running of more than one candidate for the Presidency. After 1847 the posture of the reformed Society was that uniform scientific rationality was inconsistent with personal rivalry for the chair. The actual situation was very different. The issue was always settled, however fiercely, in private before the visible process of nomination and election by Council began.

Of course these occurences involved only the tiny group of men making up the power-holding factions within the Society. For the rank and file of scientific workers, the struggle for their work to see the light of day was the main thing. The German physiologist Carl Ludwig summed up the situation quite tellingly in a letter of encouragement to Emil Du Bois Reymond who had recently been in Paris trying to secure some recognition for his ideas about animal electricity. Du Bois had been dismissed from serious consideration by his compatriots working in the German Universities. The letter was dated 19th September 1850:

"As one is gradually coming to see, your struggles in Paris were very necessary in order to win the majority of physicians over to your side. You must depict the matter from this viewpoint to those of your friends who do not agree with the trip - Riess, for example - and who probably do not understand that a popular reputation is necessary to us, after the manner of the politicians, that it is only popular acceptance that is of true utility. The fight against Matteuci will only be over when the writers of textbooks condemn him."70

* * *

Notes

1. E. T. Whittaker, "Oliver Heaviside", Bulletin of the Calcutta Mathematical Society, 1928-9, 20, 199-200, ff. 216.

2. Royal Society, NLB 3, no. 158.

3. Herbert Rix, "The Royal Society - its Fellows and its work", Leisure Hour, 1896, p. 657.

4. John Ziman, Public Knowledge, Cambridge University Press, 1966, p. 148.

5. Thomas Huxley, Diary of the Voyage of HMS Rattlesnake, Julian Huxley (ed) London, 1935.

6. Royal Society, Register of Papers.

7. Huxley Papers (ICL), 3.327.

8. R. M. Macleod, "Of Medals and Men: A Reward System in Victorian Science 1826-1914", Notes and Records of the Royal Society, 1971, 26, vol. 1, 81-107, ff. 97. John Tyndall, who was also to be an X club member, was involved in a controversy over one of the Royal Medals in 1853 - a crucial phase in the formation of his career.

9. Stimson, SA, Hall, ASN, Lyons, TRS.

10. F. W. Edridge-Green, Science and Pseudo-science. The Necessity for an appeal board for science, John Bale Sons and Danielsson Ltd, London, 1933.

11. Hall, ASN, p. 125.

12. Edridge-Green, ibid., p. 68. The author goes on to record that: "A late Secretary of the Royal Society told me that he could ensure the acceptance or rejection of any paper simply by the selection of the man to whom he sent it."

13. Royal Society, Referee Reports 10-11. Wilde was refereed by Rücker and Ayrton, RR 11, no. 53; by Ayrton, RR 11, no. 59; by Sir William Thomson and John Perry, RR 11, no. 50. The system of inter-locking refereeing carried on amongst the members of the dominant group of Cambridge mathematical physicists is immediately apparent as a consistent strand running through the Referee Reports, volumes 6-11.

14. Evelyn Sharp, Hertha Ayrton 1854-1923 A Memoir, Arnold, London, 1926, p. 204.

15. T. S. Kuhn, The Structure of Scientific Revolutions, University of Chicago Press, London, 1970.

16. Edridge-Green, ~~ibid~~., p. 45.

17. Robert H. Murray, Science and Scientists in the Nineteenth Century, The Sheldon Press, 1925, p. 347.

18. S. Brush, "Herapath," DSB.

19. Robert Merton, The Sociology of Science, Chicago University Press, 1973.

20. A. J. Meadows, Communication in Science, Butterworths, London, 1974, p. 42.

21. Joseph Larmor, Memoir and Correspondence of George Gabriel Stokes, Cambridge, 1907, 2 vols., vol. 1, p. 37.

22. Huxley Papers (ICL), 4.279. Huxley and Foster were very much disposed to oppose Stokes and what he stood for. When the layout and content of Stokes' "Life and Correspondence" was being finalised, members of his immediate family agonized at length with Larmor over the extent to which Stokes little known personal and sentimental side should be revealed.

23. Meadows, ~~ibid~~., p. 42.

24. Merton, ~~ibid~~., p. 443.

25. A. S. Eve and C. H. Creasey, Life of John Tyndall, Macmillan, London, 1945, pp. 94-104. The paper was about testing a law of Lenz and Jacobi bearing on relative conductivity. Tyndall asserted that the paper was both unoriginal and mistaken. He went on to "beg that these observations be received with caution". (Royal Society, RR 3, no. 155.)

26. Royal Society, RR 3, no. 157.

27. Eve and Creasey, ~~ibid~~., p. 95.

28. Stokes Papers (CUL), RS 938. The prominence of William Flower and Richard Owen is the key factor. Controversy could not be fully repressed by the Royal Society when the participants held powerful positions of influence within it.

29. Ibid. Stokes' notion that no earlier (eminent) author of papers printed by the Royal Society should be contradicted has very far-reaching implications in any discussion of the place of the Royal Society in late Victorian science.

30. Ibid., RS 374. This mode of procedure is not exceptional. In Stokes' time many papers were refereed by men well-known to be acrimonious rivals of the author.

31. Royal Society, Register of Papers, 1868.

32. Edmund Gosse, Father and Son, Oxford University Press, London, 1974, pp. 60-62.

33. B. Barber, "Resistance by Scientists to Scientific Discovery", Science, 1961, 134, pp. 596-602.

34. Merton, ~~ibid~~., pp. 439-459.

35. Record of the Royal Society, 1949, p. 200.

36. Leonard Huxley, Life and Letters of Thomas Henry Huxley, John Murray, London, 1900, 2 vols., I, p. 91.

37. Merton, ~~ibid~~., p. 456.

38. Huxley, ~~ibid~~., I, p. 97.

39. D. Lamb and S. M. Easton, Multiple Discovery, Avebury, 1984, p. 146. Planck's difficulties with his paper followed a similarly blank and hostile response to his doctoral dissertation in 1879. Helmholtz was one of those who rejected Planck's Munich thesis. Its author stated: "I found no interest let alone approval even among the very physicists who were closely concerned with the topic. Helmholtz probably did not even read my paper at all, Kirchhoff expressly disapproved." (M. Planck, Scientific Autobiography translated by F. Gaynor, Philosophical Library, New York, 1949, p. 18).

40. Stokes Papers (CUL), K 46.

41. Sir Archibald Geikie, A Long Life's Work, Macmillan, London, 1924, p. 385.

42. Royal Society, MC 17, no. 163.

43. Royal Society, MM XV, no. 73. In 1899 W. F. R. Weldon relayed a letter from Francis Galton in Luxor to the Royal Society. The letter, with which Weldon heartily agreed, stated that Galton wanted no more to do with the Evolution Committee. The Committee broke up shortly afterwards having achieved nothing. From the outset it had been a most improbable mixture of Biometricians and Mendelians, The residual group of younger Huxleyites who were still influential in the Royal Society at the time supported the Biometrical position.

44. Royal Society, Larmor Papers, La. 1565.

45. Paul F. Cranefield (ed.), Two Great Scientists of the Nineteenth Century: Correspondence of Emil Du Bois Reymond and Carl Ludwig, John Hopkins University Press, Baltimore and London, 1982, pp. 58-60.

46. Royal Society, Council Minutes 1840-1900. Matteuci was President of the Florence Academy. Edward Sabine visited Matteuci in Italy, as a personal friend.

47. Royal Society, MC 8 no. 144.

48. Robert John Strutt, Life of Lord Rayleigh, Arnold, London, 1924, p. 169.

49. Stokes Papers (CUL), RS 398.

50. Leonard Huxley, Life and Letters of Joseph Dalton Hooker, John Murray, London, 1918, 2 vols. II, p. 144.

51. Royal Society, RR 12, no. 175.

52. Rollo Appleyard, Pioneers of Electrical Communication, Macmillan, London, 1930, pp. 211-260, ff. 227.

53. Royal Society, Register of Papers. During the same period Stokes communicated ten papers and contributed none of his own. Ray Lankester communicated nine and produced none himself.

54. E. B. Poulton, John Viriamu Jones and Other Oxford Memories, Longmans Green and Co., 1911, p. 240.

55. Huxley Papers (ICL), 4.288.

56. Robert John Strutt, Life of Lord Rayleigh, Arnold, London, 1924, p. 168.

57. Stokes Papers (CUL), T 484. Tribe was a chemist.

58. Stokes Papers (CUL), RS 8A.

59. Larmor, ~~ibid.~~, p. 37.

60. Stokes Papers (CUL), C 1184. The Stokes-Crookes collaboration is extensively referred to over the series of letters C 1062-C 1215. In this specific case, Stokes was not well-equipped to advise on so delicate a matter. His somewhat studied obtuseness left him little conscious of personal antipathies between individuals. His daughter Isabel contributed a telling anecdote to Larmor's biography of her father (Larmor, ~~ibid.~~, p. 37).

61. Stokes Papers (CUL), W1076, W1077, W1093, W1114.

62. D'Albe, ~~ibid.~~, p. 300.

63. Stokes Papers (CUL), S 593.

64. Stokes Papers (CUL), W 56.

65. Stokes Papers (CUL) RS 1162 A.

66. Tyndall Papers (RI), 1430.

67. Royal Society, PLB 2, no. 71.

68. White's Journals, p. 271.

69. Royal Society, NLB 9, no. 301. Not everyone was favoured with this advice. This led to the cursory "reading in" of a great many papers at the end of the session in June. This was known to some of the Fellows and Officers as "the slaughter of the innocents".

70. Cranefield, ~~ibid.~~, p. 63.

CASE STUDY ONE: C. K. AKIN AND THE SOCIAL MEANING OF LUMINOUS AND OBSCURE RADIATIONS

During the R.S. session of 1863-64, John Tyndall was achieving a considerable consolidation of his reputation as an investigator. By the latter date he had had five memoirs published in the Phil. Trans. on the subject of Radiation, and this series was to be marked by an invitation to deliver the Bakerian Lecture to the R.S. for the third time. Much to Tyndall's chagrin, he was anticipated in an important new development within this field by a little-known Hungarian investigator, Dr. C. K. Akin, who had been working under considerable difficulties on the subject for a number of years. Tyndall and Akin had both been impressed by the experimental work of George Stokes at Cambridge, which had demonstrated the property of fluorescence in the emission of visible light from certain substances exposed to ultra-violet radiation. It was reported later by both Akin and Tyndall that they independently conceived the notion of similarly shortening the wavelength of the invisible heat-rays at the other end of the spectrum so as to produce visible radiation. James Challis, Plumian Professor of Astronomy at Cambridge coined the term but did not develop the research.[1] Akin did not have the wherewithal to purchase apparatus or anywhere suitable to conduct the work. Although Stokes had two years earlier described the central implication of Akin's preliminary note to the R.S. of June 1863 as "a great discovery", the Hungarian failed to secure the support of Cambridge for his work.[2] Regnault at Paris was similarly unforthcoming and it was not until Akin made an approach to G. Griffith, the deputy Professor of Experimental Physics at Oxford, in November 1862 that he was assured of any sort of facilities for conducting the work.[3] At the time of his preliminary R.S. note in June 1863, Akin was unavoidably absent from Oxford. His hope that

Griffith would continue the work in spite of this, proved to be a vain one so he withdrew his paper. The next publication target was predictably the August B.A.A.S. meeting at Newcastle but the impecunious Akin was not able to get his work into a finished form. However he did read two short papers on the subject at the meeting and was voted £45 towards the costs of his research. At this point Tyndall acknowledged Akin's priority in the matter by virtue of his date of publication, but maintained that he had conceived of the central idea much earlier and independently.[4]

Tyndall's biographers ascribe responsibility for the events which followed to the oversensitive and tetchy behaviour of the younger man in aggressively rating Tyndall for entering an investigation which he had first claim on. The studied partisanship of Eve and Creasey led them to support Tyndall's contention that he showed the dark hot focus in the course of his spring lecture at the Royal Institution on Heat in 1862, whereas Akin had merely secured priority of publication. Whilst the struggling Hungarian desperately tried to assemble the means to perform the experiments which were the obvious upshot of his observation, the thwarted Tyndall in his established place at the Royal Institution and existing eminence within the R.S. (he was first voted on to the Council in 1857) bridled at the unwonted disruption of his prestigous Phil. Trans. series on Radiation. In April 1864 the two men met and suggested various means of collaboration. Tyndall's later testimony maintained that no agreement was then reached while Akin strongly maintained that it had. The acrimonious exchange of opinions which had taken place in the pages of the Philosophical Magazine in 1863 was renewed. John Tyndall was throughout his career a diligent martyr to the mendacities of actual and suspected plagiarism. The modern concept of multiple discovery was seldom granted much

credence in the purview of Victorian scientists who were inclined to view it as a fiction elaborated by the unoriginal to extenuate their unacknowledged collaborations. When Akin emerged precipitately in his path, Tyndall had been successfully colonising the field of radiation for some time. Eve and Creasey's adulatory attitude towards their subject inclined them to place complete faith in his spoken and written testimony. However, their contention that Tyndall first heard of Akin: "sometime towards the end of 1863 . . . from a friend (probably Stokes) at a dinner of the Philosophical Club", does not accord with a letter from Akin sent to Tyndall at the R.S. in September 1862. In it he asked Tyndall to correct the linguistic errors in his paper because he wanted it included in the next number of the Phil. Mag. of which Tyndall was then the co-editor. Akin remarked that Stokes had agreed to do the corrections for him but was away in Ireland.[5] Bearing in mind that the 1863 B.A.A.S. meeting provided publicity for what Akin wished to be known as "calcescence", one reads with some surprise Eve and Creasey's assertion that it was an article in the Saturday Review for January 1864 that provided Tyndall with his first written account by Akin of his work.[6] Aside from this, Akin's 1863 B.A.A.S. contributions were reported in the Athenaeum and the Reader. An inspection of Akin's letters to Stokes at the R.S. reveal that the Secretary took a sympathetic view of the younger man's work, bolstered by the support which he readily offered Stokes when Becquerel made an attempt to claim the credit for the discovery of fluorescence. On this occasion Akin noted that his papers were being given a rough passage and continued:

> "With regard to Becquerel's claim to the discovery of fluorescence I think I have sufficiently indicated my sense of it in the papers and in fact can scarcely speak of it without feeling indignation at the preposterousness of the claim and pity at the short-sightedness of the man who had certainly let slip an opportunity for making a brilliant discovery."7

One month later, on July 27th 1863, Akin wrote again to Stokes:

> "I have to thank you for the interest which, as I have learnt you have been exerting in behalf of my papers. I drive the greater comfort from this, your kind intervention as I shall have probably to appeal to the British Association for the means to be able to carry out the experiments projected - in which case of course your favourable opinion would be of decisive importance."8

Akin succeeded in obtaining £45 for his work from the B.A.A.S. in 1863, as was earlier noted. By the end of the following year Akin was still held back by want of funds, stating to Stokes his wish that the experiments for his Phil. Trans. paper were out of his hands: "that is to say [being conducted] by persons not covetous to take the whole credit to themselves and to deprive me of my own just share"[9] At this time Akin was planning his application for an R.S. Government Grant and wondering how much to request, bearing in mind that Balfour Stewart was receiving £150 for his work on the boiling point of mercury. The consequences of having become embroiled in a lively controversy with a prominent man had, for Akin, come to their full term by March 1865. The President, Sabine, wrote to Akin during February advising him that his Grant application was against his own interests and that it would most likely not be given. Thomas Hirst described in his Journal how he had gone along to the Royal Institution on the 11th of November 1864 to see Tyndall's incandescent ("or rather orange-red") platinum rendered so by obscure rays solely. Hirst remarks that the reason of his going was the possible need to bear witness against Akin and his claim to the same discovery.[10] Following the publication of his paper "On Luminous and Obscure Radiations" in the latter part of 1864, Tyndall wrote to Akin on the 3rd of November:

> "I have to say then, that from this 3rd of November 1864 to the 3rd of November 1865, I shall not make known publicly or privately, any experiments on 'Ray-transmutation'."11

Without alluding to the event at the R.I. on the 11th of November pointed out above, Eve and Creasey note censoriously that: "The following December Akin's attack was continued in the Philosophical Magazine. Three entries in his Journal showed that Tyndall suffered severely from these attacks which involved his personal honour."[12] Bearing in mind the circumstances, one can scarcely imagine how Akin could have omitted consideration of that particular aspect of Tyndall's character from his treatment. The support which had originally been provided to Akin by Stokes ranged from invitation to attend the R.S. for the ordinary Thursday meeting on several occasions, to a testimonial for him in respect of a vacant examinership of London University. After Akin's return to Hungary, the two men's correspondence continued until 1872. Hirst first saw Akin at the R.S. ordinary meeting on the 16th February 1865, while ten days later he was helping Tyndall to compose his reply to the "preposterous claims".[13] Akin still did not realise at this point that he was due to be overwhelmed. On March the 12th, the R.S. Soirée presented Tyndall's experiments on "ray-transmutation" at the climax of which the Prince of Wales saw him light a cigar at the dark focus. A few days later, Hirst wearily noted that Akin's protests had continued and that: "Brewster had been gained over by Akin, through Tait." Hirst recorded with satisfaction that Francis, the editor of the Phil. Mag. and one of his familiars, had decided that no more of the controversy would be printed in his pages.[14] The apparent even-handedness of this policy scarcely reflects the inequity of the dispute's outcome. Tyndall had won all the plaudits for the discovery, while its original author languished in straitened circumstances supported only by erratic sniping from north of the border. This came from Tyndall's opponents in the earlier Mayer controversy, led enthusiastically by P. G. Tait. At a meeting of the R.S. Council at the end of the following May: "it was decided that Dr. Akin's application for

£35 for experiments on Ray Transmutation should not be entertained."[15] Just a few days earlier, on the 25th May Akin had written to Stokes at Burlington House urgently requesting a reply regarding his Grant application because of his imminent departure for Paris where the apparatus for his experiments could be obtained more cheaply:

> "I imagine that the interference of other persons with my researches will not prevent the Royal Society from extending to me their support - as little as the similar occurrence in the case of Mr. Crookes prevented them from extending their support to him."16

Akin is here referring to the dispute over priority in the discovery of thallium, between Crookes and the Frenchman Lamy. The comparison bears scrutiny, especially in respect of the crucial juncture in their scientific careers at which Crookes and Akin had each to face their respective ordeals at the hands of a fallible and prejudiced institutional structure for the evaluation of scientific performance.[17]

During late November 1866 Tait wrote to Stokes informing him that he could not attend the R.S. on December the 6th to be present at the reading of his paper. He had nonetheless arranged for Balfour Stewart to go to the meeting because he was more conversant than Tait was himself, with the experimental part of the paper:

> "You will see that de la Rue, Stewart and Loewy have virtually published, in their second paper, my ideas for which I asked the grant. I am very anxious to make the experiment before long. Thomson agrees with me and that, otherwise, I shall be "akinized", if you understand the word."18

By May 1867 a disaffected Akin was back in Hungary in charge of the physical cabinet at the Academy of Pesth. In the course of a long letter to Stokes he reflected on his experiences in England and the term used by Tait to denote intellectual piracy:

> "though like an ever open sore it is never absent from my mind. It is just like my usual lot, just like what I have been accustomed to in life, that I should now be accused of selfishness for having

> fought, it may be tooth and nail against a robber on the intellectual highway. Your friend who coined the term which you mention, probably little knew what its import to me was . . . it was not my rights, which I knew I could never get, that I defended, but the principle of right in the abstract, of common honesty and of fair dealing. If a proceeding like that may justly be styled a selfish one, then I must submit to the charge."

Akin did not give a clear answer to Stokes's inquiry as to whether he would visit England: "the scene to me of much that I should like to forget."[19] It seems to be a regular feature of coercive orthodoxy to diminish its opponents beneath conventional consideration by a systematic withering neglect. The victims of knowing calumny, becoming dogged and heated in their righteous self-defence are at once shorn of their original credibility and their capacity to threaten the authority to which they made their original appeal. It is therefore not coincidental that the misguided outpourings of carping cranks are frequently indistinguishable from the tantalised appeals of authentic victims. The R.S. archives for the nineteenth century contain numerous vivid instances of this process in action. By 1872 Akin was wholly estranged from the world of science.

The usual explanation of P. G. Tait's campaigns against Tyndall tends to stress the former's irascibility and the clash between his religiosity and Tyndall's increasingly loud materialism. The present case suggests that the storms of brickbats which frequently descended upon his head may actually have been deserved by his conduct.

* * *

Notes

1. James Challis was Senior Wrangler and First Smith's Prizeman of 1825. In 1835 he became Plumian Professor of Astronomy at Cambridge. The D.S.B. contributor on Challis describes him as "a spectacular failure as a scientist".

2. Stokes Papers (CUL), RS 470.

3. Eve and Creasey, ~~ibid~~., p. 107.

4. Ibid., 108.

5. Stokes Papers (CUL), A 615.

6. BAAS Reports., 1863, p. 11.

7. Stokes Papers (CUL), A 618.

8. Stokes Papers (CUL), A 619.

9. Stokes Papers (CUL), RS 475.

10. Hirst Jornal, 1706.

11. Tyndall's Journal quoted in Eve and Creasey (~~ibid~~., p. 109).

12. Eve and Creasey, ~~ibid~~., p. 109.

13. Hirst Journal, 1716.

14. Ibid., 1725.

15. Ibid., 1734.

16. Stokes Papers (CUL), RS 499.

17. Frank A. J. L. James, "Of Medals and Muddles, the context of the Discovery of Thallium: William Crookes' early spectro-chemical work", Notes and Records of the Royal Society of London, 39, no. 1, September 1984, pp. 65-90.

18. Stokes Papers (CUL), RS 548.

19. Stokes Papers (CUL) A 622.

CASE STUDY TWO: SCIENCE AND GOVERNMENT IN THE FIELD OF COLOUR VISION

During March of 1892, Herbert Rix despatched a letter to one of the R.S. Secretaries which made a rather curious reference to an exhibit of colour photography which was under consideration for the forthcoming Soirée. Rix observed that the newly ennobled President Lord Kelvin had judged the exhibit to be suitable and that it had been booked for the Royal Institution in the form of lectures. He concluded enigmatically:

> "so, suspicious as the subject matter is, I suppose the exhibit will not be unsuitable."1

At the end of April the report of the R.S. Colour Vision Committee was presented to the Board of Trade. The colour photographs were accepted for the Soiré, then on June the 9th, Rix wrote again to the Secretary R.S. stating that the Soirée Committee had decided to order their exhibitor to omit several parts of his accompanying description for the Soirée's printed programme: "as being matters of individual opinion".[2]

Interest and concern about the nature of colour vision was generated increasingly as the levels of marine and rail traffic multiplied during the last quarter of the nineteenth century. In 1889 the Board of Trade had a number of experts it was able to call on, one of whom was Dr. F. W. Edridge-Green. At this time the groundswell of interest which the subject was generating in public life prompted Lyon Playfair to warn Stokes (then in his penultimate year as P.R.S.) that the matter of colour blindness would be before the House as a question relating to maritime signals. Playfair intimated that the Members would expect to be addressed by their P.R.S. on the subject (he was the sitting M.P. for Cambridge University) but they were to be disappointed in this as they had been in virtually every other case where so eminently scientific a member might reasonably have been expected to speak. It became rumoured in scientific circles that Edridge-Green as the Board

of Trade's most dynamic scientific advisor on colour vision was endeavouring, with some success, to embarrass the officially approved Holmgren wool test in his work with Cosmo Monkhouse.[3] On the basis of his own new theory of colour vision which sought to usurp to some extent the established position of the Young-Helmholtz-Maxwell theory, Edridge-Green was allegedly proving able to select numerous colour blind subjects of a particular nature, who would consistently pass the wool test. His solution was the use of a lantern test which, he maintained, successfully detected these cases. Out of jealous regard for the pre-eminent position as the Government's chief scientific adviser which the R.S. had long held, its leading managers directed a letter to the Board of Trade soliciting a request for consultation on the whole question of colour vision. This letter was sent on the 25th February 1890 and preserved on page 51 of the Board's memorandum on the subject. At that time Edridge-Green had been an appointee to the Board's International Code of Signals Committee for nine months.[4] Thomas Gray replied to the Stokes letter of February 25th in the most disapproving terms, regretting the ethical basis of the Society's rather hostile initiative in the following words:

> "If we wanted the opinion of the Royal Society we were quite capable of asking for it."5

Gray went on to report his extreme misgivings about entrusting the matter to the Society because at the ordinary meeting on the subject which he attended at Burlington House there had been no one there except Professor Ramsay who knew anything about the subject. When an R.S. Colour Vision Committee became inevitable, Gray requested that Edridge-Green be the Board's representative on the new body. Gray died suddenly soon after and Stokes made his reply on the 31st March 1890:

> "It is obviously desirable that we should be put in connection with someone who is well-acquainted with the wants of the Mercantile Marine and the Railway Service and the Committee would I doubt not have gladly availed themselves of the services of a man like the late Mr. Gray. But I cannot find that Dr. Green has any official connection with the Board of Trade and if he be suggested on scientific grounds I would observe that it would be rather for the Royal Society than for the Board of Trade to select him."6

For the ensuing thirty-three years the R.S.'s officers stoutly denied that any of the above quoted correspondence ever took place. When much later questions were asked in the House of Commons by Mr. Arthur Lynch it was established that Edridge-Green's alleged lack of official connection with the Board of Trade was based on the fact that he had received no payment for his services.[7] When denying the facts of the case to Edridge-Green in 1889 Michael Foster stated:

> "My dear Edridge-Green, what do you take us for? We do not do things of that sort. I can assure you that the Government asked us to settle the question, and had your name been suggested by the BoT we should have been only too pleased to have had you on the Committee. I can assure you you are wrong as all the letters pass through my hands before they go out."8

Foster and Stokes in fact were both frequently given to write from home or any other places that they happened to be. The R.S. records contain no trace of Stokes' letter of the 31st of March, nor yet of Foster's letter to Stokes of two days earlier which evidently prompted it.

> "My dear President,
>
> Rix tells me that the Board of T. have nominated that fellow Edridge-Green to serve on colour vision c'tee in place of Gray - now he does not in any way represent the interests of the Board of T. and indeed there is every reason to believe that <u>he got</u> the BoT to nominate him - he will be a horrible nuisance on c'tee. Can you not see Hicks-Beach on this sub. on Monday or Tuesday and point out to him that while delighted to accept Mr. Gray or anyone in Mr. Gray's (position) (relationship) [Foster's deletions] place who could be what Mr. Gray would have been, a representative man, we did not expect the BoT to nominate a person whom we could have

> appointed ourselves if we had thought him desirable. It seems to me a bit of scandalous carelessness on the part of the Board and I shall certainly ask the C'tee to decline to accept the BoT's nomination - but it would be much better to arrange it favourably . . .."9

Despite repeated requests and ultimately the vindication of his lantern testing technique by its almost universal adoption, Edridge-Green was unable to acquire documentary evidence of the Royal Society's systematic deceptions from the Board of Trade until 1922. As one might expect, by the time he was permitted to redeem his credentials the issue was dead and Edridge-Green's working life was virtually over.

Earlier, Edridge-Green had quite shone in his chosen direction of life, winning his year's gold medal for an M.D. thesis on colour perception. At the same time as his position with the Board of Trade was approaching its critical juncture with the R.S. officers striving to appropriate scientific control of colour vision, Edridge-Green submitted his first paper dealing with his novel interpretation to the Society. He later reflected on this action:

> "I then committed the most foolish action of my life, namely in reading a paper full of new facts to the Royal Society in which I pointed out that the generally accepted method of testing for colour blindness were very defective and suggested others now universally adopted."10

The paper was received on the 28th January 1890, read at a lively and lengthy ordinary meeting on February 6th at which it was vociferously opposed by the mathematical physicists led by Lord Rayleigh, and subsequently archived on February 24th. Dr. Lauder Brunton communicated the paper, of which only the title was printed in the Proceedings (XLVII, 176). In the event Rayleigh as Secretary R.S. had referred the paper to himself.[11] There is evidence showing that Edridge-Green had been aware to some extent of the opposition to his unorthodoxy as much as nine months earlier. On the 1st May 1889 Rix replied to his inquiry as to who would be on the Committee considering his request

for a Government grant. The applicant was told that no such information was forthcoming, in line with usual practice.[12] The R. S. Colour Vision Committee was appointed in March 1890, immediately following Edridge-Green's exclusion from the work that the Board of Trade had appointed him to do, and the prompt consignment of his paper to the archives. The new Committee consisted of Rayleigh, Stokes, William Thomson, Mr. Church, Dr. W. Pole, R. Brudenell Carter with William Abney as its Secretary. The physicists were all committed supporters of Holmgren's wool test, which was duly vindicated in their report issued in April 1892.

William de Wiveslie Abney, eldest son of Canon Abney of Derby, became an authority on scientific photography following a career in the Royal Engineers. A year after the rejection of his first paper Edridge-Green took three cases of dangerous colour-blindness, all of whom could pass the wool test, to Burlington House where their feat was demonstrated to Abney. Michael Foster then spoke encouragingly to Edridge-Green to the effect that he had a strong case which should be published by the R.S. However, he would not offer to assist in this himself as he had: "always held a contrary opinion."[13] Edridge-Green later heard through the communicator of his second paper that he had received a letter from Foster marked "Strictly Private" which advised him to withdraw the paper as being quite unsuitable for publication by the R.S. Many years later this same paper was published in the Proceedings. In the course of his attempts to circumvent the barriers put in his way by the Society, Edridge-Green sent abstracts of his papers to Helmholtz who acknowledged the significance of the new facts and inserted a reference to their author in the second edition of Physiologischen Optik. At home the Opthalmological Society followed the R.S. lead and refused to publish a paper for which it had originally made a particular request. Following the appearance of the

Colour Vision Committee Report, the Board of Trade established a colour ignorance test based on the Committee's endorsement of Holmgren's technique of colour matching. This signalled the demise of Edridge-Green's radically different colour naming method. The Board's test failed to detect a single instance. Quite apart from the inherent defects of the Holmgren test, a successful performance could be faked and many of its subjects had already passed exams for positions of responsibility. Edridge-Green's volume entitled Colour Blindness and Colour Perception appeared in 1891 as a part of the "International Scientific Series". Its author was inclined to suspect Dr. William Pole of being responsible for the sharply dismissive review of the work which appeared in Nature. This was surprising to Edridge-Green as Pole had been sympathetic towards him and appreciative of his work. It transpired that Pole, although used by the editor of Nature as an authority on colour vision for a number of years, had been roughly displaced to make way for another. Pole had actually been the communicator of Professor Holmgren's paper: "How do the Colour Blind See the Different Colours?", printed in the Proceedings by the R.S. in 1881.[14]

On the 22nd June 1891 Michael Foster arrived late to dine alone at the Athenaeum, having spent the day at Swindon testing employees of the Great Western Railway for colour blindness as a part of the work of the R.S. Committee. Foster had been accompanied on this excursion by Stokes, Rayleigh and Thomson.[15] Over 100 people were submitted to the Holmgren test while Edridge-Green, who arrived inexplicably late had time to test only 8 men with his lantern. Following the Colour Vision Committee's report of April 1892, Abney, the Committee's Secretary, became the chief beneficiary of Edridge-Green's "favourably arranged" disappearance from the scene. Abney was appointed as permanent colour vision advisor to the Board of Trade in his place. When Edridge-Green

had a further paper communicated to the R.S. three years later in 1895 Rayleigh referred the work, entitled, "The Perception of Luminosity at Different Points of the Retina", informally to Abney on the 8th February. His report recommended its withdrawal.[16] William Abney continued to prosper throughout the new decade. He was appointed to the Secretaryship of the Department of Science and Art in 1899, and made a K.C.B. in the following year. The 1890's saw the executive group of the Royal keeping up an impenetrable front in the face of Edridge-Green's attempts to have his work publicised. In February 1892 he had a paper communicated on binocular vision which prompted the following response from Rix in a letter to Michael Foster:

> "It seems that Dr. Edridge-Green's paper was already communicated by Dr. H. Hicks, who indeed wrote to me to say so. I have therefore been obliged to register it."17

When Edridge-Green had been fully discredited by the R.S.'s careful dismissal of all his papers and the official approval of the wool test evinced by the Committee's report, his career was ruined. He was cast in the rôle of a disappointed crank without the capacity to detect his own errors. The B.M.A. repeatedly refused him a research grant whilst he could obtain no other appointment, scholarship or any other kind of support for his work. In 1904 he went to Cambridge at his own expense to pursue his research, but was utterly frustrated by the academic authorities. In the account of his experiences connected with promotion of the new theory of colour vision, Edridge-Green alleges a network of influence with the leaders of natural science at Cambridge. Of course in a number of vital cases the same individuals occupied key rôles within both centres of power concurrently. After two years at Cambridge during which he had been treated with blank disregard, his supervisor addressed him in the following manner:

> "You don't know the elements of the subject. Who are you to criticise the greatest men of all time? The subject has been thoroughly worked out. It is a difficult subject and it took even me some considerable time to properly understand it and what I don't know about the subject isn't knowledge."18

The target of this tirade later asserted that the R.S. was "almost only an appendage to Cambridge". When Edridge-Green's second paper was read at the R.S. in February 1892 Rayleigh immediately stood up and said, in words reported by its author:

> "I can definitely state from my own experience that every statement in this paper is erroneous."19

This account has dealt in close detail with the case of Edridge-Green because it gives tangible support to the foregoing critique of the popular view of the R.S. in the late nineteenth century. This complacent view projects an inaccurate picture of the Society's scientific and moral transfiguration in 1847. Important and premeditated diversions from the notional path of pure rationalism (fondly constructed by the high-minded writers of official histories) are more usefully seen not as quirks of malpractice, but as fundamental to the social nature of all scientific activity. This outlook, obvious enough in itself, can be seen as a reworking of Faraday's emotional plea on behalf of human sympathy on the occasion of his first meeting with Tyndall at the Royal Institution. There they were to work together in spite of serious religious, philosophical and scientific differences. Faraday said to Tyndall:

> "Science itself is not the principal thing, we are men and ought to have human feelings."20

As I have attempted to show, the easy identification of the rational structure of a guileless natural world, with the intellectual and moral disposition of its human investigators, provides a barrier to understanding of the social construction of the meaning of scientific activity.

In the case of Edridge-Green, one has to look far wider than individual animosities before it becomes possible to form an estimate of the real cause of his woes. Two wholly separate schools of thought developed to treat the subject of colour vision during the nineteenth century. The imposing group of Cambridge physicists who held such great sway at Burlington House for the last third of the century and beyond had naturally cleaved to the approach which became known as colourimetry. This consisted of measuring precisely the response of the eye to colour stimuli and calibrating this with objectively fixed standards of light and colour. This approach was in accord with the high regard in which Thomas Young was held in mathematical and physical circles at Cambridge. The full embrace there of Young's wave theory of light produced by association a favourable medium for the growth of commitment to his trichromatic theory of colour vision. The modification of the theory by Maxwell and the Cambridge physicists (backed up by an important contribution from Helmholtz) ensured that by the late nineteenth century, the whole subject of colour vision had been marshalled within the frontiers of physics by a group of physicists who controlled nearly all the institutional barriers to entry upon the subject. The mutually reinforcing power centres of Cambridge and Burlington House embodied the scientific vested interests of men who although very powerful, had been challenged by the aggressive incursions of the scientific naturalists to an ever more alarming extent as the century wore on. Consequently they reacted with concentrated vigour when a man approached them seeking to put the august might of Cambridge physics to rout with a few simple observations from what were seen as the stilted underworlds of physiology and psychology. The inconvenient viability of Edridge-Green's views could quite well pale into insignificance beside the vastness of the outrage he was seen as trying to perpetrate. The phenomenological approach to the question of colour

vision stressed the interpretive basis of the experience of light and colours as a variable cerebral function. This outlook necessarily saw little merit in the three-receptor theory with its accompanying attempts to calibrate retinal responses as if the eye were a sort of scientific instrument which had been universally "set". R. Steven Turner has succintly noted what is strikingly apparent from a scrutiny of the literature concerned with the history of colour vision theory; that the colourimetric camp and its support of the Young-Maxwell-Helmholtz theory has remained its almost exclusive concern. Turner, in the course of a review of a recent example of this sort of historical approach, remarks that from the pages of the book in question the reader would remain quite unaware that:

> "by 1890 most German psychologists had rejected the Young-Helmholtz theory as an adequate account of colour vision or that the facts of colour mixing as revealed by Maxwell were widely held to be incompatible with other, very different theories."21

By an irony which owes nothing to coincidence, the historigraphical neglect of the phenomenological approach represented by Edridge-Green mirrors faithfully its contemporary treatment by the colourimetrical school within the R.S.

Rayleigh had read Maxwell's work on colour vision as early as 1865 and five years later at the Liverpool meeting of the B.A.A.S. followed the reading of a paper on the subject by his master, with one of his own.[22] Rayleigh's son's biographical account of his father was published in 1924, the year of Edridge-Green's played-down vindication. This came about when Edridge-Green gained access to the Board of Trade's correspondence with the Royal Society on colour vision for the years 1889 to 1892. Robert John Strutt, for reasons which have earlier been made apparent, felt constrained to insert a quite disconnected paragraph into his biographical account:

> "Experiments made at a much later date for a departmental Committee under the Presidency of Sir Arthur Acland, of which Rayleigh was a member, showed plainly that those who failed with the wool test failed also on tests with distant coloured lanterns."23

In June 1900 Stokes wrote to Abney assuring him that the Colour Vision Committee was not going to be recalled as an insufficient case had been made out for the frailty of the wool test. Stokes had ascertained this by soliciting opinions from the members of the original committee. A year later Secretary R.S. Arthur Rücker sounded the first warning note from inside the self-electing oligarchy of the R.S. executive by inviting yet more reconsideration from Stokes:

> "If Mr. Green's statements are correct the question as to whether the recommendations of the Committee should be in any way altered may have to be considered and Foster and I think that the best thing to do is to send Mr. Green's communication to you and ask you whether you think it desireable to call the Committee together again."24

Nothing was done, and Abney dismissed the increasing regard in which Edridge-Green was held by opthalmologists by defining them as not being involved in the subject "in a scientific [i.e. colourimetric] way". By 1906 Edridge-Green had the support of the German leader of colour vision theory von Kries who repeated some of his work. Professors Bayliss and Starling started to back his cause, the latter securing for the now middle-aged Edridge-Green a Beit Fellowship at University College London. At an ordinary meeting of the R.S., Green was permitted to read a paper, during the discussion of which Rayleigh objected to the division of the spectrum into 18 monochromatic divisions. He maintained that he could resolve thousands. (Rayleigh had for many years been given to administering the wool test to guests resident at Terling, his country seat, in the form of a parlour game.) Green adjured his illustrious persecutor not to declaim prior to any knowledge of the facts, which he had been offering to show him since 1890:

> "This remark was received with considerable applause by the Royal Society as there were many present who were aware that the facts were as I had stated."25

The next day in Green's laboratory at U.C.L. Rayleigh was astonished to be able to discern just 17 monochromatic divisions on the home-built spectrometer. Shortly afterwards Green succeeded in having a paper published by the R.S. for the first time. In 1909 the Royal Navy adopted Green's lantern test. By 1914 it was in universal use by railway companies. The Board of Trade were introducing a pirated version for all their colour vision testing. A bead test devised by Green was used as the colour vision test for entrants to National Service throughout the First World War.

The anatomical absence of triple-nerve fibrillae necessitated by the Young-Maxwell-Helmholtz theory could now be recalled by researchers without putting their prospects in jeopardy. These structures had been conjured up by Hering and von Kries in order to deal with the phenomena of colour vision under the ruling theory. At the close of his book, Green seems to show a degree of misunderstanding of the system in the toils of which he had fared so ill. In suggesting an independent appeal board for science he leaves unsaid any suggestion of an alternative source of legitimacy for this body.

Although his methods were uniformly adopted, Edridge-Green remained a "non-person" for all practical purposes in the scientific sense. Although he was appointed as Special Examiner and adviser to the Board of Trade in matters relating to colour vision and eyesight in 1920, Green was never proffered any form of redress by the R.S. He received a niggardly acknowledgement from Rayleigh who had so glowingly restored the honour of J. J. Waterston, but continued to be refused a Government Grant and excluded from the Fellowship. In 1923, with all but the last few details of his case brought into the open Green wrote to Burlington

House asking for access to his original archived paper of 1890. The Secretary William Hardy replied after a delay of 10 months. In a memorandum to the Assistant Secretary F.A. Towle, Hardy said:

> "The paper he refers to was, I suppose, archived. Can I hush it up? How can we prevent him publishing an amended version of the original?"26

Having stalled for a further three years Hardy wrote to Green on the 4th August 1926 to say that he was about to go away on holiday.[27]

* * *

Notes

1. Royal Society, NLB 6, no. 128

2. Royal Society, NLB 6, no. 251.

3. F. Holmgren, Method of Testing for Colour, LNWR, 1880.

4. Royal Society, CD 1079.

5. F. W. Edridge-Green, Science and Pseudo-science. The necessity for an appeal board for science, John Bale Sons and Danielsson Ltd, London, 1933, p. 11.

6. Ibid., p. 52.

7. Royal Society, CD 1079.

8. Edridge-Green, ~~ibid~~., p. 14.

9. Stokes Papers (CUL), F 262. In this instance Foster's duplicity is unmistakable. Having assured Edridge-Green that "we do not do things of that sort", he is here at pains to effect something of precisely "that sort". Foster aims to achieve this by making sure to "arrange things favourably".

10. Royal Society, CD 1085.

11. Royal Society, Register of Papers. The paper in question is designated Ap.66.22. in the General Card Catalogue.

12. Royal Society, PLB 3, no. 162.

13. Edridge-Green, ~~ibid~~., p. 14.

14. Royal Society, Register of Papers.

15. Hirst Journal, 2783.

16. Royal Society, RR 12, no. 314.

17. Royal Society, NLB 6, no. 2.

18. Edridge-Green, ~~ibid~~., p. 20.

19. Ibid., p. 24.

20. Eve and Creasey, ~~ibid~~., p. 43. The point being made here is the obverse of that expressed in the quotation.

21. R. Steven Turner, "Colour Vision", British Journal for the History of Science, 1983, 16, p. 298.

22. Strutt, ~~ibid~~., p. 44.

23. Ibid., p. 177. These tests took place in 1912.

24. Stokes Papers (CUL), RS 2176.

25. Edridge-Green ~~ibid~~., p. 24.

26. Royal Society, CD 1086 and 1087.

27. Royal Society, CD 1089.

CHAPTER FIVE

THE ROYAL SOCIETY'S FOREIGN RELATIONS

Late in 1884, the Government of Siam sent an inquiry to the Royal Society via its legation in Paris. The Royal House of Siam were eager to know why the sun had turned green.[1] This sort of incident was not typical of the Royal Society's participation in scientific internationalism during the second half of the nineteenth century. The Society's occasional involvements were mostly restricted to the prestigous celebrations usually associated with the presentation of medals. Most scientific projects which were dignified by the description "international" were actually concurrent involvements on the part of several nations who were more or less explicitly in competition with one and other for the plaudits which would follow a spectacular success. Such was the case when Britain and Germany were loosely associated in connection with work on tropical diseases in Africa towards the end of the century. Secretary of the Royal Society, Michael Foster, expressed his regret at the relative lack of success being enjoyed by the Royal Society's Fieldworker in 1896.

> "From such scant notices as have reached us Koch seems to have been successful with the Rinderpest. We always felt that the Rinderpest was much more hopeful than the Tsetse, and promised to be a much shorter business, more easily producing kudos, and it is rather hard that we have lost that. Still, if we do get at the bottom of the Tsetse, our ultimate reward will be all the greater."2

International science has often been assumed to have been a noble agent in the cause of civilisation. In fact, the development of international science from the seventeenth century onwards, illustrates not so much the gathering momentum of a global sense of spiritual brotherhood as the consistent denial of that ideal. International science has grown up out of the conflict between the constantly reiterated version of its beneficence, and the far more effective impetus of self-interest, political expediency and nationalsim.[3] In 1897 the Royal Society

smartly lost interest in a plan for an international conference to bring about the uniformity of the calendar when the Astronomer Royal found out who was promoting it. The current incumbent, William Hunter Christie, wrote to Burlington House pointing out that:

> "if I remember rightly Monsieur Jardini de Quarenghi was the gentleman who, in connection with the universal time question proposed Jerusalem as the Prime Meridian."4

Christie was referring to the 1889 International Conference at Washington which was concerned with the determination of an international prime meridian.

The high-handedness of international scientific dealings at the institutional level is frequently counterposed by the refreshing candour of personal relationships between individuals across national frontiers (see note (3)). In the late nineteenth century, as in the present, individual entrants to the international scientific domain were members of a very small and exclusive group of the most spectacular domestic scientific performers. Of the tiny group of men with well-known names who published to the international community, some would be recognised in foreign centres of study. For these few, the praise of foreign rivals was the next crucial stage in the accumulation of prestige to be undertaken after the conquest of home fields. The praise of foreign rivals carries the most weight because usually it is the hardest won in the face of an obvious and inevitable shade of national bias. The elaboration of international means for the evaluation and reward for scientific performance was a predictable outgrowth of the general expansion of scientific activity. It was facilitated in practical terms by the power which the most highly accredited scientific men (on the national level) achieved in the scientific societies. In many respects the process resembles the escalation of the means of allocation of prestige, rewards, and power in other fields of endeavour, such as international sport. In both cases we can roughly reduce the false

portentousness and vainglory of internationalism to one thing: the ambitious elevation of each particular department of plaudit-seeking to its highest imaginable level. In this rarefied atmosphere the twin peaks of Nobel prizes and Olympic Gold Medals beckon almost indistinquishably.[5] The Royal Society regularly distributed honours to foreign men of science and so was the vehicle for many amicable international contacts. Apart from the medals and foreign memberships which were the preserve of the very few, there was occasionally some sharing of practical problems of the sort faced by workers in the same field. In the case to which the following quotation relates, the French astronomer Antoine d'Abbadie sought Stokes' assistance in a vital matter in which there was known to be some earlier English experience.

> "But enough on my darling hobby. I scarcely expect that you will have time to read my long letter. If however you can proceed thus far, I request as your answer to receive from you a number of the Proceedings of the R.S. which may be worth its weight in gold to me. It is vol. XIII (Dec. 10&17) and contains Sir J. South's Obsun's [sic] on the tremors caused by trains in the neighbourhood of any observatory. The Bayonne railway runs within 600 metres of mine. Judgement for damages has been reserved, and if I can prove that my pillars shake, I shall get enough money to purchase my transit instrument, available between trains."[6]

The question of scientific merit was central to the award of the Royal Society's foreign memberships and medals. The formally meritocratic procedure which the Society self-consciously promoted as the sole mode of entry for its ordinary fellows was not invoked throughout the century in the international sphere. The maintenance of a mainly personal basis to such involvements clearly ran counter to the trend which defined impersonal bureaucratic methods as correct for an institution increasingly composed of middle-class newcomers to the expanding professoriat.[7] When the new sort of certificate which allocated space for details of publications as part of its printed format was stipulated for the

proposal of ordinary fellows in 1863, the foreign certificates remained immune to such harbingers of modernity.[8] For the proposal of a foreign member a blank sheet was employed. On it were placed the supporting signatures and a recommendation that might be as brief or as idiosyncratic as the proposer willed. The proposal and promotion of putative foreign members was usually performed by Fellows who were old friends of their nominees. Of course this was also very frequently the case in the election of ordinary fellows, but in keeping with the letter of the 1847 statute reforms, the fact was never officially acknowledged. The compromise of the ideal of impersonal meritocratic selection was a natural outcome of the factional division of the active Fellowship.[9]

The highly personal nature of the foreign business of the Royal Society is clearly shown in the incidents surrounding the award of the Copley medal to Michel Chasles in 1865. Chasles was the former teacher of Hirst, his proposer. Hirst visited his mentor in Paris at irregular intervals and kept up a strong relationship. The other nominees for the 1865 Copley medal were Plücker, Regnault, and Poncelet. Stokes was the supporter of Plücker having entertained him during visits to England and kept up a fairly regular correspondence. W. H. Miller of Cambridge and Price of Oxford were similarly involved on a personal basis with Regnault and Poncelet respectively.[10] When Chasles was unable to attend the Anniversary meeting of the Royal Society in order to receive his medal, Hirst responded on his behalf to the President's words of congratulation.[11] Three weeks later Hirst travelled to Paris with Michael Foster in order to deliver the medal personally. When they arrived at his house in the Passage Saint Marie, Rue de Bac, Chasles had been studying Weld's History of the Royal Society and "knew precisely the value of what he had received."[12]

Nearly two years later national passions were inflamed when with seeming eagerness some prominent Frenchmen (including distinguished men of science) seized upon the Vrain-Lucas letters which sought to glorify France at Newton's expense. It was particularly embarrassing for the Royal Society that Chasles decided to take a leading part in the promotion of the letters. Hirst was provided with a Government Grant of £20 by the Royal Society for him to travel to Paris and photograph the letters alleged to have passed between Newton and Blaise Pascal.[13] Hirst was given the delicate duty of marshalling the English case in support of Newton's originality, and then facing Chasles with the result. Eventually after a good deal of nationalistic posturing, the letters were generally condemned as (rather weak) forgeries.[14] These incidents illustrate how the actual conduct of international science under the aegis of the Royal Society, was of a highly personal nature. Although the Foreign Secretary was supposed to conduct the Society's overseas contacts, this was rarely the case. Almost invariably the Fellows who tried to boost the case of a particular savant at Burlington House were his personal friends. They would take care of the correspondence and hospitality requirements themselves without reference to the Royal Society's official channels. This of course meant very little work for the Foreign Secretary. When Joseph Lister was pursuaded by Michael Foster to accept the Presidency of the Royal Society in 1895, a reluctant Edward Frankland was cajoled by Lister in his turn to accept the Foreign Secretaryship which would thereby be vacated. Foster tried to win Frankland over by asking him to agree to fill the post for just a year. Lister assured him that:

> "Your doing so will give uniform satisfaction, and I can assure you from personal experience that the duties are of the very lightest description."15

The procuring of medals and Foreign Memberships by their British friends for eminent men of science from abroad was not a wholely erratic process. The success of foreign scientists in securing Royal Society honours appears to have been attended by some rather obscure tacit conventions. The rough standard of reciprocity which the major factions within the Society operated in regard to domestic matters seems to have extended somewhat into the international field. As Adam Sedgwick's Copley medal was followed a year later in 1864 by Darwin's, so that acquired for Chasles by Hirst in 1865 was followed a year later by success for Stokes in providing the Society's highest honour for Julius Plücker. Huxley managed to get the medal for his old mentor Karl Ernst von Baer in 1867. In 1870 the leading Cambridge and Scottish physicists were able to thwart Tyndall's promotion of von Mayer by ensuring the prior award of the Copley medal to his rival Joule.[16]

The style in which the various European academies transacted their business did not necessarily resemble that which was usual in London. Walter White described an ordinary meeting of the Royal Society in 1862 at which the foreigners present were Forchhammer, Dove, Regnault, Delesse, Stas, Frémy & Captain Belavenitz. White reported as follows in his journal:

> "Stas said to me after the discussion he was astonished at the dispassionateness of the speakers, that such a discussion in France would have become violent and personal."17

At this time, meetings of the Institut in Paris were open to the public and "very numerously attended although held about the middle of the day", according to John Stenhouse. He suggested to Sharpey that the public should be admitted to the ordinary meetings of the Royal Society because papers of high quality and the discussion accompanying them often took place "in the presence of a mere handful of auditors."[18] In 1872, Hirst was taken to the Academy (Institut) in Paris by Chasles. He placed Hirst in the centre of the meeting in front of the President. From

there Hirst heard a heated controversy between Frémy, Balard, and Pasteur.[19] The interplay of personal friendship, self-interested pomp, and nationalism was not always kept in a decorous state of balance. In early December 1892, the arrangements for James Glaisher, J. J. Sylvester and A. G. Greenhill to represent the Royal Society at the 70th birthday celebrations of the French mathematician Charles Hermite in Paris, seemed to be satisfactorily settled. Without warning the representatives, apart from Greenhill, withdrew. Assistant Secretary Herbert Rix wrote to Archibald Geikie, as Foreign Secretary to inquire as to official policy in the matter.

> "I must send you the enclosed letter from our Fellow Dr. Glaisher. You will see that he brings a very serious charge against Hermite, and I imagine it may be just a question whether the Royal Society, under the circumstances, should be represented."20

The correspondence was all sent on to Greenhill who still wished to go to Paris. Rix wrote to him dissociating the Royal Society from his attendance at the Academy.[21] Glaisher's charge concerned an incident which took place six years earlier in connection with a prize competition organised by the Academy. A Cambridge man, Smith, solved the problem which constituted the competition but had to share the prize with a German whom the Cambridge contingent took to be an unrepentant plagiarist. The fragility of even individual commitment to the ideals of the scientific cosmopolite is starkly revealed in Hirst's account of the affair. Whilst in conversation with the Academician Halphen over dinner in 1886 the conversation seemingly turned to the matter of the disputed prize competition.

> "He told me once more the story of the blunder committed by Hermite and Camille Jordan relative to the prize which a year or two ago was divided between Smith and an unknown German. In proposing the prize the Academi [sic] des Sciences was evidently ignorant of the fact that Smith had already solved the problem of the 5 [?] squares and had even published his solution in outline. The German,

> who was unknown to fame, had evidently seen Smith's solution; but the Academy looked on both as of equal merit and divided the prize. I ventured to say that the French Savants, before proposing such prizes, ought to make themselves a little more conversant with what has been done in other countries. Halphen evidently did not like my remark, and replied with a tu guogue one. Before he was a member de l'Institute he entirely agreed with me; but he had evidently forgotten the fact. His friend Collet was decidedly of my opinion."22

Britain and Germany: Rivalry in an Unequal Partnership

The Royal Society's organisation of the International Catalogue of Scientific Papers in 1898 reveals something of the tensions which had developed in international scientific affairs at the end of the century. The domination of international scientific publication by Germany and the German language spurred the Royal Society into taking the initiative in this matter.[23] A few years earlier the Society had achieved something of a coup in terms of international prestige by securing Greenwich as the base for the prime meridian. The difficulties involved in acquiring prestige from the International Catalogue were different because Germany could almost afford to ignore the Royal Society's attempt to annex this area of activity. German control of the vital channels of scientific communication was so broad and pervasive that in the absence of co-operation from the Kartel of German academies, the Royal Society's assumed rôle of leadership would have been rendered ineffective and redundant. As the time of the Conference on the International Catalogue approached, the Royal Society's Officers became increasingly disturbed at the haughty indifference of the Germans. The Kartel Academies did not reply to increasingly desperate inquiries from Burlington House. When the conference was imminent a series of last-minute telegrams were sent out. These somewhat petulant pleas do not accord with the image of refined self-assurance which has been portrayed as the august stance of the Society in its wider institutional

and political setting by the end of the century.[24] The following quotation is from a telegram sent to Dyck, the representative of the Munich Academy on the 6th October 1898.

> "Absolutely essential you should attend conference to watch proceedings on behalf of your academy and Kartel. We are informed German Government anxious to participate but not ready. Armstrong. Royal Society."25

A further anxious request was despatched to Weiss at the Academy in Vienna. It culminated in the sentence: "we rely absolutely on your attendance." In order to reinforce the plea sent to Munich the Royal Society's Assistant Secretary Robert Harrison was required to send a further telegram to Arthur Rücker (Royal Society Secretary) at Leads where he was attending a festival at the Yorkshire College.

> "For Rücker at Festival. Armstrong begs you to beg Dyck to come in any case to represent Cartell or his Academy. Harrison."26

The German claim of a lack of warning about the proceedings was plainly a sham got up to form a pretext for leaving the Royal Society to its own devices. The Kartel and the German Government (which also refused to send representatives) had known of the general arrangements for the conference since May 1898.[27]

International transactions in science are inevitably occasions which prompt comparisons between individuals and nations on the levels of personal material advantage and overall scientific performance, respectively. At the beginning of the second half of the nineteenth century the technologically linked expansion of the German scientific establishment had not become a dominating trend. English science was not then preoccupied with its later sense of having been eclipsed by German efficiency and generous funding. When Hermann von Helmholtz visited George Airy's house in the course of a visit to London in 1855, he remarked in a letter to his wife:

> "Airy's house and family life were arranged, as we should say, in style, but it is so with most of the English professors."28

In the following year the prominent German physiologist Emile Du Bois Reymond was building the conviction that his professional future lay in England. He informed his friend Carl Ludwig that: "it is more than likely that in a while I shall take up a post there like A. W. Hofmann."[29] When the financial basis of scientific work in Germany expanded rapidly in the third quarter of the century, the frailty of Britain's international scientific reputation became increasingly apparent. The possibility of continuing the tradition in which Britain's handful of heroic, isolated amateurs forged her scientific identity, was not entertained by the progressive party whose agitation brought the Devonshire Commission into being. The pre-requisites for doing science at the highest level were rapidly changing. In 1886 Edward Frankland wrote to Huxley lamenting the vastly superior facilities for research on the Continent.

> "In regard to chemical and physical laboratories Rome, Zürich and Naples have left us far behind. Zürich is casting aside a chemical laboratory at least equal to any in this country & has nearly finished a new one on a magnificent scale. The Bundesrath voted the money £70,000 without discussion."30

At the time of the Devonshire Commission the Zürich Polytechnic had 60 professors and lecturers. At South Kensington there were 12, and in all the departments of Owens College, 17.[31] Had Du Bois Reymond come to this country in the 50's when he was of a mind to do so, his working environment as a physiologist would have developed on very different lines from that which he came to enjoy having remained in Berlin. In 1878 he told Carl Ludwig:

> "Würtz was with us today, wide-eyed looking at what we have done with part of the billions."32

The growing disparities between the scale, wealth, and prestige of the British scientific establishment and those of the leading European nations tended increasingly to inform the disposition of the Royal Society in its foreign dealings. A certain defensive pugnacity is evident in the Society's part in numerous incidents. Good examples are the Vrain-Lucas forgeries and the manoevrings in respect of the International Catalogue, both of which have been described earlier.

Huxley wrote an article for The Times early in 1887 which declared that the emulation of German feats by copying her methods was the only way of saving this country's industrial position. A form of resigned sycophancy towards German scientific work became the normal outlook of even leading researchers in this country. In connection with a paper which he had recently submitted to the Royal Society, the Sheffield amateur Henry Clifton Sorby declared himself: "proud to think it is requisite, and that an Englishman should be able to correct and extend German work."[33] In the closing years of the century few took any pains to disguise their eagerness to follow the German model. Following a meeting of the Committee of the B.A.A.S. in 1896 its Secretary Douglas Galton communicated with the Royal Society.

> "At the meeting of the British Association, in September, it was resolved to take means to extend the scientific usefulness of the [Kew] Observatory, especially in its relation to technical work in connection with various industries, on the principle of one branch at least of the Reichsanstalt at Berlin, in order to relieve the higher branches of application of science, to industrial work in Britain, from its present dependence on French and German establishments."[34]

For many years the time-burnished grandeur of the Royal Society had formed a sufficient source of prestige. The lack of well-organised research over a broad front of scientific fields in this country appears not to have held a great deal of significance for many Fellows of the Royal Society. A precious few first-rate men were still acquiring

accolades for this country in the international arena. This made it possible for most of the Fellowship at the close of the century to see the lustre of the Society as undimmed by the proliferation of highly industrious professional men of science in Germany. In the words of Joseph Hooker already quoted, the ordinary Fellows were quite content "to put F.R.S. after their names" and care little else for what went on at Burlington House.[35] The official lists of proposals of foreign men of science for the Foreign Membership of the Royal Society portray a clear picture of German domination of much of the wider scientific domain.[36]

Proposals for Foreign Membership R.S.

Year	France	North America	Germany
1878	5	1	16
1888	2	2	5
1899	3	5	18

According to the outlook of the devotees of the amateur tradition in British science, "organisational science" in the German fashion was an ignoble distortion of the field of play open to the heroic individual. In any case the traditions bound up in the Royal Society ensured that its accolades were highly prized by foreign savants. Eminent foreigners could still be impressed when they visited Burlington House. Geikie described the opinion which Louis Pasteur formed of the Royal Society's apartments.

> "One day he [Pasteur] drew an amusing contrast between the scanty accommodation accorded to science in the Palais de l'Institut with what he called the palatial quarters provided for the scientific societies at Burlington House. I was able to assure him that we had almost outgrown the quarters assigned to us."[37]

From an early date the prestige value of entertaining illustrious scientific foreigners had been realised. In 1861 the B.A.A.S. formed a plan to attract more scientific visitors from abroad. Huxley and Hooker did not approve of this plan to subsidise the hotel and travelling expenses of foreign visitors by means of a subscription list to be circulated amongst the British Association's members. Thinking the whole idea misconceived and likely as a result to attract far from illustrious visitors, Huxley wrote to Hooker in the following terms.

> "If the British Association want to play the host, the British Association, to my mind should do it using its own funds. I am most willing to do anything my means will permit for legitimate scientific purposes but considering the great doubts (and I think you share) as to the utility of the BA itself, I confess I am not greatly disposed to give money to make it more attractive to the golessmuches who meet together at foreign [-]."[38]

At the end of the century there were a few voices still raised against the dominant rôle of the Royal Society as the allocator of the major resources of scientific kudos available to practitioners in this country. When the question arose of the Royal Society's possible entry into the predominantly German International Association of Academies, national feeling tended to obscure disaffection within the Society itself. This situation is presented in detail in the present writer's MSc. thesis already cited. The Professor of Mathematics at Edinburgh, George Chrystal, was not pleased by the reaffirmation of the Royal Society's dominion over British science which he saw in its affiliation to the I.A.A. Chrystal later told Joseph Larmor that he thought the I.A.A. a "trumpery matter" born of "Congressional mania". More significantly, Chrystal commented on the nature of British involvement in the Kartel-based I.A.A. He told Larmor "there is no doubt that the Germans outmanoevred you."[39]

The relative perspective which developed between Britain and Germany in scientific circles was fraught with the barbed generalities

which usually afflict international relations. Reference has already been made in an earlier chapter to Helmholtz's address at the Deutscher Naturforscher Versammlung which took place at Innsbrück in 1877. Helmholtz expressed the opinion that Germany stood at the forefront of the struggle with traditional authority, that there "was greater fearlessness of the entire truth."[40] A decade later, Francis Darwin developed an exploration of the opposite of that state of affairs. Having recently returned from a visit to Germany, he was convinced that the reason for the lukewarm reception of his father's work in that country lay in the organisation of its science. He stated this view in a letter to Huxley.

> "I was tremendously struck in G. by the terror of offending big guns exhibited by the Privat Docents - one of these at Würzburg would not even work in the laboratory for fear of getting at logger-heads with Sachs. A man called A. B. Frank was nearly starved, and was made miserable for 15 years by Sachs and Hofmeister, because he held unorthodox view on geotropisms - which have since been accepted.* Fortunately Sachs has not the whole patronage and de Bary is a gentleman and a fair minded man. If things were as bad in 1860 it would be quite reason enough - what do you thing?
>
> * I have heard Sachs boast how at last Frank came 'with his hat in his hand' and then he recommended him for the post he now has."41

What is not generally realised is that the communication most often received from abroad at Burlington House was from British men of science forced to go abroad for lack of paid scientific work in this country. Once elected to the R.S. these men were obliged to apply for a deferral of the time of their formal admission to the Society. For a significant number of these men, it would be a long time before they were able to be in London at a time suitable for their admission. For the men who could not break into the domestic scientific scene, the empyrean heights of true international recognition must have seemed distant indeed.

* * *

Notes

1. Royal Society, MC 13, no. 9. The Royal Society eclipse expedition of 1875 had included Siam as one of its observation stations. Arthur Schüster had been in charge there. The explosion of Krakotoa was presumably responsible for the sun's unusual colour.

2. Royal Society NLB 14, no. 285. Foster was writing to Sir Walter Hely-Hutchinson who was responsible for the Whitehall end of the arrangements.

3. Paul Forman, "Scientific Internationalism and Weimar Physicists", Isis, 1973, 64.

4. Royal Society, MC 17, no. 40. The Royal Society used the prospect of Greenwich being established as the universal prime meridian as a means of putting pressure on the Government in the matter of this country's continued membership of the Bureau de Poids et Mésures.

5. A. Harrison, "Internationalism in Science", UMIST, M.Sc. thesis, 1983, p. 6.

6. Royal Society, MC 7, no. 72.

7. Scientifically irrelevant personal knowledge and non-scientific social accomplishments continued to form the basis of many elections to the ordinary Fellowship of the R.S.

8. Royal Society Certificate Books.

9. In 1885, Huxley's friend and physician Sir Andrew Clark was a candidate for the Fellowship of the R.S. Huxley himself backed a number of candidates with very slim scientific credentials. At the time of Clark's candidature it seems that there was a move afoot to "abolish the practice of making first rate doctors F.R.S". Huxley felt that his forebearance regarding Clark was being repaid by the cynical opportunism of his "younger scientific friends . . . bent on bringing in their chum - [Pye-Smith]." Huxley continued his letter to Hooker, declared that he would: "sacrifice his friend for a principle but not someone else's friend". (Leonard Huxley, Life and Letters THH, vol. 2, pp. 118-120).

10. Hirst Journal, 1755.

11. Ibid., 1763. Chasles told Foster and Hirst that as well as congratulations of friends he had been contacted by the Minister of Instruction.

12. Ibid., 1767.

13. Ibid., 1822. The main import of the letters was to show that the young Newton acquired most of what was considered new and important in his work, from France. Hirst carefully destroyed these copies many years leter when the issue was settled in Newton's favour.

14. Ibid., 1819. The R.S. Assistant Secretary Walter White discovered certain syntactic anomalies in the "Newton Letters" which showed that they had been transcribed in part from a French translation of some of Newton's letters. Hirst asserted that this was "annihilating" to the French case. It seems that the culprit was assisted by copies of nine eminent signatures cut from letters in the Desmaizeaux collection at the British Museum in 1833 (Ibid., 1822). In 1888, Hirst mentioned that one Vrain-Lucas had been put on trial and convicted of forgeries in 1870.

15. O.U. microfilm of Frankland Papers (R.F.C.). Frankland was not overindulgent in the discharge of what were, in any case, very light duties. His name scarcely appears in the official records of foreign business.

16. Record of the Royal Society.

17. White's Journal, p. 179.

18. Royal Society, MC 7, no. 127. In 1887 a "very large meeting" described by Foster - it was Stokes' second Anniversary Meeting as President - contained seventy or eighty persons (Huxley Papers (ICL), 4. 302). Stenhouse was a chemist.

19. Hirst Journal, 1926.

20. Royal Society, NLB 7, no. 87. It is important to note that Geikie's involvement stemmed from the breakdown of personal relationships in this case.

21. Royal Society, NLB 7, no. 98. Professor A. G. Greenhill was Professor of Mathematics at the Royal Artillery College Woolwich and probably not in full sympathy with Cambridge mathematicians who held sway over their discipline in the Royal Society.

22. Hirst Journal, 2277.

23. Harrison, ~~ibid~~., pp. 205-207.

24. Lyons, passim., Hall, passim.

25. Royal Society, NLB 17, no. 194. In 1894, Geikie went to considerable trouble to ensure a rapid election for Edward Suess to the foreign list of the R.S. In addition to being a renowned geologist, Suess was Secretary to the Vienna Academy. A short time later, Suess supported wholeheartedly the R.S. initiative over the international catalogue scheme (R.S. Council Minutes, 1894).

26. Royal Society, NLB 17, no. 194.

27. Royal Society, NLB 17, no. 195. The leaders of the Royal Society were unhappy with the equal status of French, German and English in the production of the Catalogue. Germany did take part, as indicated in note 23 above.

28. Leo Königsberger, Herman von Helmholtz, Clarendon Press, Oxford, 1906, p. 111.

29. Paul F. Cranefield (ed.), Two Great Scientists of the Noneteenth Century: Correspondence of Emil Dubois-Reymond and Carl Ludwig, John Hopkins University Press, Baltimore, 1982, p. 86.

30. Huxley Papers (ICL), 16.264.

31. D. S. L. Cardwell, The Organisation of Science in England, Heinemann, London, 1972, p. 117 and p. 119.

32. Cranefield, ~~ibid~~., p. 119.

33. Royal Society, MC 10, no. 114.

34. Royal Society, MC 16, no. 330.

35. Huxley Papers (ICL), 3.316.

36. Royal Society Council Minutes.

37. Archibald Geikie, A Long Life's Work, Macmillan, London, 1924, p. 247.

38. Huxley Papers (ICL), 2.242.

39. Royal Society, Larmor Letters, La. 603(2)C-D, no. 268. Chrystal felt that as a paid official he was obliged to comply with the Royal Society of Edinburgh's exclusion from the I.A.A. Chrystal was Secretary of the R.S.E.

40. Königsberger, ~~ibid~~., p. 307.

41. Huxley Papers ICL, 13.53.

CHAPTER SIX

THE X CLUB

In common with much recent historiography, it has become conventional for historians of the social relations of Victorian science to look askance at the "managed" appearance of institutional history. The paramount reality of scientific organisations is then sought within the often rich and complex domain of informal private dealings among significant individual participants. The "official" account of the Royal Society according to this approach is merely an obstacle to understanding the real nature of the institution. The concealment of the institution's "real" workings is taken to be originally arranged by the contemporary power-holding group. Several authors have contributed to the basic assertion that for fifteen years following 1870 the X club decided the course taken by British Science, both in broad concept and detailed execution.[1] Ruth Barton has concluded that the X club worked behind the scenes as "the cabinet of a liberal party in science" in power between 1870 and 1885 and under whose veiled aegis "science became central to English Culture".[2] Barton states that there is documentary evidence of conspiracy among the X club members in connection with the R.S. A close scrutiny of the Society's records and the correspondence of particularly active club members reveals little more than the natural and ordinary acts of mutual aid which colleagues who are also close friends routinely render one and other. Concerted action on the part of the members at no point becomes manifest to the extent which would confirm the original version of the "X club thesis".

An early instance of co-operation between future X club members took place on the occasion of the withholding of John Tyndall's Royal Medal in 1853. Hopkins, the well-known Cambridge mathematics coach who taught George Stokes, William Thomson, and Arthur Cayley, organised opposition to Tyndall as the recipient of the medal on the grounds that the work on

which it was based had been contributed to by others.[3] Louisa Tyndall later denied that the charges had any substance. Nonetheless, Huxley rose to protest immediately after the award of medals at the R.S. Anniversary Meeting of 1853. Just as it was being formally proposed that Rosse's Presidential Address should be printed, Huxley inquired as to why his friend was not standing beside Darwin, who received the only Royal Medal to be awarded in 1853. Huxley then insisted, to the audible pleasure of a number of the Fellows, that Tyndall's letters declining the medal be read to the meeting. An X club member first appeared on the Council list of the R.S. in the shape of Joseph Hooker in the year of Huxley's spirited defence of Tyndall. In the course of her account Barton asserts that the X club members achieved little on the R.S. Council between 1852 and 1868 with the exception of the Copley Medal campaigns in favour of Darwin during 1863 and 1864.

It seems rather odd that in the process of failing to make much of a mark on the Society's running during a period of 16 years, the X club representatives (Hooker and the diplomatic Lubbock during 1863; Hooker and George Busk in 1864) should score a resounding victory in the major battle for the reaffirmation of Darwin's status after the publication of the Origin. Sabine's attempt to compromise the triumph on behalf of the opponents of Darwinian evolution at the medal presentation did not detract from the obvious enhancement of Darwin's intellectual renown provided by the medal. With the exception of the Senior Secretary William Sharpey, the Officers of 1863-4 were all possessed of some sort of religious outlook. This does not, however, enable the prediction of their response to the Origin. It seems more likely that Darwin's Copley medal was facilitated by means of a wider mobilisation of influence including Charles Lyell who spoke for the Origin when Sabine glibly excluded it from the grounds of Darwin's medal at the 1864 Anniversary Meeting.[4] In addition there appears to have been a certain ceremonial element attaching to the

contemporary conventions of the Council. This tended to produce a rough parity as to medals and honours between the two sides of the Darwinian debate. In 1863 Darwin's nomination for the Copley medal had been blocked in favour of the Rev. Adam Sedgwick. Huxley's ire at the exclusion of the Origin from Darwin's credentials by Sabine in 1864 may have been as much excited by the apparent flouting of the informal convention which decreed parity of R.S. honours, as by the predictably hostile behaviour of Darwin's natural enemies. One year prior to the "Origin's" publication in 1859, the Copley Medal went to Lyell. In 1860 a joint Copley award was made to the German physical scientists Weber and Bunsen. A year later the award was made very much in keeping with the wishes of Darwin's opponents. It went to Louis Agassiz whose recent work treated much of Darwin's evidence in a similarly plausible manner but interpreted according to the assumptions of separate creation. The medal went to the chemist Thomas Graham in 1862 and Sedgwick in 1863. It seems that the temporarily balanced power of the opposing factions made it impolitic for one to merely suppress the other. This made necessary some sort of accommodation. This system was extended to the alternation of B.A.A.S. Presidencies a few years later. In this way the mathematical physicists who emerged as the leading scientific defenders of Christianity took turns with leading scientific naturalists to provide the figurehead of the Association. The simple assumption that the X club was the only self-aware pressure group within the leading scientific institutions of the time provides at best an incomplete explanation of those bodies' actual workings. The approaches which have been made to these issues (note 1) come perilously close to endorsing the central paradox of any cogent conspiracy theory viz.: that the plausibility of any conspiratorial activity increases in direct proportion to the paucity of tangible evidence supporting it. There were occurrences which suggest that the R.S. was not run solely on a diet of sweetness and light distilled from the free play of open rationalism. That the

capacity and will to enact a secret factional policy successfully was the exclusive preserve of the X club is not certain. The inclination to wring an anodyne order from the distracting chaos of historical events has been described by Mark Beach in connection with an American parallel of the X club thesis:

> "few traits seem more American than the ability to see a cabal where none exists. Our propensity to see human intelligence underlying turmoil and guiding change seems particularly strong during periods of severe social unrest."5

The first suggestion of the elevation of X club members to any kind of executive role beyond council membership came with William Sharpey's nomination of Busk and Huxley as potentially useful Library Committee members in 1859. In his letter to the Junior Secretary, Stokes, he cited as a contributory factor to their eligibility, the adjacence of Burlington House to their "business shops".[6] There can be little doubt that concerted action took place covertly before the future club members were fully fledged. It appears to be equally likely that such machinations did not cease as soon as the club got fully into its act, as it were. Barton asserts that "there is no hint of organised oppositon"[7] to X club sway between this time and the natural end of its members' careers. This version of events naturally invites the question of why it should be that the members' concerted, combative energies were required in the acrimonious struggle. Victory must surely have been certain in the absence of "organised opposition". In the year following the reform of the R.S. (also the year of Hooker's election to the Fellowship) Walter White noted in his journal the evidence he had seen of the surface ripples of conflict:

> "In consequence of Lord Northampton's intended resignation it is proposed that the Royal Society shall obtain additional apartments and give soirées. There appears to be some motive actuating the promoters of the change beyond that which manifests itself in their proceedings. Lieut.-Col. Sabine

> today in reply to Robt. Brown said he did not intend to be present any more at the committee of Physics as they occupy themselves with unimportant matters. According to Mr. Wheatstone the reason is that his papers on meteorology and magnetism in the Phil. Trans. for 1847 were not rewarded with the Royal Medal."8

During the early summer of 1861 it became clear that Sir Benjamin Collins Brodie P.R.S. was not going to recover his eyesight. The Society's executive had deferred the issue of Brodie's successor for nearly a year by asking him to stay on in name only, as a mark of respect. In reality, this did no more than make a virtue of necessity. There was a disabling lack of consensus respecting the choice of a new President. Sharpey described his view of the situation to Stokes in October 1860:

> "I confess I feel quite at a loss to suggest a man as Sir B. Brodie's successor. . . . some are probably unwilling to accept and others too uncertain of being elected without opposition."9

The issue could be evaded no longer as the end of the 1861 session loomed. Sabine was convinced of his own suitability for the position. Huxley was not of the same mind and sought to open the way to an opposition candidate by stalling the reading in Council of Brodie's letter of recommendation. In it the President named Sabine, the incumbent Treasurer of the Society, as his choice of successor. Huxley suggested to the Council that the prompt reading of such a recommendatory letter put great pressure on any member who might like to suggest someone else. This early skirmish of Huxley's failed, as had his debut on Tyndall's behalf in 1853. Sabine was elected unopposed and the jealously protected appearance of universal scientific amity was preserved.[10] In these events there is little to support the picture of the Royal Society as a forum of unsullied procedural rectitude. It is that appearance that the supporters of the X club thesis have routinely implied as the backdrop to the bold imprint of the club's doings. Sabine was not

shown excessive respect by the R.S. Council and Fellows once he had ascended to the Presidency. In July 1863 the Council considered an attempt to block one of the President's papers. This prompted a number of his supporters to circulate a list of signatures supporting the publication of the paper without waiting for the approval of the Council. One of the signatories was the Cambridge crystallographer W. H. Miller who had become Foreign Secretary of the R.S. in 1856.[11]

The X Club in its Context

In recent years, two distinct schemes of interpretation have been applied to the complex processes which underly this phase of change within Victorian science.[12] The familiar picture of the interplay of Victorian scientific institutions and networks of influence as the battleground on which the sociologically mortal combat between scientific naturalism and christian orthodoxy took place has formed the foundation of most recent work in this field. Rather earlier, attention was given to a Marxian form of the sociology of science. This position held that scientific activity was inevitably projected from the changing economic infrastructure. According to the lights of such men as J. G. Crowther and J. D. Bernal, the decay of religious authority was a consequence of the secularisation of society's central value zone. The process which the scientific naturalists pioneered is thus seen as no more than epiphenomenal within the inexorable Marxian world machine. The world view of the historical actors concerned is given very short shrift in this austere cosmography. It requires no special interpretation of the intriguing world of Victorian science. We have perforce to imagine that the whole thing sprang de novo from shifting gears in the engine room of the global historical mechanism. Berman has pointed to a third tradition marked by such atheoretical contributions as the pioneering empirical work of Cardwell.[13] The writers who

have treated the subject of the X club in any detail have tended to adopt the first approach outlined above. Macleod is exceptional in giving no place to religion in the career of the club which he saw as being a means by which an élite sought to channel the scientific enterprise towards the service of their own material and social interests.[14] Jensen raises the question of theological opposition to the social ambitions of Victorian science but remains vague about whether these ambitions constitute the sum total of "the cause of science".[15] Barton gives ample attention to the rôle of the X club as a means of defending the version of modern science elaborated by the leading scientific naturalists, from the clerical threat. Her account remains largely incomplete because it places the forces of theological reaction in English society at large (London and the home counties, except for the annual forays necessitated by the annual B.A.A.S. gatherings) and does not trace their representation and activities within scientific institutions and personal networks. Little has been discovered about the conflict between science and religion within such bodies as the Royal Society. Despite the bold assertions that "for the Royal Society there is documentary evidence of conspiracy among the X", and that "there is no hint of organised opposition" to the clubs pre-eminent influence, no clear answers are forthcoming to the following questions.[16]

i) In the absence of concerted opposition within the R.S., why was the X unable to simply please itself on all issues? What was the X endeavouring to overcome?

ii) If the "cabinet of a liberal party in science" is a fitting description of the X club, which is usually reckoned to have won the major battles before its members left the scene, how was it that the "unorganised" forces of reaction reasserted themselves so readily in the Royal Society after 1885?

iii) What were the manifestations of the conflict between scientific naturalism and Christianity within the scientific societies, particularly the Royal?

In its usual form, the X club thesis provides little enough insight into these matters. At the same timeit appears to render further inquiry into them somewhat superfluous. The "documentation" of the club acting as an invisible force at a distance against an amorphous opposition has produced little more than circumstantial suggestions of its actual activities. As a preliminary to suggesting a rather different place for the club on the stage of Victorian science, some light ought to be thrown on the question by an examination of the actual representation within the R.S.'s executive of, on the one hand, Christians, and on the other, scientific naturalists.

The Religious Disposition of R.S. Officers 1850-1900

Office Held/Years	Man/Years Incumbency of Men Committed to Christianity	Man/Years Incombency of Men Committed to Scientific Naturalism
P.R.S. 1848-1905	38	19
P.R.S. 1880-1900	10	10
Sec. R.S."A." 1850-1900	50	0
Sec. R.S."B." 1850-1900	3	47
Treasurer R.S.	42	8
Foreign Secretary	27	24
TOTALS:	170	108

The total of man/years spent on the R.S. Council by X club members was 92 out of the total of 840 covering the full span of the nine members' active careers. For the club's allegedly most potent phase between 1870 and 1885 the X members accounted for 51 of a total of 315 man/years of Council membership. A rough estimate of the number of Copley Medals awarded to nominees of the leading group of Christian Fellows within

the R.S. compared with the number awarded to men championed by the scientific naturalists gives the following results:[17]

		Christian-Promoted	Scientific Naturalist-Promoted
Copley medals awarded	1850-70	6	3
	1870-1900	10	10
Successful proposals of Candidates for Foreign Membership R.S.	1879-1900	22	28

Bearing these figures in mind, one can only presume that if there was no concerted opposition to the X club in the Royal Society than its active members can have experienced little difficulty in getting their own way. Two important questions emerge from the foregoing discussion: firstly, from evidence of events which actually took place within the Royal Society and the wider sphere of British science, is it possible to discover the extent to which the X club held sway? Secondly, if an effective coterie of strategically situated individuals did exist and was at thezenith of its powers roughly between 1870 and 1885, was its membership simply that of the X club? For present purposes it will be assumed that an affirmative answer can be given to the first question. It will be dealt with in greater detail in a later chapter. The second question forms the point of departure for the next section.

"Not Just Nine Eminent Men"[18]

The penchant for foisting a "secular clerisy" on to the history of Victorian science as the integrating key to its apparently disparate affairs has defined a large rôle for the X club. Huxley's bland assertion that the X was simply a means of keeping a group of increasingly busy men in touch with one and other has been largely set aside.

Disregard of the founder's testimony is also applied to its reaffirmation in 1900 by his son Leonard in his Life and Letters. Barton considered the friendship model of the X to be simply Huxley's extension of subterfuge - to Barton the club's staple function - to the formulation of its public image. Viewing conviviality as but the tip of the iceberg, Barton endorses the power-based interpretation of William Irvine in his pioneering revelation of the "strong" version of the X club thesis of 1955.[19] Unfortunately neither of these two authors proved able to raise their argument from its foundation in wishful conjecture. In a private letter to Edward Frankland in January 1888, Huxley stated his view of the club's real purpose. It seems unlikely that Huxley would attempt to misrepresent the club to a veteran fellow member at the virtual end of its life:

> "If I had been present I should have represented Satan and opposed all round. I never could see the use of enlarging the X or continuing its existence after we all drop off.
>
> The club has never had any purpose except the homely personal object of bringing together a few friends who did not want to drift apart. It has happened that these cronies have developed into bigwigs of various kinds - and therefore the club has incidentally - I might say accidentally - has a good deal of influence in the scientific world. But if I had to propose [?] to a man to join and be told to say, 'Well, what is your object, I should have to reply [as] the needy knife grinder - Object, God bless you Sir, we've none to show'."20

The frequent assumption of the X members' conscious intention to function as the "fluid cabinet [of British science] united by the reliable intimacy of an eating friendship", has usually been accompanied by a painstaking logical reconstruction of a suitable criterion for recruitment. Barton construed the X club's division of intellectual labour in the following terms:

> "As a symbol of the unity of science the membership of the club represented all branches of scientific inquiry."21

Such a view is scarcely borne out by the facts. Hooker, Huxley, and Busk overlapped considerably in their competence. Spottiswoode and Hirst were concerned with what at the time were held to be the non-urgent departments of mathematics. Spencer bore no direct relationship to scientific practice at all. Tyndall's special place in experimental physics, while worthy in itself and bringing him ample plaudits, did not equip him for supporting the cause of scientific naturalism at the highest level of technical achievement sustained by the Cambridge mathematical physicists. The indulgent contemporary view of Lubbock was one of a busy and talented generalist. His detractors now detect in him the inherent weakness of the thoroughgoing dilettante. Frankland's prominent position in English chemical circles is undeniable, but it must be recalled that by the time the X club had become allegedly the "scientific party in power", he had largely given up original research for the more lucrative pursuits of water analysis and providing legal testimony as an expert witness.

The contention that the club was, from the outset, of a premeditatedly conspiratorial nature remains unconvincing for a number of other reasons. At the fifth meeting on March 2nd 1865, the members agreed that Busk should ask James Fergusson to join the club.[22] Fergusson was a well-known antiquarian and collector. He declined. No new members were ever admitted. There appears to be no evidence of behaviour remotely approaching the scheming ways of the cabalist in the conduct of at least four members. They were Hirst, Spottiswoode, Lubbock, and Spencer. Frankland, although not averse to involvement in schemes, appears to have restricted himself to those which were concerned with his own betterment. As I shall attempt to show in some detail in the next section, a case can be made for disregarding the X club (and its members, excepting Huxley and Hooker), as the exclusive means by which

an ambitious group of scientific naturalists sought to impose their will upon the forces of religious and scientific conservatism. Against this background it might be presumed that the potent, manipulative function of the X club was, whilst not envisioned in its foundation, nonetheless the way in which it developed. In this case the attitude of Huxley to the admission of new members (note 20) becomes almost impossible to comprehend. Few people would dispute the crucial part played behind the scenes by Huxley himself. With this interest in marshalling influence it seems strange that when club members pressed for the admission of a tenth member in 1874, and again in 1888 for a more ambitious restocking of the club Huxley demurred. On the former occassion the suggested recruits were Richard Strachey and Francis Galton. Both men were influential in different areas; both possessed respectable scientific credentials and Strachey had been a close friend to both Hooker and Huxley for many years. Fourteen years later, following the deaths of Spottiswoode and Busk and Huxley's retirement from the chair of the Royal Society due to failing health, a number of names were put forward. It is predictable that those (such as Frankland) robust enough of health to maintain an enthusiastic attendance at the meetings would be more likely to approve the recruitment of some younger and more reliable diners. Nonetheless Huxley's attitude of disapproval is hard to fathom. Particularly this is the case against the background assumptions of the usual version of the X club thesis. Surely it would not appear contrived to assume that the leader of an informal group lapsing through age and infirmity as the de facto "government" of British science would welcome the reinforcement of his depleted and largely retired "cabinet" by well-liked and influential fellow travellers? Huxley insisted on the essentially social nature of the X in a letter to his closest friend in March 1888. In it Huxley's emphasis again suggests that the

friendship model of the X club might not have been a publicly useful fiction. It may have been the simple truth.

> "The X really has no raison d'être beyond the pleasant attatchment of its original members - Frankland told me of the names that had been mentioned & none could be more personally welcome to me, especially Strachey and Foster and Evans - but somehow or other they seem out of place at the X. However, I am not going to stand out against the general wish and I shall agree to anything that is desired."23

It could be that one of the principal functions of the club has been a convenient vehicle for an increasingly entrenched view of the conflict between science and religion. The X club neatly locates the vanguard of secularism. Attention might usefully be paid to the question of where the real focus of the movement's power lay. In the course of his speech of thanks for the Darwin Medal of 1894 (which was obtained for him by Michael Foster) Huxley sounded a serious note of warning to the supporters of Darwin's theory. His concern was for the erosion of the authority of both Darwinism and the wider enterprise of scientific naturalism due to the enfeeblement and complacence of its original supporters. In this context Huxley would surely have wished to bring the new "Young Guard" into the X as it had been the operational core of scientific naturalism. It is also worth remembering that the club's contemporary secrecy was successfully maintained despite the grand scale of its alleged effects. The very active world of men's clubs in Victorian London would have been unlikely to engender quite the level of naiveté which is required for the smooth operation of the conventional X club thesis. A great deal of store has been set by the probably apocryphal account of the X club's activities overheard by Huxley. From behind his newspaper in the smoking room of the Athenaeum Huxley is supposed to have heard a scientific colleague's account of the rôle of the X club - "they govern scientific affairs, and really, on the

whole, they don't do it badly." It suggests a somewhat broad perception of the club's place not borne out by other evidence of general contemporary awareness of its existence and endeavours.[24] The lack of awareness of the X club is illustrated by a further step in the management of Hirst's rather difficult career by his X club colleagues. In 1872 Spottiswoode and Huxley were both consulted by Goschen, the First Lord of the Admiralty, regarding Hirst's suitability to take charge of the new Royal Naval College at Greenwich. Hirst's two supporters went separately to boost his case with Admiral Key. Huxley mentioned to Tyndall that Goschen: "was naturally considerably surprised by the fact that we coincided by recommending Hirst."[25]

When the club first formed on the third of November 1864 at St. George's Hotel in Albemarle Street, the meeting was, according to Hirst: "very pleasant and 'Jolly'."[26] The club could quite well maintain its inscrutability in the early years. Few of the members had got anywhere near the wide limits of their growing reputations when the ritual charm of the mysterious algebraical summons was at its freshest. There were annually recurring discussions about the likely candidates for election to the R.S. Fellowship, regular rakings over of vexed issues such as the method of selection of new Council members, and reports of current Council affairs. However, collusion, canvassing, and campaigning evidently did not become manifest to the leading representatives of other scientific and religious interests. As the power of individuals within the club grew one might well imagine the visibility of even their covert Council and Committee dealings would reach the level of recognition by alert enemies and allies alike. This was not the case. A lack of common purpose and even schism progressively became a bar to concerted action by the X club within the main scientific bodies. In 1864 when Darwin received the Copley Medal and Tyndall the Rumford, there

were two X members on the Council list: Busk and Hooker. On the same day as these awards were made Hirst was unanimously voted on to the list for the next R.S. Council, and remarked in his journal:

> "For this I have no doubt to thank my friend Sylvester to whom I wrote on the following day."27

As a mathematician and a Jew, J. J. Sylvester can scarcely be seen as in cahoots with the X club. This incident stands as an illustration of the possible misguidedness of applying the standard "X club thesis". There is a contradiction between the supposedly increasing X club power and an accompanying enlargement of its capacity for complete secrecy. When a mere two club members are supposed to have achieved the "break-through" of Darwin's Copley Medal and the bonus represented by Tyndall's Rumford Medal, surely the election to the R.S. Council of their friend and fellow X club member would have been a matter of ease?

From all accounts it is clear that contemporaries vitally involved with running the Royal Society did not perceive the existence or doings of the X club. The papers and letters of the Cambridge mathematical physicists contain no allusion to it whatever. More telling is the fact that the letters of prominent scientific naturalists who were both active in the cause and very close allies of Huxley and Hooker were likewise silent on the subject. Few of the club's special dinner guests seem to have guessed at the extent of the X's extra-culinary activities. Indeed, from this quarter also there is scarcely so much as a mention of the club's existence. George Airy was in rough accord with the supposed unifying factor of the X club: their commitment to keep the course of science unimpeded by religious prejudice. When Sabine was finally dislodged from the chair of the Royal Society in 1871, Tyndall "sounded" Airy for his willingness to become the new President. Huxley made a similar approach to Lyell. Airy made a half-hearted and ungracious P.R.S. (he was both too deaf and too parsimonious

to please the Fellowship at large) and withdrew from the position in what was considered to be indecent haste. Nonetheless Airy's candidature had been actively entertained by several leading X club members.[28] Airy revealed his utter ignorance of the X club, its alleged programme, and personal relationships in a letter to Stokes of the 20th January 1873:

> "The tenor of the nominations on Thursday astounded me. It must have been the result of a 'caucus'. I cannot conveive that the man who carried such a preponderance of names (I do not yet say votes) is the proper person. I will not now enter on personal reasons but may perhaps see you for ten minutes before meeting on Thursday 23rd if I can manage to come then."29

Airy wished to see the Duke of Devonshire in the chair, whereas the man installed in the chair was Hooker. At this time Spottiswoode was the recently installed Treasurer and Huxley the new Junior Secretary. Busk, Hirst, and Hooker were Council members at the time of the meeting described by Airy. After the death of Spottiswoode in 1883 the X meetings were transferred to the Athenaeum Club. There a particular "X corner" came to be associated with the club in the same way as the "Indian corner" was held dear by the denizens of the Raj. Guests had always been an occasional feature of the meetings, where they were by no means kept from forming a full impression of the X club's significance. Barton remarks on the garrulous reporting of the X club's standing by its visiting American diner John Fiske, who disclosed to correspondents back home that the club was the most influential scientific coterie in England. Barton describes Fiske as: "the only contemporary who found the X club remarkable". Fiske appears fleetingly as the only contemporary with any inkling of the club's existence and "real" significance. The "X corner" of the Athenaeum was open not only to occasional visitors by special invitation, it was quite open to the scrutiny of the rest of the dining room. Hirst noted in his journal in

early January 1889 that the meeting had been attended by only Hooker, Frankland, and himself. Hirst had received a telegram of apology from the absent Tyndall who hoped that his old friend would read it to the meeting. This he did: "and to Sir. F. Leighton who happened to sit at the adjoining table at the Athenaeum."[30] In the same vein, Hirst made frequent reference to being joined by various individuals such as A. W. Williamson and Henrich Debus for cigars and conversation following X club dinners. The openness of proceedings at the Athenaeum during the evening of the 4th February 1890 is clear from the following quotation. This evidence makes it seem all the more strange that the X club should have escaped the notice of contemporaries, especially by men who frequented the Athenaeum and held compelling vested interests in acquiring and utilising vital information about any key network of influence.

> "Shortly before 7 Hooker and Huxley came in. The latter and Spencer did not speak to one another. At 7 Hooker, Huxley, Frankland and I sat down to our X dinner, antedated by Lubbock but he did not put in an appearance until 7.30, and disappeared soon after dinner was over. The rest of us smoked our cigars downstairs; Rusden hovering about us, and Lockyer silently reading a book near us."31

Such carelessness of what nowadays would be termed security was no more than a convenient extension of earlier public evidences of the club's activity. This was made manifest in the country excursions accompanied by the married members' wives and the sharing of accommodation at B.A.A.S. meetings. In the early phase of the club's career the original contingent of wives (who seemingly got along well with one and other) were thoroughly involved in forming its social shape and tenor. In these early times dinners often took place at members' houses where the guest list included nearly the whole X membership. This naturally fell to the originally wealthy members: Spottiswoode and Lubbock. Such a gathering took place in the former's "magnificent drawing room" at his

house "Combe Bank" on the 7th February 1866, where all but Tyndall and Hooker were present.

Variety of Outlook and Purpose Among the X

According to the lights of the rudimentary conspiracy theory underlying the conventional "exposé" of the X club, evidence of its members' concerted actions should be extremely scarce. Such scarcity being taken as confirmatory of both the existence and thoroughness of such actions. Scarcer still, within the dictates of this approach, should be any evidence of a lack of unified purpose and active dissension. From a reading of the small literature dealing with the X club it is not clear whether the lack of evidence of the club's effective actions is to be taken as a denial or a confirmation of its vital rôle in Victorian science.

Spottiswoode

William Spottiswoode was unusual among the other X members in having as a personal background the wealth deriving from his family's printing firm. His father's plan for Spottiswoode's education took him from Eton and Harrow to Oxford where he obtained a first-class degree in mathematics from Balliol College. Having succeeded to his father's position as the Queen's printer in 1846 at the age of 21, Spottiswoode settled into his vocation as a wealthy amateur man of science. He undertook a journey of probable Humboldtian inspiration into the little-known regions of Eastern Russia in 1856. Four years later he set out once more on similar expeditions in Croatia and Hungary. Judged with cold hindsight Spottiswoode is remarkable only for his strict conformity to that styleof the scientific life held up to obloquy by the Royal Society's reforming party of the late-1840's. The 1870's saw him dabbling in experimental physics under the postal guidance of George Stokes. Prior to that, simply keeping up his mathematics had formed

the greatest part of his practising concern with science. Hirst described Spottiswoode's mathematical capability in the following unflattering words:

> "his treatment is far from being so [i.e. geometrical]. He removes himself too far from the object of research and loses himself in symbols."32

Hirst later admitted in the pages of his journal that Spottiswoode's scientific accomplishments were not sufficient to grant the eminence seemingly reflected by his interment in Westminster Abbey. This was brought about by a pressure group of F.R.S.'s joined by Hirst. He was apparently oblivious of the strongly felt disapproval of Hooker and Huxley. Hirst was also quite unaware of the wider implications of the situation which had come about with alarming rapidity in the year following the death and Abbey burial of Darwin the year before. Spottiswoode succumbed to typhoid contracted whilst in Italy. Six years later Hirst reflected on the opposition to the lionisation of Spottiswoode. Not recanting his own support, Hirst detailed what were, from other sources also, clearly his major qualities: "I do not regret having helped to the interment of him there. He was a noble and exceptionally high-minded man, at all events!"[33] Huxley also admired Spottiswoode as one of his best friends and that he: "comes under the A1 class of 'people with whom you may go tiger hunting'."[34] William Spottiswoode was a minor fixture in the lower levels of Victorian High Society. Hirst found himself dining alone with Mrs. Spottiswoode one evening in December 1866 in consequence of her husband having gone out with Lord John Russell, Sir Henry Holland, and Gladstone. When the exiled Emperor of Brazil sojourned in London in 1871, it was with the Spottiswoodes that he stayed. From that vantage point his ex-Excellency was able to form friendships with a number of X members. Spottiswoode was a member of the Royal Asiatic Society and a man of some moment in

the Athenaeum Club as early as 1865 when he was 40 years of age. He made an attempt to secure the election of Hirst in that year which, although it failed illuminates a rather interesting fact. Spottiswoode and Tyndall were able to acquire the support of the scientific members of the Athenaeum Committee: Sabine and Murchison. Both these men might well have been expected to have been constitutionally opposed to the godless Darwinians Tyndall, Hirst, and Spottiswoode. Five years later the latter successfully supplanted J. P. Gassiot who was Edward Sabine's nominee for the place of Treasurer of the Royal Society. Spottiswoode's maintenance of strict moral rectitude appears to have been complete in all departments of his life. Regarding his own failed Athenaeum candidacy Hirst clearly stated his belief in Spottiswoode's integrity, and his rather surprising faith in Edward Sabine and Murchison.

> "I do regret, I confess, that I was the cause of their influence being for once unsuccessfully exerted. Health and strength being granted to me therefore I shall feel it to be due to them to prove hereafter - if indeed proof were needed - that they are incapable of <u>ever</u> using their powerful influence unworthily."[35]

Spottiswoode does not fit at all comfortably within the model of the X club which centres on the covert deployment of collusive power by the leaders of a burgeoning "secular clerisy". As a commercially wealthy scientific amateur of ordinary attainments he forms an odd point of contact with the Cambridge mathematical physicists who maintained Christianity against the trend towards materialism. When the time came to fill the Royal Society chair vacated by Hooker in 1878 (in order to set a precedent of a five-year tenure) Spottiswoode was the scientific naturalist most acceptable to the Cambridge physicists. The latter were baffled by Rayleigh's chariness and comparative youth which coincided with Stokes' practical need for the salary of the Secretaryship which he had held since 1854.[36]

Spottiswoode was neither a schemer nor an active guerilla in Huxley's "liberal army". It was Spottiswoode who secured the election of Rayleigh to the Royal Society in 1873. When Sir William Thomson took the Presidency over from Stokes in 1890 he remarked to the latter that Spottiswoode appeared to have been too lenient with the authors of inferior papers.[37] In the course of his Presidential Address to the Anniversary meeting of the Royal Society in 1881, Spottiswoode lamented the passing of the isolated amateur tradition in this country with its age of heroes. Similarly incongruent with his suggested place within an ambitious élite of thrusting scientific power brokers is the section of his 1881 Anniversary Address touching on Royal Society Council procedures. The President gave a detailed account of how the rule of limited tenure governing membership of the Council was ineffective in obviating the dominant influence of "old hands". He described how the latters' sure touch, maintained through cyclical reappearances from a pool of like-minded peers, could comfortably set aside the efforts of largely ineffectual novices. Spottiswoode was scarcely speaking in the X club interest. Nor was he materially assisting the upkeep of the club members' largely untarnished ethical reputations. Spottiswoode was not intending to resign at the end of his five-year term in tacit support of Hooker's precedent. Hooker's act was largely a response to the ten-year Presidency of Sabine whom both he and Huxley regarded as an untrustworthy enemy. Spottiswoode had taken no steps in the direction of vacating the chair when Hirst visited him at Combe Bank on the 5th June 1883. He found the President prostrate on his sofa labouring under the misapprehension that his complaint was ague, an old enemy. In reality he was suffering the typhoid fever he had been infected with in Italy from whence he had just returned, having left the matter of the May Soireé at Burlington House to John Evans. The President died on the

27th June.[38] This account has dwelt somewhat on the details of this rather shadowy X member because he has usually been presented rather cursorily. It seems that only on the flimsiest grounds can Spottiswoode's career be co-opted into the mainstream of what has been supposed to be typical X club behaviour.

Frankland

Edward Frankland made contact with Tyndall at Queenwood College where both were teaching in 1847. Both were from humble social backgrounds. Impelled by dedicated ambitions to raise themselves the pair set off for Marburg in the following year. Tyndall's close friend Hirst made the same journey in 1849. In this way was established the friendship group of three striving provincials which went on to form half of the main body of the X club. The other half was formed by the three Naval naturalists: Huxley, Hooker, and George Busk. Tyndall formed the common factor between the two groups, having got to know Huxley at B.A.A.S. meetings in the early 1850's. Tyndall wrote to Frankland at Basle on the first of August 1856 to suggest a joining of forces with: "Hooker and Huxley, two excellent fellows who know you by renown . . . ."[39] Nine months prior to the formal founding of the X club, Frankland was acting as a prominent performer in the aggressive defence of the Darwinian banner of scientific naturalism. Hirst noted in his journal that on the 31st of January, having dined at the Busk's in company with the Lubbocks, he accompanied them to the Royal Institution to hear Frankland speak on the glacial epoch. The background to his lecture was coloured by the continued endeavours of the custodians of Christian precepts to deny Lyell's Principles in order to preserve the literal interpretation of Mosaic time. Hirst concluded that the lecture was really the work of Tyndall (who had done much to assist Frankland's appointment as Professor at the Royal Institution the year before). Frankland was

responsible for the hypothetical section of the lecture. This apparently dealt with his view of how the moon, gradually cooling over a vast span of time in a like manner to the earth, formed large caverns within itself into which the lunar seas disappeared. The speaker also reported his detection of traces of glacial activity in the region of the crater Tycho. Hirst thought his friend was needlessly hazarding his reputation with these "wild speculations". Nasmyth declared that the lunar section depicted was heretical.[40] The lunar siting of evidence of the shortcomings of the Biblical account of creation was an occupation whose popularity remained undimmed for quite a number of years. Perhaps it is no coincidence that the 1860's witnessed a widespread interest in the nature of the moon's surface, with especial interest in detecting evidence of change. On February 16th 1869 Frankland wrote to Huxley to express his pleasure at the effect of one of the latter's recent lectures:

> "the 'lunar politics' are splendid and altogether the lecture will frighten the parsons more than anything they have encountered for a long time."[41]

Apart from John Lubbock and William Spottiswoode who were born into the comfortable financial circumstances of their family firms, Frankland was the only X club member to make a lot of money. He differed further from his two upper middle-class colleagues in that he made money from the commercial exploitation of his scientific knowledge. His pioneering work in chemistry was complete within a year of the formation of the X club, by which time he had moved into the place vacated by Hofmann at Jermyn Street. Both there and in the new Huxley Building at South Kensington to which is department moved in 1872, Frankland was unable to apply himself to his own strictly scientific research. He became ever more immersed in the profitable business of water analysis. His humble beginnings quite possibly played a part in

forming such a determined view of the importance of financial security.[42]

Early in the course of his shared experiences with Hirst and Tyndall, Frankland began to diverge from the course of earnest Germanic self-examination in which they indulged. The pragmatic versatility (which in himself replaced the high-minded aspects of the Victorian "wars of discussion" in which his friends played such a prominent part) was later derided by some of them as mere licence for the pursuit of social mobility. Huxley's antipathy towards "commercial gents, chemical traders and experts" has been fully aired in an earlier chapter dealing with the commercial involvements of Fellows of the Royal Society. This attitude was shared by Tyndall, and Hooker for whom campaigning devotion to duty and service was uppermost. When Frankland produced a highly detailed vindication of his behaviour in the face of repeated charges of his having conducted private work for profit in the laboratories at South Kensington, Huxley admitted some overlapping of loyalties. In a letter of June 3rd 1884 to his close friend J. F. Donnelly at the Department of Science and Art, he admitted that he regretted the extent to which the repetition of the charges had influenced him.[43] Frankland was a lifelong admirer of the dignity and doings of the Royal Society. However he failed to achieve office except for the minor place of Foreign Secretary at the end of his life. The main reason for this would seem to have been the disapproval of those of his comrades who formed the effective focus of power within the Society. During the spate of activity following Spottiswoode's death in June 1883, Huxley addressed a summary of his thoughts on the succession to Hooker:

> "Who have you to suggest? The only thing I am clear about is to keep out traders on the one hand and mere noblemen on the other. It is the turn of the biologists and I can think of no one but Lubbock. I know what there is to be said on the other side - but if you can tell me of anyone more suitable -

> Stokes won't do . . . as President he means stagnation or retrogression - Williamson won't do - he means crotchets and impracticability in excelcis. Frankland won't do - Biologists and Chemists and Mathematicians aside what do you think about Tyndall? Nothing I should like better personally but how about him as a man of business and conducting negotiations with a Government department. I am afraid Johnny would upset the coach in his first drive - It's a tangled mess."44

By the 1880's with much of the X members' sense of unified urgency lost in the rush of accumulated life, weaknesses inherent in many of the bipartite links within the X club became apparent.

The two most enduring relationships were between the two pairs of oldest friends. From the Naval naturalists' side of the original club structure this involved Hooker and Huxley, and from Tyndall's Marburg group it was his own very close ties with Hirst which proved to be the most resilient. Hirst was far from being enamoured of Frankland or his ways by the time that the last phase of the club was under way in the late eighties. At the meeting on the 8th November 1888 when the club was 24 years old, Hirst had an argument with Frankland about the propriety of scientists appearing in the rôle of expert witness in courts of law:

> "He was rather severe on Dewar for doing what he himself has done for years. . . . He defends the practice so far as he himself is concerned but abuses Dewar, and inconsistently admits that scientific non-partisan referees might be employed solely. Hooker was entirely of my opinion and added that he had always refused to go into the witness box. Lubbock weakly defended the present practice as being most in accordance with English modes of procedure!"45

At this time Frankland was a Royal Society Council member, and at the meeting described above he informed the members present (Hirst, Lubbock, Hooker) of the decision to award the next Copley Medal to Huxley. Tyndall wrote to Hirst a few days later telling him that he did not approve of this action. At the X meeting on the 11th April of the following year Hirst described Huxley as being "in great force" whereas

Frankland "talked far too much and very loosely, as usual".[46] As will become clear in the next section, the crucial relationship amongst the X club members was that between Huxley and Hooker. Despite all sympathetic accounts of the power of the X club as a "secret cabinet" ruling British science, it seems that the power of the X club identified as scientific naturalists was narrowly focussed on the person of Huxley himself. Its effects were brought about through the personal relationships which he had formed over many years with a small number of strategically placed men in official circles. It is certainly the case that by the time of the Presidential crisis concerning Stokes' entry into Parliament in 1887, the outlook of Frankland, Hirst, and Lubbock had drifted far from the position maintained by Hooker and Huxley. The greater weight of the latter view can be assumed roughly from the extent to which it was sought and followed by the influential members of the rising younger generation of scientific naturalists. Frankland's reading of the 1887 situation was such as to give Hooker considerable pause for thought. At the time the inner circle of "Huxleyites" were trying to force Stokes' resignation as President as the penalty for his concurrently sitting as M.P. for Cambridge University. Hooker addressed Huxley as follows:

> "I was much impressed by Frankland's advocacy at the X of 'a member for the Society', my blood ran cold and my very soul sank within me. You and I and perhaps very few others know the power for good that the Society can exert, and that a suspicion of an infection of party would snuff it out."[47]

Exactly one month earlier Huxley informed Hooker that he had been in contact with Frankland about the Stokes situation hoping to influence him "meantime saying nothing of what we are about".[48]

In 1892 Frankland was, in common with Lubbock, still physically fit and eager to continue the X club dinners. During the winter of 1886 whilst Huxley languished in the miseries of his latest breakdown

of health, Frankland, who was his contemporary at 61, reported to his erstwhile colleague at South Kensington that he had been enjoying "a fifteen mile spin along the hard smooth roads on my tricycle".[49] During the same year he had been walking in the mountains of Italy with Cannizzaro's architect son for his guide. By 1892 the problem of absenteeism at X meetings had reached crisis point with Frankland. He tried to push along two schemes for improving the situation. The first for admitting new members was quashed by Huxley, while the second for holding the meetings as house parties with the diners also being accommodated for the night was shelved by Hooker.[50] Where Spottiswoode was not a schemer by his natural disposition, Frankland was deliberately excluded from the full confidence of the persons who exerted the most effective influence.

The "Xquisite Lubbock"

John Lubbock was born into the wealthy setting of his family's banking business. Lubbock's social status allowed him to develop a position as the leading dilettanté in the London scientific societies for forty years. Through activity as an amateur naturalist Lubbock got to know Hooker and Huxley. His elevated social position lent a lustre to Lubbock's activities which could not have been attributed to his scientific accomplishments. He was made F.R.S. in 1858 for his work on the reproduction of Daphnia. Two years later he was sitting with Huxley and Hooker on the platform of the Section D meeting of the B.A.A.S. on Saturday June 30th at Oxford which witnessed the much-celebrated confrontation between Samuel Wilberforce and Huxley. In an attempt to revitalise the reputation of this "forgotten man", R. J. Pumphrey has suggested that Lubbock's effectiveness as a propagandist of Darwinism outstripped that of the overly polemical Huxley. There appears to be very little in the way of evidence to substantiate the claim. The Lubbock family home was very near to Downe where Darwin

lived for 24 years until his death in 1882. For nearly a decade from 1865 Darwin utilised his young friend's main talent as a lucid sounding board for his ponderously forged notions. The family moved into "High Elms" when Lubbock inherited it from his father in 1865. Darwin said that of the three men whose company he could take seriously, Lubbock stood ahead of Huxley and Hooker. Despite this recommendation, the reclusive old valetudinarian was to receive an unpleasant surprise at the hands of his urbane promoter. On a Sunday afternoon in February 1877 Sir John, as Lubbock was by then known, turned up at Downe with a group of dignitaries including Lord Playfair and William Gladstone. The latter declaimed mightily at the behaviour of the Turks.[51] The bonds of friendship and common commitment to the cause of defending the Origin of Species from its natural enemies had been slackened three years prior to the surprise visit of Gladstone. Darwin and Lubbock fell out over a tract of land adjacent to Darwin's House.[52] The quarrel over the "Sandwalk", where Darwin had liked to walk with his children and grandchildren, cooled relations between the two men for the remainder of Darwin's life. Some attention has been paid to Lubbock's personal relationship with Darwin because this seems to be the central factor which confirmed his attatchment to the X club members. It appears that Lubbock did not enjoy a close personal friendship with any of the club's members. Indeed it could be maintained that his inclusion was the only aspect of the club's formation which conforms with Barton's picture of it as a premeditated cabal whose members were selected for their strategic significance within scientific and wider networks and influence.

If strong personal friendship was not important in understanding Lubbock's career in the X club the same could not he said of the wife whom he married in 1856 when he was 22. Lady Lubbock sounds to have

been fascinating company in a world of rather prim and conventionally limited womenfolk. During the 1860's Tyndall and Hirst remained firmly under her spell. As bachelor and widower respectively the two maintained a rather more regular series of visits to the Lubbock family homes than strict prudence might have suggested. Lady Lubbock's unusual independence of mind made her attentions as a companion and confidant much sought after. Hirst recounts the following incident in his journal:

> "Lady Lubbock 'received' at Price's Hotel Dover Street; all the members of the X were there, besides Sir W. Armstrong, Spedding and several others. The X dined afterwards at Brooke's Hotel, Mr. Benham not being able to accommodate us. The dinner was ordered in a very spirited way by Lady Lubbock without consulting me or any other member of the X. It was an unauthorised act of interference which was as successful in its results as it was audacious in its character. None of my acquaintance, except Lady Lubbock, could have done it."53

This indomitable individual mothered six children and maintained active membership of a club in Albermarle Street where she took friends to dine. Her death in 1879 was a great blow to the X club generally and changed many of the accepted forms of their socialising and conviviality. The "double X" excursions involving the members and such "yv's" as there were did not survive her passing. The installation in 1884 of Alice, daughter of Lieutenant General Pitt-Rivers, as the new Lady Lubbock at High Elms marked the end of Lubbock's participation in the central life of the X club. Thenceforth he was known chiefly for arriving late at the meetings, showing his face and then sidling on to further engagements elsewhere. Despite his somewhat soured relations with Darwin, Lubbock was very promptly into action following his death in 1882 to mobilise opinion in favour of his interment in Westminister Abbey. It appears that Lubbock's labours in the Darwinian cause were largely of an evasive and emollient nature. He made attempts to secure

a broadening of the Anglican church's doctrinal formulations so that Darwinists might perserve a form of amity and order by remaining within the Church. For all that Lubbock seemed to take the lead in April 1882 it is difficult to see his endeavours as any more than the desire to superintend grandiose formalities. It will be argued in a later chapter that Huxley himself was not so eager to put a gloss of magnanimity over the rival explanations of man's place in nature. Lubbock, with an exceptionally well-heeled foot in both camps, sought the symbolic trappings of a complacent coexistence. This could only have a short-term significance for those who remained in sympathy with an increasingly obsolete position. Huxley would not sign Lubbock's petition to the Dean of Westminster.[54] As a very popular author on many subjects apart from his attatchment to insects and prehistory, Lubbock did not duck the opportunity to be among the pall bearers. He gave untiring devotion to all aspects of what is nowadays passed off under the rather nebulous title of public relations. Lubbock's place as a glamorous and popular generalist is incompatible with the usual prototype of the earnest and single-minded member of a rising secular clerisy. As the foremost of those who intervened to prevent Darwin's wish of a quiet burial at Downe being fulfilled, Lubbock cannot truly be held to have exemplified the underlying meaning of Darwin's interment in the Abbey as it has been related by James Moore:

> "By appropriating it [Darwin's corpse] the new leaders of English culture were able to redeem its political value. Like the mind gone out within, the body now served them well, in a last symbolic rite testifying to their authority, the extreme unction of a rising secularity."55

As a regular church goer and member of the Society of Antiquaries, Lubbock was not merely a rich scientific naturalist.[56] The most clear-cut act which Lubbock performed for the defence of Darwinism was his reluctant Presidency of the Linnean Society which he was goaded into by

Hooker in 1881. The Huxley-Hooker plan in this regard was, with Busk's connivance, to block the dangerously plausible anti-Darwinian St. George Mivart from the chair. Lubbock learned a good deal about diplomatic equivocation in the course of establishing his record for the greatest number of Societies presided over by one man. His part in the parliamentary campaigns over the Ayrton affair and later over the anti-vivisection bill were judged to be adequate. Nonetheless, Hooker at one stage registered his amazement at the credulity of Lubbock when confronted with official blandishments.[57] He maintained his personal friendship with Gladstone after dissociating himself from the latter's Home Rule policy. This would have done little to recommend Lubbock to Hooker and Huxley who had acquired first-hand impressions of Gladstone during the Ayrton business. This pliability was utter anathema to Tyndall who somewhat crazily described Gladstone as the "wickedest man of our day and generation" in 1890.

Lubbock's ambition extended to the Presidency of the Royal Society. This was revealed in his constrained attitude towards Huxley following the latter's election to the chair on a temporary basis after Spottiswoode's sudden death in 1883:

> "the only intimate friend who is absolutely silent is Lubbock. So I suppose he thought the pear was for him"58

On the 22nd September 1883, Michael Foster, the Secretary of the Royal Society, wrote to Huxley stating that: "Lubbock is not quite the man."[59] Foster's aim was to foil the aspirations of Stokes' supporters and John Evans' personal ambition by obliging Huxley to fill the place. The opinion of the remaining member of the effective triumvirate - Hooker - had been voiced in an earlier letter to Huxley on July 6th. He singled out Lubbock's wealth as the most damning source of objection to him as P.R.S. Hooker declared himself to be completely united with Huxley in emphasising:

> "the immense importance of keeping the chair free for poor men - this is a cardinal point with me. . . . one of my chief objections to Lubbock was his wealth, he is too much a 'washed out' Spottiswoode."60

Hirst thought that Lubbock's best seller, The Pleasures of Life published in 1889, no more than "trivial tittle tattle".[61] The easy condescension of his long-standing fellow X member's grand social manners was similarly unattractive to Hirst, whose everday life was by this time focussed on the Athenaeum Club. He noted in his Journal for the 5th February 1889:

> "Lubbock dined at the Athenaeum today, with the Archbishop of York. He had asked me to share his dinner table; but on observing that he had also secured the Archbishop, I retired to my own quiet corner."62

The most unmistakable evidence of Lubbock's long-term exclusion from the effective nucleus of the X club emerges through his blithe support of Stokes during the Presidential crisis of 1887. Lubbock saw Stokes' seat in the House of Commons as wholely compatible with, even complementary to, his position as President. This outlook was diametrically opposed to that of Huxley, Hooker, Foster, and the influential "younger brethren" of the biological side of the Fellowship. In his general approval of the fittedness of the evangelical physicist Stokes for the chair of the Royal, Lubbock was joined by Frankland and Hirst. Hooker, Huxley, Frankland, Hirst and Lubbock made up the active X membership in 1887 although Huxley's frailty prevented him from attending very often. Hooker's dismissive references to the "mere convenience" which curtailed Lubbock's commitment to the club's survival suggest that the convenience of such absences was mutual. In the course of reporting recent developments to Huxley in April 1888 Hooker told him:

> "I will do what I can to keep up the X and issue the monthly card in the hopes of better days - but must confess that with only Frankland and Lubbock it will be a cold dinner. If I could make sure of any of the others I would not mind."63

Hooker, Huxley, Frankland, and Lubbock were in Haslemere Churchyard at Tyndall's December funeral in 1893. It was ten months after that of his life-long comrade Hirst at Highgate cemetery. On that occasion, the illness of Huxley and Tyndall prevented their attendance. Lubbock and Frankland were absent for their own reasons.

Hirst never entered into the behind-the-scenes administration of British science which is usually held to be the effective secret life of the X club. However, he was well-liked by some members and personally attached great significance to the X meetings even when the more convivial members were unavailable through their being "out to grass". Huxley remarked in 1885, year of the club's majority, that Hirst would rather have gone to the X club dinner alone and remained so than "pass the day over."[64] William Irvine's contention that Tyndall's funeral represented "the last meeting of the X club" can be seen as rather misleading in the light of the evidence presented here. With Hirst and Tyndall dead the only strong relationship which remained was Hooker's with a Huxley too enfeebled to foster an effective presence. As will shortly be detailed, the "business" side of the X had never been in any hands but those of Hooker and Huxley and their powerful friends outside the X club.

Herbert Spencer, George Busk, Thomas Hirst

Numerous aspects of the X club careers of Spottiswoode, Frankland, and Lubbock tend to support the conclusion that they were not involved with the club's alleged central function. Whatever the real extent of the X club's operation as a "secret cabinet" of British science, these three members were only peripheral to it. Herbert Spencer also can be seen as ancillary to the notion of the club as a decisive clearing house for the resources of power and influence associated with the cause of scientific naturalism. In the department of conviviality, Spencer's

influence was far from negligible. It seems that certain reservations were already held on the Spencer question by the time of the X club's second birthday. Darwin reported to Hooker on a meeting with Spencer at "High Elms" in October 1866: "I plainly made out that Lady Lubbock thinks him like you do, not a small bore".[65] Spencer's legislative style of conversation must on occasion have formed a rich mixture with Lubbock's manner of overbearing reasonableness. As President of the Society of Authors under his title of Lord Avebury he proposed Spencer for the 1902 Nobel prize for literature. In spite of the colossal claims of what Hooker referred to as his "all-true-istic" views, the X members showed no marked taste for hearing them. So keen an attender at the X club dinners was Spencer that the only means of keeping exposure to him within manageable limits was to close the meetings as early as possible. The spirit of his complaint to Huxley as the club treasurer in December 1885 would seem pathetic in a less proud and objectionable man:

> "And so you sat till 10. Well, really, this is too bad. Considering that I am always the one to protest against the early dissolutions that habitually take place, that you should seize the occasion of my absence for making a night of it, is adding insult to injury."66

In the club's early years the necessity of keeping Spencerian loquacity in check had already manifested itself. At the 40th meeting Spencer called the diners to order for allowing its conversation to become broken up rather than remaining general. The other members in regular attendance kept up the habit of forming "binary factors" for conversation in order to prevent Spencer from holding forth. At the 46th meeting in 1870: "conversation was very metaphysical. Spencer v. the field." Four months later Spencer was fighting "the battle of the ladies" single-handed against the club's visitor Professor Masson.[67]

Spencer's extravagant life work - his philosophical design - assumed for him a broad scientific competence. Some of the available evidence has shown this competence to have had poor wearing qualities. George Darwin sent a letter containing his appraisal of Spencer's nebular hypothesis essay to Sir William Thomson on the 29th of January 1883:

> "I should describe it as a clever essay for a boy who knew no mathematics - but as the work of a great philosopher - oh! The Rev. O. Fisher is one of his great authorities. I have tried to be civil by eluding some of the worst parts . . .."[68]

Another of Spencer's foibles was the reiteration of his complete ignorance of Comte's work at the time when he wrote his own treatise on sociology. Spencer's sense of vulnerability to criticism for want of insight into the everyday realities of the natural sciences remained close to the surface during his dotage. Mostly out of London, his time was divided unequally between Brighton and loafing listlessly about the rooms of the Athenaeum. Spencer's craving for scientific authority gave Hirst a sleepless night in March 1890. He browbeat the Athenaem's two leading wranglers, George Darwin and Hemming, into apparent sympathy with his views on the properties of the parabola which Hirst had not approved. Hirst privately maintained that the "Great Philosopher", as he termed him, could not comprehend his own errors however painstakingly they were explained.[69] He thought that Spencer's reputation would have been unable to survive an exposé of his rather modest knowledge of elementary science. This deficiency was kept from wide public scrutiny by the discretion of both friends and surprisingly his enemies as well. The incident described above, involving George Darwin and Sir William Thomson is interesting in this regard. In 1889 Spencer moved into new living accommodation with three maiden ladies and immediately set about the education of his cook. For the most part, his life became devoted to coddling his self-obsession and marked by a

lack of interest in his old friends. Spencer's attitude to honours has frequently drawn the attention of his publicists. He never became F.R.S. seemingly because pride forbad his allowing possibility of his being passed over at the first attempt. In 1883 he declined the Institut's nomination of him as a Corresponding Member because at the same election Sir C. Sumner Mayne had been made an Associate. Spencer seems to have feared being stigmatised by the possession of honours which he saw as unequal to his stature. In 1895, the year of his death, Huxley lamented the way in which Spencer, having accepted membership of the Lyncei was "trampling on Pour le merité". The public row between the two of them was never healed and did not even form a source of regret to Hooker and Huxley, who both came to detest Spencer with great gusto. The latter, who had walked in the London parks on most Sunday afternoons with Spencer in the late 50's and early 60's, declared to Hooker that:

> "A four hundred horsepower Evolution engine couldn't make Spencer into a gentleman. . . . As if the fellow had not sucked my brains for thirty years!"[70]

The exasperated Hooker gave full and free expression to his view of Spencer's place in the X:

> "he was always a damper. Comprehensive as his intellect or capacity is, his views are so cribbed and confined that you have no freedom of motion in conversing with him, and his ego is so crushing that I prefer getting out of its way: in short I never esteemed him. . . . the throne he fancies he occupies, high above that of any sage that ever lived, I rather feel a profound pity. . . . As it is he is the skeleton in the club's closet and I have had difficulty in keeping the door shut upon it."71

The grouping together of Spencer, Busk, and Hirst in this somewhat cursory fashion reflects the assumption that these three were either irrelevant to the Machiavellian manoevrings of the "X club proper", or merely peripheral in capacities such as R.S. Council membership where

they (excluding Spencer) would book the X club line on issues such as elections of new Fellows and Officers. It could be construed as rather odd therefore, that Busk should figure in the few glimpses which evidence provides of the Darwinists acting together premeditatedly in the Council Chamber of the Royal Society. In 1858, Hooker give his vote in favour of a candidate for the Fellowship against his own judgement. He deferred to Busk's strong view expressed privately before the annual June Council which was crucial in selecting the 15 new Fellows of each year. The candidate so energetically supported by Busk was one Williams, and while Hooker was unsure of whether he would be elected, did feel it necessary to inform Huxley of the situation.[72]

George Busk was the oldest X member having been born in 1807 at St. Petersburg. He studied medicine at St. Thomas's and St. Barts's in London from whence he moved to Greenwich as Assistant Surgeon of the hospital ship "Grampus". Having served as Surgeon aboard the "Dreadnought", Busk retired in 1855 to do science in London. He had been elected to the R.S. five years earlier at the beginning of the period of close friendship between Busk and his wife Ellen and Huxley who was struggling to gain official recognition and financial provision for the completion of the work on his extensive "Rattlesnake" data. The year before Busk attended the first meeting of the X club at the age of 57, he acquired a certain limited celebrity by his writing on the topical question of the Moulin Quignon jawbone.[73]

Ellen Busk and her husband joined Huxley and his wife at Tenby where they were honeymooning in 1855. Like the first Lady Lubbock, Ellen Busk was of great importance as a female companion to several of the younger unmarried X club members. The X club meeting which took place immediately after her death in February 1890 was described by

Huxley as being under her shadow. The Busk family was drastically reduced to two daughters: Elinor and Fanny, in the four years to 1890. George Busk himself was a close friend of the "naval zoologists" Huxley and Hooker but rarely took an influential rôle in the scientific life of the capital city. Busk's place fits uncomplicatedly with Huxley's evocation of the friendship model of the X club.

Hirst's career has been discussed in detail elsewhere. It is not always noted that he had drifted away from the other X club members by the 1880's. Hirst's lack of intimate knowledge of the Huxleyite stance on numerous issues is made manifest by his Journal.[74] As a widower of long standing, Hirst was very particular about his quite often solitary routine. Although still very keen on the X club meetings up to the end, he had cooled towards Huxley and Hooker due to his own sympathy for Spencer. Hirst never suspected the depth of Huxley and Hooker's antipathy towards Spencer. By the 1880's Hirst was reliant on the regular diners at the Athenaeum for company and conversation. He dined regularly with J. J. Sylvester the Oxford mathematician, Herbert Spencer, Kerr (one of the club's leading scandalmongers), and two other new obscure members, Massey and Westmacott.[75] Hirst had always been much closer to Ellen than George Busk. When the first Lady Lubbock died in 1879, the lip-service which Hirst had previously paid to Sir John Lubbock, virtually ceased. Hirst was out of sympathy with Huxley and Hooker over Spencer, and only ceased to be in awe of Spottiswoode's wealth with the latter's untimely death in 1883. Hirst's embittered and ineffectual arbitration of the squabbles and ensuing coldnesses of Tyndall and Debus points to a melancholy conclusion. The physical fraility which terminated the meetings of the surviving X club members served as a welcome pretext for ending what was no longer a pleasure for most of them.[76]

John Tyndall's Decline and Death

Tyndall's suprising remoteness from the main focus of Huxleyite influence has been described in another section of this work. His life and work have, like the other leading X club members' contributions, been dealt with in some detail by historians. One interesting aspect of this coverage is the way in which Tyndall's deterioration into hopeless drug addiction has been missed or concealed. As early as 1868 Hirst noted in his Journal that Tyndall had begun to use brandy to counter sleeplessness.[77] Tyndall's bachelor status was seen as a problem by some of his friends. Two years after Hirst's recognition of Tyndall's problem with insomnia, Hooker was entertaining Hirst and Tyndall at his home. According to Hirst, their host was "too evidently" displaying the suitability of another guest, Miss E., as a wife for Tyndall.[78] The extent to which he indulged in laudanum and chloral during the 1870's is unclear. That time was Tyndall's hey-day as a controversialist in the cause of materialism. Hirst noticed a change in him following his return from America in 1873. Hirst noted in his Journal that following that date Tyndall was more sensitive to real or presumed slights. As often happens in such cases, the person concerned perceived a trend which was the virtual opposite of that noticed by observers. Tyndall expressed the opinion that he possessed a new magnanimity.[79] Of course, Tyndall's emotional development during the 1870's can readily be attributed to the normal exigencies of the ageing process on a volatile temperament. What is clear is that by the time Tyndall's situation at the Royal Institution was becoming untenable during 1886, his narcotic addiction was well-advanced. Hirst describes aspects of these events in some detail. Early in August 1886 he saw Tyndall alone at the Royal Institution with the intention of having a serious talk with him:

> "about himself and the sad condition in which I found him at Hindhead at the beginning of July. He spoke calmly and with perfect self-possession, tried to reassure me. He assured me that the affair was at an end and would not re-occur. His tone was not at all that of a man who is conscious of any moral weakness on his own part."80

At Tyndall's final lecture at the Royal Institution, the electrical engineer, W. H. Preece, noticed that the speaker:

> "maundered on well over his usual hour, repeating himself again and again."81

Unpleasantness accompanied James Dewar's takeover of Tyndall's position at the Royal Institution. Eve and Creasey, in their biography of the Irishman, emphasised the shabby behaviour of Dewar towards their subject during the transitional period at the Royal Institution.[82] Eve and Creasey's interpretation omits the fact that in his last two years as Professor at the R.I., Tyndall had become embarrassingly incapable as a result of his addiction. A letter from Huxley to Hooker in March 1884 regarding the prospects of a forthcoming trip on Spencer's yacht, throws new light on Tyndall's situation at this time.

> "And I should have the gravest doubt about Tyndall [making the trip on Spencer's yacht] - Dewar can manage him and has, in fact, saved him out of the fire this time but, I know noone else who can."83

In late June 1888, Hirst noted in his Journal that Tyndall was continuing his overindulgence in opiates. By this time, two years had elapsed since Hirst had found him in such a "sad condition" at Hindhead. At about that time, Tyndall began to take the proprietary sedative "Bromidia". Hirst reported his friend to be strong but still unable to sleep.[84] During the last two years of the 1890's Tyndall's close friends clearly felt that their reserves of feeling for him were being run close to exhaustion. In December 1888, Hirst strongly resented Tyndall not turning up at the X club meeting, largely because he was left with the relatively uncongenial company of Hooker, Frankland, and Lubbock.[85]

Popular concern over Tyndall's health was sufficient to prompt the appearance of morning and afternoon bulletins during the later part of April 1891. Tyndall himself wrote to the newspapers to have these suspended and at the same time explain some aspects of his illness.[86] When Huxley made one of his increasingly rare forays in the summer of the same year he found Tyndall "quite bright and Tyndalloid" at Hindhead.[87] The interplay between Tyndall's physical illnesses and his addiction to narcotic drugs over a period of many years was known to a few of his closest friends, but not even hinted at in the public domain. When Tyndall died in December 1893, Hooker was in no doubt as to the cause of his friend's deterioration and final undoing. Writing to Huxley on December the fifth, Hooker lamented that:

> "another of us is gone - I suppose it was chloral at the last - and all along."88

The story put about by Tyndall's immediate circle prior to the inquest was of an innocent mistake over medicine bottles on the part of his wife Louisa. Although accepted by the inquest and Eve and Creasey in their highly laudatory biography of Tyndall, this explanation has to be regarded as being somewhat beside the point in view of his long history of addiction.[89] John Donnelly wrote to Huxley on the day following Hooker's letter. Donnelly seemed to quite casually suggest that Tyndall's final demise might have been deliberate.

> "Why cannot they let a poor fellow take an overdose of chloral without having an inquest?"90

Understandably, the tenor of a number of statements made by Tyndall's actual (and erstwhile) intimates at the time of his death was one of relief.

* * *

Notes

1. Ruth Barton, "The X Club. Science, Religion, and Social Change in Victorian England". Ph.D. dissertation, University of Pennsylvania, 1976. R. M. Macleod, "The X Club. A Social Network of Science in late Victorian England", Notes and Records of the Royal Society of London, 1970 (April), 14, pp. 305-318. J. Vernon Jensen, "The X Club: Fraternity of Victorian Scientists", British Journal for the History of Science, 1970 (June), 5, pp. 63-73.

2. Barton, ibid., pp. 17-19. This assertion is very dubious. It holds forth no prospect of an explanation of why scientific naturalism failed in all its main endeavours. Neither does it reflect the fact that the representatives of land and inherited wealth trained as classicists remained in virtually uninterrupted charge of the national life of this country for many years after the close of the nineteenth century.

3. A. A. Eve and C. H. Creasey, Life of John Tyndall, Macmillan, London, 1945, p. 46. This was probably the earliest of the damaging encounters which Tyndall was to have with the forces of Cambridge science.

4. M. J. Bartholomew, "The Award of the Copley Medal to Charles Darwin", Notes and Records of the Royal Society of London, 1975, 30, no. 1, pp. 209-217.

5. Mark Beach, "Was there a Scientific Lazzaroni?", George H. Daniels (ed.), Nineteenth Century American Science, Evason, Illinois, 1972, 115-132, ff. 115.

6. Stokes Papers (CUL), R.S., no. 137.

7. Barton, ~~ibid~~., p. 128.

8. White's Journals, p. 82.

9. Stokes Papers (CUL), R.S. no. 200.

10. Huxley Papers (ICL), 11.81.

11. Stokes Papers (CUL), M 547.

12. Barton, passim. M. Berman, "'Hegemony' and the Amateur Tradition in British Science", The Journal of Social History, 1975, 1, pp. 30-50.

13. D. S. L. Cardwell, The Organisation of Science in England, Heinemann, London, 1972.

14. Macleod, ~~ibid~~.

15. Jensen, ~~ibid~~.

16. Barton, ~~ibid~~., 128, 132.

17. Royal Society Council Minutes 1850-1900. Record of the Royal Society, 1948.

18. William Irvine, Apes, Angels and Victorians, Weidenfeld and Nicholson, London, 1955, p. 236.

19. Ibid., pp. 235-238.

20. O.U. microfilm of Frankland Papers (RFC). Frankland was the most frequent member of the X club in the matter of attendance. He was present 186 times.

21. Irvine, ~~ibid.~~, p. 236. Barton, ~~ibid.~~, p. 6.

22. O.U. microfilm of Frankland Papers (RFC). Fergusson was prominent as an Art Historian.

23. Huxley Papers (ICL), 2.312.

24. Irvine, ~~ibid.~~, p. 236. Huxley allegedly reported this as an amusing incident. Huxley's rejection of the picture of the club as an exclusive brokerage for power and position in English science is firmly supported by his opposition to the addition of new members in the 1880's. The persons concerned (particularly Strachey and Foster) were very close to Huxley and occupied significant positions in the scientific world. At a time when the original Huxleyites were waning due to advancing years, it is almost inconceivable that Huxley would have opposed the admission of intimates who occupied influential scientific offices - if the Irvine-Barton conception of the club were correct.

25. Huxley Papers (ICL), 8.131.

26. Hirst Journal, 1704.

27. Ibid.

28. Barton, ~~ibid.~~, p. 145.

29. Ibid., p. 117. Bearing in mind that Airy was virtually recruited for the chair of the Royal Society a few years earlier by the leading members of the X club, it seems remarkable that he knew so little of the nature of Hooker's support in 1873.

    It must be remembered that middle-class, male, professional London was a profoundly clubbable place at this time. Those whom modern historians take to be the interested contemporaries who "must have known" what was going on, would have had their own semi-clandestine arrangements of an intriguing nature.

30. Hirst Journal, 2569.

31. Ibid., 2659. These items of evidence support the interpretation of the X club as a primarily social entity. The adjacent reader (of the quotation), Lockyer, possessed a high estimate of his own importance in the scientific world and took part in two regular social events of a similar nature. In 1867 Lockyer inaugurated a series of weekly "smokers" at Hampstead. These meetings went on for many years, usually on Wednesday evenings.

32. Ibid., 1732.

33. Ibid., 2622.

34. Leonard Huxley, Life and Letters of Thomas Henry Huxley, 2 vols., London, 1900, vol. 2, p. 50.

35. Hirst Journal, 1720.

36. Huxley Papers (ICL), 30.180.

37. Stokes Papers (CUL), K 290.

38. Hirst Journal, 2130.

39. O.U. microfilm of Frankland Papers (RFC).

40. Hirst Journal, 1560.

41. Huxley Papers (ICL), 16.251.

42. Colin A. Russell, "Edward Frankland and the Cheapside Chemists of Lancaster", Annals of Science, 1978, 35, 253-273, ff. 267.

43. Huxley Papers (ICL), 14.46.

44. Ibid., 2.250. Frankland achieved office in the Royal Society only after the X club era.

45. Hirst Journal, 2555. Clearly the work of the paid expert witness was intimately bound up with advocacy. Its assumptions denied the ethos of the sort of impartial knowledge-seeking which would in due course reveal the "new Nature". This was a crucial matter for the X club to be divided over in the long term. The fact that these differences were never reconciled lends further support to Huxley's portrayal of the club as being primarily a social entity.

46. Ibid., 2592.

47. Huxley Papers (ICL), 3.316.

48. Ibid., 3.308.

49. Ibid., 16.268. In the following year Frankland arrived in Mentone - "in hopes of catching an earthquake - but was disappointed".

50. O.U. microfilm of Frankland Papers (RFC).

51. Irvine, ~~ibid.~~, 211-212.

52. James R. Moor, The Post-Darwinian Controversies: a Study of the Protestant Struggle to come to Terms with Darwin in Great Britain and America 1870-1900, Cambridge University Press, 1979, p. 99.

53. Hirst Journal, 1798.

54. Irvine, ~~ibid.~~, p. 228.

55. Moore, ~~ibid.~~, p. 111.

56. Horace G. Hutchinson, Life of Sir John Lubbock, Lord Avebury, London, 1914, 2 vols., R. J. Pumphrey, "The Forgotten Man - Sir John Lubbock", Notes and Records of the Royal Society of London, 1958, 13, 49-58.

57. Huxley Papers (ICL), 3.169.

58. Ibid., 2.258.

59. Ibid., 4.218.

60. Ibid., 3.274.

61. Hirst Journal, 2593.

62. Ibid., 2578.

63. Huxley Papers (ICL), 3.325.

64. Leonard Huxley, ~~ibid~~., vol. 2, p. 260.

65. Irvine, ~~ibid~~., p. 166.

66. David Duncan, Life and Letters of Herbert Spencer, D. Appleton and Company, New York, 1908, p. 249.

67. O.U. microfilm of Frankland Papers (RFC).

68. Kelvin Papers (CUL), D 20.

69. Hirst Journal, 2632.

70. Huxley Papers (ICL), 2.434.

71. Ibid., 3.360.

72. Ibid., 3.241.

73. A. Geikie, Annals of the Philosophical Club, London, 1917 p. 41.

74. Hirst appears to have been wholly unaware of numerous aspects of the Huxleyite outlook. For example, he seems to have been ignorant of the ridicule to which the Huxleyites subjected both the person and deeds of Sir Roderick Murchison. Hirst was unknowingly at variance with Huxley's inner circle regarding the burial of Spottiswoode in Westminister Abbey. A third instance of Hirst's remoteness from the inner circle of Huxleyite scientific naturalists is provided by his apparent unawareness of the central rôles of Richard Strachey and William Thistleton-Dyer.

75. Hirst Journal, microfiche no. 77, passim. Sylvester was undoubtedly a difficult man. Ironically he suspected Hirst (one of the least conspiratorial of men - especially as judged within the X club membership) of acting with others to arrange matters against his interests. In a letter to Hugo Müller written in February 1873, Sylvester remarked of Hirst that he was: "thoroughly insincere . . . probably bound up by some engagement to a cabal with

whom he is mixed up, being an inveterate plotter." (Ibid., 1953). Of course, Hirst was bound up with a cabal but was unaware of its wider endeavours and far from its centre. Indeed, Hirst's naivete in many connections appears to be quite authentic. There was a common inclination on the part of Victorian professional men to see sinister machinations covertly manipulating familiar institutional appearances, from within the upper echelon of the institutions themselves. This is vividly reflected in the works of Conan Doyle and G. K. Chesterton.

76. Throughout the 1880's the group comprising Heinrich Debus, Tyndall, and Hirst was frequently riven by petty squabbles (Ibid., 2416, 2562, 2273). At the X club itself, nearly all of the strategies put forward to facilitate conviviality were rejected. These included the rearrangement of expenses on a pro rata basis, the possibility of further "double X meetings" including wives for summer excursions, and the idea of making X club meetings into house parties where the members would visit each other and stay overnight (ibid., 2560).

77. Ibid., 1830. Tyndall possessed a wild streak in his character without doubt. In 1865, Hirst noted that during Alpine excursions it was Tyndall who wanted to set aside occasional days for climbs involving greater risks than were routinely countenanced (ibid., 1729).

78. Ibid., 1842.

79. Eve and Creasey, ~~ibid~~., p. 161. In 1871, Tyndall wrote of a scientific critic: "ten years ago, I should have been at the throat of Zöllner, but not now."

80. Hirst Journal, 2294.

81. E. C. Baker, Sir William Henry Preece Victorian Engineer Extraordinary, Hutchinson, London, 1976, p. 351.

82. Eve and Creasey, ibid., p. 248.

83. Huxley Papers (ICL), 2.272.

84. Hirst Journal, 2300. It is well-known that the medicinal use of narcotic drugs was very common in polite London society at this time. In November 1885, Hirst himself began to take cocaine for the neuralgia he had begun to suffer from earlier in the year (ibid., 2219). Over a period of a few weeks in 1886, Hirst noted Tyndall's "shocking condition" at Hindhead, the physicist William Grove's use of chloral for his depressive condition, Tyndall's resort to "Bromidia", and Spencer's reduction of his medicinal alcohol intake in favour of more opium (ibid., 2294-2810).

85. Ibid., 2560. Eighteen months later, Hirst repined vigorously at Tyndall's social outings having become confined to large glamorous dinners. At that time, Tyndall was still having difficulty sleeping with the assistance of laudanum (ibid., 2685).

86. Ibid., 2770.

87. Huxley Papers (ICL), 2.385.

88. Ibid., 3.413.

89. Eve and Creasey, ~~ibid~~., p. 279.

90. Huxley Papers (ICL), 14.159.

CHAPTER SEVEN

THOMAS HUXLEY AND THE PLACE OF SCIENTIFIC NATURALISM IN THE ROYAL SOCIETY

The X club was not the main vehicle for the exercise of the power of scientific naturalism in the Royal Society. This has been shown by the members' lack of common purpose and the rarity of concerted actions. From the effects which were produced, it is clear that the scientific naturalists' cause must have been consistently promoted within the Royal Society. The questions then remain of determining who was involved in this activity and the extent to which they acted in a co-ordinated fashion. A good deal of evidence suggests that for many years Thomas Huxley was the key figure in organising efforts to secure the interests of the scientific naturalists within the Royal Society. Huxley's skilful handling of the oligarchic government of the Society was combined with the benefit of strong personal relationships with a range of strategically situated officials in the wider orbits of science and government. One of the assumptions of the "X club thesis" holds that effective control of the Society was maintained by keeping as many X club members as possible on the Council list. The full Council, including the Officers, amounted to twenty-one men, each holding one vote. The X club (which Barton has identified as: "the cabinet of a liberal party in science . . . it was the party in power between 1870 and 1885.") held an annual average of 3.4 Council places for that fifteen-year period. X club members held an averge of 1.86 positions as Officers of the Royal Society over the same period.[1] Even if the remainder of the "X club thesis" is accepted, these figures indicate a flaw in the notion of the club as a "cabinet" working along democratic lines within a parliamentary Royal Society. The oligarchic nature of its government made it unnecessary for a numerically strong coterie of X club members to pack the Royal Society's Council. Joseph Hooker

appears to be the only X club member to have been deeply involved in the sphere of influence formed by Huxley's bilateral relationships. Hooker's place within the Royal Society made his dealings with Huxley especially significant. Hooker's correspondence offer two interesting views of the inner workings of the Royal Society; one taken from the time of his relatively powerless position on the Council as a rising man of promise, and the other taken from the retrospective outlook of his retirement from official life. In 1861 he addressed Huxley as follows:

> "The dearth of botanists makes me think that the R.S. Council may think of proposing me again for Council. Should you hear of anything of the kind will you give me warning, as I must decline . . . I hate the Council, its electing, medals, and dodges . . . and am clear for following my old Dad's example and drawing off London."2

Twenty-six years later, the best part of which was spent right at the centre of things both as Huxley's closest comrade and for five years as P.R.S., Hooker summed up the nature of the Society's government as follows:

> "Again the Society is not a homogenous body in the sense or to the degree that many similar associations for more special objects are. Few of the Fellows trouble their heads about its administration, or care to do so - they are content to put F.R.S. after their names, and even of the Councils, not many are impressed with a sense of their duties. The government is truly oligarchical; and practically that of the officers. Now I believe this is as it should be . . . ."3

If Hooker is taken at his word and Huxley is presumed to have been the key figure who mobilised the power of scientific naturalism in the Society, then one would expect to see Huxleyites occupying vital places within the alleged oligarchy. The rhetoric of reform and democracy which has commonly been applied to the Royal Society as it emerged from the statute changes of 1847, clearly does not fit Hooker's view of the situation.[4]

William Sharpey

The first individual to become an Officer of the Royal Society and as such to support the interests of scientific naturalism was William Sharpey. Even though it frequently appears that Hooker and Huxley burst upon the scene at Burlington House as the earliest promoters of anti-clerical doctrine, they were doubtless assisted in their ambitions by Sharpey. Born in 1802, Sharpey exerted a great influence over British physiology as Professor of Anatomy and Physiology at University College London from 1836-1874.[5] He was elected to the Fellowship of the Royal Society at the age of 37, serving on the Council from 1844. Nine years later, election of the Royal Society's Junior Secretary Thomas Bell to the Presidency of the Linnean Society caused him to resign his former position. Sharpey was elected while a year later the retirement of Samuel Hunter Christie made Sharpey the Senior Secretary. George Stokes was the newcomer to the secretariat on the physical side. Sharpey was involved socially with Huxley and his friends during the 1860's. This is made clear by a series of entries in the journals of Thomas Hirst. Early in June 1872, Lady Lubbock gave a dinner party for most of the members of the X club to which Sharpey was also invited.[6] Sharpey's commitment to the removal of clerical restraints on scientific progress is clearly displayed by his own behaviour. This view is strengthened by his formative influence on other nascent activists in the cause of scientific naturalism, and by the choice of successor that he made for his place as biological Secretary. In a speech to the B.M.A. in 1862 Sharpey dwealt at length on the harmful effects of the suppression by churchmen of views such as those of Darwin:

> "Faint as some may deem the prospect of success of Mr. Darwin's great attempt, let none condemn its tendency . . . surely such an issue could but tend to lighten and exalt our conceptions of creative wisdom."7

Michael Foster and Joseph Lister each in their turn became favourite pupils of Sharpey at University College. Both went on to become Officers of the Royal Society. Like Sharpey, Foster studied classics at first. Both developed careers which were predominantly given over to university teaching rather than their own research work. Foster accompanied Sharpey on his last continental touring holiday.[8] In 1872 when he had decided to retire from the Secretaryship of the Royal Society, Sharpey questioned Huxley about his willingness to take on the job. Hirst's account of the Council meeting on the 16th of May during which Sharpey's replacement was to be decided, shows that the advent of Huxley was far from being welcome on all sides:

> "Busk proposed and I seconded a resolution to the effect that it was 'Desirable that Mr. Huxley should be recommended as Secretary to the Society in place of Dr. Sharpey who retires.' The proposition took some of the members by surprise and it was decided that the vote on it should be postponed."9

Early in his Royal Society career, Sharpey earned a reputation for engaging in forceful in-fighting on both the Council and the sectional committee for physiology. The clearest example of this concerned the award of the Royal Medal for physiology in 1845. This has been described in detail elsewhere.[10] Sharpey's conduct prompted the fierce antipathy of Thomas Wakley who was then the editor of The Lancet. The author of a biography of Wakley described Sharpey as: "the profound physiologist and autocrat of the elections of the Royal Society".[11]

For nearly twenty years, Sharpey shared the secretariat with Stokes. Although occupying a lofty position in British physics, Stokes was not conversant with the style of internal politics which determined the outcome of all important issues which the Society encountered. The religiously orthodox Edward Sabine and his natural opponents took the largest parts in the crucial dealings within the successive groups of officers which held sway between 1853 and 1871. An event such as

the award of the 1864 Copley Medal to Charles Darwin is rather simpler to explain on the basis of strictly reciprocal arrangements between members of an oligarchy, rather than a simple majority of pro-Darwinists in the Council vote. Sabine was the Treasurer of the Royal Society from 1850 to 1861 and then President from the latter date until his retirement in 1871. He sought to hold back the Darwinists within the Royal Society and was at the centre of the furore over the presentation of Darwin's Copley Medal in 1864.[12] Huxley was Sabine's principal opponent during the controversy, which came three years after he had tried to block Sabine's path to the Presidency of the Royal Society.[13] As a mere Fellow with the reputation of being Darwin's most vociferous supporter, Huxley had proved unable to keep Sabine from the chair.

The presence of Sharpey as Senior Secretary of the Royal Society must have greatly assisted the emerging group of scientific naturalists whilst they were finding their feet in the various departments of the world of Victorian science.

Michael Foster

Huxley's time as Secretary lasted from 1872 to 1881. His replacement, Michael Foster, had been favoured as Huxley's protegee for many years. The two men first met at some point in the late 1860's. In 1870, Huxley gave his course in practical biology at South Kensington for the first time. Foster was engaged as a demonstrator along with Ray Lankester and Rutherford. The two men worked together in connection with the South Kensington examinations then later in 1870 Huxley recommended the thirty-four year old Foster for the newly established Praelectorship in Physiology at Trinity College, Cambridge.[14] When Foster's candidacy for the Royal Society was under consideration in the Council meeting on the 11th of April 1872, Thomas Hirst spoke in his favour. Foster was elected. A month later he was at Huxley's house

for the latter's birthday dinner.[15] This was the period immediately preceding Sharpey's withdrawal from the Royal Society Secretaryship due to failing eyesight. Huxley had a high regard for Sharpey's performance as Senior Secretary. Huxley's letter of thanks to Hirst for his congratulations following Council's selection of the new biological secretary reflects this:

> "I have a fancy for the office as you know and hope that I will be able to do the work satisfactorily, though I think that anyone coming after dear old Sharpey will have a difficult task to do as well as he has done . . . ."[16]

Foster succeeded to the office in 1881 as Huxley's nominee and was himself a follower of Sharpey's teaching from his days of training at University College London. Sharpey's supporters, led by Huxley, were able to secure a Civil List pension of £150 per annum for him following his retirement. The succession of the three pro-Darwinian biological secretaries Sharpey, Huxley and Foster ran from 1853 until Foster's sudden death from a burst oesophageal ulcer in 1907. Foster's rise to eminence within the British scientific establishment was clearly attributable to the agency of Huxley at each of its crucial points. Foster himself was not slow to point this out to his benefactor in April 1870 when Huxley's intercession had recently provided the recently widowed Foster with his Cambridge Praelectorship:

> "From the time I trembled before you at a corner of the Royal Society's old tea-room or when you made me blush at Aberdeen in the reddest and hottest manner by patting me on the back after my first little shot, up to last week I have had nothing but help from you . . . ."[17]

Acknowledging that without Huxley's help, his livelihood from science would have been "scanty enough", Foster shows that as early as 1870 he had a detailed knowledge of Huxley's ambitious schemes for the reform of English scientific life. The particular case referred to by Foster in early 1870 was the liberalisation of the Ethnological Society.[18]

Huxley's intention to install his protegee in the biological Secretaryship in 1881, following his own retirement, is evident from a remark of Foster's addressed to the man he privately referred to as "the General":

> "As to R.S. of course if things turn out and Flower won't stand - I will see if I can conscientiously follow your bidding."19

Huxley was already in the habit of keeping Foster informed of the private transactions of the Royal Society's Officers in 1873. At that date Foster was 37 years old and had been a Fellow of the Society for just one year. A week prior to the Anniversary Meeting on St. Andrew's Day 1873 "the General" relayed to Foster his confident sense that with Spottiswoode established as Treasurer and himself as Secretary, the installation of Hooker as the new President would be accomplished without difficulty. The chair had fallen vacant because of Airy's resignation. It had first been received at the end of October 1872 but Sharpey and Stokes succeeded in staving off the rapid withdrawal of the new President by a year in order to preserve the dignity of the office and the Society.[20] Huxley summed up the situation surrounding Hooker's election to the Presidency in a letter to Foster dated 23rd November 1873:

> "I don't suppose there will or can be any battle at the R.S. Airy has conducted himself more lunatically than ever - we are well quit of him."21

The close relationship continued throughout the 1870's involving Huxley in briefing his acolyte in the affairs of the Society's management. In 1875 Foster co-operated with Huxley and Hooker in supporting the claims to the Fellowship of Ray Lankester.[22] During most of the 1870's Huxley had little difficulty in getting his own way. Despite the fact that Airy turned out so poorly as P.R.S., he had been acceptable to Huxley and his circle as a means of keeping the chair from the Duke of Devonshire.[23] Airy was followed as President by Hooker then Spottiswoode.

With Huxley himself as Secretary in succession to Sharpey and John Evans as the new Treasurer following Spottiswoode's promotion in 1878, the Huxleyite hold on the Society's executive was nearly complete.

After Spottiswoode's sudden death in 1883 Huxley was appointed as the provisional P.R.S. until fuller consideration could be given to the matter in the weeks preceding the next anniversary meeting. Throughout this vital period Foster kept in close communication with Huxley, providing him with detailed information about the attitude and behaviour of Stokes. The guileless Senior Secretary was perceived as the only viable nominee of the conservative "old guard". Foster was convinced that the Council would back Huxley unanimously if Stokes would not allow himself to be put forward. Ever since the reform year of 1847 contested elections for the Presidency had been avoided with equal care by virtually all parties in the cause of preserving the seriousness and dignity of the Society's public image. Against this background Foster wrote to Huxley on the 22nd of September 1883:

> "I insisted that the interests of Science and of the Society really narrowed us to two men - yourself and himself - and continued that I understood that he did not wish for it . . . to my surprise he harked back from his definite refusal . . . I told him that you had said to me that you would on no account allow yourself to be nominated if he desired the post, and urged him to make up his mind and decide. . . . I think the matter stands thus - the great body of all the fellows and all the younger and working ones want you and will howl if you are not appointed - "one or two older fellows" don't want you and I fancy they have got at Gabriel and are egging him on to allow himself to be nominated - I don't think he will consent - if he does I think he must go in. . . . If you were to fall away we should be in Queer Street. . . . If Stokes stands I shall be in despair - but other wise there is nothing for it but for you to take it."[24]

A few days later Foster addressed another letter to Huxley noting that he intended to "see Stokes and run him in a corner."[25] Foster's design was to play on Stokes' basic modesty. He travelled from Cambridge to

London by train specifically to let Stokes know that the three officers A. W. Williamson, John Evans and himself were behind Huxley.[25] Stokes allowed his name to be dropped and his opponent was duly installed at the next anniversary meeting. The desire of Huxley and his circle to keep out Stokes must have been a very strong one because they were quite aware that Huxley's exhausted and feeble condition was likely to prompt an evacuation to distant foreign parts. On the firm advice of his doctor Sir Andrew Clark, Huxley set off for Italy. The trip was envisaged even at the time Foster was working to secure the Presidency for his mentor. At the close of the letter of 2nd October referred to above (note 25) Foster passed on the light-hearted words of his sister Mercy. She had said that if she could break down and go to Venice then she wouldn't mind being P.R.S.

Foster kept the absent President fully informed of Royal Society affairs during his European sojourn. In December 1884 the crossing of their letters in the post prompted Foster to remark: "We are a sort of Corsican brothers each writing to each just when the other is writing."[26] Evans' request for a formal letter appointing him Vice-President of the Royal Society to act in Huxley's absence was passed on in the same communication. As the President's state of health underwent no dramatic change for the better, so Foster's estimate of Evans as a possible replacement steadily grew. By this juncture the need to keep Stokes from the Chair was looming far larger for Foster than it was for Huxley. On December the 2nd 1884 Huxley received the following words from his reliable lieutenant:

> "Evans is in great force and I think he has very much strengthened his position in view of Presidentship."27

Foster's hopes that the Treasurer's administrative acumen and well-received Presidential Address to the Anniversary Meeting in 1884, would

be sufficient to outweigh Stokes' scientific eminence were to be disappointed. The Society's long-serving Secretary became P.R.S. one year later.

Foster's forebodings about the prospect of Stokes in the Presidential chair were fully borne out by events. When he suggested to Stokes that the Society should produce a volume covering the progress of science during Victoria's reign in time to coincide with the celebration of her jubilee in 1887, Foster was not encouraged by the Presidential response. He relayed the nature of the outcome to Huxley by letter on November 7th 1886:

> "I have mentioned it to Mumbo Jumbo [Stokes] - but that is not much use. I have no doubt Mumbo Jumbo has written to the Pres. R.S.E. [Sir William Thomson at the Royal Society of Edinburgh] but of course he has not shown me what he has written - and of course I shall not ask him - Alas for the days that are gone!!"28

Foster largely disregarded Huxley's retirement from the executive of the Royal Society. Over a year after the promotion of Stokes to the vacant chair, Huxley was wintering in Lisbon. There he received a letter containing Royal Society news with which Foster had enclosed a letter addressed to Stokes from the Treasury. He had done this, he reported, "to save time". It was Foster's hope that he and Huxley could iron out an agreed policy on the matter and persuade Stokes not to deal with the matter before the Royal Society Council. It is clear from these events that the oligarchic method of government which the officers had enacted when Huxley and his circle were in full force, was not disrupted by Stokes' promotion to the chair. Regarding the Treasury letter, Foster declared unequivocally: "My own view is that we should deal with it ourselves."[29] On April the 22nd 1887 Foster wrote to Huxley again on Royal Society official business. He enclosed H. Seeley's paper in order that Huxley could determine whether it was suitable to become the Society's Croonian Lecture for that year.

Foster concluded by consulting Huxley about what he termed "the Parkerian crisis". This concerned the opposition of the influential "younger brethren" on the biological side to the continued publication of the lengthy (and non-Darwinian) papers of W. K. Parker. Foster suggested that a special committee to decide the issue ought to have Huxley and W. H. Flower representing the senior biological Fellows with the two leading "younger Brethren" representing the junior position.[30] With Huxley back in London and Stokes as P.R.S. preparing to enter Parliament as the member for Cambridge University, the stage was set for what A. J. Meadows has described as "a major dispute of the eighties".[31] The affair centred around Huxley's leadership of the group which objected to Stokes' dual rôle. Its unsatisfactory outcome from the point of view of the scientific naturalists reflected the slackening grip of Huxley on the late Victorian scientific world, and the failure of any worthy successor to take up his former position. The Presidential crisis of 1887 will be detailed in a later section.

Michael Foster acted for Huxley throughout the troubled later months of 1887 which witnessed the attempt to remove Stokes from the chair of the Royal Society. Promoted to Senior Secretary following Stokes' elevation to the chair in 1885 (Lord Rayleigh took over Stokes' Secretaryship), Foster continued to consult "the General" until virtually the end of his life. In April 1888 Foster sent details of a new scheme for the Government Grant to Huxley for his perusal.[32] Later in the year Foster consulted him regarding possible replacements for Williamson who was retiring as Foreign Secretary. The questions of Stokes' Presidency and his replacement in 1890 were regularly discussed by the two men. On March 7th 1895, four months before his death, Huxley received two requests from Foster which typified the nature of their relationship. Foster's words clearly testify to the staunchly

agnostic position which he held in common with his master and erstwhile patron. Foster expressed the wish to speak to the ailing Huxley about:

> "The Pres. R.S. to take office Dec. next!! I want to see your review of A. Balfour. It would be rather amusing to write a parody on B's book bolstering up some d-d idiocy or other on the same lines. He and Salisbury run on the same lines. Is this the basis of Conservative Statesmanship? Thank Ether I am a radical."33

Balfour was to become Prime Minister in 1902. His book was an able defence of orthodox intellectual traditions which was comparable in its intentions with Lord Salisbury's 1893 Address to the British Association.[34]

Joseph Hooker

In October 1897 Thomas Huxley had been dead for over two years. Joseph Hooker's letter to his widow Henrietta confirms the particular closeness of the relationship between Huxley and himself.

> "Well I miss him and Tyndall and Spottiswoode more than I can tell, but him most of all. The X club died with him and I have never had the heart to ask for another meeting, even to wind up. Spencer I never see; Frankland very rarely; I still go to High Elms once a year for old acquaintance's sake, but it is another house to what it was."35

On several occasions Hooker showed that his outlook was rather more militant than Huxley's own. An instance of this occurred in September 1872 during the Ayrton incident when Hooker's position as Director of Kew was threatened by official interference.[36] Huxley related to Tyndall his regret that he had not been on hand to advise Hooker during the latter stages of the controversy:

> "I wish I had stopped in town, as I was minded to do, until it was all over. I believe I might have mended matters. . . . my judgement was clear that Hooker should have taken no steps whatever after it, but should have waited until he got some official statement from Gladstone of what was expected of him. Then there would have been something to go upon."37

As had been described in the foregoing chapter, Hooker formed one-third of the group of naval zoologists who joined the X club in November 1864. However, it is evident that activism in the cause of Darwin was far from being the X members' exclusive preserve. In a letter assuring Darwin of strong support, written in late 1863, Hooker names Bentham, Oliver, and Thomson as his biological "inner circle". Beyond that group, Hooker mentioned Lubbock and "half a dozen others" as being dependable promoters.[38]

The concentration of historians' attention on the affairs of the more well-known X club members' affairs has consistently oversimplified the way in which the period has been portrayed. Two now forgotten men figured in the vanguard of the movement to secure for Hooker the 1887 Copley Medal. In common with those named above as his most intimate and influential biological friends of 1863, Hooker's leading Copley supporters did not hail from the small group of men usually cited as the leading "Hookerites". One X club member, Frankland, was among the group which also contained Michael Foster, George Darwin, Arthur Gamgee, and A. W. Williamson.[39]

Despite Hooker's alliance with Huxley on virtually all matters religious and Darwinian, it appears that he did not give firm public emphasis to his own position until nearly a decade later. During the stormy meeting at the Oxford B.A.A.S. in 1860 which provided Huxley with his first starring rôle as the champion of Darwinsim, Hooker has generally been attributed a far smaller part.[40] At the 1868 B.A.A.S. meeting held at Norwich, Hooker espoused strong views which offended many people. His opinions, like Huxley's and their many followers, came to be known as agnostic, following Huxley's coining of the term two years later as a banner beneath which to fight his battles in the Metaphysical Society.[41] Thomas Babington told Hooker that his Norwich

Address had shocked and surprised a lot of his friends. The reverberations of Hooker's disclosures continued into the new year. He wrote to Darwin in January 1869:

> "I have got tremendously pitched into for quoting (Spencer) in my address as I expected; and for declaring the power above to be inscrutable. My last flagellation is from Pritchard the Astronomer who blames me for not being complimentary enough to the Almighty."42

Hooker's stance was lessaggressively polemical than Huxley's throughout the long and various phases of the evolution debate. This enabled Hooker to lead the way in assuming the highest positions in the establishment of British science. He preceded Huxley by one year in becoming a member of the Royal Society Council in 1853. Of course, this took place some years prior to the publication of the "Origin" and is perhaps attributable to Hooker's well-established position at Kew which granted him a higher status than the still struggling Huxley. Nevertheless Hooker was the first prominent Darwinist to be made President of the B.A.A.S. at Norwich in 1868. Huxley took up the same mantle two years later at Liverpool.[43] In 1872 Sharpey's retirement saw Huxley installed as Stokes' fellow Secretary of the Royal Society following the tied vote in the Council which has already been detailed. Following Airy's hasty withdrawal from the Presidency a year later, it was Hooker who was elected as the first Darwinist P.R.S. Eleven years later Huxley remained unacceptable to a section of the Fellowship when his supporters sought to install him for a full Presidential term. A detailed picture of the events which preceded his election in November 1883 will be given later. Hooker's rôle as trailblazer for his (p. 274) friend in the matter of acquiring formal honours continued into their retirement. They received the Copley Medal in successive years. On November 15th 1888 Huxley wrote to the previous year's medallist noting the peculiarities of the case:

> "You would have it that the R.S. broke the law in giving you the Copley and they certainly violated custom in giving it to me the year following. Who ever heard of two biologists getting it one after another? It is very pleasant to have our niches in the pantheon close together."44

Politically Hooker's position was very similar to Huxley's. The late 1880's brought with them the furore over the Home Rule Bill drawn up by Gladstone's Liberal Ministry. In common with a great many others on the right wing of that party, Huxley and Hooker found themselves firmly opposed to Gladstone's audacious remedy for the Irish problem. Because this put them at odds with the younger activists from the biological side of the Royal Society, the two veteran campaigners acted together to deter Tyndall from issuing his inflammatory manifesto opposing Irish Home Rule in January 1888. Confirming his own and Hooker's opposition to Tyndall's robustly phrased document, Huxley wrote:

> "Hooker's and my chief difficulty is that any manifesto prepared by a man of science is pretty certain to be followed by a counterblast from a certain number of them (among the junior more especially) and on all questions of principles our respected colleagues are, for the most part, so sluggish that I doubt if many, even of those who think with us, would make a public profession of faith, and a fiasco would be worse than nothing."45

Spottiswoode's wealth and social position were used by Huxley and Hooker to secure his Presidency in 1878.[46] This expedient ran counter to their strongly held views regarding access to the Society's chair for "poor men". Apart from Spottiswoode, Hooker and Huxley were the only X club members to achieve real power within the Society's offices. The issue of Tyndall's manifesto opposing Home Rule illustrates well the casually consultative nature of the X club when delicate matters were afoot. A week prior to Huxley's damper on the Tyndall manifesto quoted above, he had sent the impetuous Irishman a note stating that he and Hooker were:

> "full of doubts and difficulties but they can be set forth best at the X to which I hope you are coming on Thursday next."[47]

The dealings between Huxley and Hooker point to the conclusion that the two men had, quite early in their Royal Society careers, come to an important conclusion. This was that they alone (of the X members) possessed the correct combination of authentic scientific renown and political skill required for effectively controlling events. The way in which this control was handled, and the estimates which Huxley and Hooker made of their fellow X members' capacities will be examined in the next section. If Hooker's portrayal of the oligarchic nature of the Society's government is taken to be accurate, then any thoroughgoing X club "cabinet of a liberal party in science" as suggested by Barton would need to have contained more persons who actually held positions of power. The very cohesive power-holding group which she proposes would probably have made some attempt to provide a "succession" for its ageing members, yet this is not suggested in most standard accounts of the period.[48] The men deliberately groomed to become the future leaders of scientific naturalism were protegees of Hooker and Huxley. Due to accident and illness it was Hooker's who tried (and largely failed) to assume the place of their predecessors.

The Fate of the Young Huxleyites

The most notable career which was foreshortened by death within the scientific naturalists' camp, was that of William Kingdon Clifford. Born in 1845 at Exeter, Clifford was neither biological nor Huxley's protegee, but in the 1860's and 70's received general X club encouragement. Clifford seemed to be on the way to doing great things in the Liberal-Darwinian cause until illness intervened. Clifford's scientific authority was founded on his success in the mathematics competitions at Cambridge in 1867. He was second wrangler and second Smith's Prizeman and was granted a Fellowship in Trinity College. Clifford

then took what was a very rare step for a nineteenth century Cambridge mathematician and renounced all his previously held views on politics and religion.[49] The young scientific naturalists of Huxley's London circle naturally found much to admire in this rare sort of ally who possessed such a clear view of the (to them) arcane heights of mathematics. The claim to such potent credentials had hitherto been limited for the most part to Cambridge scientific men with pronounced leanings towards Christian orthodoxy and political reaction. Clifford is reputed to have taken the unprecedented step of being more sceptical than Huxley himself.[50] Thomas Hirst described in his journal the sort of reception which Clifford received when he moved out into London scientific society during the year following his Cambridge successes. The occasion was a dinner of the Society of "B's" which brought together informally the more clubbable members of Section B of the British Association:

> "Spottiswoode was there for an hour and brought Clifford with him. Clifford is the Lion of this season. Everybody is anxious to entertain him. I hope only his head will remain unturned."[51]

Clifford was not averse to advertising his agnosticism in light-hearted ways. Oliver Lodge reported an example of this in a brief account of the Dublin meeting of the B.A.A.S. in 1878:

> "On the Sunday Professor Jellett had been holding forth in church. Clifford from across the street shouted: "Hello Lodge, have you been to Section Hell?"[52]

In 1871 Clifford had left Cambridge to become Professor of Applied Mathematics at University College London. Elected to the Royal Society in 1874, he started to show signs of pulmonary disease in 1876. Clifford died on the island of Madeira three years later. Writing in the year of his own death, Huxley described the loss of Clifford and Francis Balfour as "the greatest loss to science."[53]

Francis Maitland Balfour has been identified as Huxley's scientific heir by William Irvine.[54] The younger brother of the future conservative Prime Minister A. J. Balfour, he was assisted in the transition between Eton and Cambridge by Michael Foster. There it was found that he had a great natural facility for biological work.[55] In establishing a reputation for his work in embroyology, Balfour achieved sufficient distinction in the Natural Science Tripos to earn him a Fellowship in Trinity College. Elected to the Fellowship of the Royal Society in 1878, he was awarded its Royal Medal three years later. In 1882 he was appointed Professor of Animal Morphology at Cambridge. He fell to his death from a precipice on one of the peaks of Mt. Blanc in the summer of the same year.[56] Michael Foster raised the suggestion that Balfour's work was to have formed the natural continuation of Huxley's own. Balfour's place in the future leadership of the wider movement of scientific naturalism had he survived remains a matter for speculation.

The decade from the mid-1870's to the mid-1880's was one of heavy mortality for the small group of prominent scientific naturalists. Irreplacable losses occurred among the pioneers themselves by the deaths of Lyell in 1875 and Darwin himself in 1882. The "liberal party in science" could still less afford to lose its most promising young men whose careers had yet to come to fruition. Following the demise of Clifford in 1879, that of Darwin and of Balfour in 1882, came the shockingly sudden death of Spottiswoode from typhoid fever in the next year. The handful of talented younger men was thus depleted just at the time when Huxley and his close allies from the early days were falling prey to physical frailty and contemplating retirement. However, a further blow had yet to fall. Henry Nottidge Moseley studied under the first Linacre Professor at Oxford, George Rolleston. Having obtained a first class degree in 1868 he went to study in Vienna with

his close friend E. Ray Lankester. By some now obscure means, Moseley managed to get himself included in the party travelling with Norman Lockyer to observe the solar eclipse of 1871.[57] At roughly the same time, Rolleston communicated Moseley's paper on worm dissection to the Royal Society. These various types of publicity succeeded in ensuring a place for Moseley on the "Challenger". One of the expedition's main concerns was the study of the ocean floor. The Government had become interested in this subject following the first successful trans-Atlantic telegraph cable six years earlier. When the expedition returned Moseley's outstanding performance was noticed by Huxley and Hooker, among others, and Moseley obtained a Fellowship in his old Oxford College in 1876. Three years later he was made F.R.S. Rolleston himself had no gift for research, but following his death in 1881 the support of Darwin and Huxley enabled Moseley to achieve election to the Oxford chair.[58] As a leading member of the biological "younger brethren" referred to by Michael Foster in connection with the "Parkerian Crisis", Moseley and Ray Lankester were named as the foremost of the younger biological Fellows.[59] Thomas Hirst disapproved of Hooker's prôtegée William Thistleton-Dyer and found himself in disagreement with Foster and Moseley as well. Hirst thought that the provision of personal grants to cover subsistence would have a demoralising effect on the young applicants. The three younger men were prepared to allow for this possibility. The discussion took place after the three had returned from a meeting of the Government Grant Committee at Burlington House.[60] Later in 1887 Moseley was afflicted with a neuro-muscular disease which incapacitated him. An alarmed Foster told Huxley in mid-November that the mania afflicting Moseley necessitated three nurses.[61] In 1891 he died leaving yet another large gap in the front rank of the Huxley-Hooker succession.

The Hookerites

Sir Richard Strachey

During the Dundee meeting of the B.A.A.S. in 1867 a group of people was staying in the town at the Royal Hotel. The persons concerned were noted by Hirst in his journal:

> "Mrs. Strachey, Spottiswoode, Tyndall, Lubbock, Busk, Hamilton, Fergusson all arrived and constituted our party at the Royal Hotel. A very pleasant one it was."62

From that date onwards Hirst makes frequent references to the inclusion of the Stracheys in dinner arrangements involving X club members. Prior to the date of Richard Strachey's final return from India his wife was often inclined to attend such functions alone. Shortly after the B.A.A.S. meeting described above, Hirst found Mrs. Strachey at Hooker's house one evening when he also had been invited. These social meetings continued throughout the 1870's. Joseph Hooker and his wife made arrangements to lodge in the same house as Strachey, Thistleton-Dyer, and Lawson for the duration of the Bradford B.A.A.S. in 1873.

Richard Strachey hailed from a line of distinguished servants of the British Raj. Following a colourful military career as an officer in the Bombay Engineers during which he had his horse shot under him in the First Sikh War, Strachey became a renowned constructor of irrigation works and railways.[63] Whilst surveying in the Himalayas, Strachey was given responsibility for a botanical collection which was made by one J. E. Winterbottom and Joseph Hooker. Winterbottom died before he could write up the results, leaving Strachey to complete the work which appeared in the Journal of the Royal Geographical Society fifty years later in 1900. Strachey was elected to the Fellowship of the Royal Society in 1854 for his work in collecting data for the study of natural history.

On the 28th June 1877 Hooker set out for the U.S.A. where he planned to lead an extensive botanising operation. Strachey and his wife were members of the party.[64] It is clear that Strachey was a thoroughly committed member of scientific naturalism's high command. He took the initiative over several issues and was consulted as a matter of course by Huxley and Hooker in situations where other X club members were not. Strachey's explicit endorsement of the materialist position was reported on by Michael Foster. In 1888 he went to listen to the lectures on geography which Strachey delivered at Cambridge.

> "it was really very charming, the most complete evolutionary and 'materialistic' views uttered by what seemed to be the very mildest old gentleman - it was delightful - but as I told him, if he had attempted to do that 20 years ago the priests of Baal would have risen up and stoned him."65

Hooker attended one of the lectures and pronounced: "its matter excellent but very dry".[66] Strachey reached the rank of Lieutenant-General in the Indian army and was the grandson of the first Baronet. Lady Strachey had religious views comparable with those of her husband. She was fiercely agnostic and did not worship when in residence at the family's London house at 69, Lancaster Gate. In the country, however, things were different and Lady Strachey (who had had all her children christened) submitted to the convention governing upper-class Sunday behaviour and went to Church.[67]

Strachey was active in the attempts which were made to reform the Philosophical Club of the Royal Society at the start of the final decade of the century. In 1888 Hooker was one of the last survivors of the original forty-seven members of the club. Another pioneer, William Grove wished to see it disbanded if attendances did not improve, while Hooker and Huxley wanted to see the end of the club because it had accepted as members men whose eminence was based on commercial success. Huxley dealt with the subject in a letter to his great ally in March 1889:

> "If the Phil. Club had been kept pure it might have acted as a check on the intrusion of the mere trading element - but there seems to be no reason now against Jack and Tom and Harry getting in and the thing has become an imposition. So I go with you for extinction before we begin to struggle in the mud. I wish I could take more part in what is going on - I am anxious about the Society altogether . . . ."[68]

At a Philosophical Club meeting during the following November, Strachey and Thistleton-Dyer made attempts to open the club to guests from among Fellows who were not members, and non-Fellows respectively. Dyer's motion was rendered ineffective by the restriction of such visits to one per session and Strachey's motion was lost.[69] Three years later Strachey tried again to reform the Philosophical Club, endeavouring to institute the Royal Society Club's rules dealing with guests in the interim. It seems that all efforts were in vain. Dyer resigned from the club following the failure of Strachey's final effort. Seven years later the two Royal Society dining clubs were merged.[70]

Strachey was a member of Council in the Royal Society for four terms between 1872-1891. In 1889 he represented the Royal Society at the International Congress for the determination of the Prime Meridian where he acted as one of the secretaries. During the crisis brought about by Huxley's attempt to oust Stokes from the Presidential chair of the Royal Society during late 1887, Strachey was consulted by Hooker and his response passed on to Huxley by letter:

> "Strachey agrees and 'will take any part that may be decided upon'."[71]

Three years later, with Stokes' term as President completed in spite of Huxley's best efforts to displace him, Hooker looked to Strachey as the leading contender from among the scientific naturalists for the Presidency of the Royal Society:

> "I have been much exercised about the P.R.S. Evans it is said has been touting for it. Rayleigh and Foster would not take it, and William Thomson has

> been too often asked to try him again, as it was thought - so I put up Strachey as a typical Indian Scientific man; and had not Thomson relented in time (most happily) I hope Strachey might have been carried. Lubbock was, I fancy the only alternative to keep Evans out."72

Hooker's suggestion of Strachey as the tenth X club member was due for discussion at the club's 90th meeting on the 5th November 1874. However, in the absence of its author (who was at that time in his first year as President of the Royal Society) the matter was not raised.[73] At the beginning of March 1888 Hooker, Frankland, and Lubbock recorded the unanimous opinion that the X club should be augmented by up to four members. The names suggested were Michael Foster, Richard Strachey, John Evans, and Francis Galton. As has been shown earlier in the previous chapter, Huxley halted all movements towards X club recruitment.[74] His view was held consistently and applied to Foster and Strachey who were both close, long-standing friends and among Huxley's most influential allies within the British scientific establishment. Strachey served on the Royal Society's Statutes Review Committee with Frankland and Ray Lankester. The Committee reported late in 1890. The warmth of Huxley's feelings towards Strachey can be judged from the response he made to Hooker's request for support in obtaining a Royal medal for their old confederate in 1894:

> "I am not competent to judge of his work, you are and I do not see why you should not suggest it - I would give him a medal for being R. Strachey - but probably the Council would make difficulties."75

Early in August 1887 Huxley and Hooker began to prepare the ground for their attack on Stokes for (as they saw it) improperly combining the rôles of P.R.S. and M.P. Hooker specified Strachey as one of the crucial men to consult about the situation:

> "Shall I ask Hirst or Foster or Strachey or any of the 'wise heads' to meet us?"76

Despite his important position within the Huxley-centred network of influence within late Victorian science, Strachey was definitely not cast in the mould of the conspirator. When he returned from India in 1873 Strachey had been appointed to the Royal Society's Meteorological Committee. The Cambridge wrangler, W. N. Shaw, became one of Sir Richard's underlings in later years when he had been appointed to a senior position in the Meteorological Office. In writing Strachey's obituary Shaw testified to his former chief's rectitude in reporting that:

> "He would not even let us indulge in the semi-official pastime of abusing the Treasury."77

William Thistleton-Dyer

Thistleton-Dyer's education was an unusual amalgam of mathematics, medicine, and natural history. In 1870 he graduated from the University of London having already served for two years as Professor of Natural History at the Royal Agricultural College at Cirencester. Dyer spent the next two years as Professor of Botany at the Royal College of Science in Dublin.[78] During that time he began to correspond with Hooker whom he met in 1872. This connection was to be the basis of Dyer's successful career. Initially it produced for him the Professorship of Botany at the Royal Horticultural Society which took Dyer to Chiswick and to South Kensington. There the young botanist almost inevitably became subject to Huxley's influence. During the summer of 1873 Huxley was in poor health which he hoped to improve by means of a leisurely touring holiday in France with Hooker.[79] Dyer took over the summer course at South Kensington for that year and in the following year was appointed demonstrator for Huxley on the same course, along with Sidney H. Vines. Vines was later to have a significant effect on British botany and to become Oxford's Professor of that subject. Hooker became concerned about Dyer's lack of dedication at this time and expressed the feeling to Huxley:

> "I do wish that Dyer could buckle to scientific work, he will never get on to F.R.S. at this rate - he dissipates time and talents."80

Nevertheless, Hooker appointed Dyer to be assistant director at Kew a year later in 1875. When the Jodrell laboratory was completed in the next year, Dyer took charge of it. It seems to have been especially difficult for men of promise in the study of natural history to achieve the scientific heights projected for them in the epoch following Darwin's great announcement. Dyer stands as a clear example of this trend. Hooker described him as a leading figure of "the new school of physiologists" in a letter to Darwin in 1878. Dyer made a substantial contribution to the imperial schemes of economic botany run from Kew. In addition he superintended the rising stars of the new school of British botany in the persons of Walter Gardiner, Dukinfield H. Scott, Bower, and Marshall Ward. However, Dyer did not fulfil the high hopes which Hooker had cherished for him. He became neither able nor willing to succeed to the positions of dominant influence which Huxley and Hooker had occupied. In 1877 Dyer married Hooker's eldest daughter and followed his father-in-law into the directorship of Kew eight years later.[81] Admitted to the Fellowship of the Royal Society in 1880 at the age of 37, Dyer served on the Council twice. In common with Huxley, Hooker, and Frankland whose contributions to original research were severely curtailed once they achieved scientific eminence, Dyer's energies were mostly absorbed by a heavy administrative load.

It seems that Dyer's first informal contact with Huxley concerning the conduct of biological science in Britain occurred in 1880. In November of that year Dyer wrote to his future mentor concerning the extent to which he and Ray Lankester thought that George Rolleston (Professor of Physiology) was retarding the progress of biological science at Oxford:

> "as titular Professor of Physiology he knows practically nothing of the subject and Foster and Pye-Smith will tell you he has done everything in his power to discourage it as an honours subject. . . . I have been spending a few days in Oxford and I confess that as far as science is concerned it seems to me a mere city of the plain."82

Dyer's letter describes how Lankester had opposed Rolleston using too little tact and that Moseley had better go somewhere where his studies would be safe from Rolleston's "religious proclivities". Thenceforward Dyer frequently briefed Huxley on the issues of current concern to the rising "biological set". His first request for Huxley's intercession came in connection with the further unfolding of the Oxford struggle between the reactionary Rolleston and the aggressive Huxleyite Ray Lankester in 1882, following the latter's resignation.[83] Huxley received Dyer's hearty encouragement to stand for a full term as President of the Royal Society in 1883. As will be recalled, Spottiswoode's untimely death had pitched Huxley into the chair on a provisional basis in the June of that year.

The part which Dyer took in Huxley's 1887 campaign to terminate Stokes' Presidency following the latter's entry into the House of Commons is of some significance. Dyer wrote to Nature endorsing the views set out by Huxley in the same journal two weeks earlier. Both men shared a strongly held objection to the blithe unconcern that they saw as Stokes' attitude. The grave peril in which they felt the President's dual rôle placed the public image of the Society seemingly did not move the bulk of the Fellowship to action. Nevertheless, Dyer was nervous about the outcome of his foray into the public arena:

> "My heart was in my mouth when I sent if off. I should not have entered the fray but for loyalty to you. But Lockyer and Roscoe were good enough to say that they thought well of it. And I really think it has answered its purpose in stiffening up the younger men."84

It is evident that Dyer was intended to replace the ailing and retired Huxley as the champion of scientific naturalism in public controversy. In the year following his first timorous shot in the Stokes affair, Dyer launched an attack on George Romanes. Hooker and Huxley spent a good deal of time being angry about Romanes. They felt that he did not comprehend Darwin's doctrine yet at the same time tried to acquire for himself some of the credit due to it.[85] At the beginning of 1890 Dyer was persuaded by Lankester and J. F. Donnelly (both active Huxleyites as will be related below) to reply to "the Duke of Argyll's imbecile letter in Nature".[86] At the time Argyll was engaged in an elaborate attempt to discredit Darwin by demonstrating his fallibility in the matter of coral reef formation.

As it happened Dyer did not prove able to take up where Hooker and Huxley had left off. In the first place he did not possess Huxley's well-known facility with words or his robustly effective style in the conduct of public scientific controversy. At the time of Argyll's letter to Nature Dyer wrote to Huxley clearly demonstrating the disarray of the new leading lobby of scientific naturalists. It is significant that Dyer was reliant on the tactical advice of Lankester who was well-known for his indiscrete professional behaviour:

> "[Lankester] wants to lie by a while which I think is advisable. The difficulty, however, which I feel as much as he would is how to meet such infernal rubbish with anything like moderation. I hate controversy . . . . How I longed for the temporary use of your magical hand!"87

Kew's new Director (he succeeded Hooker in 1885) was far from being the committed radical that he might be supposed to have been. He gave firm support to Walter Gardiner's new theory of plant life which placed the basis of vegetable life in the action of protoplasm rather than in osmotic action. The predominantly German stress on osmosis was set aside by the approach of the younger British workers. In writing to

Huxley on this subject in 1887 it seems that Dyer was providing him with new information on this developing trend:

> "The young men are building up amongst them a complete and truly biological theory of plant life which will I hope completely smash up the stupid physical theories of the Germans which have hitherto led us into bondage."88

However, there was evidently more than a hint of irascibility in Dyer's radicalism. It has been suggested that despite his supervision of the new physiological school in Britain he hankered after the old method of systematic botany. After his voluntary retirement at 62 Dyer extended his indulgence in the ancient botany of Pliny.[89] Dyer was autocratic in his running of Kew. He failed to endear himself to other members of Huxley's circle as is shown in the following encounter between Hirst and Dyer at the Philosophical Club in 1891. Even though Hirst was in poor health and was destined not to survive long into the new year, it is remarkable that he was unaware of the Huxleyite motives which allied Strachey and Dyer against the "chemical trader" Sir Frederic Abel:

> "Dyer was once more 'serious and sententious'. He <u>appeared</u> to make an attack on Abel for his conduct of the Colonial [sic] Institute. Abel replied well and judiciously; but Strachey, to my great surprise, said of his speech sarcastically, that his powder 'was <u>not</u> smokeless'. I did not know what the whole affair meant. M. Foster, Geikie, Rücker, and even Maskelyne, were 'noisy'. Sir W. Grove was present but was very quiet and looked ill. Dewar supported Abel, by whispered comments, he was not audible."90

After the sudden death of Edward Frankland early in August of 1899, the question of his replacement as Foreign Secretary of the Royal Society naturally arose. Dyer declined to serve, giving as his reasons pressure of work and his lack of scientific stature in comparison with the two previous incumbents Frankland and Williamson.[91]

<u>Huxley's Most Durable Disciple: E. Ray Lankester</u>

An examination of the Royal Society careers of Dyer and Lankester reflects their failure to realise their early promise as heirs to the

leadership of Huxley's network of influence. It should be borne in mind that the whole context of Victorian science had changed since Huxley fought his way to prominence. Darwin's theory had become fully established within British biological science so that youthful newcomers to the field had perforce to make their mark by modifying some part of Darwinist doctrine. During the third quarter of the nineteenth century critics of natural selection, such as St. George Mivart and Fleeming Jenkin, were widely heard. The general complacence which commonly saps the vitality of a long-established theory formed the backdrop for these developments. No Huxleyite was able to acquire anything approaching the dominant stature of "the General", as Huxley was known by intimates. Regardless of any personal shortcomings of Huxley's likely successors, the unity born of adversity which facilitated the emergence of his own pre-eminent position was no longer forthcoming. It was the heady quality of the early engagements with the clerics that made so special the place which Huxley occupied. These conditions were no longer in force during the last two decades of the century. Hence the feelings of anger and frustration experienced by members of the old guard when they saw Darwin's theory left undefended in the hands of its enemies and "being messed about with" by its presumed friends.[92]

E. Ray Lankester's struggles against Rolleston at Oxford have already been touched on in connection with Dyer. Rolleston refused to have the crucial modern work of the German physiologist Gegenbauer translated by the Oxford University Press. The autocratic powers held by Oxford professors enabled him to stifle all new developments within the subject under his control.[93] Lankester was an admirer of Huxley of many years standing when he received his first major gift of patronage from his master. Despite donnish disapproval of the 1860 B.A.A.S. meeting, Huxley was called upon by Exeter College Oxford to advise on

the filling of a Fellowship in science in 1872. Huxley chose Lankester. The new Fellow was not content in his new cloistered setting and soon wrote to his patient sponsor asking him to look out for an alternative position. During the summers of 1873 and 1874 Lankester assisted Huxley with running the summer course for schoolmasters at South Kensington. Later in 1874 Lankester resigned from Oxford to become Professor of Zoology and Comparative Anatomy at University College London. Despite his inability to become an effective manager and lobbyist within the highest echelon of Victorian science, Lankester retained Huxley's support for his academic career. In 1891 Huxley gave his support successfully to the move to have Lankester installed as the new Linacre Professor of Physiology at Oxford. This was achieved against the wishes of the Archbishop of Canterbury who wished to see his cousin, a man called Hatchell, settled in the same post.[94] Over the Questions of Lankester's departure for Edinburgh and Moseley's inclination to take the empty place thereby left at Oxford, Dyer concerned himself greatly and requested Huxley's intervention. These events have been described earlier. Two years later ill-health was gradually forcing Huxley to the point of resignation from the chair of the Royal Society and his Professorship at South Kensington. It is obvious from the words of his superior, J. F. Donnelly, that Huxley was likely to try to promote Lankester as his successor. Donnelly was another leading Huxleyite whose connections with scientific naturalism will be described in the next section. In his position of Director of the Department of Science and Art, Donelly did a great deal to assist Huxley, but was emphatically against the installation of Lankester in his place. Donnelly's letter to Huxley closed the subject with the words: "so don't you try it on".[95]

Lankester became involved with Moseley through their Oxford connections. As has been noted, they travelled to Vienna together in 1868. Both men's careers kept them in the forefront of their department of science with the result that by 1886 Michael Foster regarded them as the leaders of the "young brethren" i.e. the most promising and active young biologists in the Royal Society. Moseley's rise was terminated by the onset of his illness in the following year. Lankester's prospects as a leading Huxleyite were damaged in the same year but in his case the blow was self-administered. Lankester allowed his lack of discretion to lead him into direct opposition to Huxley over the running of the Marine Biological Association. As President of the Association and an official Fisheries Inspector in 1886, Huxley could not support the younger members' eagerness to tie the Association to the Government Fisheries Board. The Marine Biological Association's Honorary Secretary was Ray Lankester. The proposal which angered Huxley concerned the younger members' wish to appoint a scientific adviser to the Government Fisheries Board. Huxley was sensitive on matters concerning the interplay of science and government. This was further shown in the following year when Stokes as Huxley's replacement as P.R.S., entered Parliament for Cambridge University.[96] Regarding the Government Fisheries Board, Huxley felt that a scientific adviser appointed by the Marine Biological Association would occupy an untenable position. As a representative of science the appointee would not be accountable to the scientific community at large. Huxley went further and admonished the young, thrusting element within the Association for their ignorance of the official procedures of fisheries administration. The opposition of the "young bretheren" who three years earlier had supported Huxley for the Presidency of the Royal Society was relayed to him by Foster. The ailing Huxley was unable to come amicably to terms

with the wane of his influence. Although there was widespread opposition to his summary dismissal of the proposed government adviser as a "scientific Frankenstein" Huxley fastened his resentment on the person of Lankester. By mid-summer of the following year, Huxley had signalled his wish to resign as President of the Marine Biological Association. It seems that he focussed the blame for this state of affairs on Lankester without investigating the extent of support for the younger man:

> "I am sick and tired of working with a man when I cannot trust him . . . and I wish you would consider the question of a President in my place."97

Huxley had been co-opted by Foster to act as Lankester's proposer for election to the Athenaeum in 1886. He was urged to do so on the basis of scientific eminence. A year earlier, Foster had written to Huxley including a sharp sketch of Lankester's behaviour towards colleagues.

> "There is a mixture in him of the most barefaced conscienceless selfishness with a certain good-natured and vigorous power of work, and meek acceptance of rebuke which completely fascinate me and lead me to do things for him that my judgement does not approve of."98

The dedicated wish to promote the appearance of unity through the solemn display of a public singleness of purpose delayed Huxley's departure from the Association for several more years. Early in April 1888 Foster wrote to Huxley to tell him that Lankester was to be replaced by a new director who would also act as secretary to the council. Foster went on:

> "As to the MBA it would be a good thing if you could make up your mind to allow your name to remain as President until '<u>after the building is opened</u>'."99

The fact of Huxley's receiving the support of Evans and Foster over this issue should not disguise the fact that the influence of Huxley and his immediate circle over the new generation of scientific men was

diminishing. At the same time Huxley's sway in official circles was still great. Later in the 1890's Evans and Foster were themselves both severely impugned by the leaders of the Royal Society's active men of science. The facts surrounding these developments will be presented in a later section. At all events, it seems clear that Ray Lankester (p.343) thoroughly disqualified himself from any realistic chance of taking over from Huxley the responsibility of becoming the public champion of scientific naturalism. At the time when trouble first broke out in the Marine Biological Association, Huxley informed Evans (Treasurer of the Royal Society 1878-1898) of Lankester's inability to present a statesmanlike public image of science over the matter. Huxley stated that Lankester was "flooding the papers with letters under pseudonyms which criticised Mundella - Huxley's man official contact in fishery matters. Huxley advised Mundella to proceed normally in his department.[100] Lankester was quite belligerent enough to take on Huxley's mantle, but lacked the skill and cunning to triumph over his adversaries. As in the case of Huxley himself in the matter of the Marine Biological Association, Lankester's adversaries were often desperately ill-chosen.

Ten years earlier, Lankester had been largely responsible for the sensational exposure and trial of the slate-writing medium Henry Slade. Lankester's divisive critique of the scientific support of spiritualism personified by A. R. Wallace was printed in The Times on the 16th of September 1876.[101] Lankester demonstrated many years later that his lack of discretion had not been modified by experience. During the Great War he took a leading part in trying to have Arthur Schüster removed from the Secretaryship of the Royal Society because of his German ancestry.[102]

J. F. Donnelly

J. F. Donnelly's initial contact with the scientific world came in 1850. As a Captain in the Royal Engineers he was put in command of a company of sappers whose task it was to prepare the South Kensington site for the Victoria and Albert Museum. Donnelly served in the Crimea but on his return took advantage of his relationship with Sir Henry Cole at South Kensington to gain an appointment as an Inspector with the Department of Science and Art in 1859.[103] Donnelly served as Director of the Department between 1874 and 1899. He was therefore chief to all those involved with the Royal College of Science at South Kensington including Huxley, Frankland and Lockyer. Huxley and Donnelly provided each other support throughout the period of their official connection. Their relationship developed early into close friendship with regular collaboration on important issues. The unity of outlook which developed was probably due in part to Donnelly's wholehearted endorsement of Huxley's views on religion. In a letter of 1889 to Huxley he roundly declared that:

> "After all, religion is at the bottom of all troubles and cussedness . . . ."[104]

An example of the close and active co-operation between the two men is provided by an incident which might otherwise be taken as consistent with the conventional "X club thesis". In July 1872 the struggle between Joseph Hooker and Acton Ayrton of the Office of Works was at its height. The memorial in support of Hooker which was produced and signed by four X club members (and signed by seven other men) was referred to Donnelly by Huxley for his opinion prior to its submission to Parliament.[105] Huxley needed prolonged leave of absence from South Kensington in advance of his retirement from both his professorship and the Presidency of the Royal Society in 1885, due to ill health. In a letter of 21st November 1884 to Huxley's wife, Foster explained that he

was intending to invite Donnelly to the Society's Anniversary Dinner in order to arrange an official insistence on the extension of Huxley's leave.[106] Foster and Donnelly collaborated again early in 1885 when the question of Huxley's absences through ill-health seemed to threaten dire consequences for his pension rights. In the event Huxley received a generous pension of £1200 per year to which was added a Civil List pension of £300 annually. Writing to Donnelly, Huxley reflected that the Liberals would see his Civil List pension as a pay-off from the Tories for his tenacious attacks on Gladstone.[107] These attacks were continued by Huxley on subjects ranging from the physical nature of the Gadarene swine to the Home Rule Bills.

As has already been noted, Donnelly was an active member of the group of Huxleyites who sought to take over from their ailing leader some of the responsibility for defending Darwinism from the continued attacks. The case which was recorded in an earlier section concerned the joint effort of Donnelly and Lankester to pursuade Dyer to respond critically to the Duke of Argyll's letter in Nature. In connection with the broadening of the scope of the Royal School of Mines, its removal to South Kensington, and the modelling of the Royal College of Science on Huxley's plan, Donnelly played the part of tireless ally to his friend. Their co-operation in these endeavours has been described in detail by Cyril Bibby.[108]

It was not always possible for Donnelly and Huxley to focus their co-operative efforts on the positive side of the career of English scientific education. In February 1883, Donnelly came across rumours (not for the first time) that private work was being carried out for payment in the South Kensington Chemistry Department under the charge of Edward Frankland. The case was a particularly delicate one because of the long term relationships between all three men. Action had to be

taken because the recently reorganised and renamed Normal School of Science and Royal School of Mines was suffering from shortages of both money and students. Its progress was therefore being monitored closely by its paymasters and detractors. At first Donnelly wrote to Huxley in general terms not indicating any particular branch of the school, because of his wish to avoid unpleasantness with an old associate like Edward Frankland. This showed considerable loyalty because Donnelly already had in his possession: "two letters from Dr. Percy Frankland, written on official paper, stating the terms on which analyses would be made for publication, and not for publication respectively."[109] Donnelly himself verified unofficially the case of work done for the Indian Railway Department which was named in a later renewal of the accusations against Edward Frankland's department. Frankland's lengthy self-exoneration covered all the five charges against him. They ranged from the suggestion that he neglected his official duties in order to run a private laboratory to the absence of any evidence of Frankland's own original investigations.[110] Despite Huxley and Donnelly's evident relief at the strong position taken up by Frankland in response to these charges, the affair left its mark. When Frankland retired in 1885 he experienced stout opposition to his wish for a benevolent interpretation of his pension rights. His experience stood in stark contrast to the generous treatment which Huxley received at the same time. For Huxley, commercial connections of any sort gave him grounds for grave suspicions. Frankland's commercial involvements had proliferated since the later 1860's and this had set the two men apart to a great extent.

Donnelly was the major intermediary between Huxley and the Treasury in virtually all matters connecting him with the running and expansion of scientific education. Donnelly adopted a posture of combative but intelligent opportunism in these dealings which resembles closely

the approach of Huxley. This should give little cause for wonder bearing in mind that Huxley himself was offered the post of Science Director at the Department of Science and Art in 1874, but preferred to support Donnelly's candidature.[111] There is no doubt that Huxley's ambitions for the advancement of British science were among Donnelly's prime concerns. This naturally brought down upon his own head the suspicions and disfavour of Government departments. In a letter to Huxley in February 1885, Donnelly described the happenings during a committee meeting concerned with the Science Museum. The committee members were Ralph Lingen, Mitford, Frederick Bramwell who was Chairman, and Donnelly as Secretary. Lingen and Mitford represented the Treasury:

> "The simple ignorant cussedness of the first two cannot be imagined - much less described. . . . [Lingen had been reasonable and straightforward at first.] Then we came to meetings and putting things on paper and he not only went right round but showed a capacity for ignorant obstinateness which surprised even me. . . . Lingen must deeply regret that the Treasury, yes the Treasury selected him to be Chairman to sit on <u>us</u>."

Donnelly later remarked that he kept his dealings with the Chancellor of the Exchequer and the First Lord of the Treasury "just within the bounds of official decency".[112]

Huxley had many battles with officialdom and consistently held strong views which led him to disapprove of the acceptance of official decorations by men of science. In the late summer of 1892, Michael Foster joined with Donnelly in recommending Huxley to accept the appointment of Privy Councillor. Although not disapproving of politically allocated honours for heads of offices such as Hooker and William Flower, Huxley felt that only the membership of the Privy Council was "fit recognition for an independent man of science or letters".[113] Many years earlier Huxley had made this known to Donnelly who remembered it and in due course petitioned Lord Salisbury for Huxley's appointment.

The frail Huxley was amazed that Salisbury, as leader of a Conservative Ministry had acceded to Donnelly's request. On June 21st 1892 Huxley sent his thanks to the man he referred to as his "bosom friend".

> "My dear Donnelly,
>
> You have been and done me at last. . . . I have always been dead against orders of merit and the like. . . . As for yourself it is only one more kindness on the top of a heap so big I shall say nothing about it."114

Ruth Barton's contention that the accession of Huxley to the Privy Council marked an advanced stage of the penetration of the political establishment by science is clearly questionable. There is little to suggest any specific ways in which science was becoming "central to English culture".[115] Huxley's entry into the Privy Council was not politically meaningful, by his own admission. The wane of scientific influence in Whitehall and the rapid decline in the importance of scientific naturalism at the end of the nineteenth century is described in detail in a later section on Huxleyite influence in the government of the R.S. from 1885-1900.

* * *

Notes

1. Ruth Barton, "The X Club. Science, Religion, and Social Change in Victorian England", Ph.D. dissertation, University of Pennsylvania, 1976, pages 19 and 129.

2. Huxley Papers (ICL), 3.97. In January of the following year, Hooker told Huxley: "I pray 3 times a day with my back to London that I may not be called there" (ibid., 3.98).

3. Ibid., 3.316.

4. Barton, ~~ibid~~., Dorothy Stomson, Scientists and Amateurs, Greenwood Press, New York, 1968, pp. 199-230. Marie Hall, ASN, passim.

5. R. Peppiatt, "William Sharpey", St. Bart's Hospital Journal, September 1973, pp. 266-315.

6. Hirst Journal, 1941.

7. Peppiatt, ~~ibid~~., p. 292.

8. Ibid., p. 291.

9. Hirst Journal, loc. cit.

10. Marie Hall, ASN, pp. 84-87.

11. Peppiatt, ~~ibid~~., p. 313.

12. M. J. Bartholomew, "The Award of the Copley Medal to Charles Darwin", Notes and Records of the Royal Society of London, July 1975, 30, no. 1, pp. 209-217.

13. Huxley's actions in this matter have been described in an earlier section.

14. Sir Henry Dale, "Sir Michael Foster", Notes and Records of the Royal Society, 1964, 19, pp. 10-33.

15. Hirst Journal, 1940.

16. Ibid., 1943.

17. Huxley Papers (ICL), 4.173. Foster was one of the very few non-members to whom Huxley made explicit mention of the X club (Leonard Huxley, op. cit., vol. 1, p. 55).

18. Barton, ~~ibid~~., pp. 156-164.

19. Huxley Papers (ICL), 4.217.

20. Stokes Papers (CUL), S 589.

21. Huxley Papers (ICL), 4.62.

22. Ibid., 4.205.

23. The Seventh Duke of Devonshire was dismissed by the Huxleyites as a suitable candidate for the chair of the Royal Society. Bearing in mind that William Cavendish (the seventh Duke) had become second wrangler in the Mathematics Tripos and First Smiths prizeman 1829, the Huxleyite objections to this particular nobleman as a Presidential candidate must have been of a more ideological nature than their usual opposition to aristocratic dilettantés. Airy, who had been installed largely at the instigation of the Huxleyites in 1870, was himself in favour of the Duke of Devonshire as his successor in the chair of the R.S. The Huxleyites were themselves bent on installing Hooker, and easily succeeded in this. In January 1873 Airy wrote to Stokes asking if anyone knew how long the Duke's stay in London was during the season (Stokes Papers CUL, A 442). For William Cavendish see J G Crowther, Statesmen of Science, Cresset, London, 1965, pp. 215-220.

24. Huxley Papers (ICL), 4.218.

25. Ibid., 4.222.

26. Ibid., 4.236.

27. Ibid., 4.233.

28. Ibid., 4.279.

29. Ibid., 4.284.

30. Ibid., 4.288. Parker has already been described as the chief beneficiary of the Royal Society Government Grant. Foster suggested H. N. Moseley and Ray Lankester as the two committee representatives of the "younger brethren".

31. A. J. Meadows, Science and Controversy: a Biography of Sir Norman Lockyer, Macmillan, London, 1972, p. 226.

32. Huxley Papers (ICL), 4.325.

33. Ibid., p. 248.

34. William Irvine, Apes, Angels, and Victorians, Weidenfeld and Nicholson, London, 1955, pp. 354-358.

35. Huxley Papers (ICL), 3.416.

36. R. M. Macleod, "The Ayrton Incident: A Commentary on the Relations of Science and Government in England 1870-3", Arnold Thackray and E. Mendelsohn (Eds.), Science and Values, Patterns of Tradition and Change, New York, 1974, pp. 45-50.

37. Huxley Papers (ICL), 8.128.

38. Leonard Huxley, Life and Letters of Joseph Dalton Hooker, John Murray, London, 1918, 2 vols., vol. 2, p. 68.

39. Royal Society, PLB 1, no. 420.

40. Irvine, ~~ibid~~., pp. 3-9. A recent inclination to grant a larger rôle to Hooker cannot easily be applied to events beyond the actual Cambridge B.A.A.S. meeting of 1860 (J. R. Lucas, "Wilberforce and Huxley: a Legendary Encounter", Historical Journal, 1979, 22, pp. 313-330).

41. Ibid., p. 253.

42. Leonard Huxley, ~~ibid~~., vol. 2, p. 120.

43. B.A.A.S. Report, 1870.

44. Huxley Papers, 2.332.

45. Ibid., 8.264. This letter was from Huxley to Tyndall, 8th January 1888.

46. Barton, ~~ibid~~., pp. 145-150.

47. Huxley Papers (ICL), 8.262.

48. This is briefly touched on in Irvine, not at all in Jensen, Macleod, and Barton.

49. A. Geikie, Annals of the Philosophical Club, London, 1917, p. 71. Clifford began as an Anglo-Catholic. Karl Pearson was told by his college lecturer in mathematics, Percival Frost, that Clifford was the best pupil that he had ever taught as a coach. (Karl Pearson, "Old Tripos Days at Cambridge, as Seen from Another Viewpoint", The Mathematical Gazette, 1936, 20, 27-36, ff. 30.)

50. Barton, p. 81.

51. Hirst Journal, 1828. Clifford dined as a guest of the X club.

52. Oliver Lodge, Advancing Science, London, 1931, p. 46.

53. Leonard Huxley, Life and Letters of Huxley, vol. 2, p. 396.

54. Irvine, ~~ibid~~., p. 308.

55. Geikie, ~~ibid~~., p. 78. Huxley, ~~ibid~~.

56. Ibid.

57. J. L. Heilbron, H. G. J. Moseley: Life and Letters of an English Physicist, University of California Press, London, 1974, p. 7.

58. Ibid., p. 22.

59. Huxley Papers (ICL), 4.288. Parker was a low church Christian who had not been able to come easily to any stable compromise with the implications of natural selection. His enormously prolix Royal Society papers dealt only with morphological matters. The Darwinian "younger brethren" wished for Parker to be curtailed or suppressed.

60. Hirst Journal, 2412. Hirst was dining at the Athenaeum and spending most of his evenings there at this time, so it was quite natural that he should meet the three younger men there after they had repaired from Burlington House.

61. Huxley Papers (ICL), 4.294.

62. Ibid., 3.206. Hirst Journal, 1812. James Fergusson was a Scottish Art Historian who intended to be a member of the X club in 1864.

63. Proc. R.S., 1909, LXXXIV.

64. W. B. Turrill, Joseph Dalton Hooker, London, 1964, p. 166.

65. Huxley Papers (ICL), 4.321.

66. Ibid., 3.320.

67. Betty Askwith, Two Victorian Families, Chatto and Windus, 1971, p. 45.

68. Huxley Papers (ICL), 2.340.

69. Hirst Journal, 2644.

70. T. E. Allibone, The Royal Society and its Dining Clubs, Pergamon Press, Oxford-New York, 1976, pp. 266-268.

71. Huxley Papers (ICL), 3.308.

72. Ibid., 3.359.

73. O.U. microfilm of Frankland Papers (RFC).

74. The import of this for the credibility of the well-known "X club thesis" which stresses the club's central but covert rôle in late Victorian science has been discussed in detail earlier.

75. Huxley Papers (ICL), 2.454. Hooker and Huxley were clearly on very good terms with Richard Strachey and his family. On one occasion something of a stir was caused at the Athenaeum when Strachey left his very young daughter Pippa with Huxley in the club's rooms and the latter amused her by playing "cockatoos" (Askwith, ~~ibid~~., p. 95).

76. Huxley Papers (ICL), 3.301. This is clearly supportive of the view that the group most bound up with "running scientific affairs" in the interests of scientific naturalism was not the X club itself. Huxley did not want non-members who were part of his "inner cabinet" to be recruited into the club despite the willing compliance of the other members.

77. Proc. R.S., 1909, LXXXIV-XCII, ff. LXXXIX.

78. D.S.B. on Dyer.

79. Irvine, ~~ibid~~., pp. 277-279. Huxley had recently lost a good deal of money in legal costs incurred in a dispute with a troublesome

neighbour over drainage at the new Huxley family home in Marlborough Place. Friends feared that this might prompt another breakdown of Huxley's health. Charles Darwin helped to organise a collection of money among Huxley's friends which would enable him to settle his immediate money troubles and take a foreign holiday. A sum of £2,100 was collected in this way.

80. Huxley Papers (ICL), 3.227.

81. D.S.B., p. 341.

82. Huxley Papers (ICL), 27.182.

83. Ibid., 27.193.

84. Ibid., 27.214.

85. Ibid., 3.334.

86. Ibid., 27.222. Nature, XLI, 1890, Dec. 26th, p. 173. Loc.cit p. 294. Loc.cit. p. 366.

87. Ibid., 27.210.

88. Ibid.

89. D.S.B., p. 342.

90. Hirst Journal, 2832. Hirst was here referring to the Imperial Institute. Sir Frederic Augustus Abel, the inventor of smokeless gunpowder, found it easier to wield influence in the capital than in the provinces. In 1890, Ellen Busk informed Hirst that the recent B.A.A.S. meeting at Leeds had been sparsely attended because of "the President's want of popularity". The President was Abel (ibid., 2727).

91. Royal Society, MC 17, no. 287.

92. Huxley Papers (ICL), 27.232.

93. Ibid., 27.182.

94. Cyril Bibby, T. H. Huxley, Scientist, Humanist, Educator, Watts, London, 1959, pp. 183-185. Lankester did not achieve high office in the Royal Society. In 1898 he became Director of the Natural History Museum.

95. Huxley Papers (ICL), 14.39.

96. As has been detailed earlier, Huxley's concern with the interplay of science and government was founded on an ideological position which sought far more than moral probity in the conduct of the relationship.

97. Royal Society, MM XIII-IX, no. 2. Huxley wanted Evans to take his place as President. It should be remembered that Lankester was virtually the founder of the Marine Biological Association.

98. Huxley Papers (ICL), 4.254. Huxley did not despair of Lankester, and in 1891 played a crucial rôle in getting him elected to the Linacre Professorship of Physiology at Oxford.

99. Ibid., 4.325. The building, in Plymouth, was due to open in June of that year: 1888.

100. Royal Society, MM XIII-IX, no. 8.

101. Roy Wallis (ed), On the Margins of Science, p. 225.

102. Sir Arthur Schüster, Biographical Fragments, London, 1932. Lawrence Badash, "British and American views of the German menace in World War I", Notes and Records of the Royal Society of London, 1979, 34, 91-121, ff. p. 96.

103. Meadows, ibid., p. 113. Hirst Journal, 1811. W.H.G. Armytage, The Rise of the Technocrats, p. 102.

104. Huxley Papers (ICL), 14.104. Donnelly became Director of the Department of Science and Art after Huxley had turned down the suggestion that he take on the position. Huxley was influential in obtaining it for Donnelly, and supported the latter steadfastly during his long term of office (Bibby, ~~ibid~~., p. 118).

105. Macleod in Thackray and Mendelsohn, ~~ibid~~., p. 62.

106. Huxley Papers (ICL), 16.228.

107. Leonard Huxley, Life and Letters of THH, vol. 2, p. 117.

108. Bibby, ~~ibid~~., pp. 253-257.

109. Huxley Papers (ICL), 14.26.

110. O.U. microfilm of Frankland Papers (RFC).

111. Biddy, ~~ibid~~., p. 118.

112. Huxley Papers (ICL), 14.48 and 14.104. It has been suggested that Ralph Lingen developed a more co-operative attitude towards science in the 1880's (Macleod in G. L'E. Turner (ed.), The Patronage of Science in the Nineteenth Century, pp. 154-155).

113. Leonard Huxley, Life and Letters of THH, vol. 2, p. 322. Here Huxley deliberately co-opted a very high honour to be the one suitable for men of science.

114. Ibid., vol. 2. p. 323.

115. Barton, ~~ibid~~., p. 203.

CHAPTER EIGHT

RUNNING THE ROYAL SOCIETY 1870-1885

Within the section devoted to miscellaneous notices in the edition of Nature for the 8th September 1870 there appears the following item:

> "We regret to learn that the health of Gen. Sir E. Sabine the distinguished and venerable President of the Royal Society is at the present moment such that he is likely soon to demand relief from the pressure of those duties which he has hitherto performed with so much credit."1

This seemingly routine reference was actually an impudent act calculated to encourage Sabine's resignation. It was born of the enormously increased self-confidence and stature of the generation of biological scientists who had come to full maturity after the publication of the Origin of Species in 1860. The machievellian method employed by Norman Lockyer, Nature's founding editor was not lost on the Royal Society's Senior Secretary William Sharpey. He wrote to his fellow Secretary George Stokes a week after the appearance of the Nature item, remarking that:

> "Lockyer should not allow disagreeable references to enter Nature. Noone likes to be told by a journal that he is unfit for duty. There have been two letters on business by the president written the last day or two, from which I can only gather that he is in usual health."2

In 1870 Huxley was 45, Hooker 53 and Sabine was 82. The growing confidence of the Huxleyites was reflected in their preparations for the end of Sabine's Presidency. Having decided on their preferences, Huxley was deputed to sound out Charles Lyell's willingness to stand for election as Sabine's successor. Tyndall approached George Airy for the same reason. These preliminaries took place two years before Sabine finally gave up the struggle and retired in 1871.[3] This foray of 1869 was the first attempt by Huxley and his main supporters to annex the crucial responsibility for nominating new members of the Society's tiny oligarchic government. By this time the key Huxleyites

were well-established in their scientific careers and possessed formidable scientific reputations. Up to this time none of them including Huxley himself had held any office within the Royal Society. During 1869 only Tyndall held a place on the Council, yet within twelve months the Huxleyites were able to win an important victory out of the crisis which developed over the appointment of a new treasurer in 1870. Huxleyite representation on the Council of the Royal Society had been small throughout the late 1860's. In 1868 there had been no representative at all, whilst Huxley himself served as the lone upholder of his cause in 1867. This being the case it is all the more remarkable that they should have moved with such assurance in pursuit of power in the Society at the end of the decade. At this time the Darwinian cause was gradually winning ground from the clerical interests. It is helpful to recall that within the necessarily short-term view of the participants the struggle must have appeared very much as a stalemate requiring constant attention for its maintenance.

Throughout the 1860's the only one of the Royal Society's officers who was not a committed Christian was the physiologist William Sharpey. The election of officers known for their explicit Darwinism was clearly a matter of great importance. The controversial British Association Presidencies of Hooker and Huxley took place in 1868 and 1870 respectively. The issues which were involved are given detailed consideration in the next chapter. The admission of Darwinists to the most senior positions in British science could be staved off no longer. Sabine's last three years as President of the Royal Society earned him much unpopularity amongst the relatively young generation of active Huxleyites. Sabine's letters show him to be a highly ambitious though usually diplomatic individual. He seems to have calculated for the unpopularity which marred his last few years in office. That period

can perhaps best be seen as a deliberate holding action. Sabine spent his last three Presidential years struggling to stem the tide of perturbing influences by replacing himself, the Treasurer, and part of the Council with senior opponents of Darwin.

Late in 1868 Sabine made approaches to the Duke of Argyll with a view to his taking over the Presidency of the Royal Society. Argyll was one of Darwin's most influential opponents and his election would have gratified the conservative interests. However, the Duke declined Sabine's offer of nomination.[4] By late October of that year, Sabine was clearly convinced that the Council contained a substantial group of Fellows who were prepared to vote together in order to block the President's intentions. His recommendations for the 1869 Council list were thwarted in just this way. The Society's Assistant Secretary Walter White reported in his journal Sabine's response to the Council's defiance. According to White, the President declared:

> "That he would not be President to fight a faction and that he intended to resign soon."5

It seems that the unpopularity which marred the twilight of Sabine's Presidential career was not unknown in earlier days. When Sir Benjamin Collins Brodie's blindness prevented him from delivering the President's Address on St. Andrew's Day in 1860, Sabine as Treasurer and Vice-President read it for him. Anticipating this event, White noted in his Journal that, "many dislike the General".[6]

The much vaunted changes in the Royal Society's Statutes in 1847 did not transform it in quite the way or to the extent that some historians have maintained.[7] The simple notion of a fashionable dilettante social club turning rapidly into a strictly meritocratic forum for earnest and fair-minded truth seekers has been examined and rejected in an earlier section. In 1860 the Society was composed of roughly equal numbers of scientific and non-scientific Fellows. Bearing this in mind

it is not surprising that accounts of the meetings of the Philosophical Club during the decade following 1847 contain a good deal that does not fit the model of enthusiastic modernism so frequently used to distinquish the reformed Society. The Philosophical Club was founded in 1847 to preserve the spirit of the changes then made. The Club's method of recruitment was intended to ensure that it would continue to operate as a pressure group representing the leading active scientific practitioners of the day. The subjects which preoccupied many of the Club's meetings show clearly that even this most shining expression of the good intentions of 1847 was far from being strictly scientific. During the 1850's discussions of mythical beasts, freaks of the weather, and geographical curiosities are much in evidence.[8] At a Philosophical Club meeting on October 27th 1853 Sabine displayed an egg-shaped bottle made of green glass, the neck of which had been broken off. The chronicler of the meeting describes the bottle as one of several found east of Nova Zembla on the Siberian coast. At Sabine's prompting a Royal Society Skara Sea Bottle Committee was appointed to go thoroughly into the whole question. It was settled before the next meeting of the Philosophical Club on the 24th of November by: "a Norwegian at Lloyds [who as Sabine reported] had identified the bottle exhibited at the last meeting as one of those used by fishermen of his country as floats for nets".[9] It is clear that Sabine commanded a strong groundswell of support from the Fellowship at large. This was based on his record of participation in the traditional scientific pursuits of maritime adventure and Humboldtian data collection in distant romantic climes. The first generation of Darwinists themselves bore a closer relation to that obsolescent way of doing science than is sometimes supposed. Darwin, Hooker, Huxley, and Spottiswoode are all associated with great journeys. Of them, only Huxley was later identified with the

professional ambience of university-based research which their activities had helped to encourage. Darwin and Hooker still found things to admire in Humboldtian science in 1881. In that year Hooker wrote inviting Darwin's agreement with his continued support of Humboldtian science:

> "It is the custom to disparage Humboldt now as a shallow man, but when I think of what he did through his own observations during travel . . . I am constrained to regard him as the first of scientific travellers; do you?"

A few days later Darwin replied in the affirmative.[10] Against this background the ambitions of the Huxleyites, invigorated by the startling new rallying cry provided by the Origin of Species might have seemed close to being a betrayal from within. Sabine had encouraged Huxley to stay in this country when the scarcity of scientific employment prompted his interest in a post at the University of Toronto.[11] The group which had effected the reform of the Royal Society naturally experienced resentment at the Huxleyite attempt to supplant their leadership of the Royal Society at the end of the 1860's. Hooker was elected in 1847 shortly prior to the reform of the statutes, but most of the Huxleyites owed their early admission to the Fellowship to the support of the reforming group.

Sabine's high-handed treatment of the "Origin" during his presentation of the Copley medal to Darwin (not personally) at the 1864 Anniversary Meeting has been detailed elsewhere and will be re-examined in the next chapter.[12] By the end of the decade the "Young Guard" of British science felt that Sabine's retirement was long overdue. The "pushing and shoving" indulged in by the Huxleyites in order to hasten his departure is well-illustrated by Lockyer's barbed item in Nature which opens this chapter. Matters were fortuitously brought to issue following the death of the Society's Treasurer William Allen Miller in

1870. He was taken ill at the B.A.A.S. meeting at Liverpool and died two months later.[13] When the new session of the Royal Society opened in the late autumn of that year, it was well-known that Sabine wished his close friend J. P. Gassiot to become the new Treasurer. Gassiot was 73 at this time and as conservative as his promoter. The two men also shared a similar religious opposition to Darwinism. The Council meeting which was to select a new Treasurer met at 1.00 p.m. on Thursday 27th October. It was known beforehand that Gassiot was to be opposed by William Spottiswoode. Tyndall was Spottiswoode's proposer. Despite being the sole Huxleyite on the Council, Tyndall's canvassing was surprisingly successful and resulted in the ensuing vote being tied. Following a week's adjournment, the victory went to Spottiswoode by a margin of ten to six. In the words of Thomas Hirst:

> "After a division and some external agitation by the Council of the Royal Society [it was decided] that Spottiswoode should succeed Miller."14

William Sharpey had been well aware of Sabine's eagerness to settle Gassiot in the Treasurer's place. Shortly before the Council meeting of the 27th October, Sharpey wrote to his fellow Secretary George Stokes seeking his support for Spottiswoode. Sharpey's case was founded on both the likelihood of Gassiot's nomination being attributed to the exercise of undue Presidential influence and the particular suitability of Spottiswoode. The tenor of Sharpey's approach was nicely calculated to engage the sensibilities of George Stokes.

> "the recommendation of high reputation in mathematics and great general attainments and high culture and he would seem to me to be better fitted to occupy a position which is next to that of President and indeed represents the Society in his absence."15

Thoroughly beleagured by this juncture, Sabine had not only lost his nominee for the Treasurership but also his personal selections for the new Council list. He had earlier primed W. A. Miller and

Stokes on the strength of his wish to see Richard Owen and Charles Wheatstone included in the next Council. Sabine was baffled by the absence of these two names from the list which Sharpey sent him. Sabine sent an aggrieved letter to Stokes on the 31st October 1870.

> "You will have anticipated that the list which you have sent me will not receive my individual vote - The next year will inaugurate a new era, so far as the influence of a President is concerned. I deem it highly expedient that my successor should, in the first year of his Presidency have the advantage of the presence in the Council of the two eminent men whose names I have mentioned."16

Sabine's thwarting carries a particular irony. In November 1854, the President , Lord Rosse, wrote to the Society's apartments in Somerset House from Parsonstown stating his objection "in principle" to some aspects of Council procedure. Basically Rosse was tired of being associated with the Society's unsuccessful requests for Government funds in connection with particular scientific projects. Rosse attributed these failures to the absence from the "reformed" Council of powerful members on an effective personal basis with the government of the day. The two suggestions which Rosse had had turned down concerned Lords Ashburton and Argyll.[17] Sabine replied for the Society, emphasising the paramount importance of the free expression of the Council as a whole in determining the Council list for the coming session. Sixteen years later Sabine's motive in wishing to exercise undue influence in forming the new Council list was admittedly somewhat different. He wished to protect traditional interests. These seemed to be threatened by the disturbingly rapid advance made by the burgeoning power base underlying Spottiswoode's strong run in the contest for the Treasurership. Knowing the oligarchic nature of the Society's government, Sabine was understandably alarmed at the infusion of: "new blood, including younger men, into the Council".[18] The atmosphere of crisis did not abate in the short term. Walter White wrote in his Journal

an entry for the last day of 1870 referring to a recent conversation with Dr. Percy, the Professor of Metallurgy at the Royal School of Mines in Jermyn Street. Percy told White of a move afoot to prevent the paid officers of the Society from voting. Following a Royal Society meeting a month later White noted:

> "In the tea-room talk afterward I heard that there is an intention to set aside others of the chief officers as well as the President."19

Sabine's position was becoming extremely uncomfortable at the beginning of 1871. All his personal wishes had been ignored. His hope that the Council would finally act as a dutiful cabinet by following his suggestion for his successor as President was similarly disappointed. Sabine's scheme was for Lord Salisbury to take over for two years after which the Fourth Earl of Rosse was intended to take his place. The irony of Sabine's adoption of the very stance for which he had chastised the third Earl, William Parsons, in 1854 appears to have been lost on those most interested in the General's departure sixteen years later.

Sabine failed to secure the election of his nominees Owen and Wheatstone to the Council. He might then have realised that the choice made for the new President would be even less likely to please him than the previous - more "Sabinite" - Council's choice of a new Treasurer had been. The only question remaining in which Sabine retained any real influence was that of when he would go. The lack of deference which the "Young Guard" displayed towards the veteran magnetician had been ruthlessly highlighted at the Council meetings which dealt with filling the vacant Treasurership on the 27th October and 3rd November 1870. Alexander W. Williamson drew the attention of the latter meeting to the 1848 resolution restricting Presidential terms to four successive years. The "rude scene" in the Council chamber which this caused, led to the arousal of some sympathetic support from conservative

interests. Chief among Sabine's defenders was Gassiot who privately printed and circulated a pamphlet decrying the attack upon his mentor.[20] On the 24th November Hirst reported that at a meeting of the Philosophical Club, Gassiot:

> "eased his mind by denouncing in strong terms a remark of Williamson's in the Council of the Royal Society to the effect that the period during which the President should hold office ought to be limited."21

Sabine's humiliation was completed by the installation of the Astronomer Royal George Airy as the new President on St. Andrew's Day 1871. Airy had the approval of the Huxleyites and shared their anti-clerical views.

A Digression: The Struggle Between Airy and Sabine

The rout of the conservative interests was made worse by the fact that Airy had for many years been Sabine's bitter opponent over the financing and control of research in terrestrial magnetism. In 1866 Airy accused Sabine of using his position as President of the Royal Society to corruptly influence the amount of funds and scientific recognition accorded to the Kew observatory. Sabine used the observatory as the focal point for his ambitious plans to build up terrestrial magnetism as the dominant concern of large-scale British science. He could only achieve this by annexation of power, responsibility and resources from the hitherto sacrosanct domain of the Astronomer Royal, George Airy. The Kew instruments had originally been bought by J. P. Gassiot in 1842. Gassiot, who had made a fortune in the Spanish wine trade, showed his generosity when it had become clear that Kew Observatory was to be taken over by the B.A.A.S. His long-term ally, Sabine, had in the earlier years of the programme of magnetic research relied upon his ability to successfully play on the larger aspirations of the B.A.A.S. Sabine's success in this endeavour is attested to by

the history of what has become known as the "magnetic crusade". Quite regardless of the threat to his personal position, Airy was alarmed by Sabine's seemingly compulsive collection of apparently pre-theoretical data in colossal quantities. Airy was given to alluding to Sabine's huge store of magnetical maps as reflecting his "chartism".[22] In December 1860 their power struggle was evidently still in full spate. At that time Sabine wrote to William Sharpey stating:

> "I had a note from Airy to the effect that as I consider it proper that the Kew record [of magnetical observations] should be continued over the next decennial minimum, a fortiori it must be more proper that the Greenwich record should also be so continued!"23

In February 1866 Airy took his long-running source of resentment to the Royal Society and made what Sharpey described as an "unseemly suggestion". Airy alleged that Sabine had manipulated the Council to secure unfair advantages for the Kew Observatory in the matter of instruments. The Secretaries showed a great concern for the Royal Society's public image in their response to the situation. Sharpey wrote to Stokes telling him that he was loath for the Council to formally rebuff Airy's charge lest it should seem to recognise its plausibility. Neither was the Senior Secretary in favour of Sabine writing on the subject as an individual lest that should seem to validate Airy's charge. Nevertheless Sharpey felt that he should be upbraided, "considering the rude and offensive way in which Mr. Airy puts forward these assumptions." Stokes managed to coax Sharpey into softening his approach to Airy on the subject. The style of the reproof was "still more toned down at the meeting of the Council". Sharpey did not want the Council explicitly to deny that they had covered up the truth of the matter. He suggested that:

> "in some private way or non-official way [Mr. Airy might be] disabused of his presumably genuine but mistaken conviction and yet in a way that if necessary might come to be known."24

Three months later Airy had been forced to withdraw his "vile charges". Stokes was still not at peace however. In his letter acknowledging Airy's grudging withdrawal he voiced his dissatisfaction with the fact that the Astronomer Royal, "still speaks of the words now withdrawn as having been at any time justified by the letter of the P&C [President and Council]."[25] As the extent of the magnetic research programme shows, Sabine nearly always got the better of the struggle. Having used the B.A.A.S. to his advantage up to the mid-century, he became centrally involved in the running of the Royal Society itself thenceforward. Five years after the incident related above, Sabine was forced from the chair of Royal to be replaced by Airy: his bitterest opponent of eminence since Charles Babbage.

"A New Era"

Although Airy was one of the two men "sounded" for the Presidency by the Huxleyites in 1869, it seems that he was the outcome of a compromise of their true wishes. Seventy years of age and still heavily involved in the affairs of Greenwich according to the fiercely autocratic character of his regime, Airy cuts an unlikely figure as the harbinger of a new era. The men who represented the infusion of new blood into the Council did not yet possess the confidence to put forward one of their own number for the Presidency. Airy personified the roughly "correct" combination of scientific eminence and seniority formally required by the reformed Society. His tenure was an embarrassing failure.

Despite the success which the up-and-coming younger men felt they had achieved in despatching Sabine, the office-holders of December 1871 did not impress Hirst by their vigour or sprightliness:

> "Attended the first meeting of the new Council of the Royal Society. Airy presided for the first time. He conducted business well on the whole, but being deaf he could not consult with his Council

> as much as a President should do. It was jokingly remarked that 'our new President was deaf on both sides, our Senior Secretary deaf on one side and blind on the other, and our Junior Secretary [Stokes] generally dumb!"26

The sense in which Airy's nomination had formed a compromise is reflected in his acceptance by the Society's leading conservative elements headed by the chronically reticent Stokes. The later's personally preferred nominee in 1871 was the 7th Duke of Devonshire, a nobleman with a strong Cambridge background in mathematics. The limited evidence available seems to point to Stokes having been put under pressure by the other officers. They were well aware of his tactical ineptitude and easily ensured the abandonment of the Duke's nomination. Walter White noted the central fact of the incident in his Journal for April 25th 1871:

> "That Mr. Stokes dreads Huxley's being President so accepts Airy."27

When White's Journal was published in 1897 by his nephew this entry was to cause Stokes a good deal of chagrin. The strength of Stokes' feelings at that juncture two years after Huxley's death, combined with the terms of Foster's oddly avuncular reassurances help to resolve a matter of some interest. The two communications show the extent to which social relations remained amicable between the leading figures in opposed idealogical camps. So far as the scientific naturalists were concerned, the studied avoidance of personal invective and the overt pursuit of narrow factional interests was a central part of their ideal public image. As the aspiring technical managers of an ever-improving science-based society, the scientific naturalists sought to distance themselves as much as possible from the methods and mannerisms of party politics. They did not always succeed.

The summer of 1872 reveals the executive of the Royal Society in a state of transition which might have continued in a sedate fashion for

years but for two sudden events. Sharpey decided that his worsening eyesight had reached a stage which meant he could no longer carry out his duties and decided to retire. He had accomplished a great many helpful acts behind the scenes for the Huxleyite cause and the X club members in particular. His last official act in connection with the Royal Society was Sharpey's most effective one. It resulted in the election of Huxley in his place. Writing to the new Junior Secretary much later Michael Foster informed him that the Fellows were pleased and surprised to find that one, "whom they looked to rather as a not distant President, was willing to undertake the duties of the office."[28] Despite the inevitability which Foster's hindsight lends to the election of Huxley to the vacant Secretaryship, it was not so straightforward at the time. A tied vote in Council had been the result of the converted move for Spottiswoode as the Society's new Treasurer eighteen months earlier. In a like fashion Huxley's strength in the contest for the Secretaryship took some of the members of the Council by surprise. The vote was postponed for a month. The proposition was confirmed on the 20th of June.

Four months later Sharpey received Airy's preliminary letter of resignation from the Presidency. On the occasion that Stokes and Spottiswoode had visited Airy at Greenwich to communicate the offer of the Presidency, Walter White reported that "he accepted without reserve."[29] Airy's nomination had been supported by a unanimous vote of the Council. Airy was the first poor man to take the chair in the sense that he possessed no personal fortune. In March 1871 he applied to the Society for £100 to cover out-of-pocket expenses. Airy specified these as being "such as in my habitual life I am anxious to avoid."[30] Despite the fact that the change was fully in keeping with the meritocratic spirit of the 1847 reforms, the bold statement made by the President elect elicited an unfavourable response from many influential

conservatives among the Fellows. Sabine's place among these critics is unsurprising considering the decades of emnity which had marked the relationship between the two men. Following Airy's application for expenses, the Society resolved to pay for its own soirées (traditionally the President's treat). His request for other expenses were ignored. Airy's attitudes to the accumulation of wealth and the proper relation of this activity to the scientific life were a strange but firmly held mixture. He tried sternly to warn Stokes off academic pluralism in 1855 when the latter was struggling to increase his income beyond the limit imposed by his Fellowship of Pembroke College Cambridge and his Royal Society Secretaryship.[31] The conviction of Airy's opinion was doubtless reinforced by the Civil List pension of £300 per year which had been awarded to him by Peel's first Ministry in 1835.[32] Airy actively supported the movement opposing the endowment of research set up by Captain Noble in 1880.[33]

On October the 28th 1872 Sharpey wrote to Stokes stating that:

> "Sir G. Airy's letter which I herewith return has come upon me like a clap of thunder. . . . I for one can scarcely admit of the <u>sufficiency</u> of his reasons for thus so abruptly withdrawing. With Spottiswoode and one or two good vice-Presidents we can transact business without the presence of the President being absolutely required."[34]

Sharpey went on to note that until a replacement could be found, the Society's Officers would be able on occasion to visit Greenwich to consult with the unwilling President. In his preliminary letter of resignation, Airy had given as his reason the impossibility of curtailing the demands made upon his time by Greenwich.[35] Airy expressed the fear that his health was in imminent danger (this was by no means an unusual notion in middle-class Victorian circles) and that he would not serve beyond November 1872. Sharpey tried to impress upon Stokes that precipitate action could only damage the prestige inherent in the office

of President. The Senior Secretary wanted to avoid demeaning any hastily co-opted replacement.

> "We could not well ask the Duke of Devonshire to decide at a moment's notice in order to relieve us further difficulty now caused by the President. It is no use to speak of Owen. Hooker would not be a perfectly satisfactory appointment. Wheatstone if he could be persuaded might do very fairly. Grove I fear could scarcely now give the time - but there is no saying. But in any case the compliment is tarnished by our being obliged to ask an eminent man to stop a gap."36

So far as the official attitude of the Royal Society was concerned, Airy's reputation never recovered from this abrupt withdrawal. When he died twenty years later, the Assistant Secretary Herbert Rix addressed an extraordinary inquiry to Airy's successor as Astronomer Royal. Rix wished to learn where and when Airy was to be buried: "and whether you think that the Royal Society ought to be represented at the funeral?"[37]

The Council met to nominate a new President on the 16th January 1873. Airy was taken aback by the weight of opinion for Hooker, which he attributed to the operation of a "caucus". Not relishing the prospect of Hooker as his successor, Airy arranged to see Stokes to put his objections forward prior to the next Council meeting on the 23rd of January. His initial letter to Stokes on the subject dwealt on the capacities of past Presidents to cope with acrimony.

> "The best President I have seen in troublous circumstances was the Duke of Sussex . . . the worst was Lord Rosse. I never saw General Sabine in stormy circumstances. Does anybody know the usual annual length of the Duke of Devonshire's town residence? or the probability that he on the one hand or the favourite on the other hand could attend?"38

Airy had not supported Hooker during the painful and protracted Ayrton incident. Sympathy among the Huxleyites for the man whom they had backed for the Presidency in 1869 was wearing extremely thin in November 1873. A week prior to Hooker's inauguration as the new President Huxley told Michael Foster that Airy had been behaving: "more lunatically than ever - we are well quit of him".[39]

Despite their mutual antipathy, Airy and Sabine ended up similarly out of favour at Burlington House. Their campaigns and ambitions had become increasingly outmoded by the rise of new forces with new aims. Sabine informed Stokes in May 1873 that he had: "little disposition . . . at any time to offer a word on Royal Society matters; or even the wish to do so."[40] After twenty-two years service within the Royal Society's executive since 1850, Sabine died quietly at one o'clock in the morning of the 26th June 1883. At the time, the current President William Spottiswoode was in the throes of the typhoid fever which was shortly to kill him. Sabine's death was largely overshadowed by news of the incumbent President's fate.

Huxleyites in the Chair 1873-1885

Huxley found himself formally at the centre of things when he took over from Sharpey as Secretary of the Royal Society in December 1872. The new era contemplated by Sabine two years earlier had begun, but not in the way he envisaged. Many Fellows disapproved of the rapid nomination of the first Darwinian President of the Royal Society. Hooker's social position as a salaried scientist within a Government-controlled institution (Kew) did nothing to improve his suitability in the eyes of his conservative detractors. The opposition to Hooker was not able to mobilise sufficient influence to flout the informal convention which proscribed contested elections for the Presidency. Shortly before the Council which would consider nominations in January 1873, Thomas Hirst noted in his Journal one important outcome of an X club meeting on the 2nd of the month:

> "we communicated to Hooker that he might be nominated as the next President of the Royal Society."41

At the Society's Council meeting two weeks later Hirst spoke in favour of nominating a "purely scientific man not a man of rank".[42] He noted that five or six Council members held out for the Duke of Devonshire.

The campaign for Hooker was well-planned. Spottiswoode occupied the Treasurership, Huxley himself was the new Junior Secretary, and the Council included: Hirst, Busk, and Hooker himself. Airy's communication with Stokes regarding the availability of the Duke of Devonshire was sent only a week before the crucial meeting of the Council. On the 23rd of January 1873 Hooker's nomination was carried by a ⅔ majority.[43]

The significance of the coup which was carried out so swiftly and surely is reflected in Huxley's assurance to Foster, his protegé, given a week prior to the installation of the new President on the 30th of November 1873:

> "I don't suppose there will or can be any battle at the R.S."[44]

An abiding fear of the oligarchic governments of the Royal Society was that influential dissenters might rise during the Anniversary Meeting and declaim their opposition to the structures of the Council. Enormous store was set by the preservation of an aura of statesmanlike altruism about the formal doings of the Society. This image of "scientific" tranquil honesty was even more important to the scientific naturalists than it had been to their traditionally-minded predecessors at the head of British science. A few days after the 1873 Anniversary dinner, Huxley was delighted to relay to Foster news of "a splendid anniversary down at the R.S. on the occasion of Hooker's inauguration. The like not known in the memory of the oldest F.R.S . . . ." Huxley went on to describe briefly an incident which shows clearly that the significance of the first Huxleyite Presidency was not lost on Richard Owen, one of the leading opponents of scientific naturalism from the outset.

> "Also Owen came and made a malignant all of himself to an extent not known in the memory of the same senile party."[45]

Hooker's presence at the head of the Society appears to have rekindled the vitality of the sometimes lustreless events staged at Burlington House. At the first soirée of his Presidency the attendance was twice as great as any previously seen. During his term as President, Hooker further restricted the terms of admission to the privileged class of Fellows and instituted a scheme of subsidies which reduced the subscription charge to £3 per annum.

The scientific naturalists were concerned with redefining the nature of the scientific enterprise for a society in which all could be "scientifically enfranchised". Hooker contributed towards this end by opening the Royal Society's rooms for an annual evening reception of a less formal character than the traditional conversazione. This took place for the first time in 1875.[46] By the mid-1870's when Hooker was well-established as President, the sense of threat to the Darwinian interest from leaders of the church in society at large had diminished to little more than a heroic memory. The main thrust of the scientific naturalists' struggle had resolved itself into the zealous promotion of their version of the basic nature and future of science. This had perforce to take place at the expense of the rival version held to be the far less publicity conscious mathematical physicists. It was from the latter that the religiously inspired critique of the Darwinian world view now came. The debate was contained within the scientific community once more. Hooker's main accomplishments as President were directed towards making the Society more accessible to the sort of acolytes who would be most likely to answer the demands of a regime based on scientific naturalism. Hooker's measures have been explained as being simply the obvious extensions of the major reforms of 1847.[47] In fact they originated in an entirely different set of circumstances.

In 1874 Hooker had fifteen committees of the Royal Society with which to deal. He was glad of the support given him by the other officers. He remarked to Darwin of the committees that: "they relax me as metaphysics do Huxley".[48] Hooker pointedly included an account of some of the internal affairs of the Society in his Presidential Address for 1875 as he believed that most of the Fellows were entirely ignorant of them.[49] For a central figure in the Society's strictly oligarchical government this act of enlightenment could be seen as something of a luxury. Hooker's objection to the Council and its "dodges" in the 1850's has already been noted. At that time he stood outside the Society's ruling group. The resignation of the Royal Society's Foreign Secretary William Hallowes Miller shortly after the installation of Hooker as President was thought by some to indicate continued opposition to this very public success for the Huxleyites. Walter White noted in his Journal on the 23rd of January 1874 that:

> "Prof. Newton says it came out throught the ladies that Prof. Miller resigned because he did not like Dr. Hooker as President; that many Fellows think the same, and that a large party is in favour of the Duke of Devonshire."50

Joseph Hooker's reforming style appears to have broken through some of the stiffness of the Society's august self-image. Predictably, some of his aspirations fell victim to the binding forces of institutional inertia which frequently subvert the sweeping ambitions of any opposition party of long standing when finally it comes to power. Hooker wanted to streamline the procedures of the Council in such a way as to concentrate even more power in the hands of the officers. After joining the Council in December 1872 he mentioned to Huxley that:

> "I too was struck by the more- unbusiness-like-way - than ever of the Council. Half an hour's preliminary work of Secretary and officers might have saved three at least of the six hours."51

Despite their very strong position within the executive, the Huxleyites had not achieved any modification of Council procedure two years later. Hooker referred to his Royal Society routine on Council days as: "great pulls - 1-6 p.m. continuous followed by dinner followed by the meeting at 8½."[52]

Hooker's Presidency stood as a retrospective reproof to his erstwhile tormentor Acton Ayrton, the Office of Works and Gladstone's Ministry which had permitted the hounding of Kew's Director two years earlier. A comparatively large amount of attention had been given to the anti-clerical posture of the Victorian scientific naturalists. This had tended to distract attention from the preoccupation of the Huxleyites with less sensational aspects of the autonomy of science. By the mid-1870's the complacent manufacturers and party politicians stood out in the minds of the leading scientific naturalists as the most immediate threats to their cherished vision of the future. A scientific movement corrupted by the interests of profit and party was anathema to them.[53] It was judged to be somehow morally incapable of driving the process of social change within which truth and technological power would advance in step towards the perfection of civilisation.[54]

As a government employee, Hooker had early to clarify his position regarding the "proper" relationship between science and government. As had already been described in an earlier section , Hooker (p. 225) was not always ideologically pure as Huxley. He was not averse to the occasional use of his position as President of the Royal Society to secure personal advantages in his situation at Kew. Huxleyites in general found themselves in a potent dilemma in the matter of financing science. Scientific activity of all kinds became increasingly expensive as the century wore on. The sort of money involved could only be obtained from the Government. The natural democratic corollary

of Government endowed science was of course the accountability of its leading practitioners to the party politicians comprising the government of the day. In the outlook of scientific naturalism this implied a fatal compromise of the autonomy of science by the very persons whose invidious social position it aspired ultimately to replace. The Huxleyite watchwords: honesty and truth did not sit well with clear commitment to using their arch enemy's financial favours as a means to eventually bring about his downfall. The anti-democratic basis of the scientific naturalists' political ambitions is evident from the following extract from a leading article in Nature. It appeared on January 19th 1880 and so was roughly contemporary with Hooker's Presidency (1873-1878).

> "The Science of Statesmanship
>
> Yet there is surely no reason why political action, the conduct of the State should not be guided by scientific method quite as much as the conduct of a scientific exploring expedition such as that which has so recently sailed over the North-East Passage. . . . to elevate [politics] it into something like a science of national life and progress."

The writer then sets out his recommendation that Darwin's methods should be generalised, "if he [the aspirant to authentic progress] really desires to arrive at the true principles of scientific statesmanship." Having quoted Darwin at length and compared the moral tenor of his work with that of the "special pleader" of normal politics, the Nature editorialist concludes:

> "if scientific statesmanship, and not mere party prejudice were the guiding principle in the conduct of public affairs, this nature would be more fitted than ever to survive and play the leading part in the affairs of the world."55

Hooker had been made well-aware of the corrupting influence of political involvement in science back in the 1850's over the dubious political patronage heaped on the Schlagentweit brothers. This was

reinforced in a more tellingly personal way during the Ayrton incident which dragged on from 1871 to 1873. Kew's Director felt much threatened by the politicians headed by Gladstone. On August 31st 1871 Hooker put his case very strongly in a communication to the Government, yet he had little faith in achieving anything.

> "I fear that Gladstone will pick up Ayrton's pieces, kiss them and put them together again - and nobody a bit the wiser or better . . . I have made Ayrton my enemy for life, that I care nothing about and D. Galton too - (who has behaved like a sneak) and that I am little sorry for."[56]

Sir Douglas Galton was a cousin of Francis Galton and was directly involved in science. He had been appointed as a specially qualified Director of Works to be responsible in part for the heating system at Kew. Galton failed to support Hooker in the struggle against Ayrton. Hooker and Huxley felt that they had been shown a stark illustration of how their identity between science and honesty stood up to wholehearted co-operation of scientists with the existing system of party political government.[57] The Ayrton incident produced a considerable stir. The waters were further muddied by the coldly agressive interference of Richard Owen. He had been the enemy of Hooker and Huxley for twenty years. His staunch support for opponents of Darwin is well-known. At the beginning of 1873 Owen began to launch attacks on Hooker whom he saw as a direct rival in biological science and as a threat to his power as Superintendent of the Natural History Department of the British Museum. Huxley described the situation in a letter to Tyndall as it appeared to him on the first day of 1873:

> "The tail of the Ayrton-Hooker storm is drifting across the scientific sky in the shape of fresh attacks by Owen on Hooker."[58]

Kew's embattled Director emerged from the ordeal much chastened. Certainly his taste for isolationism at Kew was broken down and a few months later he was the willing President elect of the Royal Society.

Subsequently he cultivated in himself the same mixture of polished diplomacy and administrative guerilla tactics which had made Huxley: "a devastatingly efficient political operative."[59] Hooker's more combative disposition was given full rein when the Treasury refused his two requests (separated by a full year) to refurbish a small house adjacent to Kew Herbarium for the use of J. G. Baker. Baker was the Herbarium's first assistant. Hooker went in person to see the Chancellor of the Exchequer to whom he pointed out that like Baker, the Chancellor (Sir Stafford Northcote) was a Fellow of the Royal Society. Hooker suggested that a deputation from the Society visit Northcote and thereby shame him by creating a minor scandal. The Treasury officials stalled for a short while longer then agreed to Hooker's request.[60] When he had become the Royal Society's President-elect in 1873, Hooker tended to take a jaundiced view of any connection between science and government. He was very suspicious of the Government's intentions regarding the outcome of the Devonshire Commission which had been set up in 1870 by Gladstone's Liberal Ministry. The Commission had been prompted by the initiative under Hooker's Presidency in 1868. Late in the August of 1873 Hooker thought that he saw official duplicity being used in the matter of the transfer of some scientific men employed at South Kensington to the British Museum. As he told Huxley:

> "It shows the utter baseness of this Government and supports my original view that Lowe's granting the Commission was a mere blind though I did suppose that it was more for the purpose of passing the subject on to the next ministry than for leaving him to carry on his own schemes unobserved."61

Huxley was not so suspicious of Lowe with whom he had connections reaching back to his time in Sydney with the "Rattlesnake". Lowe was Vice-President of the Committee of Council on Education from 1859-1864, Chancellor of the Exchequer from 1868-73, and Gladstone's Home Secretary from 1873-74: the last year of his first ministry.

It has been suggested that already in 1854 the President of the Royal Society had come to occupy a position analogous to that of a constitutional monarch.[62] There are however few indications that twenty years later Hooker's conduct was ordered by any such democratic constraints. He explicitly endorsed the purely oligarchical government of the Society in his private communications with other Huxleyites to whom he emphasised the unfittedness of the Fellowship at large to take part in running the Society. Hooker's programme derived its style and the substance of its initiatives from a rigorous interpretation of Huxley's version of scientific naturalism. When W. H. Miller resigned the Foreign Secretaryship in 1873 he was replaced by the Comtist and distinguished chemist Alexander W. Williamson. He was clearly no agnostic but as the author of the "rude scene in Council" which finally saw off the dotard Sabine, had many things in common with the Huxleyites. After a rapid period of change the Royal Society executive contained only one Christian believer in 1875. It contained also only one political conservative. The two were combined in the person of George Stokes who at that time had been serving as Secretary for twenty-one years. The Council list for December 1875 is as follows:[63]

President	J. D. Hooker
Treasurer	W. Spottiswoode
Senior Secretary	G. G. Stokes
Junior Secretary	T. H. Huxley
Foreign Secretary	A. W. Williamson

Prof. J. C. Adams	J. N. Lockyer
Maj. Gen. Boileau	R. Main
E. V. Cardwell	Prof. D. Oliver
W. de la Rue	Prof. E. A. Parkes
Capt. F. J. D. Evans	Lyon Playfair
Edward Frankland	William Pole
Albert Gunther	Reverend B. Price
J. Wharton Jones	W. W. Smyth

This securely Huxleyite executive represented a complete reversal of the 1854 situation. At that date William Sharpey took his place as the only officer out of the five who would develop Huxleyite sympathies.

Until 1870 when Spottiswoode filled the Treasurership left vacant by the premature death of W. A. Miller, Sharpey stood alone within the Society's executive as an ally of the scientific naturalists.[64]

The foregoing Council list reveals a few well-known conservatives (also Christians) such as de la Rue, Günther, and Price. The list also contains two influential supporters of the mainly Huxleyite executive in the shape of Lockyer and Frankland. Apart from Playfair and Adams, the remainder were (and are) relatively little-known men whose service on the Council was a reward in itself. They were selected for their limited renown which tended to make them easily susceptible to the voting pattern of certain more influential others. The replaceable "small men" of the Council were distributed according to their field of study to produce a semblance of even representation. They served on a scheme of rotation which usually had so long a period that they served only one or at most two terms of office in the whole of their scientific lives. The pivotal veterans of the Council returned on a much more regular basis as will be shown in the next section. The robust appearance of openness and balance about the Council lists of this era is mostly a sham. A quiescent and pliable Council was a pre-requisite for the smooth running of the Society's covert oligarchic government.

1878-1885: the Last Phase of Huxleyite Control

The beginning of the 1870's seems to have produced a growing feeling among many of what might loosely be termed the progressive party in science that the tide had turned in their favour. In the event, the seventies proved to be a time of difficulties and deferred hopes. Frank Turner has described the unfulfilled optimism of the opening of the decade as a "false dawn".[65] Hooker's sceptical view of the Devonshire Commission turned out to be accurate in many respects. Hooker and Huxley were in control of the Royal Society executive at a time

when the latter's "episcopophagal" powers were at their height. Regardless of this the late 1870's witnessed the conduct of science being impeded and its autonomy threatened. The attitude of the State and the general public towards science never began to approach the zealous vision which the scientific naturalists projected as its future. To them, science was not reducible to a mere collection of techniques. As Turner has emphasised, scientific naturalists "deliberately equated the progress of science with the march of civilisation".[66] Despite the fact that the endowment of science by the State on a national basis did not take place in the 1870's in accordance with the reports of the Devonshire Commission, the Huxleyites were far from happy. They appear even to have felt threatened by the relatively small amounts of Treasury funds which continued to reach the tiny establishment of British science. This was presumably because for them the complete autonomy of science was paramount. The scientific naturalists dogged insistence on the autonomy of science at virtually any cost is explicable in two distinct ways. Undeniably there are clear reasons (given above behind their attempt to distance science from the unabashedly jealous contingencies which motivate party political interests. What is never suggested in the Huxleyites own accounts is the extent to which their consistent jealous concern for the autonomy of science served their own interests as its leaders. Their refusal to compromise over the dilemma which linked the endowment and control of British science undoubtedly contributed in large measure to keeping it poor during the last quarter of the nineteenth century. In marking out all party political interests as corrupt, the Huxleyites inevitably attributed altruism to their own position. Frank Turner rejects the version of its predicament which the "Young Guard" of British science promoted during the last third of the nineteenth century. It is clear that many of the Huxleyites'

opponents perceived the extent of their material and social ambitions. Confusingly, much of the resentment of these comprehensive Huxleyite aspirations was expressed in the narrow terms of the religious debate surrounding Darwinism. The pivotal place which the relationship between science and government held in the overall outlook of scientific naturalism is highlighted frequently in the existing evidence of this period. The attitudes usually taken up by Foster, Hooker, and Donnelly when the issue of government patronage was to the fore are marked by deep suspicion, extreme caution and virulent probity, at least in their public dealings. There is some evidence which suggests that despite the Treasury's ingrained partiality for retrenchment under such chiefs as Ralph Lingen, more money might have been available. The divisions within the Victorian scientific establishment were deep and complex. They undermined the initially limited confidence which successive governments placed in science despite the efforts of the various factions to maintain to preserve a public image of amicable disinterestedness. The astronomer David Gill was working on a star catalogue of the southern skies in 1886 when he wrote to Stokes at the Royal Society touching on this question. At the time Gill was being financed by a sum of £300 from the Society's Government Grant. From the Cape of Good Hope he offered the following opinion to Stokes:

> "I think there is not the remotest chance that parliament will challenge the proposal of the Treasury to vote £500 for observing the eclipse. Members have told me over and over again that so long as scientific men are agreed as to what they want, and the Treasury can be persuaded to agree with them, that Parliament will always support votes for scientific purposes. They only wonder that we don't ask for more!"67

The professional scientist occupies a weak position in trying to maintain control over his own situation let alone the establishment of science in general. The imperative need for government finance has

provided an ever present source of trouble to those for whom a strictly determined relationship with party political interests was important. The positions taken up by the scientific naturalists during the 1870's and 1880's mostly proceed from this one simple fact. The facts of the dilemma involving the need for government money and the ambition for private professional control of science were only beginning to be learned. The campaigning zeal with which the Huxleyites promoted their identification of scientific progress with civilisation appears remarkable today. Even as recently as thirty years ago, this would surely not have been the case. Science is currently in bad odour for a number of well-known reasons. This very pervasive reinterpretation of the meaning and worth of the scientific enterprise inevitably colours the current historical appreciation of the world view and endeavours of the scientific naturalists.

At the zenith of Huxleyite influence any incursion into the scientific world from that of politics was reviled as an offence and an outrage. As the government patronage of science rarely consisted of an unconditional offer of money, such strong feelings found frequent occasions for their expression. The Royal Society's Treasurer John Evans received a robust version of this outlook from Huxley in 1886. The extract below forms a digression prompted by the willingness of Ray Lankester and the recently formed Marine Biological Association to become involved in the Government's Fisheries Inspectorate.

> "I have heard a good deal lately of the history of the various surveys and other state organisations for scientific work in the United States - and the dirt thereof is enough to make one sick - I do believe that State science is capable of becoming even more corrupt than a State church and that is saying a good deal for it. As for the American fish connection, I have often testified to the good work it has done. But it also has a shady side and in the hands of a less moral man than Baird it might play the deuce. I should think it my duty to leave no stone unturned to prevent the establishment of anything of the kind in this country."68

As has been outlined earlier, the 1870's seemed to hold for the Huxleyites the promise of a coming re-establishment of British science in the shining image of their technocratic utopia. The failure of their ambitions was not brought about by repressive power of the pulpit. Whatever it amounted to earlier the potency of this opposition had passed away by this time. Some of the most significant religious opposition to Darwinism came from within science itself, promoted by the Cambridge mathematical physicists.[69] The decline of the power of scientific naturalism appears to have been less a consequence of the theological debate than the fierce opposition of the Huxleyites to any relinquishment of their power in favour of their governmental paymasters. So far as the scientific naturalists were concerned, anti-climactic feelings must have been inescapable at the close of the 1870's. The Devonshire Commission had brought forth little whilst physiological science had become increasingly beleaguered by the anti-vivisection movement. Physiology was the context in which the careers of a number of leading scientific naturalists took place. The anti-vivisectionists' efforts culminated in the Cruelty to Animals Act of 1876 which imposed quotas and licencing on the practice of physiological science.

In the same year the annual Government Grant to the Royal Society was augmented by a further £4,000 which was to be allocated according to the opinions of the Society's appointees. Money for research was desperately needed, but the fresh resources brought little joy to the Huxleyite executive. They saw the money as a snare which might lead to direct party political control of science.[70] The specialised Government Grant Committee Boards were empowered to allocate "personal grants" for the subsistence of researchers for the first time. Despite the commitment of the mid-century statute reforms to democratise science, and the explicit steps taken by Hooker and Huxley to keep the Society's

Presidency open to "poor men", the new personal grants were viewed with the greatest misgivings. Their greatest fear was that the allocation of the new subsistence grants would be seen as corrupt and that young men of science would become mere vassals of government. It is ironical that the utopian designs of the new meritocratic scientists required a world stocked with the independent wealthy amateurs of a byegone age in order to flourish. Huxley warily tested Stokes with his distrust of the new Government Grant of 1876.

> "I don't know what your feeling may be about the administration of £4,000 - but I look upon it as about the most troublesome business that the Royal Society had yet undertaken."71

During the 1870's expensive solar eclipse expeditions were becoming an accepted call on the Treasury while Gladstone's Liberal Ministry was responsible for financing the "Challenger" expedition from 1872-74 at the rate of £3,000 per annum. Twenty years later the Society's officers were still dealing with the vexed question of "honoraria" paid to various contributors to long overdue sections of the voluminous expedition report.[72] At several times during their overlapping incumbencies, the Huxleyite officials of the Royal Society issued formal statements clarifying the rôle of the Society as trustee of the grant rather than its beneficiary. The treasury consistently construed the Government grant as an allowance for the Society itself. When applications were made for special projects, Treasury officials nearly always suggested that the annual grant was available for such purposes. The Council Minutes of the Royal Society for 1869 contain a reference to a letter received by the Council from a group of leading Huxleyites. The letter urged that the Society should formally repudiate the official implication that £10,000 which had been paid by the Government for the conduct of meteorology represented any sort of "boon or favour" to the Society.

The New President: 1878

Joseph Hooker's wife died suddenly in November 1874. Stokes decided to make a necessity of the virtue of democratic openness which he had failed to enact on a number of earlier occasions. Stokes had realised that his opponents were in power. Those opponents naturally endeavoured to bring about the election of individuals who were not congenial to a man of Stokes' background. Having mentioned Spottiswoode, the Senior Secretary suggested the compromise of placing his own ally [de la Rue] in the vacant Treasurership if Spottiswoode was elevated to the chair. Stokes' position, confronting as it did a solidly Huxleyite executive, was an awkward one which he was ill-equipped to deal with. His letter continued:

> "I don't think the officers ought to take the initiative by proposing one of their own body; nor do I think they ought to do covertly - by prompting one of the members of the Council - what would not be proper to do openly."73

Evidently Stokes intended to revive the ostensibly democratic function of the Council. The last time that the President had been chosen by the Fellows predated Stokes' letter by nearly half a century. In the election of 1831 Augustus Frederick, Duke of Sussex, defeated John Herschel as the nominee of the "declinist" reforming party. Such contested elections were considered to be symptomatic of a breakdown in the proper running of the Society throughout the nineteenth century. Overt divisions of opinion were generally held to be incompatible with the Royal Society's durable self-image as an institutional analogue of the doctrine of the universality of science. The Huxleyite embrace of notions such as "scientific statesmanship" (which was to operate through the exclusive power of science to reveal truth) made the appearance of unity even more important during the last third of the century. Stokes asked Huxley to ensure that an interval would separate the announcement

of Hooker's intention to resign and the election of his successor. In the meantime Stokes wanted no reference to the resignation to appear in the Council minutes and no mention of it to be made to the newspapers. Stokes' intention was to prevent the hapless Council from being presented suddenly with the executive's usual fait accompli. By allowing the Council a genuine interlude before the choice of a new man, Stokes hoped to thwart the smooth installation of the next Huxleyite nominee. Coincidentally the purportedly democratic function of the Council would be "re-established".[74] Stokes was to be disappointed in this. Spottiswoode was nominated for the chair and with a final turn of the screw, John Evans was made the new Treasurer. Evans proved to be a reliable ally of the Huxleyites. He battled against the stolidly conservative influence of Stokes, in support of Michael Foster during both Stokes' Secretaryship and Presidency. In 1878 Stokes was unwilling to seek the chair himself because it would have meant the forfeit of his Secretary's salary. The position of President was not only unpaid but actually entailed some outlay. Two years earlier Stokes had consulted privately with Hooker about the possibility of augmenting his income by becoming a member of the Meteorological Council. At this time Hooker had been eager to enhance the prestige of scientific meteorology and hinted to Stokes that a few busy years on the Meteorological Council might well result in the creation of an appointment for him as "a highly paid Scientific Director".[75]

At the time of Airy's withdrawal, the opponents of scientific naturalism favoured the 7th Duke of Devonshire for the Presidency. Stokes was clearly committed to the "iron duke" in 1873 as he had been at the time of Sabine's departure two years earlier. To his father-in-law the Reverend Thomas Romney Robinson, Stokes made the following observations in a letter dated the 1st December 1877:

> "Among other solutions there is one that will occur to everybody. For a combination of exalted social position with the highest moral and intellectual qualities the Duke of Devonshire stands pre-eminent. He is universally respected and if we come to have a nobleman at all I think that he is par excellence the man."76

Second Wrangler and First Smiths prizeman of 1829, William Cavendish the Seventh Duke of Devonshire was nearly as taciturn as Stokes himself. The Duke had vastly augmented the already large wealth of his estates by developing the smelting of rich haematite iron deposits near Barrow. Hence his acquisition of the nickname normally associated with the Duke of Wellington.[77] It is clear that the mathematical physicists were by no means averse to the prospect of a noble industrialist as their President. The amateur interest in science which the Duke indulged himself in having performed notably at Cambridge in the Mathematical Tripos, was thus preferred to a man of practising professional position within science by a significant proportion of the Fellowship. Ironically, the man smoothly installed by the Huxleyites in the Duke's stead in November 1878 was comparable with him in several respects. William Spottiswoode's impressive social connections in Victorian society have already been noted. He was wealthy and had direct industrial involvements through manufacturing. His basically amateur scientific pursuits had begun, like the Duke's, in an original academic concern with mathematics. Spottiswoode was a genteel exponent of scientific naturalism who might almost have been calculated to give the least possible offence to the forces of reaction within science.

In the course of his Presidential Address in 1880, Spottiswoode remarked that he had often heard complaints about the lengthy period of office of individuals elected to serve on the Council. Showing a considerable degree of either naiveté or brazenness, the President went on to describe the manner in which Council procedures facilitated the

oligarchical rule of the Society's Officers. The Royal Society's charter dictated that ten Council members retire every year. By custom six of these were selected according to seniority with the other four being determined according to a rank order of attendance at the meetings. The President continued:

> "Experience, however, appears to show that for a member serving on the Council for the first time, there is so much to learn . . . that his first year is occupied quite as much in ascertaining his duties as in actually performing them. This objection is in some degree met by selecting for the ten incoming members five who have served before and five who have not so served."78

It is not difficult to see how the domination by the officers of the self-electing Council became greater during the second half of the century despite the Statute modifications which took place at its midpoint. The representatives of the power-holding group were not blatantly foisted on the Society en masse, but consistently reintroduced into the Council as individuals over many years. The transitory residuum of novice Council members who served for only one two-year term or two terms separated by many years are a singular feature of the Council lists covering the years 1870-1900. The ruling group was thereby able to maintain its hold over the conduct of the Society and maintain its public image of regulated fairness. Spottiswoode's speech blithely pointed to the enormous influence of the Society's executive as having been almost an accidental by-product of the radical propriety of Council election procedures:

> "I am aware of the great convenience attaching to our present impersonal mode of selecting the members to retire in each year. . . . But the great confidence which the Society has, especially of late years, placed in its most permanent officers and the power which naturally accrues to them from the comparatively short tenure of office by the other Memebers of Council, appear to me to be points of which the Society should not lose sight."79

Spottiswoode was, with Lubbock, of a far more elevated social background than the other X club members. Nevertheless Spottiswoode's outlook and values were typically Huxleyite. He shared with Hooker and Huxley their grave misgivings about the £4,000 addition to Government money which the Royal Society was responsible for allocating. Also in common with the leading Huxleyites, Spottiswoode fully approved of "electing from time to time men of eminent distinction in other avocations of life than those of strict science."[80] It has been asserted that Hooker's resignation in 1878 was prompted by his desire to set a precedent for five year presidential terms.[81] Spottiswoode took advantage of his Presidential Address in 1881 to tell his hearers that Hooker's resignation and that of Huxley from the Secretaryship were prompted by their unwillingness to spend more of their lives "an running hither and thither". As has been noted earlier, Spottiswoode was not himself intending to resign at the end of a five-year term in 1883. His sudden death from typhoid fever (contracted in Italy) in July of that year revealed basic divisions within the Huxleyite group. Disunity began to show over the question of Spottiswoode's burial. One faction was eager to press the case for his interment in Westminster Abbey. Huxley opposed the initiative but regretted the public display of division even more. The following extract from his expression of regret refers to the risk of damage to more than personal relationships:

> "It has long been too obvious to me that the relations of some of us at the X are getting very strained. . . . We shall smash the X completely if we get into public and open antagonism over this business - without doing any good that I can see . . ."[82]

Those who took Huxley's view believed that Spottiswoode's science alone did not merit a place in Westminster Abbey. There was a strong feeling that the bolstering of the deceased President's claim with references to his social position and philanthropic activities not only damaged

his reputation, but also tended to diminish the value of Darwin's burial in the Abbey a year earlier. The comparison between the two events is drawn in more detail in a later chapter.

Fending Off the Philistines

As has been described earlier, the apparently "strong" position of scientific naturalism within British science at the start of the 1880's was threatened, in the view of its leading practitioners, from a number of directions. Several of the perceived menaces were unconnected with the institutionalised conflict between the agnostic leaders of scientific naturalism and the Christian mathematical physicists. As might be expected, the leading figures within the two groups frequently adopted mutually antagonistic positions over a host of other disparate issues. The question of the autonomy of science formed an increasingly serious dilemma as the need for Treasury funds increased. The ultimate material and social ambitions of the scientific naturalists assisted in holding many of them wholely committed to autonomy. Occurrences such as the Ayrton incident, the opposition to the Government Fund of £4,000 and the furore over the Marine Biological Association's eager flirtation with officialdom reveal these tensions in operation. Only in the light of this background can the accession of Huxley to the chair in 1883 be interpreted accurately. Two years earlier he had given up the Secretaryship of the Royal Society. Failing health and a host of other calls on his time made this desirable. Huxley doubtless thought that with his faithful supporters installed in the executive - especially his protegée Foster replacing him as Secretary - his departure from the central position of the Society's affairs could be managed without penalty. After the President's shocking death in 1883 the whole situation was changed. Fresh threats brought new doubts. The wider programme which the scientific naturalists envisaged for the amelioration

of society required new expression independent of the self-evident success of Darwinism which had provided much of its early reforming impetus.

The X club members were at the height of their influence but far from being secure from dangerous attack. Rumours of private work being conducted at Frankland's South Kensington laboratory were again being circulated in the early months of 1883. The Science and Art Department's Director John Donnelly was in contact with Huxley on the matter throughout this period. Another potent threat to the freedom of science was posed by the anti-vivisection movement which remained an active force in the 1880's. From 1876 licencing and the allocation of quotas to practitioners of vivisection were a constant reminder of the power which others had to constrain their ambitions.[83] Michael Foster occasionally aspired to utilise the position of the President of the Royal Society, George Stokes, as the Member of Parliament for Cambridge University. As will shortly be made evident, Huxley and Foster heartily disapproved of Stokes' dual rôle. Nonetheless Foster sometimes requested his co-operation when the vivisection question was due to come up in the Commons. As it turned out he might have saved himself the trouble. Stokes did not speak in the Commons despite an exemplary attendance record, and made no exception for matters connected with science. On one occasion Foster had heard it rumoured that trouble was afoot.

> "the anti-vivisection people are going to be troublesome in the House on Monday. . . . I will look in at Lensfield [Cottage] on my way in one Monday morning."[84]

It should also be recalled that at this time the basic conflict underlying the Ayrton controversy had in no sense been resolved. The full extent of what the Huxleyites regarded as official carelessness of scientific authority and independence was given a further demonstration in 1881 over the question of lighthouse illuminants. On March the 18th

of that year John Tyndall resigned from his post as advisor to Trinity House. Tyndall resented what he saw as the discounting of scientific truth by corrupt politicians for their own base motives.[85] On a broader front than these isolated dealings, the Huxleyites still aspired to the interrelation of science and the State, but on a strictly devised basis of their own choosing. Huxley was involved with the Government at this time, in vital dealings concerned with the emergence of the Normal School of Science at South Kensington. Huxley's intention was to redirect the underlying rationale of the institution by liberating it from what he saw as the cramping association with the Royal School of Mines. Controversy linking science and Government was frequently focussed by the problems thrown up by the running of the Natural History Department of the British Museum. There was trouble over the removal of the specimens from Bloomsbury to the new building in Cromwell Road at South Kensington. This was begun in the summer of 1880 and completed three years later.[86] In all these complicated dealings, Huxley's central concern was that science should at no level and in no department be taken out of the control of its own leading practitioners and internal managers. He wished to avoid at all costs the reduction of scientists to the status of mere salaried technicians working at the behest of party political interests. The logic of the situation did not impress itself so clearly on many of his circle. Frankland's blithe recommendation of a "Member of Parliament for the Royal Society" (Stokes) and Lankester's easy endorsement of an ill-defined rôle for a new Government Marine Biologist reflect a lack of common purpose on this issue. When Spottiswoode's death prompted the need for decisive action, Huxley made his position clear to Hooker:

> "As I think I told you before now - I do not think it is desirable that anyone so closely connected with the Government as I am should hold the post [of P.R.S.] and personally I do not desire it,

> having more than enough worry and distractions already. Who have you to suggest? The only thing I am clear about is to keep out traders on the one hand and mere noblemen on the other."[87]

Huxley discounted the other available members of his coterie as serious candidates. Having been urged on by the younger men "not to leave the place open for the Philistines" Huxley forced his resolve to "take a header and have done with hesitation."[88] Between the two of them Huxley and Foster could easily manipulate the tactically naive Stokes. Evans' vain ambition to adorn himself with the Presidency was frustrated by Huxley who had only to ignore it. The Council met and unanimously elected Huxley on the 5th July 1883. The new incumbent frequently denied that the personal prestige conferred by high office held any attraction for him. The suspicion naturally aroused by such robust protests seems to be justified in this case. Three days prior to the crucial Council meeting Huxley addressed an archly phrased letter to his intimate friend Hooker. The letter contains Huxley's reaction to a considerable jolt recently given to his self-esteem by Hooker over the burning question of the selection of a new President:

> "and if, as you seem to think the interests of the Society would be as well-served by his [Evans] occupation of the Presidential chair as by my taking the post - I don't see why I should make the sacrifices which are involved in my accepting it. Vanity blinds us all and I really did not think he and I were exactly on the same scientific level in that there was any question of the presidency 'lying between us'. I withdraw from the competition altogether having never intended to enter the lists. In fact I should never have thought of the Presidency if some of the younger men of science had not brought pressure to bear on me."[89]

Hooker replied patiently in soothing tones. The temporary Presidency which Huxley accepted on the 5th of July amounted, in practical terms, to no more than a holding action. After the summer vacation speculation about the nomination of a full-term President began almost

immediately. Huxley's technique was very simple and consisted of trapping Stokes - the only other serious candidate - by the implications of his own reputation for modesty. Michael Foster's outlook was one of relaxed confidence when he addressed a letter to Huxley on the 22nd of September 1883:

> "I had half a mind to come up and talk to you about the P.R.S. - but my natural indolence conquered and I did not. I had a talk about a week or so ago with Gabriel. . . . If Stokes stands I shall be in despair - but otherwise there is nothing for it but for you to take it."90

Three days later Foster wrote to say that he would "see Stokes and run him in a corner."[91] On October 2nd Foster went to London from Cambridge specifically to tell Stokes that the three other officers including himself, were backing Huxley. The Royal Society had for many years conformed to the convention of avoiding any sort of contest for the Presidency. The method used by the currently dominant faction was invariably the same. In this case the leading Huxleyites informally established their own candidate then prepared for the stage management of the position of any rival so that his motive could only appear disruptive and ambitious.

It would be natural to assume that after twenty-nine years as Secretary of the Royal Society, Stokes might have garnered some insight into its inner workings. This appears not to have been the case. Stokes was an unusually artless man neither equipped for nor inclined towards involvement in the Society's worldly affairs. After Stokes' death, Foster diplomatically wrote that his fellow Secretary had been exclusively concerned with the Society's scientific papers.[92] The distinction between the physical and biological secretaryships was for the most part a convenient fiction. The allocation of the two posts was used to placate rivalries between the increasingly divided specialists in physical and biological sciences. Stokes devoted himself almost exclusively to a postal handling of the internal scientific work of the

Society. During their nine years as co-Secretaries, Huxley had dealt with the external scientific concerns such as the "Challenger" expedition. Huxley also took on most of the Secretaries' share of the Society's domestic work which was also contributed to by Evans the Treasurer.[93] Stokes' influence over the papers by direct intervention and his policy regarding referees was enormous. At the same time his perception of the internal politics of the Society seems to have remained negligible. On St. Andrew's Day 1883, Huxley took the chair as the Society's new "full term" President. Having agonized over the need to keep the position open to "poor men", Huxley in the end excluded a rival in Stokes who was neither so well-off financially as himself nor remotely aristocratic. When elected in July as interim President, Huxley had rather truculently informed Hooker that his election proved:

> "that a poor man - who does not mean either to entertain or be entertained one whit more than before - can hold the post."[94]

Huxley's Presidency

In the summer of 1882, Francis Balfour died in the Alps. The loss of his scientific heir seems to have prompted Huxley to try to marshal his extensive activities. The death of Spottiswoode and his own election to the Presidency brought an end to such aspirations. Ailing and already planning a recuperative trip to Italy prior to his installation as President in 1883, Huxley conducted his two-year term largely by correspondence with Foster from distant European resorts. Evans delivered the President's Anniversary Address for 1884. Foster told his chief in early December 1884 that in his view the Treasurer's smooth performances as Vice-President had "very much strengthened his position in view of the Presidentship."[95] This notion naturally appealed to Foster who was less than enthusiastic about the prospect of serving

under Stokes as the next obvious incumbent after Huxley. Foster and Donnelly co-operated to secure official sanction for Huxley's absence from South Kensington. Under the threat of a complete breakdown (the never distant worst peril for the busy Victorian valetudinarian) Huxley set off for Italy. The dire prediction which his doctor Sir Andrew Clark had made for Huxley himself was soon overshadowed. At the railway station en route for Italy he learned of his daughter Mrs. Marion Collier's serious illness. Huxley made his way to her residence at Lucerne. Her condition eventually proved to be fatal.[96] Foster's methods of communicating with his chief during this absentee Presidency indicate Huxley's strange state of mind as well as his former protegé's staunch loyalty. Following the opening of the new session at Burlington House in 1884 Foster would not directly address Huxley on the subject of London business and sent the following letter to Huxley's wife Henrietta on November 21st 1884:

> "The General may like to hear - provided that the letters R.S. do not bring on nausea - in which case you will hide all this from him, that our first R.S. ordinary meeting passed off very well. Evans seemed to enjoy sitting in the big chair his eyes beaming over and through and beneath his spectacles - & Gabriel sat with pursed lips and nose cutting the air - and the twin brethren Ayrton and Perry came in together and sat at their old seat - and in fact everything went on as usual. . . ."[97]

Huxley was never willing to accept a man of Evans' dilettante accomplishments as a prospective President of the Society. Evans was connected through his wife and mother to a paper-making business. As a prominent numismatist his interests were largely antiquarian and these he indulged more fully following his semi-retirement from the firm in the 1880's. The Royal Society's pride in its purely scientific constitution during the years after the mid-century reforms did not prevent Evans from succeeding Spottiswoode in the office of Treasurer in 1878 Despite Foster's optimistic promotion of Evans as a pliant future

President, he was not generally popular among the Huxleyites. Hirst had little patience for the urbane Treasurer's manner. After a meeting of the Philosophical Club in 1886 Hirst wrote in his Journal:

> "Evans was as usual full of witty stories and repartee. It becomes wearisome. He is too clever by half. There is no true humour in him. He is simply sharp. One becomes weary of his cleverness."[98]

Evans was a staunch Tory but not a party man. According to the Dictionary of Business Biography he distrusted all politicians, especially Gladstone. This background fitted Evans well to be a supporter of Huxleyite principles. Foster's carelessness of Evans' glaring lack of scientific credentials as a prospective President for 1888 would seem to indicate that he was used as a lackey ever since acquiring the Treasurership in 1878. Evans had never fitted into the social orbit of the X club. In 1887 Hirst recalled "how he used to weary and worry my poor Lady Lubbock."[99]

When Huxley's failing health forced his retirement in 1885 he did not support Evans as his successor. At this, his circle realised that they "had no other man". This predicament was similar to that suffered by the mathematical physicists two years earlier when their reliance on Stokes had been complete.[100] It is remarkable how little special scientific eminence was associated with a number of leading Huxleyites. Foster expressed his regret at the tiny amount of original work he was able to accomplish due to the teaching and administrative duties imposed by his establishment of a school of physiology at Cambridge. William Sharpey had produced seven original papers during his career whilst Müller, one of the main inspirations of the new English schools of physiology, had produced 300. Huxley, Hooker and Dyer were largely taken up with running scientific institutions. When it became known that Stokes was destined for the chair in October 1885 no opposition

was presented. Lord Rayleigh moved with a similar lack of hindrance into the vacant Secretaryship. There is no mistaking the suddenness with which Huxleyite domination of the executive had been brought to an end. As will become clear, Huxley's influence remained as a major force working through Foster for a number of years. Nonetheless the days of wholely self-confident power-broking were over. Even prior to 1885 the Council had not always behaved submissively towards the upper management. Foster wrote in December 1884 to Huxley in Italy where he was unsuccessfully convalescing.

> "I forgot to tell you that the stupid Council chose Dana and Cornu for For. Memb. We [had] von Baeyer and Kowaleski [in] a tie or nearly so three times and then gave it up. We have now two vacancies and we propose very soon to fill these up with von Baeyer and Kowaleski."101

George Gabriel Stokes P.R.S. M.P.

By 1885 the scientific naturalists of that movement's brief heroic age had spent most of their force. Stokes' scientific eminence was such that with Huxley out of the way, his precedence could not be denied. The first effect of Stokes' election to "Newton's chair" (as it was portentously celebrated by some contemporaries) was a great upheaval in the method of dealing with the scientific papers sent to the Royal Society. Stokes had dealt with them in his own peculiar way for thirty years, developing a vast correspondence. As will be recalled from the preceding chapter dealing specifically with the papers, Stokes engaged actively in the modification and development of papers which he liked. Recalling Stokes' obsessive diligence in this regard, the telescope maker Grubb related the following impression to Stokes' obituarist in 1905. According to Grubb, Stokes' letters were written:

> "sometimes in railway trains, sometimes at the R. Society and sometimes at home. He was wonderfully painstaking in answering any queries, so much so that I sometimes hesitated to ask him even a simple question fearing it would encroach upon his time

> for he went so deeply and so minutely into every aspect of the question that in some cases I had as many as five postcards or letters from him in 24 hours each describing some new view of the particular subject I had enquired about."102

Stokes' simplicity in nearly all non-scientific matters made his concentration on the papers a fortunate match with a rational division of labour. Despite this happy congruence his time was mostly spent in Cambridge. His unavailability for any more thorough involvement with the running of the Society must have been a welcome long-term boon to the Huxleyites. In 1883 Huxley emphatically dismissed the prospect of Stokes as the new President: "the best of Secretaries, as President he means stagnation or retrogression."[103] Here is unequivocal evidence of Huxley's unrealised programme of change for the Royal Society.

Historians have paid little attention to Stokes' career at the Royal Society. Considering the extent of his influence over the papers for so many years, an examination of some of his background would seem to be worthwhile. In the course of a discussion of Stokes' religious thought, David Wilson suggested that:

> "Perhaps even his supreme conscientiousness in pursuing his duties as Secretary of the Royal Society and in corresponding with those seeking advice reflected an Evangelical seriousness of purpose."104

There may be some truth in Wilson's contention, but it is clear from the correspondence itself that Stokes had in the past been driven by more immediate and material considerations. When he had held the Secretaryship for just a few years some influential Fellows registered their disapproval of his residence at Cambridge. Late in November of 1860 the Society's Treasurer Edward Sabine wrote to the Senior Secretary William Sharpey about a letter that he had received from the President Benjamin Brodie. The letter was actually written by Brodie's wife because he himself was suffering the loss of sight which was shortly to bring about his retirement from the chair and the advent of

Sabine as President. Brodie pointed out that Stokes' late assurance that he would stay overnight in London on Thursday nights in order to make himself available at Burlington House during Fridays had not been carried out. The undertaking had been forced on the Junior Secretary by an agitation set up by Charles Babbage on the occasion of application being made for an increase in the salaries of the Secretaries. Brodie's letter continued:

> "even this plan has not been carried out and I have reason to believe that much discontent on the part of the Fellows has arisen in consequence. . . . Mr. Weld informs me that Babbage has been occupied in looking over the minutes relating to the Secretaries; and I suspect that not only he, but others may be inclined to bring the subject of the imperfect performance of the duties of the Junior Secretary before the Society at the Anniversary Meeting."105

Sabine and Sharpey were united in the wish for a statement of Stokes' commitment to the reform of his conduct. They wanted to be able to pre-empt any attack at the Anniversary Meeting by Stokes' reading of his own statement early in the proceedings.[106] Stokes' reply to the President was a protest of innocence. His statement confirms that the Junior Secretaryship had been almost exclusively taken up with the Society's scientific papers since his appointment in 1854. Stokes' meticulous care (and often substantive involvement) with the papers was continued until he was elected to the Presidency in 1885. The long-serving Assistant Secretary Walter White retired during December 1884. His successor Herbert Rix was a graduate of London University who had previously worked for the Society in a more menial capacity. When Stokes' election to the Presidency was decided, the potential workload for the new Assistant Secretary reached alarming proportions. Huxley wrote to Foster from Rome on the 8th of January to stress his disapproval of the amount of time spent by the Secretaries on the drudgery of proof-reading. Huxley thought that the Assistant Secretary should do this work.

> "People grumble at the delay in publication, and are quite right in doing so, though it is impossible under the present system to be more expeditious, and it is not every senior secretary who would slave at the work as Stokes does."107

In the attempt to give his long distance pep talk as much currency as possible, Huxley wrote to Evans on the same day about the folly of "going on cutting blocks with our Secretarial razors."[108] Foster's reply highlights the blinkered isolation of Stokes within the existing group of Officers. The relationship between Foster and Stokes was to get a good deal worse before long.

> " - Gabriel seems anxious not to lessen his proof-reading duties - I have warned Rix that I shall use him a good deal but I doubt if GG will much."109

Stokes' term as President was not relished by the Huxleyities. Twenty years earlier when Sabine was surveying likely Presidential prospects of the future, he discounted Stokes despite his scientific eminence. The two men also had a good deal in common on the counts of politics and religion. Sabine told Walter White that Stokes "has no governing faculty - besides his pecuniary resources are insufficient."[110] Five years later Sabine was still President of the Society but a significant development was indicated by Huxley's selection for the Presidency of the British Association meeting to be held in Liverpool. In November, some two months after the meeting, a storm suddenly blew up over Stokes' conduct as the representative of the Royal Society over the matter of Government money which was wanted quickly to finance a solar eclipse expedition. Stokes' inability to operate effectively in science's internal political domain was emphatically confirmed on this occasion. On the 11th of November, Huxley sent him a robustly phrased letter informing him of the emergency meeting of the B.A.A.S. Council which his conduct had occasioned.

> "My dear Stokes, I am afraid that you and I have very different notions as to the proper way of

> transacting public business. If I were to follow your advice namely to treat a formal and well considered resolution of the Council of the British Association as if it had a meaning which everybody concerned in passing it well know to be the reverse of its real signification I think I should commit a serious breach of my duty. . . ."111

Thomas Hirst was one of the Secretaries of the Association at the time and entered an account of the special meeting in his Journal. It took place the day after Huxley's communication to Stokes was written.

> "Stokes . . . mismanaged the matter deplorably and moreover treated the British Association in a manner which could not be permitted. Huxley made a temperate but in reality scorching exposé of the whole affair. Stokes was present but hardly said a word either in excuse or otherwise. In consequence of his remarkable sang froid or perhaps peau epais no storm occurred. Had Stokes taken any other course he would undoubtedly have heard unpleasant things, for his conduct had been both dictatorial and exceedingly ill-advised."112

In 1870 the Huxleyites had not yet achieved real power in the Royal Society. Not surprisingly they were more than willing to mobilise the British Association as a rival platform from which to attack reactionary influences within the Royal. The postscript to Hirst's Journal entry suggests the involvement of Sabine as President of the Royal Society in Stokes' conduct.

* * *

Notes

1. Nature, September 8th 1870.

2. Stokes Papers (CUL), RS 755. Once the Huxleyites were established in power at the Royal Society, this sort of thing ceased. It began again fifteen years later when Stokes entered Parliament whilst serving as P.R.S.

3. Barton, p. 145.

4. White's Journals, p. 210.

5. Ibid., entry for 31st October 1868.

6. Ibid., p. 139. Sir Benjamin Collins Brodie was P.R.S. 1858-61.

7. Lyons, Stimson, Hall et. al.

8. A. Geikie, Annals of the Philosophical Club, London, 1917, passim.

9. Ibid., p. 123. Royal Society, Manuscript General Series Catalogue.

10. Leonard Huxley, Life and Letters of J.D.H., vol. 2, pp. 223-224. Francis Darwin, Life and Letters of Charles Darwin, John Murray, London, 1887, 3 vols, p. 247. The X Club members nearly all had experience of scientific journeying in their early careers.

11. Marie Boas Hall, "The Royal Society in Thomas Henry Huxley's time", Notes and Records of the Royal Society of London, March 1984, 38, no. 2, pp. 153-159, ff. p. 156.

12. M. J. Bartholomew, "The Award of the Copley Medal to Charles Darwin", Notes and Records of the Royal Society of London, July 1975, vol. 30, no. 1, pp. 209-217.

13. Hirst Journal, 1884.

14. Ibid., 1886.

15. Stokes Papers (CUL), S 587. Lubbock was also proposed for the Office of Treasurer. These criteria for high office within the Royal Society, founded as they are on the value of wealth, cultivated taste and social élan, are not the criteria one would initially anticipate some twenty-seven years after the alleged demise of dilettantism.

16. Stokes Papers (CUL), RS 764.

17. Royal Society, MC 5, no. 181.

18. White's Journal, p. 218. It seems likely that the opposition between the young Huxleyites and the Humboldtians (such as Sabine, Murchison) was partly generational, as well as ideological and theological.

19. Ibid., p. 228.

20. Marie Hall, All Scientists Now, Cambridge University Press, 1984, pp. 106-107.

21. Hirst Journal, 1887.

22. A. Harrison, "Internationalism in Science", unpublished M.Sc. thesis, 1982, UMIST.

23. Royal Society, MM 19, no. 24.

24. Royal Society, MC 7, no. 303.

25. Royal Society, PLB 1840-79, (426), no. 241.

26. Hirst Journal, 1922.

27. White's Journal, p. 236.

28. Leonard Huxley, LL THH, vol. 1, p. 356.

29. White's Journal, p. 232.

30. Royal Society, MC9, no. 181. The R.S. executive as a whole shared in the disparagement of Airy for not adopting the pre-1847 outlook of noblesse oblige. This attitude was clearly marked in Sir Edward Sabine who, despite having been closely identified with the democratic reform of the Society, had wanted Lord Salisbury to succeed him in the chair (White's Journal, p. 235). When Helmholtz visited London in 1853 he found that Airy's home circumstances were arranged "in style, but it is so with most of the English professors" (Leo Königsberger, Herman von Helmholtz, Clarendon Press, Oxford, 1906, pp. 111-112). Airy had a well-deserved reputation as an autocrat and eccentric. According to Lockyer, Airy once arrived in Dublin with no money and, finding his way to the doorstep of a fellow mathematician, he sought to establish his identity by inviting questions of a mathematical nature (Hirst Journal, 2845).

31. Stokes Papers (CUL), A 274.

32. Roy M. Macleod, "Science and the Civil List 1824-1914", Technology and Society, 1970, 6, pp. 47-55, ff. p. 49.

33. R. M. Macleod, "The Support of Victorian Science: the Endowment of Research Movement in Britain 1868-1900", Minerva, 1971, 9, p. 224. Airy was always very sensitive about money and his own antecedents. Brought up on the farm of his uncle Biddell at Bury St. Edmunds, he saw agricultural associations as a grave flaw in the scientific personae of others. This came out in his attitude to William Hopkins, the prominent Cambridge mathematics coach, whose wife was being petitioned for in respect of a government pension in 1871 (White's Journal, p. 236. Stokes Papers (CUL), A 373).

34. Stokes Papers (CUL), S 589.

35. Stokes Papers (CUL), S 589.

37. Royal Society, NLB 5, p. 448.

38. Stokes Papers (CUL), A 442.

39. Huxley Papers (ICL), 4.62.

40. Stokes Papers (CUL), RS 932.

41. Hirst Journal, 1963.

42. Ibid., 1966.

43. Leonard Huxley, Life and Letters JDH, vol. 2, p. 132.

44. Huxley Papers (ICL), ibid.

45. Ibid., 4.64.

46. W. B. Turrill, Joseph Dalton Hooker, London, 1964, p. 164. Mea Allen, The Hookers of Kew, London, 1967. Hooker made a point of writing his own speeches for the Anniversary Dinners of his Presidential term. This was a marked contrast with previous practice (Leonard Huxley, Life and Letters of JDH, vol. 2, p. 140).

47. Marie Hall, ASN, p. 145. Hooker reduced the membership fee for Fellows of the Royal Society, inaugurated the new form of soirée, tried to rationalise administrative procedure, and resigned after five years.

48. Leonard Huxley, ibid. Hooker was widowed during his Presidency of the R.S. and needed new use for his time. The first Lady Hooker had been Frances Henslow. She died suddenly in 1874 at the age of 49. Hooker later married Hyacinth Jardine, widow of Sir William Jardine (Mea Allen, ~~ibid.~~).

49. Huxley, ibid.

50. White's Journals, p. 264.

51. Huxley Papers (ICL), 3.227.

52. Leonard Huxley, Life and Letters JDH, vol. 2, p. 136.

53. Within the X Club itself Frankland was well-known to be a "man of coin", Tyndall was preoccupied with the interests of the Unionist Party, and Spottiswoode lived in the aura of London high society.

54. C. Chant and J. Fauvel (eds.), Darwin to Einstein: Historical Studies on Science and Belief, Longman/The Open University Press, 1980, pp. 56-62.

55. Nature, January 29th 1880, p. 295.

56. Huxley Papers (ICL), 3.144.

57. Leonard Huxley, Life and Letters JDH, vol. 2, p. 161.

58. Huxley Papers (ICL), 8.131.

59. William Irvine, Apes, Angels and Victorians, Wiedenfeld and Nicholson, London, 1955, p. 274.

60. Leonard Huxley, ~~ibid.~~, vol. 2, p. 137. Northcote (1818-1887) was a Conservative statesman who rose from the Presidency of the Board of Trade in the Earl of Derby's Ministry of 1866, to the Chancellorship in 1874 when Disraeli began his term as Premier (DNB). Tyndall referred to Northcote as "that amiable mollusc" (Hirst Journal, 2341).

61. Huxley Papers (ICL), 3.206. Many years earlier, Hooker had developed a highly suspicious attitude towards officialdom. In 1858 he wrote to Huxley expressing his anxiety about the future of Kew. He said that he would do "anything to keep out of the K. Gore [Department of Science and Art at Kensington Gore] peoples clutches." (Ibid., 3.36).

62. Marie Hall, ASN, p. 96-97.

63. Royal Society, CM, 1875.

64. George Stokes, the junior Secretary, was committed to his evangelical faith. Edward Sabine as President of the R.S. was firm in following out the implications of his Christianity. William Allen Miller, the Treasurer until 1870 was a steadfast believer. His namesake then occupying the Foreign Secretaryship was a believer also.

65. Frank M. Turner, "Public Science in Britain 1880-1919", Isis, 1980, 71, p. 591.

66. Ibid., pp. 591-595.

67. Stokes Papers (CUL), G 147.

68. Royal Society, MM, VIII-IX.

69. There is no sense in which Darwin is here being closely linked with the rise of technocracy. The Huxleyites however were starkly technocratic in outlook.

70. Leonard Huxley, The Life and Letter of JDH, vol. 2, p. 230.

71. Stokes Papers (CUL), RS 883.

72. Royal Society, NLB 10, 467.

73. Huxley Papers (ICL), 30.180.

74. Ibid. Stokes' inclination to open and democratise the procedures of the Council had not been consistently acted upon by him in the past. On numerous occasions he had fitted in with (and enacted) the usual covert oligarchic practices.

75. Ibid. Stokes Papers (CUL), RS 1187.

76. Joseph Larmor, Memoir and Scientific Correspondence of George Gabriel Stokes, Cambridge, 1907, 2 vols., p. 40.

77. J. G. Crowther, Statesmen of Science, Cresset, London, 1965, p. 218.

78. Proc. R.S., November 1880, 31, p. 79. From the tone of The Times' criticisms in the early 1890's and the private responses of the Huxleyites to these outbursts, it seems that Spottiswoode's candour produced no effective change in the situation he was alluding to.

79. Proc. R.S., November 1880, 30, p. 79. Clear evidence reveals that the covertly oligarchic control of the R.S. was not common knowledge during the last two decades of the nineteenth century (Huxley Papers (ICL), 3.316).

80. Ibid., p. 41. Spottiswoode's family and George Stokes maintained connections through their religious community of feeling (Stokes Papers (CUL) S 1262).

81. Hall, ASN, p. 117.

82. Huxley Papers (ICL), 2.250. As has been described earlier, Huxley was firmly of the belief that Foster and Strachey were far more "ideologically pure" in their conception and promotion of scientific naturalism than were Frankland, Spottiswoode, Hirst, Spencer, Tyndall, Busk, and Lubbock of the X Club membership.

83. Stewart Richards, "William Rutherford", Notes and Records of the Royal Society of London, 1987, pp. 193-209.

84. Stokes Papers (CUL), F 269.

85. R. M. Macleod, "Science and Government in Victorian England: Lighthouse Illumination and the Board of Trade 1866-86", Isis 1969, 60, pp. 5-38, ff. p. 6. Tyndall strongly identified with the poor Irish inventor Charles Wigham, and fiercely resented the way in which existing vested interests were able to "tailor" the scientific aspects of the question to the tastes of a far from neutral government department.

86. W. T. Stearn, The Natural History Museum at South Kensington, Heinemann, London, 1981, pp. 59-61.

87. Huxley Papers (ICL), 2.250.

88. Ibid., 2.256. Huxley was clearly stung into his final resolve to exclude Evans. At the July Council meeting, this is what Huxley did. Writing to Hooker shortly afterwards, he declared: "Between ourselves I think Evans thought the pear was ripe for him - and was considerably astonished when I intimated my change of heart before the Council meeting yesterday."(Ibid.).

89. Ibid., 2.255.

90. Ibid., 4.218.

91. Ibid., 4.220.

92. Larmor, ~~ibid~~., pp. 97-100.

93. Ibid., ff. p. 9.

94. Huxley Papers (ICL), 2.256.

95. Ibid., 4.233.

96. Irvine, ~~ibid~~., p. 309. Huxley's daughter Marion died in 1889. The artist Collier then married another Huxley daughter, Ethel. This involved Huxley in the highly contentious contemporary issue of marriage to the deceased wife's sister. Huxley attended the wedding which took place necessarily in Christiania.

97. Huxley Papers (ICL), 16.228.

98. Hirst Journal, 2310. Dewar referred disparagingly to Evans, who was involved for many years in a family paper-making firm, as "the pulp-maker of Nash Mills" (Ibid., 2520).

99. Ibid., 2411. When Hirst was in the habit of staying at the Lubbock family home "High Elms" frequently, his room became headquarters for other smokers who were staying. This time was somewhat before smoking became fully accepted into professional and polite society (Ibid., 1790. The year was 1867.).

100. Huxley Papers (ICL), 4.218.

101. Ibid., 4.237. Von Baeyer was very prominent at the time in the earth sciences generally. Kowaleski was involved in work connected with Darwinism. Dana and Cornu were physicists.

102. Stokes Papers (CUL), L 111. The Cambridge Chronicle, quoting the Times obituary declared: "It is not too much to say that for almost half a century he was more or less behind the best work done by other men." (Cambridge Chronicle, February 13th 1903).

103. Huxley Papers (ICL), 2.250.

104. David B. Wilson, "A physicist's alternative to materialism: The Religious Thought of George Gabriel Stokes", Victorian Studies, Autumn 1984, pp. 69-96, ff. 76.

105. Stokes Papers (CUL), RS 205. It seems that much of Stokes extraordinary zeal in dealing with the R.S. papers dated from this time. He was paid £300 a year and was supposed to be available to the Fellows at Burlington House. Stokes' postal devotion to duty in this respect has been described earlier.

106. Ibid.

107. Leonard Huxley, Life and Letters THH, vol. 2. p. 94.

108. Ibid., vol. 2, p. 87.

109. Huxley Papers (ICL), 4.246. The main objective of the Huxleyites here was to utilise the retirement of the previous Assistant Secretary Walter White to dislodge Stokes from his isolated preoccupation with the papers.

110. White's Journals, p. 194.

111. Stokes Papers (CUL), H 1387.

112. Hirst Journal, 1887. It is quite obvious that Huxley was ideologically at odds with Stokes and would not balk at issuing a reproof over apparent flouting of procedural conventions. What is significant about this case is that Stokes does not seem to have defended himself. Also, no others appear to have spoken up in his defence. He appears to have had not the slightest notion of the importance which the relationship between science and government held for the Huxleyites. Surprisingly, Stokes seems to have been in trouble a good deal during the early years of his long incumbency as Secretary, Lord Wrottesley issued him a stinging rebuke over alleged maladministration in 1858 (Stokes Papers CUL, W 1132).

CHAPTER NINE

1885-1900: THE BREAKDOWN OF HUXLEYITE CONTROL OF THE ROYAL SOCIETY

By 1885 the accumulated influence of the various secularising trends in English society had resulted in the effective independence of science from the sway of the Church. The issue was not dead, but even a man as out of touch with the world of affairs as the reclusive physicist Oliver Heaviside was able to state in 1894 that: "the pious people move on too. Things are not now as they were a generation ago, when Huxley and Tyndall were attacked."[1] In the summer of 1883 a new President had to be found quickly following Spottiswoode's sudden death. The threat from which Huxley felt called to defend the Society's chair was not that from Christianity. Despite the fact that Huxley was easily able to outwit his opponents and take the chair, the overall programme of the scientific naturalists was very far from reaching fruition in the time of his brief Presidency. Over the next ten years, with their anti-clerical unity of purpose enfeebled by its success, the Huxleyites' large ambitions were disappointed. Their social and material ambitions have been described by Frank Turner.[2] The scientific naturalists were seeking for themselves a highly privileged place at the head of a new professional ruling class. This lofty position was to be justified by their control of the means of production of scientific truth and technical expertise. The starkly obvious self-interest inherent in this scheme was rationalised quite simply. If the scientists were to maximise the rate of human progress then it was essential that they be in a position to mobilise the scientific method as the principal civilising agent within human control. To bring this about, the scientists would need to acquire the power of veto over existing power structures. The political status quo was in the hands of its long term custodians, classically trained members of the landed aristocracy. Science had been identified, by the Huxleyites themselves, as

the most potent form of intellectual and technical power. They also saw science as a form of power which naturally brought forth goodness and honesty. If science was to create those virtues only at the expense of the position and dignity of the narrow dissembling interests of ancient privilege and party politics, who was to mind?

In 1883, Huxley felt that science was gravely threatened by "traders, noblemen, rich engineers, and commercial gents". By this time the sort of clash between outraged biblical fundamentalism and brash materialism which had taken place at the Oxford B.A.A.S. meeting in 1860, was already a historical matter. Skirmishes still took place, but they were of an altogether more sophisticated and scientific nature. The pulpit was no longer in a position to act high-handedly in the matter. Nonetheless, when Huxley took the chair of the Royal Society in 1883, it came not as the crowning honour to a nearly complete career but out of an anxious sense of necessity. He intended to keep from the chair other possible candidates whose posture might tend to compromise the demands of the largely incomplete programme of the scientific naturalists. By 1889 one might expect complacent reflection to have replaced restless fervour in Huxley's behaviour. Instead Huxley was describing the rise of dilettanté commercial elements within the Royal Society as "seven devils worse than the first."[3] Huxleyite science was projected within an aura of altruism. The honesty and truth they held to be the moral objectives towards which science could steer society made stern demands of the Huxleyites' own public image. They emphasised the values of duty and service to the exclusion of profit and personal celebrity. Their plausibility was just as vulnerable to the taint of profit-making from applied science as it was from more traditional corruption in the form of jobbery and subservience to party political interests. Having assumed somewhat naively that their success

in challenging the traditionally religious explanations of man's place in nature was due simply to the monopoly of truth held by the scientific method, the Huxleyites seem to have been somewhat baffled by their subsequent failures. At the height of his unsuccessful attempts to force George Stokes' resignation from the Presidency of the Royal Society in 1887, Huxley displayed much of the innocent arrogance that he often identified in others:

> "in spite of the stupidity of mankind, our view of the case must make way, when people think over the matter."4

Rational discussion amongst practitioners of the scientific method turned out to be no more effective in formulating unity of outlook and purpose than had parliamentary debate amongst the scorned party politicians. The small Victorian scientific establishment took great pains to promote the impression that its members' disagreements were never fundamental because all participants could refer directly to truth by means of the scientific method. Partisanship, prejudice and self-interested advocacy were formally held to be inapplicable to the scientific world. Throughout the century the Royal Society forbad pressmen to report the discussions following the presentation of papers at ordinary meetings.

However, Huxley's disillusionment contained harder lessons than the realisation that he could no longer control public opinion within the scientific world. He was made to realise that even his own immediate circle could not agree on the wider aims of scientific naturalism once the unifying effect of clerical opposition had lost its effect. Having agreed with each other on man's place in nature the scientific naturalists were unable to agree on the place of the scientist in English society. After Huxley no-one was able to assume anything approaching his stature. The new generation of biological scientists - the "younger brethren" as Foster and Huxley referred to them - occupied a different world.

Stokes' Presidency

In July 1883 the shock of Spottiswoode's sudden death was still reverberating amongst his friends and supporters. Huxley wrote to Foster to tell him that he would not become involved in a contested election for the vacant chair. He remarked of Spottiswoode: "I hoped he would stop where he was for the next 10 years."[5] The full extent to which the summer of 1883 was a watershed in the career of the Huxleyite design for British science and society would only become apparent to hindsight.

By the time of Stokes' nomination in 1885, he had done a prodigous amount of work on the papers sent to the Society. His enormous correspondence contains numerous evidences of his function as the superintendent of what was virtually an "invisible laboratory". This is clear from the most cursory examination of his dealings with such men as William Crookes, William Huggins and the prominent Sheffield amateur Henry Clifton Sorby.[6] Stokes' Herculean atonement for his alleged neglect of his duties in 1860 had become well-known. When Michael Foster was attempting to secure Lord Rayleigh as Secretary for the papers in succession to Stokes he attempted to allay Lady Rayleigh's apprehension in the following terms.

> "Correspondence etc. ought not and certainly will not in the future be as great as Stokes had made it. It has been painful to see how his energy has been wasted in this way. Mr. Rix is a very competent person and can be trusted with much more than he now has and the Council will I think distinctly approve of this kind of work being taken off the Secretaries."7

The sincerity of Huxley's public loyalty to Stokes is open to question. What is sure is that their dread of each other's occupation of the Presidential chair was a matter that each for his own reasons wished to keep concealed. For his part Huxley was as alert as ever to the damage which public rancour between leading scientific figures might do to the public image of science that he was anxious to develop.

Stokes' artlessness remained a lifelong disposition. The taciturnity which marked his behaviour in all public affairs naturally led to the apparent sense of delicacy which prompted him to gloss over the aspects of Huxley which he found objectionable. Two years after Huxley's death in 1895 the journals of Walter White, the Society's long-serving Assistant Secretary, were published by his brother William. The book contained references to the serious differences between Stokes and Huxley which upset the former, who was clearly unable to form accurate impressions of how he stood in other people's estimation. The appearance of White's journal prompted Stokes to address to Foster a plaintive request for reassurance as to the high regard in which he hoped Huxley had held him. Ironically, Foster was the most vociferous of Stokes' private detractors. However, he replied soothingly.

> "WW's brother is much to be blamed for not having taken advice before publishing that diary. I see it has been roughly handled in Nature for Dec. 30th. Be assured that no-one, for whose opinion you care, has any doubts about your feeling towards Huxley and Huxley's towards you, or will pay any attention to the tittle tattle recorded by WW."8

Reference has been made in an earlier chapter to the clear evidence that when the Stokes' Presidency was just over a year old, Foster continued to refer vital Royal Society business to Huxley without Stokes' knowledge. Foster sent communications addressed to the President straight on to Huxley in Portugal. It may be recalled that one instance concerned a letter from the Treasury which the two wanted to deal with secretly so that the intervention of both President and Council could be avoided. In so extravagantly exceeding his authority Foster obviously attached great importance to the maintenance of a strictly Huxleyite policy in matters concerned with the relations between science and government. On the same occasion Foster also consulted his mentor on the question of the forthcoming Croonian Lecture and the militant opposition of the younger biological Fellows to the publication of "old

fashioned" anatomical work. A minor incident which took place at Burlington House some seven weeks prior to the above events illustrates one of the few things that Stokes held in common with the new Senior Secretary. This was that the focus of each man's social and scientific reference group was far from London and the Royal Society. Foster had an idea for a celebratory volume dealing with the progress of science during Victoria's reign timed to appear during her jubilee in the following year. Writing to Huxley on the 7th of November 1886 Foster could not see a way for the arrangements to be made smoothly with Stokes in charge.

> "I have mentioned it to Mumbo Jumbo [Stokes] - but that is not much use. I have no doubt Mumbo Jumbo has written to the Pres. R.S.E. - [Sir William Thomson] but of course he has not shown me what he has written - and of course I shall not ask him - Alas for the days that are past!!"9

Discouragement for the Huxleyite cause came in large measures during the 1880's. The loss of promising members of the younger generation took place against a backdrop of dissension within the ranks of scientific naturalism. Divisions ranged from what Foster referred to as the "Parkerian crisis" to the long-running resentment centred on the Marine Biological Association. The original members of the group which supported Darwin from the outset were prepared to tolerate W. K. Parker's prolix, old-fashioned Royal Society papers. Similarly, the young leaders of the Marine Biological Association were thrown against their elders for their willingness to accept vague relationships with the Government over fisheries which could have led to the compromise of their scientific authority. Lankester's pursuit of this line flew in the face of Huxley's carefully nurtured plans for the "proper" relationship between science and government. He wished to keep the respective channels of accountability distinct so that the autonomy of science could be strictly maintained. Huxley saw the easy adaptability of the

Marine biologists as being most likely to result in the jealous absorption of independent scientific authority by the traditional political power structure. For Huxley that would inevitably have diminished and corrupted science which would then have been unfit to take on what he saw as its historical rôle. In April 1888 Huxley was still formally in place as the figurehead of the Association. Those whose entreaties to remain Huxley had heeded were anxious that the show of unity should continue "until after the building is opened." Once that was accomplished the intention was to dispense with Lankester's part in the management of the Association and instal the moderate Huxleyite John Evans as the new permanent President.[10] During the same year Huxley's awkward position was emphasised when Foster asked him to propose Ray Lankester for membership of the Athenaeum Club. The Huxleyites had made concerted efforts to secure membership of that lustrous establishment for their friends and acolytes ever since they were in a position to do so. Considering the extent of the damage that Huxley felt Lankester was doing to the cause by his careless flirtation with Government in connection with the Fisheries Board he may not have agreed with Foster's assertion that: "with all exaggeration of his faults, these can't amount to an exclusion from the club . . ."[11] It is understandable for Huxley to have assumed that during the 1880's he would at least retain the great influence that he had acquired during the heroic phase of his career as Darwin's champion. Later events showed Huxley's failure to realise that although his popular standing was maintained and even increased, his influence over the scientific community itself diminished. Solidarity amongst the scientific naturalists was threatened first from within by the pace at which they achieved de facto control of British science. In 1883 Huxley even found himself at odds with Hooker over a fundamental point concerning the relationship between

science and government. As the incumbent of several official posts, Huxley felt that it would be quite wrong for him to assume the Presidency of the Royal Society. In the event he gave most of these duties up at the beginning of his short Presidential term. Whilst still deliberating about taking on the Presidency at the beginning of July Huxley received a letter from Hooker expressing a view which roundly contradicted his own.

> "I think it good for Govt. that the public should see what estimation their servants are held in, and I think it good for the Society that the P[resident] should have a foothold in the Govt. offices."12

The two men had obviously learnt very different lessons from the Ayrton affair a decade earlier. This very damaging split in the leadership of scientific naturalism was later mirrored in its ranks as illustrated by the long-running divisions in the membership of the Marine Biological Association. In general terms it can be said that many scientific naturalists favoured a rapid infiltration of government by scientific men. This would only be achieved quickly if in the short term, scientific autonomy was compromised. The intention was to restore this autonomy once science was established within government. Huxley, Foster, and Donnelly did not subscribe to this view. They saw the programme which it suggested as a dangerous snare which might extinguish altogether the independent power of science as an agent of social change. Against this background it is small wonder that Huxley was to be thwarted by opposition and inertia within the scientific community when he brought about the Presidential crisis at the Royal Society in November 1887.

1887: The End of Huxleyite Control of the Royal Society

The first mention of Stokes' imminent entry into the House of Commons appears in a letter to Huxley from Foster dated the 6th of November 1887. The position of the new Conservative member for Cambridge

University aroused strong feelings in Huxley. He endeavoured to make it clear from the outset that his opposition to Stokes' entry into the House was not itself party political.

> "As a Unionist I should vote for him if I had a vote for Cambridge University . . .. Now we are being connected with the Victoria Institute and sucked into the slough of politics."13

Stokes had also recently become President of the Victoria Institute which by its examination of the relationship between science and religion aimed to set aside the assumption of an intrinsic antagonism between the two. On the same day Huxley wrote to Hooker to say that he found it "utterly wrong and degrading to the Society - by introducing politics into its affairs."[14] At roughly the same time, Sir William Thomson sent congratulations in anticipation of Stokes winning an uncontested election. He clearly felt that no opponent should have the audacity to take part in the electoral process.

> "The enemy has kept a respectful distance since we taught them a lesson at Walpole's election and I trust the lesson has not been forgotten even though it is now more than thirty years old."15

On the 10th of November Huxley sent off to Hooker his outline of options available to them for dealing with the problem of the President's coming political involvement. Huxley detailed these options as: writing to Stokes privately, sending a letter to Nature, or speaking up at the Anniversary Meeting. Although Huxley was to lose this struggle he did not handicap himself by overconfidence. He put it to Hooker that whichever course they chose, it should be enacted "in the name of several - the more the better - of the older Fellows."[16] The singleness of purpose and solidarity which marked the Huxleyites' much earlier campaigns was lacking in connection with the developing Presidential crisis in 1887. The same degree of disarray was apparent in the case of Tyndall's rather embarrassing political activities which were

contemporary with the Stokes affair. In the same letter of the 10th of November, Huxley gave details of Tyndall's actions which ironically mirrored those of his compatriot, George Stokes. To Huxley and his most like-minded supporters, it appeared that Stokes was enabling the importation of the corrupt and inefficient ways of the House of Commons into the Royal Society. By the same token it was clear that Tyndall was eager to proclaim the authority and prestige of the Royal Society in the House. The cause was Irish Home Rule, a towering political issue of the late nineteenth century. Tyndall was a fervent opponent of Gladstone's policy and wanted to present a strongly-worded declaration to the politicians signed by Fellows of the Royal Society. Huxley knew that Tyndall would appeal to the whole body of scientific naturalists for their signatures and was out of sympathy with his intemperate friend's tone and method. Huxley was a Unionist himself yet Tyndall's actions showed him that the Irishman nurtured a quite different conception of the enterprise of scientific naturalism. Whereas he felt that Stokes' political life was wrong, Huxley felt that Tyndall's was merely clumsy:

> "It appears that at the last X he [Tyndall] was entrusted with drawing up some statement about the Unionist question for those who agreed to sign. Now he has gone and told all the world he is going to do it and sneered at Roscoe and Playfair and ignored Lubbock altogether! I saw him yesterday at lunch but had no opportunity of speaking about the matter."17

A week later, Michael Foster offered to confront Stokes, thinking that the President would offer his resignation. It is revealing that Foster was relying on the support or at least the understanding of Lord Rayleigh. The newest Secretary of the Royal Society could well have been expected to stand by Stokes. As a fellow Cambridge mathematician and Stokes' successor to the Secretaryship, Rayleigh was the natural ally of rank and traditionally-based political influence. Nevertheless

the wily Foster told Huxley that he would speak to Rayleigh and Evans "and form some plan".[18] Even Foster was not at one with Huxley at this crucial phase of the incident. The Senior Secretary saw the removal of Stokes over the issue of his parliamentary involvement as a convenient means of installing the pliable and innocuous Evans in the Society's chair. Meanwhile, Huxley reported to Hooker a long discussion he had recently had with Lockyer.

> "Stokes has assured him [Lockyer] that he was not going into parliament as a party man. . . . shows what an innocent he must be. As if Cambridge [University] would have put him into the House for any other purpose than to do their bidding."19

The talk with Lockyer must also have dealt with practical matters because three days later on the 17th November Huxley's anonymous leading article appeared in Nature. He roundly condemned Stokes' dual rôle because of the progressively corrupting influence which the taint of party politics inside Burlington House would provide. The article first established the credentials of its author, then asserted that:

> "It might be quite safely affirmed that Professor Stokes' political and ecclesiastical views were not taken into consideration by those who placed him in the chair of the chair of the Royal Society."

Having assured his readers that Stokes had not even sought the sanction of the Council or the Society at large for his departure from precedent, Huxley went on to the attack.

> "once innoculate the Royal Society with that virus [the interests of party], and the poison will spread through the whole organism. The Council practically chooses the President: it will therefore be necessary to look to the politics of the councillors . . . the Fellows elect the Council: have a care therefore to the politics of the new Fellows. We may yet see a politico-scientific caucus."

The portentous last line of the extract contains a vivid irony for those with any knowledge of Huxley's own smooth manipulation of the Society's purely oligarchic government. A few days after the publication of the Nature article Foster reported the Fellowship divided over

the issue: "some quite agreeing with 'Mr. Lockyer's views as expressed in Nature!' and others thinking it does not matter." Foster went on to describe Stokes' demeanour at Burlington House as very "chirrupy" adding that he must by then have read the article.[20] In a later statement Stokes revealed that his naiveté had its limits. Foster reported the incident as follows:

> "He regards the article in Nature as an attack on his Religion and on his conservative opinion and that it is his duty not to give in!!!"21

He did not give in. Huxley found himself unable to do more without disrupting the Anniversary Meeting in order to polarise opinion. Twenty years earlier he had shown himself quite willing to use that tactic on at least two occasions. So that the issue could be kept before the scientific world, Huxley pursuaded Thistleton-Dyer to restate the case in another Nature letter. The number issued on the 24th of November contained not only Thistleton-Dyer's effort but also two counterblasts from Alexander Williamson and Balfour Stewart. Williamson was still Foreign Secretary of the Royal Society. His standing with the Huxleyites was not high as is revealed by Hirst's remark on Williamson's final relinquishing of his position two years later.

> "The new list of officers of the Royal Society appeared today. Geikie is to take Williamson's place as Foreign Secretary. It must have required a surgical operation to effect this change, I should say!"22

Stewart's sympathies were closely bound up with his close scientific and theological ties with the leading exponents of Cambridge mathematical physics and Scottish natural philosophy. Dyer's attempt to "stiffen up the younger men" on Huxley's behalf failed to produce any marked effect before the Anniversary Meeting a week later. Huxley did not attend and received a description of what passed from Foster on the following day.

> "The RS meeting went off quietly, a very large meeting of 70 or so, a good many of whom came I fancy to back up the President in case of any difficulty. As I think I have already told you I am pretty confident that the majority of the Fellows are not on the Lord's side. Poor Stokes' address was a miserable business - in fact to my mind disgraceful - I offered to do what I used to do for Spottiswoode, write him something on biological matters and the biological medals - but he would do it all himself. He asked me to look over the medals statements which were bare plagiarisms from statements put in by the proposers . . .. The various orators touched, some successfully - others with less success, on the burning question of the M.P. Stokes M.P. said not a word."23

It seems strange that Foster could not accurately predict the view that some significant persons at Burlington House would adopt. A fortnight prior to the Anniversary meeting he noted his intention to "form some plan" with John Evans and Lord Rayleigh. In the aftermath of Huxley's collaboration with Dyer which resulted in the latter's Nature letter of the 24th November, it became clear that Evans was giving explicitly support to Stokes whilst Rayleigh was said to be "shaky".[24]

The willing assistance of Lockyer in the crisis of 1887 formed a marked contrast with his rather clumsy treatment of the Ayrton controversy fifteen years earlier.[25] When Huxley's campaign seemed to be losing any momentum which it had possessed following the 1887 Anniversary meeting, he turned to Nature again. The idea of direct confrontation with Stokes had to be given up. On the 10th of December Huxley told Hooker that if Stokes were to ask the Society for a demonstration of its confidence in him, then it: "from a mixture of motives, would at present, certainly decide in his favour and we should be beaten."[26] Huxley found himself in the invidious position of being worsted by default. It was particularly galling for so practised an intriguer as himself to be baffled by an opponent whose powers of generalship he regarded as negligible. The Huxleyite coterie which had achieved its own ends by oligarchic stealth for so long found itself alienated from

popular scientific opinion. Huxley's lack of commitment to democratic procedures is obvious in his remarks to Hooker about the prospects for a denunciation of Stokes in front of the Fellows of the Royal Society.

> "Now being beaten does not matter - but getting an authoritarian decision the other way would be very unfortunate - and all the more because the decision would really not be given on the true issue. A large number of the Fellows do not take the trouble to understand that; they object to do anything which seems hostile to Stokes personally and a good number make it a question of orthodoxy and unionism."27

At this stage Huxley stated that it would not be desirable that his own "gentle hand should be stirring the pudding again".[28] Accordingly he suggested to Hooker that Dyer was the right man to sum up the discussion in a Nature editorial. Huxley was convinced of Lockyer's full support but for other reasons the article never appeared. Apart from the daunting opposition of Stokes' supporters, Huxley was deterred by the surprising lack of solidarity in the ranks of his own supporters. Reference has already been made to Edward Frankland's warm approval of "a member for the Society".[29] John Lubbock was similarly forthright when he declared his support for Stokes in a private letter to Huxley.

> "It is odd how differently people look at the same things! Now I should have thought that the responsibility of introducing politics into the Royal could have not [to do] with Stokes but with those who attacked him. Surely also it is most desirable to have some man in the House of Commons who can set an example of independence."30

Huxley and his immediate circle were in an untenable position. He regarded the autonomy of science as a crucial precondition for the ultimate success of the wider social and political programme of scientific naturalism. However, the unity of purpose which had given such verve to the promotion of the Origin of Species was lacking in 1887. On the 15th of December 1887 Hooker made explicit reference to the "power for good" which their past position within the Society had been able to exercise. He supported Huxley's view that any blurring of the

frontier of scientific authority would spoil the Royal's oligarchical effectiveness. However, Hooker wanted to avoid at all costs an expression of the opinion of the Society at large because of the dangerous democratic precedent which this would set.

> "the matter had better be dropped for the present . . . it will cure itself . . . therefore it is better to put up with an irregularity provided that it is merely transient than to invite the Society at large to set it right."31

Having failed to force a favourable outcome to the Stokes crisis the Huxleyites had still to deal with the less serious matter of Tyndall. He had persevered with the launching of his Royal Society manifesto against Home Rule into the House of Commons. Huxley's failure to prevent Tyndall's scheme was brought about by the same causes which had thwarted his attempts to deal with Stokes. Concerted action by Huxley's group was no longer possible. The gulf between his outlook and that of his erstwhile supporters clearly appears in a letter which Frankland wrote to him prior to the climax of the Stokes crisis in 1887.

> "At the X it was considered advisable to draw up a sort of scientific declaration in favour of the maintenance of the union and Tyndall was deputed to draw it, strong but in moderate language, and we all agreed to sign. It was thought that nearly every scientist of note would sign it."32

The older Fellows of the Royal Society were noted for their staunch unionism. The Huxleyites of the X club generation were on the same side as Stokes and his supporters on this crucial political issue. A significant proportion of scientific naturalists were driven to supporting the Conservative party by Gladstone's adoption of Home Rule measures. Although Huxley was a unionist for all practical purposes, he admitted a secret admiration for the Irish Nationalist Charles Parnell in a letter to Hooker in 1890.[33] The basis on which he tried to enlist Hooker's support against the Tyndall manifesto in 1887 suggests that he exercised a much more pragmatic approach towards this case than towards that of Stokes.

> "I fully agree with you about this draft, a copy of which he [Tyndall] has sent me and it has too much the flavour of a personal attack on Gladstone. I am in doubt about the footing of the whole thing - Dyer gave me to understand that any unionist manifesto purportedly on behalf of the older men of science - was likely to be followd by a Parnellist Manifesto on the part of the younger!"34

Gladstone's policies had a great power for making Tories of the X club members. Their conservatism did not however extend so far as the right wing fervour of the veteran Royal Society reformer Sir William Grove. In November 1890 he told Thomas Hirst that a monument should be erected in honour of Kitty O'Shea who was the cause of Parnell's disgrace.[35] Early in 1888 Huxley tried to reason delicately with Tyndall in the hope of heading off the Unionist manifesto. In juxtaposing this petition with his summing up of the Stokes situation, Huxley inadvertantly portrayed the crampedness of his position at this, the twilight of his power and influence.

> "Hooker's and my chief difficulty is that any manifesto [opposing Home Rule] prepared by a man of science is pretty certain to be followed by a counterblast from a certain number of them (among the junior more especially) and on all questions of principles our respected colleagues are, for the most part, so sluggish that I doubt if many, even of those who think with us would make a public profession of faith, and a fiasco would be worse than nothing.
>
> If that question of the political Presidency were submitted to a plebiscite of the Society, I believe we should be beaten hollow. So I prefer to leave it to time. At any future election the knowledge that a vigorous minority will oppose a political President will have the more weight the less the exact dimensions of that minority are known."36

Stokes continued to serve for the remainder of a five year term as President of the Royal Society. He remained in Parliament until 1891.

In Newton's Chair

Stokes' supporters made great play of his concurrent occupation of the Lucasian chair of mathematics at Cambridge, the Royal Society Presidency, and one of the Cambridge University seats in the House of Commons. Hindsight has made the contemporarily drawn parallel between Stokes and Newton (as the only previous holder of these offices concurrently) seem rather tenuous. Stokes was deeply enamoured of the trappings of rank and ceremony. On the 7th of July 1887 he led the Royal Society's deputation to present a loyal address to Queen Victoria on the occasion of her jubilee. The ensuing scene contained what has been described as "the greatest shock" of his Presidency.[37] He found that the precedent of individual access to the throne room had been ignored. The Royal Society contingent were led through the Royal presence in company with the representatives of other bodies. As a consequence the Royal Society men were neither severally introduced nor permitted to kiss the Sovereign's hands. Two days later Michael Foster wrote to the Secretary of State for Home Affairs, demurely expressing the Society's sense of outrage:

> "The President and Council feel that they may be rightly jealous of anything which may seem to weaken the priviledges of the Society [which may be construed] by her subjects as a depreciation [sic] of science itself."38

The end of the era of direct control of large sectors of British science by the first generation of Huxleyites contributed largely to an already strong sense of uncertainty amoungst those who were aspiring to take their places. The supporters of scientific naturalism had fought long

and hard to defend the doctrine of evolution by natural selection. They then had to realise that implementation of their social and political programme would not follow inevitably from successes achieved in the 60's and 70's in the Darwinian cause. Powerful interests at the head of the existing social structure formed an immovable barrier to their political ambitions. As with any developing focus of influence, once its impetus was lost the movement could not remain static or hold on to outposts of influence already won. This process is well-illustrated by later episodes in the long-running struggle for purely scientific control of the Natural History Department of the British Museum. In 1888 Huxley reported to the Department's first Director, Sir William Flower, his involvement in a battle with the Civil Service Commissioners over one of Flower's subordinates in the palaeontological department. In exasperated terms, Huxley described how the individual concerned, a man called Newton, had been examined by his employers on the subject of the poor laws.[39]

Whilst Huxley's hopes for the eventual recasting of political and social life in the mould of scientific naturalism were shared by many, divisions of opinion about how best to bring matters to fruition increased. Huxley continued to insist unwaveringly on the scientific control of the relationship between science and government. The entire progressive party within British science was aware that their wider aspirations could not be realised without access to governmental funds and authority. Huxley knew that science could not afford to stand aloof from government or passively suffer neglect at its hands. It seems likely that it was renewed suspicion of declining official interest in science which prompted Foster to address his forceful letter of protest to the Home Office following the snub at Buckingham Palace in connection with the jubilee loyal address. The letter was accompanied by fifteen

quarto sheets establishing the precedents for the Royal Society's right of private audience. In this case and others Foster was mortified by what he saw as the loss of ground which had been won during the years of Huxleyite control of the Society. Foster attributed the loss of the earlier position of science entirely to Stokes' guileless ineptitude. On the day before he sent the Society's letter of protest about the jubilee loyal address presentation, Foster had heard of a new threat to the prestige of British science. The Astronomer Royal, W. H. Christie, told him that the British Ambassador at Paris had given notice of the Government's intention to withdraw from the Comité des Poids et Mésures. There had been no consultation with the Royal Society. Foster expressed his feelings trenchantly.

> "The President and Council . . . cannot but regard the fact of Her Majesty's Government having taken a step so closely connected with the scientific interests and with the scientific reputation of this country without giving to the Royal Society . . . any intimation of their intention to do so as indicating an absence of any desire on the part of Her Majesty's Government to obtain an expression of opinion of the scientific men of the country on the matter, an expression which the President and Council believe themselves able on behalf of the Society to give."40

The onset of winter brought Stokes induction into the House of Commons. Huxley's influence had failed and Foster was pessimistic. Of Stokes he remarked: "it will take some time to repair the damage he will have done before he gives up."[41] The Huxleyite vision of science at the head of society seemed further off than ever. The central dilemma which they faced brought their ambitious insistence on scientific autonomy into constant conflict with government as virtually a monopoly supplier of resources and legitimate authority. To this extent the Huxleyites' dilemma resembles the well-known economic contradiction between the ownership and control of the means of production. In sociological terms, the type of relationship between science and

government which Huxley initially aimed for has been designated by Terence Johnson as "collegiate control". He describes the relationship as one in which: "the producer defines the needs of the consumer and the manner in which these needs are catered for."[42] The scientific naturalists of Huxley's inner council saw the threat to the successful establishment of this situation as coming from the economically determined class interests of the political status quo. Foster, Hooker, Huxley, Dyer, Lankester and Lockyer believed that a scientific and technical meritocracy would be able to transform society with the support of a working population persuaded by their knowledge of popular science. However, the classically trained anti-scientific representatives of the British landed aristocracy held firm and dominated British politics for a further twenty-five years. Scientific naturalism as a dynamic movement with a political programme was defunct by the turn of the century. The Treasury and to a lesser extent other Government departments used the financial dependence of British science in order to undermine its pretensions to self-determination. By this means the politicians were able quite easily to impose their own definition of the situation. Johnson has typified this as one in which: "the consumer defines his own needs and the manner in which they are to be met."[43] Despite the panic measures of the Great War and numerous subsequent makeshift policies, the failure of the political ambitions of scientific naturalism set the pattern for the subservient position of British science ever since.

Much of the detailed history of the Royal Society in the late nineteenth century can be interpreted as the working out of the conflict between the two definitions quoted above. They characterise rival versions of the nature of the scientific enterprise. Of course, the presentation of scientific activity and its productions as a commodity in this way is to implicitly adopt the background assumptions of the

Huxleyites' political foes. In the event the economic power of the existing State remained quite unmoved by dreams of a scientific clerisy. Ironically the originator of the idea, August Comte, was described by Foster as a "blear-eyed little old prophet" in 1885. That year marked a serious downward turn in the fortunes of the Huxleyites - a decline from a highwater mark of achievement which stands untouched by subsequent endeavours. The hurried establishment of the D.S.I.R. in 1916 took place 30 years after Huxley's death. It did not mark the vindication of the scientific naturalists' grand design and was later interpreted as a simple political expedient. The period of "ad hoc neglect" which lasted until 1916 was described in a Nature editorial as "a sham supplemented by a few doles". The prelude to the establishment of the D.S.I.R. was the occasion of a relatively mild agitation on the part of the Royal Society through its Neglect of Science Committee in conjunction with the British Science Guild. The Committee was particularly critical of:

> " the predominance of the classicist in the administrative class of the Civil Service."[44]

It hardly needs pointing out that men of the ilk of Acton Ayrton, Reginald Welby, Ralph Lingen, and Gladstone continued to hold complete sway. The aspiring members of a new scientific clerisy were thwarted in Huxley's day as they have been ever since.

At the end of the 1880's he continued the struggle to contain what he saw as dangerous developments in the interplay of science and government. In 1887 Whitehall changed the plans for the new Imperial Institute. The high cost of a site in central London prompted Lord Salisbury's ministers to sanction a new building at South Kensington. Huxley immediately identified a threat in this unwonted physical proximity between untrammelled science and corrupt unscientific power politics. His protests were ignored.[45] When Foster received an official

suggestion that the Royal Society should make a formal appeal to its Fellows for subscriptions to the Imperial Institute, he refused abruptly.[46] In 1891 the President of the Royal Society was made an ex-officio member of the governing body of the Imperial Institute. This position was immediately delegated to W. E. Ayrton, Professor at the Central Technical College at South Kensington.[47]

Early in 1888 Foster and Huxley dealt with another long-running Royal Society issue without consulting their President. John Murray (who had taken over the "Challenger" work from Charles Wyville Thomson) had made repeated requests for more money to complete the long-delayed reports. Murray's manner and method had increasingly exasperated Foster, who was responsible for the conduct of all liaison between the Royal Society and the Government. The delicate balance which he sought in these dealings, in accordance with his commitment to the Huxleyite world picture, was disturbed by Murray's repeated insistence on additional funds. Murray finally suggested that he produce the account of "Deep Sea Deposits": "at his own risk and profit" to save further official expense. Foster was personally infuriated at this attempt by Murray to secure personal scientific credit and financial gain from the data collected by a large co-operative effort funded heavily from the public purse. More broadly Foster was worried that sanction for the allocation of scientific kudos was being placed in political hands. During the expedition and the work on the results, Murray's position was that of a government employee. Foster consulted Huxley on February the 19th 1888:

> "Welby [first secretary to the Treasury] has asked Evans whether the Gov't ought to make any recognition of Murray's services as editor of the reports - Evans in cowardly fashion told him to ask me. What do you say? I am inclined to tell Welby that if the Govt. wish to honour Murray at <u>Murray's own expense</u> they had better give him a sack of money - but if they want to do it at the expense of the nation they had better knight him or make him a C.B. But perhaps he won't see the force of this paradox."[48]

Despite his cynical dismissal of Murray's motives, Foster was very careful in his contacts with the Treasury. His determination to defend a collegiate definition of the relationship between science and government is unmistakable.

> "though in my letter to the Treasury I carefully avoided stating my opinion as to the question of adopting Murray's suggestion in order that the Treasury might definitely ask our opinion. Have heard nothing."[49]

He concluded by assuring Huxley (then retired from the Presidency of the Royal Society for well over two years) that he would inform him at once if any communication was received from the Treasury adding that if no letter arrived "I suppose you agree that we don't step."

Whilst vital aspects of the official business of the Royal Society were settled in this way without his knowledge, the hapless Stokes kept strictly to his silent role in the House of Commons. During that year (1888) he attended assiduously right up to the last day of the session in August.[50] Stokes seems to have possessed an unconscious talent for outraging the most firmly held Huxleyite principles. During June in his third year as President of the Royal Society and his first as a Member of Parliament, Stokes appeared as a scientific expert witness in a commercial case involving the Edison and Swan United Electric Light Company Limited.[51]

Michael Foster was placed in an awkward position by the election of Stokes to the Presidency in 1885. Unfortunately for Foster and Huxley there were increasing divisions of opinion among the Huxleyite inner circle. There had been disagreements before, such as that which centred round the Eyre controversy of 1865.[52] The new element in the situation twenty years later was that the disagreements touched on the vital issue of how the nature of the scientific enterprise itself ought to be projected through the agency of the Royal Society.[53]

Foster felt thoroughly beleaguered when he wrote to Huxley in March 1888.

> "The Statutes oh dear! Hooker and the Hookerites have made a dead set at them but for all that I don't think we shall put them through - But it is such hard work to get things through with that old stick as President - If Evans and I did not fight back to back I don't think anything would be done."[54]

Dissension grew up between Huxley and the remains of the X club at the end of 1886. On December the 5th Frankland wrote to him to say that at the recent meeting of the club he, Hooker, Lubbock and Tyndall had resolved to oppose Evans' scheme for the affiliation of colonial scientific societies to the Royal. The same meeting flouted Huxley's known opinion by approving the recruitment of several prominent and congenial supporters of scientific naturalism to the X club.[55] Huxley was opposed on both issues at the same time. He retained Foster's full support. At the end of February 1888 Foster wrote to him: "We must do something . . . I don't understand Hooker's obstinacy at all."[56] Foster and Hooker seem never to have enjoyed a close friendship. Huxley's personal regret at going against Hooker on the issue of colonial affiliation is apparent from his views as they were expressed to Evans.

> "My dear Evans, I have carefully considered your draft statutes and I sincerely trust that they will be approved by the Council and by the Society. We shall look very foolish if, after all the talk, nothing is done to bring us into closer relation with our colonial and American confrères - and I cannot see what evil can possibly arise from such a modest proposal as that which you make. On all grounds I am extremely loath to go against Hooker's judgement - but I cannot agree with him in this matter.
> Your scheme gives the officers no more influence than they have in the case of the foreign membership - and there so far as my experience goes it is by no means permanent . . .."[57]

Huxley's hopes for the changes were not realised. Regarding the other vexed issue of recruitment to the X club, Huxley had been informed

three months earlier of the determination of Hooker, Lubbock, and Frankland to go ahead. The latter wrote to Huxley informing him of this and that the opinion of Tyndall, who was also against new members, was to be ignored because of his lack of attendance. Frankland stated that:

> "Strong opinions were expressed, especially by Hooker against any tinkering of the Royal Society by the admission, on any modified terms, of colonial members. J. Evans new scheme was condemned. You see unless you attend the X will soon be High Tory."[58]

In October 1888 Foster sought his mentor's opinion about a replacement for Williamson as Foreign Secretary of the Royal Society and the continuing problem of Stokes' Presidency. Huxley's enfeeblement had forced a withdrawal from the day to day focus of events which left Foster's position exposed. He told Huxley: "it weighs on my mind much what is the best thing to do."[59]

1890: "The Horrible Task of Selecting a New President." (Foster)

Three members of the X club surveyed the prospects for filling Stokes' place at their meeting on the 1st of May 1890. Hooker, Frankland and Hirst had never been particularly close to each other and by this juncture Frankland was somewhat disliked by the other two. Hirst's Journal contains an entry of the time describing the most promising Presidential candidates as Lord Rayleigh, Richard Strachey, and Joseph Lister. The latter was Hirst's suggestion.[60] After the X club dinner the three members in attendance were joined in the smoking room of the Athenaeum by Williamson. He and Frankland had been enemies for many years. At the X meeting three weeks later it was rumoured that Sir William Thomson was to be the new President. He had been asked on previous occasions but had refused nomination because of his other commitments, especially his Glasgow Professorship. Hooker's own order of preferences was Thomson, Strachey (whom he himself had

proposed), Lubbock, and Evans. Hooker felt genuine approval for the first two but before their advent had told Huxley that: "Lubbock was I fancy the only alternative to keep Evans out."[61] According to Hooker both Rayleigh and Foster turned down the offer of the Presidency whilst Evans was actively "touting" for it. Thomson shared some of Stokes' capacity to combine a great scientific reputation with a simple lack of awareness of the subtleties of corporate and social life. Both men lacked tactical insight into the world of institutional politics. Thomson's capacity for obtuseness exasperated the normally deferential Assistant Secretary Herbert Rix. Early in April 1893 Rix wrote to Evans for a decision on whether a forthcoming Council meeting should be confirmed for the day suggested by Foster. The Senior Secretary was abroad at the time, as was increasingly his habit. Rix told the Treasurer Evans:

> "I do not ask the President because he would be sure to misunderstand the point."62

Foster recorded a brief account of the inauguration of Thomson as the Society's new President at the Whitehall Rooms of the Hotel Metropole on St. Andrews Day 1890. It points to the public shortcomings of Thomson and his predecessor Stokes. It is very noticeable how the scientific naturalists as a group far outshone the Cambridge mathematical physicists as public representatives, popularisers, and teachers of science. This is evident in Foster's description which formed part of a letter to Huxley.

> "Lubbock took your toast but had not much to say. . . . The new President did not make his appearance in the reception room till nearly seven so Rayleigh and I had to do all the receiving, but he made up by delivering after dinner long lectures about each of the medallists which bored everyone. . . . I think Stokey succeeded in saying less than me! but he did not kill any Fellow prematurely."63

When Thomson took over from Stokes there is little evidence of any mending of the dissaray in which the Huxleyite group had stood for some time. The previous summer Hooker and Huxley decided on a final rejection of Herbert Spencer. In July, Huxley wrote to Evans at the Royal Society informing him that he wanted to make a complete break with the Marine Biological Association. The grumbling acrimony which existed between Huxley and some of the younger biological scientists led by Ray Lankester had not abated despite Huxley's continued unwilling Presidency of the Association. Having been shamed and cajoled into retaining that position in order to foster an appearance of unity, Huxley found that Lankester and his supporters would not abandon the course with which he was in fundamental disagreement. It will be recalled that this course consisted of an unconditional blurring of the independent status of scientific authority by the involvement of the Association in Government Fisheries policy. In late July 1890, Huxley told Evans exasperatedly: "I do not wish to be responsible for any other dealings of the same sort."[63*]

1890-1895 Disquiet and Disillusion

Sir William Thomson's Presidency was concurrent with the last five years of Huxley's life. In relatively quiet retirement at Eastbourne the latter's activities became increasingly limited. Despite the fact that the period of Huxleyite domination of British science was by this time long past, Huxley's exceptional range of important contacts kept him au fait with current developments behind the scenes. At the end of 1891 he wrote to Evans in a surprisingly well-briefed manner:

> "I hear accusing accounts of a conspiracy between the trustees of the B.M. and the Prime Minister to circumvent the Treasury - I think I know whose finger has been in that pie."[64]

Even though Huxley remained extremely well-informed about the conduct of vital London issues, his seclusion in Eastbourne led to his being left behind in some matters of detail. In March 1892 it seems that the old X club policy of securing the election to the Athenaeum Club of sympathetic scientific colleagues was still in force. The usual aim of those who arranged the nomination of Huxleyite supporters was to secure the elections at the first attempt by the committee method which was applicable to scientific men of particular merit. By this means the appearance of a special status for men of science was projected. Most of the Huxleyites' leading nominees were able to avoid any contest with sundry sculptors, diplomats and poets which would have compromised the ambitious sweep of the scientific naturalists' ideological perspective. Huxley was concerned by his lack of close influence over the Athenaeum election of 1892.

> "I see that Abel has proposed Harcourt for election by the Athenaeum Committee - This seems to me to ruin our chances of carrying both Stone and Darwin. So if you think Stone should - - against Harcourt (who is a very good man by the way) bring him up again next time - I am so little in London now that I do not 'know the ropes' and I have no chance of consulting with anybody."[65]

The divisions of opinion and sympathy among leading Huxleyites continued. As Foster had wondered at the problems raised by the "dead set" which "Hooker and the Hookerites" had made at the Royal Society Statutes in 1888, so Hooker took exception to their codification three years later. In February 1891 he told Huxley:

> "I wish that Evans and Foster would cease pottering at the R.S. statutes . . . ."[66]

Rix reported to a colleague that although the statute modifications had taken three years and were a revolution in form, they had not affected the conduct of the Society significantly. Rix concluded darkly that: "the great change which really has come about has been produced by other means."[67]

In 1892 Hirst and Tyndall died. Valedictory honours continued to be bestowed on Hooker and Huxley. The Royal Society Council vote in favour of the award of the Copley Medal to Hooker in 1887 was unanimous regardless of the recipient's later (and largely accurate) assertion that he had nothing to deserve it. The award to Hooker appears to have reconciled Huxley to receiving his own Copley in the following year.[68] Hooker received the Royal Society's Darwin Medal in 1892 and Foster obtained it for Huxley in 1894. Foster simply excused himself for this act by describing the award as inevitable. Huxley gently chided him for it, describing the suitability of such honours for younger men rather than "useless old extinct volcanoes".[69] Despite the august significance and contemporary glitter of these awards, the tide of events had carried the modern scientific world beyond the immediate reach of the ageing Huxleyites. Referring in 1894 to his old battles with Richard Owen, Huxley declared that "it is almost impertinent to trouble the modern world with such antiquarian business."[70]

1894 Fin de Siècle Rumblings at Burlington House

The tacit convention which precluded public recognition of the Royal Society's strictly oligarchical government was thoroughly breached in an article in The Times, on December 1st 1892. Under the title "A Criticism of the Royal Society" the writer delivered a swingeing attack on the way in which the Society was run. He described how the list of ten council members to be replaced each year was supposed to be decided by rule-bound criteria and the ten new men put up by a general meeting each year. This ballot, the "Critic" asserted, was a fiction. The existing Council always decided the deletions and additions to its body as well as the appointment of the officers. Because of the flux of transient council members who were no more

than token representatives of their fields of study, the officers and their regularly re-elected clique on the Council effectively monopolised power within the Society. In the words of the anonymous "Critic":

> "Experience again indicates that within the council it must be very difficult to make any successful resistance to the officials. . . . They are sure to have some steady supporters in a body they have a large share in nomination . . . in an oligarchy of this kind there is someone ambitious of running the machine and blessed with leisure to indulge his taste. . . . There are favoured persons, not always the most notable improvers of natural knowledge who reappear on the council every five or six years. Others with at least equal claims are in a ten year rotation, some run to 14 or 15 years, and there are others with orbits so eccentric that they may be regarded as lost to the system altogether."71

In spite of the testimony of those who were interested in defusing the atmosphere of crisis which The Times attacks brought about, there were serious underlying tensions building up at Burlington House. Huxleyite domination of the Society was over. Up until 1887 Huxley had had the authority to stamp out internecine warfare. No-one subsequently emerged with anything approaching this stature. The social roles of the "Young Guard" of the 1860's were obsolescent thirty years later. The Darwinism of the scientific naturalists, though increasingly troubled, was established scientific knowledge in the 1880's. The disappointment of the social and political ambitions of the wider programme of scientific naturalism prompted frustration and discontent among the younger biological Fellows. Foster, Dyer and Lankester all failed to take up Huxley's mantle in any significant respect. Perhaps the place which he had held was no longer tenable, even for a man of comparable capacities. Antagonistic pressures continued to be felt from the wider society especially in the continuing strength and determination of the anti-vivisectionists. In 1876 the movement had demonstrated its ability to set limits to the scientific enterprise

by parliamentary means. The licence and quota system imposed by the Cruelty to Animals Act in that year held a grim significance for the Huxleyite doctrine of the autonomy of science. This obviously went far beyond the technical implications which the act held for the day-to-day conduct of the biological sciences. Had science been contemporarily defined as merely a collection of concepts, techniques, and equipment then the legal control of one of its research methods could not have produced the pious moral tone of the biologists' contribution to the furore. On the same day as the first Times attack on the Society, Rix wrote to a Dr. J. S. Risien Russell about a paper which the latter had recently submitted to the Society. Rix was passing on Michael Foster's recommendations.

> "Doctor Foster has spoken with Prof. Victor Horsley upon the matter and Professor Horsley is of the opinion that a simple diagram might be substituted for the somewhat realistic picture of the vivisection experiment . . . [Risien Russell's picture] is of a character scarcely desirable for publication at the present."72

To Huxley and his immediate supporters science was being distorted by the corrupt society which only the untrammelled progress of science could transfigure. Disillusionment and a sense of malaise are clear in the following letter sent by Dyer to Huxley nearly a year later.

> Nothing astonishes me so much as the provisional atmosphere in which we live. . . . Suddenly many of us come to the conclusion that the R.S., which seemed soaked in time and likely to jog along for ever is in a state of ferment. Even Lankester said to me last Sunday that he was horrified; it was the last thing he wanted to see go into the melting pot. There can be no doubt that the row about Howarth evidences the fact that there is a considerable simmering revolt. The causes of this would take me too long to explain. But the Society is virtually governed now by M. Foster and Evans. Both have managed to largely lose touch with the Fellows, especially Evans who is becoming positively hated. Now, I am very fond of M.F. and it is very difficult to me to speak out to him. One cause of the

> present dissatisfaction is that no one of the officers is resident in London and therefore inaccessible. The government of the R.S. is a pure oligarchy and I think rightly so. But oligarchical government requires tact now-a-days."73

The row over the candidacy for the Fellowship of Sir Henry Howarth was seriously dividing the Society at the time. H. E. Armstrong, Ray Lankester and George Romanes objected to the election of men such as Howarth, a newspaper correspondent whose social eminence was his chief claim. The three tried to stop Howarth's election on the ground that he had made no contribution to science. Lockyer would not commit himself on the matter because he had acquired friends on both sides. Thistleton-Dyer was personally committed to the scientific exclusivity of the Royal. He nevertheless wrote in horrified terms to Lockyer about the public show of disunity and partisanship which the movement against Howarth was producing.[74]

Dyer's assertion that the bureaucratic work of the Society was falling into confusion by neglect is corroborated by the views of the Assistant Secretaries. It was they who had in the first instance to explain the detailed failures of the executive on a day-to-day basis. On March 20th 1894 with the season of the London scientific Societies in full swing, Rix reported that Rayleigh, Evans, and Foster were all abroad. As the 1890's unfolded there was considerable expansion of the work connected with the Government Grant. At the same time, work on the national and international catalogues of scientific papers was in full spate. Stokes' obsessive diligence was no longer available to absorb the demands of the papers and publication delays increased once more. As the members of the executive became more lax in the performance of their duties, the strain upon Rix increased. He resigned at the end of 1895, providing the following explanation of his action:

> "my strength will no longer sustain the increased anxiety and burden of the office."75

Rix kept the Government Grant work for the modest payment of £50 per annum. Three months after his replacement as Assistant Secretary by Robert Harrison, Rix wrote to the Council to sound "a warning note". He maintained that the work on the International Catalogue was so heavy that the staff could frequently not catch their trains home at night. Rix himself had been working until 10 p.m. on the neglected arrangements for the forthcoming soirée. He recommended the appointment of "a working Secretary" (underlined ironically) to the planned International Catalogue Conference. Rix stated plainly that unless this was done a general collapse of the Society's normal functions could be anticipated with the onset of the rush period in May and June.[76] Robert Harrison was immediately disconcerted by the pressure of work in his new position and the lack of involvement on the part of the Society's executive officers. In May 1896 with the International Catalogue Conference looming up he wrote to Foster pointing out that nobody among the Fellows knew what was going on regarding the reception, the dinner or anything else connected with the Conference.[77]

Five weeks after Dyer's expression of his forebodings to Huxley, the Times attacks on the Society first appeared. Hooker's attitude was one of lamentation rather than outrage. Alluding to one of Lockyer's suggestions for the rejuvenation of the scientific societies he remarked to Huxley that: "I do not see the men who are able to propose judicious reforms or carry them out".[78] A few days later he wrote again.

> "The R.S. is in a bad way I fear - not from the Times articles for they are not worth notice - but Dyer pronounces the Govt. rotten and says it has not been sound since our time."79

The attacks on the Society printed in The Times posed an annual threat for five years from 1892. Foster was convinced by the attacks following the next Anniversary Meeting that the situation could not be ignored. He told Huxley: "we shall have to meet him. Moreover, the second letter yesterday in big type means that someone in Times [sic] probably Brudenell-Carter is backing him up. I take it big type means the Times thinks the matter important."[80] He went on to describe how Brudenell-Carter was a candidate for the Fellowship at the time and his aim might well have been to force the Council's hand in the selection procedure. The active Huxleyites were aghast at the exposure of the Royal Society to the glare of adverse publicity. The fortunes of the socially ambitious manifesto of scientific naturalism could only be adversely affected when the Royal Society was shown to be rife with faction and non-scientific acrimony. The contemporary enormity of The Times attacks can be gauged from the remarkably unfavourable comparison which Foster drew between them and the methods used in controversy by Richard Owen. Although by some contemporary accounts Owen was a ruthless dissembler, Foster's manner of recall became almost nostalgic in a letter to Huxley.

> "Poor old Owen had only been in his grave a few weeks before his mantle fluttered on someone else's shoulders. But old Owen had the courage to carry matters to the Society itself, whereas critic remained quiet at [the] Anniversary Meeting with his MSS thunderbolt in his pocket."81

The following year brought a further onslaught. On this occasion the anonymous correspondent displayed inside knowledge of the Society's Government Grant Committee. The article asserted that the Royal Society was dominated by biological concerns; that the Council was overburdened with men from Trinity College Cambridge; that the Council was ineffectual as a whole in comparison with its ruling caucus. The "critic" also mentioned two issues which were closely bound up with the expansive

strivings of Norman Lockyer. Firstly the editor of Nature's scheme whereby the Royal Society would confine its activities to biological sub-fields which did not possess their own special societies. Secondly, reference was made to the large amounts of money being spent in the field of solar physics which Lockyer had tried to make his own. Huxley was so enfeebled by this time that trips to London in the winter time were spoken of as "escapades". Foster wrote to him immediately with an account of the Anniversary Meeting.

> "The dinner was very successful in spite of Kelvin and Stokes being of course tedious everyone almost as gratified. . . . You will doubtless have seen today's "Times". From the look which Buckle gave me at the dinner I feel sure he had a bowie knife up his sleeve. What makes him play this game towards us, or rather towards me?"82

Despite Buckle's malign looks, Foster was convinced that the "Critic" of 1893 was James Dewar, Professor of Chemistry at the Royal Institution and Cambridge. Six months earlier, Board D of the Government Grant Committee had refused Dewar's application for funds to set up apparatus to work on liquid gases at the Royal Institution. The Times reference to Trinity College left Michael Foster in no doubt that he himself was the target and that Dewar was responsible.[83] Royal Society records reveal that at the time of Board D's refusal of Dewar's grant application he had appeared at Burlington House and read the Statutes aloud to Herbert Rix. Dewar's intention was to gain access to the minute books of the Government Grant Committee which dealt with his case. Rix informed Rayleigh of Dewar's success in this endeavour and his intention to create a storm over the issue.[84] Although usually committedto the preservation of a posture of condescending self-righteousness on the part of the Royal Society in times of crisis, Foster was here willing to make an exception. He felt that he had been as good as personally accused of "unfairly spending" the £4,000 grant and that "something should be done."[85]

The indictment of the Society for bias in favour of the biological sciences can be related to a wider reaction to the eclipse of dominant Huxleyite influence. In its hey day that influence seems to have acted very effectively in bringing on biological work. By the 1890's physiological papers far outnumbered those produced by the workers in any particular subdivision of physical science. Between October 1891 and June 1894 (three full sessions) the distribution by subject of the papers printed in the Philosophical Transactions and Proceedings were as follows (the relative proportions during the three sessions 1861-4 are provided for by comparison). The burgeoning of the biological side is clearly portrayed.[86]

Subject	1861-4		1891-4	
	Phil. Trans.	Proc.	Phil. Trans.	Proc.
Mathematics	25	4	9	2
Physics	39	15	37	41
Chemistry	20	6	3	17
Astronomy	2	0	4	8
Electrical science	0	0	5	17
Aggregate of physical sciences	86	25	58	85
Physiology/biology	24	7	46	69

Regardless of the allocation of medals, Government grants and other scarce forms of patronage, it is clear that biological science had come to occupy a disproportionately large part of the Society's publications. Forty years earlier, Emil Du Bois Reymond had, whilst on a visit to England reported to Carl Ludwig that: "physiology does not exist there."[87]

Dewar went on to cause a great deal more disturbance in pursuit of his researches. When the industrialist Ludwig Mond donated £107,000 for equipping the Davy-Faraday laboratory at the Royal Institution, Dewar occupied the newly available space with his apparatus for the liquifaction of air. Of course this meant that the international work envisaged by the founder was not possible. Eventually his son resigned from the overseeing body in protest.[88] Following the Times attack of December 1893 John Donnelly wrote to Huxley expressing his regret at the way things were developing.

> "Did you see the attack on the R. Society in the "Times" - a day or two after the dinner. Lockyer says it must be Dewar, though it is written in such good English! It gives me quite a shock to see the Royal Society thus dragged about - as if it were the Science and Art Department! In my young days the R.S. was above and beyond criticism."89

Huxley was fully aware of the damage which public exposure of factional posturing within the Royal could do. Scientific naturalism as the agent of moral progress through technical truth could not compel public opinion whilst the highest forum of science was riven by schism of a distinctly political flavour. The Times criticism did not seem to depict the upper chamber for a high-minded secular clerisy. Replying to Foster following further revelations in the Times a year later (1894) Huxley was unduly optimistic. In this particular case the source of the offence was editorial comment.

> "By the way do you see the 'Times' has practically climbed down about the R.S. - come down backwards like a bear growling all the time. I don't think we shall have any further 1st of December criticisms."90

Huxley was wrong. Public interest in the probity of the Royal was maintained by continuing rumours and criticism in the press of the Society's internal procedures. Rayleigh's son imperiously dismissed the entire Times critique of the early 90's when he came to write

his father's biography in 1925. He attributed the whole affair to the misdirected chagrin of disappointed candidates for the Council list.[91] This view is in no sense supported by the direct testimony of persons centrally involved at the time. The attacks changed the policy of the Officers from the usual "masterly inactivity" so characteristic of Huxley which Foster had stuck by in 1893. Three years later the new President Joseph Lister asked Rayleigh, the retiring Secretary to send to The Times an authoritative denial of that Anniversary's crop of charges. Lister was clearly worried about the public image of the body of which he had just become President.

> "You have no doubt seen the malicious article in today's Times about the R.S. The statements regarding the award of the Rumford Medals and the second Royal Medal are of course quite false and equally so is the insinuation that you declined to remain in the Council because you disapproved of its ways. It is felt by Rücker and other as well as myself that a few words from you in the Times would do a very great deal of good."92

When the attacks were still a rather shocking novelty in 1893, Herbert Rix wrote to T. Jeffrey Parker asking him to keep quiet about the details of the inordinate amount of money which his father had received from the Royal Society in the form of personal grants for subsistence.

> "as the Society is undergoing a fire of criticism just now it is as well not to give unnecessarily a handle to the enemy. Verbum sap."93

This period of continuing crisis came about partly because of uncertain shifts in the balance of power at the centre of British science due to the passing of the Huxleyite régime. As has been noted, the officers were frequently absent, and yet the leading younger Huxleyites (Dyer, Lankester and Lockyer) were unable to attain the crucial positions in the Society's executive. The President (Kelvin) did not travel to London from Glasgow very often. When he did the Assistant Secretary

was unwilling to trust him with even elementary points of Society business. Kelvin had long tried to defend his freedom to carry out his own scientific and technical projects from the demands of scientific administration and scientific social life. In 1859 he had tried to pursuade Stokes to try for the Glasgow chair of Astronomy in order to remove him from London and Cambridge: "those great juggernauts under which so much potential energy for original investigation is crushed."[94] By the time of his Presidency of the Royal Society, Kelvin's attitude in this direction had hardened and was responsible, in part, for the weak impact which his Presidency made on the scientific life of the capital. His personal shortcomings formed an unavoidable adjunct to this. In May 1891 Hooker remarked on the first public event which Kelvin presided over: "the R.S. soirée was miserably attended."[95]

Huxleyites who had seemed to move confidently and effectively with their leader still in harness were reduced to exasperated carping in the 1890's. Dyer provides a clear instance of this. From the time of his retirement in 1885, Huxley had encouraged Dyer to take over the rôle of leading publicist for scientific naturalism. In 1894 the reluctant Dyer addressed his chief quite plaintively:

> "Would to God in these evil days we had you in the chair instead of the inspired schoolboy Kelvin. I emphatically agree with you the day of "Societies" is past. They only afford a forum in which incompetents can talk nonsense to men who know and who have something better to do than listen. The R.S. is an exception of course. But there we are fairly successful in eliminating fools."[96]

Norman Lockyer and Michael Foster showed that they could still get certain things done when in 1893, with the support of Archibald Geikie, they secured the withdrawal of T. E. Thorpe's nomination as Foreign Secretary. The Council had already made its decision by the end of October, but they managed to get Joseph Lister's name accepted

instead. It was the idea of T. Lauder Brunton to raise the spirits of the recently widowed Lister by providing him with a new outside interest. When Lauder Brunton told him of it, Lister hesitated because of the "whiff of politics".[97] Geikie assured Lister that the position entailed little work and that the Council were eager for him to accept. Lauder Brunton was pleased that Lister's presence might help to heal the rift appearing between the physical scientists and the biologists. As Kelvin's term in the chair neared the end of its fifth year, Foster deftly arranged Lister's succession to the Presidency. By July 20th Lister was resigned to this fate, having had his reservations about being "pitted against Evans" quashed by Foster. Lister wrote to a friend describing these developments: "there was such an almost universal feeling against him [Evans] being President . . . that there was no question whatever of my running in competition with him."[98] Lister's reputation held a further significance for the struggling mandarins of British science. The humanitarian implications of his pioneering work in the field of asepsis gave credence to the Royal Society in its long fought holding action against the anti-vivisectionists. Foster was anxious to draw on all sources of authority in this struggle. In 1889 he was closely concerned with the organisation of the first International Congress of Physiologists. Foster was well-attuned to the way in which such continental alliances might help the domestic struggle on behalf of biological science.[99]

When Rayleigh resigned the Junior Secretaryship in 1895 his successor was Arthur Rücker, a genial man who always conducted the press preview of the Royal Society's soirées. He had been Professor of Physics at South Kensington for the previous ten years.[100] As has been noted, Dyer did not fulfill his early promise and did not achieve any influential office in the Royal Society. The only possibility for him was the Foreign Secretaryship which he turned down

in 1899. The vacancy was caused by the sudden death of Frankland whilst on holiday in Norway in August of that year. Dyer was far too inept an institutional intriguer to become a fully effective Huxleyite activist. He was also prone to stand a good deal upon his own dignity.[101] Dyer's final attempt to influence the procedures of the Royal Society was made in 1897. He and Ray Lankester tried to annex several of the crucial responsibilities of the Council and place them in the charge of the newly recreated Sectional Committees. These eight sub-committees were given partial responsibility for the Society's scientific papers in 1896. The Sectional Committee Chairmen were as follows: geology: Geikie, zoology: Lankester, botany: Dyer, mathematics: George Darwin, physics and chemistry: R. T. Glazebrook. In a memorandum invited by Michael Foster in June 1897 Dyer asserted that these new bodies should not operate merely at the behest of the Council. He suggested that they ought to take a major part in the selection of new Fellows and Foreign Members because these functions required quite as much specialised knowledge as the evaluation of the papers.[102] Ray Lankester had impetuously made his position clear somewhat earlier by presenting the Council with a piece of unsolicited advice in February 1897. This consisted of the reiteration of Professor Weldon's earlier suggestion of the election of Henri de Lacaze Duthiers to the Foreign Membership, in the event of the Council's determination to include a zoologist. Lankester underlined his point by adding that if the Council preferred a palaeontologist then it should be Professor Zittel.[103] On July the 24th, in the middle of the vacation, the Secretaries wrote to all the Sectional Committee Chairmen quashing all Lankester and Dyer's aspirations. They found that the Society could no longer be run in the interests of Huxleyism through the Council. The glaring division of interests between Lankester and

Dyer on the one hand and Foster on the other is starkly expressed in the Secretaries' reply. This held that the selection of Fellows and Foreign Members had to remain the exclusive right of the President and Council because it was one:

> "in respect to which any action of the committees in the way of making suggestions is attended with so many difficulties that the safest plan seems not to include this in the functions assigned to them."104

The Retreat of Active Huxleyism in the Royal Society

Tensions between the informal leadership groups at the head of British science continued to show through the Royal's grandiose façade for the rest of the decade. At the end of 1897 William White, brother of the Society's long-serving Assistant Secretary, arranged the publication of Walter White's Journal. The Journal's candid revelation of the self-interestedness, fallibility, and downright bellicosity of a number of leading men of science was a serious embarrassment to many of those named who were still living. The public image projected optimistically by the scientific naturalists based on honesty, openness, and impartial technical expertise was clearly threatened by White's little book. Mention has been made earlier of the jolt which the publication gave to the portentous but naive George Stokes. Michael Foster endeavoured to comfort Stokes and expressed approval of the diary's "severe handling" at the hands of a Nature reviewer. This writer set out to leave no doubt as to the amount of credence to be placed in the Journal:

> "it is exceedingly regretted that Mr. William White should have thought it desirable to give publicity to gossiping statements redeemed neither by wit nor by accuracy."105

Ever since the time of the tied Council vote which preceded Huxley's eventual election to the Secretaryship in 1872, the main honours and appointments had been dealt with informally.[106] As the Huxleyite

era passed away so this stable system began to break up. During both the fiasco of Stokes' political Presidency and the trauma of the Times criticisms, the Huxleyites found themselves powerless to act effectively. By the end of the century power in the Royal Society had been consolidated in other hands. The Cambridge mathematical physicists had for many years been represented unremarkably in the Royal Society by such men as Stokes and Kelvin. As the power of scientific naturalism faded in the 1890's, the influence of Cambridge increased to be exemplified eventually in the Secretaryship of Joseph Larmor. Larmor was the leading ether theorist by the turn of the century and carried the Stokes-Kelvin legacy in mathematical physics into the new one. The following quotation from a letter sent to Larmor by the Society's Junior Secretary Arthur Rücker exposes some of the interests of the newly dominant group in the executive. The business under discussion concerns the diplomatic timings necessary for the mathematical physicists to smoothly secure their choices for the Foreign Secretaryship, the Presidency, the Secretaryship, and the Rumford and Copley Medals.

> "Will you therefore be sure that Fitzgerald has done something that can be used to establish his claim [to the 1898 Rumford Medal] within that time [the past two years]. . . . I confess however that my reasons for thinking this year the best for Huggins are of another order. Next year Frankland retires from the Foreign Sec'yship. I know many think that Lockyer ought to succeed him and if this is so it will be difficult after next year to carry Huggins unless Lockyer supports him which is improbable. Of course we could elect him next year, but to give him the Copley and L. the Secy'p in the same year would have an appearance of balancing matters as in the case of the two K.C.B.'s. Also the biologists may make a push for some strong foreigner [for the Copley Medal] next year whereas if Huggins were elected now no-one would oppose Rayleigh in 1899. These considerations added to the fact that at H's age the expectation of life is only about 4 years make me lean to the plan we discussed but I need hardly say, that if the general view is that Rayleigh

> should have it without further delay I too am of the opinion that his claim is of the very strongest."107

In the event Lockyer was not put in the running for the vacant Foreign Secretaryship in 1899. The position went to T. E. Thorpe. Lockyer had instead become embroiled in a contest for the Treasurership vacated by Evans in 1898. The Huxleyites first tried to pursuade Lubbock to stand against Alfred Bray Kempe, a Cambridge man of small scientific standing. Although Lubbock was one of the earliest parliamentary supporters of proportional representation he would not be so graceless as to be a party to a contested election at the Royal Society. A Huxleyite group led by Hooker, Lockyer, and William Crookes stood out for the strict requirement for high scientific credentials in the Society's officers. The Howarth affair five years earlier had split the remains of a Huxleyite camp already thrown into disarray by the Presidential crisis of 1887. In the election for the Treasurership in 1898 their second nominee, Lockyer, was defeated. Kempe was elected and went on to occupy the office for many years. Lockyer retired from the Council altogether.[108] Dyer turned down the Foreign Secretaryship in 1899 leaving Foster as the only representative of the old oligarchy.

As a direct result, the Society's relations with Government became far less systematic. The Cambridge men's designs were far less ambitious and all encompassing than those of the Huxleyite version of scientific naturalism. As a relatively isolated intellectual elite content with its place in the traditional pattern of social and political privilege, the Cambridge mathematical physicists had no concerted policy in this direction. Huxleyities who remained in influential positions during the 1890's were bewildered by the jumble of clumsy ad hoc initiatives which the situation inevitably produced. John Donnelly remained in

office to see most of Huxley's precepts flouted regarding the running of British science. Donnelly was very surprised to find South Kensington honoured by Lockyer's elevation in May 1893. He wrote to his old ally at Eastbourne to explain the chaotic circumstances.

> "I dare say you saw with as much surprise as I did that our Astronomer had been made a C.B. before the Astronomer Royal! I had just written to congratulate him [Lockyer] - I could honestly do that for it would sweeten a somewhat acid body for sometime - when in came Acland [Donnelly's direct superior] - much disgruntled. He had never been consulted. Lockyer had asked him before to get him an honour and he had refused. He would never have thought of recommending him without consulting me! and now he felt himself in a very awkward position: not only over the other professors whom he thought deserved it as much but the Whitehall branch thought he was giving unfairly to S.K. and so on. Of course it is obviously all wrong."109

Acland wished Donnelly to tell the other South Kensington professors and Huxley that he had not taken part in the decision. Donnelly told Huxley that the President of the Royal Society Lord Kelvin had written to Gladstone and one of his secretaries had bungled. To the Cambridge physicists, politicians were people from whom honours were obtained.

Foster's Last Stand: The Natural History Museum

The development of the Natural History Museum provides a long term indicator of the fortunes of the institutionally active party thrown up by scientific naturalism. The original impetus behind the erection of the impressive new building at South Kensington was that of Richard Owen. In 1880 the move began from Bloomsbury to Waterhouse's terra cotta "Cathedral of Nature".[110] At this time, Huxley and his fellow supporters of "the right cause" had already been the dominant power in British science for ten years. Owen's original conception of the Natural History Museum had been well-overtaken by the precepts of scientific naturalism long before the opening of the new building.

The index museum which Owen envisaged for the large entrance hall depicted a structure within the natural world which the Huxleyites did not recognise in 1880. Owen's plan was overturned. The new building proclaimed its unmistakeable new role as a shrine to pure nature unadorned by metaphysics or religion. Huxley's part in shaping the new world view of which this new meaning of the Natural History Museum forms a revealing aspect has been described earlier. His part in directly influencing the character and conduct of the Museum is typical of his method of patient, long-term intrigue. When Owen retired from the Museum in December 1883 the new role of Director was to be far larger than that of Superintendent which Owen had held for so many years at Bloomsbury. For a number of years a campaign had been kept up to wrest control of the Natural History Department from its parent body, the British Museum at Bloomsbury. The independence of scientific natural history from the literary and artistic concerns of the British Museum is a clearly foreseeable aim of the Huxleyites. The agitation succeeded to the extent of the new definition of a wider role for the new Director compared with that of the previous Superintendent. The Trustees wanted the new chief to take a far more active and responsible part than Owen had taken or been permitted. Owen had further freed his time for personal research by enjoying the services of an administrative assistant. The new Director was still to be subject to the final authority of the wholely non-scientific Principal Librarian of the British Museum, Edward A. Bond. His Principal Trustees with the responsibility for appointing the first Director were: the Lord Chancellor, the Archbishop of Canterbury, and the Speaker of the House of Commons. It has been stated by one writer that the leading Liberal politician Robert Lowe wished Huxley to be the head of the Natural History Museum as early as 1863.[111] As Chancellor in Gladstone's

Liberal Ministry (1868-73) Lowe earned Hooker's profound mistrust over the Ayrton controversy. Lowe was an admirer of Darwin and an enthusiastic supporter of Huxley having met the latter as a young naval surgeon during his sojourns at Sydney during the voyage of the "Rattlesnake".[112] In December 1883 when the Trustees of the British Museum appointed William Henry Flower as first Director of the Natural History Museum, Huxley was pleased but not surprised. Four years later, he was made a Trustee of the increasingly independent new body in Cromwell Road. Huxley recounted to Hooker how he had advised Lowe during his Chancellorship to put Flower in charge and appoint himself a Trustee "to back him up".

> "Bobby no doubt thought the suggestion cheeky
> but it is odd that the thing has come about."113

Flower was a Darwinian and had supported the cause at a lively time in its history. By confounding Owen during one of the crucial controversies with Huxley, Flower earned the gratitude of the Huxleyites. As Director, Flower introduced Darwin's theories of evolution into the Museum.[114]

The continued subjugation of Flower to the authority of the Principal Librarian of the British Museum remained an affront to the zealous among the scientific naturalists. It represented the enduring hegemony of the classically trained civil servants over the scientists. To the leading Huxleyites the situation expressed institutionally the seemingly unshakeable sway of party politics over the scientific method. Such a situation would certainly preclude the hoped for development of a new scientific social order through the untrammelled elaboration of the "new nature". However, Flower's personal relations with his superior, Edward Bond, remained amicable to the last. In October 1897, Flower collapsed. He resigned in August 1898 and died on the 1st of July 1899 at the age of 67. During the period of uncertainty from late 1897, rumours circulated to the effect that the Trustees

of the British Museum intended to make Flower's successor, "a kind of lieutenant to the Principal Librarian at Bloomsbury".[115] Following a press campaign a memorial was sent by twenty-three Fellows of the Royal Society to the Trustees in July 1898. The memorial high-handedly asserted that natural history ought not to be demeaned by its control by any non-scientific officer of the Museum. The person who suffered most by the Royal Society's intervention was the likely successor from within the existing Natural History Museum staff, Lazarus Fletcher. He was Keeper of Mineralogy and in the view of the memorialists, lacking in scientific eminence. They surmised that the Bloomsbury mandarins would welcome the pliability of a time served man of little independent scientific authority. The unfortunate Fletcher suspected the chief authors of the memorial to be Michael Foster, Ray Lankester, and W. F. R. Weldon. Having received support from The Times, the memorialists' suspicions were somewhat allayed by denials of the anti-scientific intentions of the Trustees. These assurances came from the new Principal Librarian, E. Maunde Thompson, and from Flower himself. The British Museum's Standing Committee of Trustees included two survivors of Huxley's "army of liberalism" in Lord Avebury (Lubbock) and Sir John Evans. The Committee unanimously approved the appointment of Fletcher. The decision became known informally and the more radical Huxleyites led by Foster and Lankester acted quickly to express their opposition. Neither of them would have been surprised at the evident "flabbiness" of Evans and Avebury. Three days after Fletcher's appointment, they prepared another memorial for the signatures of Fellows of the Royal Society.

The Council meeting at Burlington House on the unusual date of the 7th of July (the session being over) produced an impressive display of signatures. The memorial was printed in The Times immediately.[116]

Lankester's name was a significant omission from the document, especially considering that he had personally canvassed John Murray, the successor to Wyville Thomson as head of the "Challenger" expedition work, for his signature. After a few days, Flower asked Fletcher to withdraw. Ray Lankester, with his large scientific reputation and the authority of the Linacre Professorship, was seen by the memorialists as being more able to realise their vision of the public relations potential of the huge new enterprise in the Cromwell Road. His appointment was confirmed on the 3rd of August 1898. Lankester resigned his Oxford post and took up a career which was doomed from the outset to be a running battle with the Trustees over the extent of his authority. The storm clouds gathered when his chief, Maunde Thompson, questioned the extent of Lankester's vacation in the latter part of 1899.

Lankester's natural irascibility was mingled with righteous aggression aimed at shoring up the crumbling Huxleyite cause which had urged him to seek the appointment in the first place.[117] The quarrel dragged on until Lankester was forced to submit to an almost public examination of his bona fides by the Trustees. In May 1900 yet another Royal Society memorial was launched, probably by Foster again struggling to get things done as they had often been done effectively in former times. The latest offering attacked the non-scientific management of the Natural History Museum. This time Lazarus Fletcher and George Murray, another Keeper at the Museum responded with a powerful memorandum which criticised both the opinions and methods of the Council of the Royal Society. George Murray was aware of Lankester's wish to transfer the Museum's Department of Botany to Kew where it would come under the charge of his friend (and fellow Huxleyite) Thistleton-Dyer. The Fletcher-Murray memorandum remarked on the propriety of the May Memorialists of the Royal Society and said of E. B. Poulton who had circulated it that he:

> "was himself doubtless acting in good faith . . . he was merely an instrument in the hands of more astute persons to whom anonymity is for the moment an advantage. We have good reason for asserting that one of the parties to the drafting of the Memorial is Sir Michael Foster . . ."

Fletcher continued to the effect that Foster's methods reflected a:

> "great lack of candour and fair play in this matter of dealing with the Royal Society Council and in this mode of attempting to secure the official help of the Royal Society in support of an attack on the Trustees of the British Museum or on ourselves."118

Murray and Fletcher concluded that the covert intriguing led by Foster had been sufficiently disreputable to warrant an inquiry into all the circumstances of the Memorial of May 1900 and the original one of two years earlier. Foster was the leading signatory of the latter which had secured Lankester's appointment to the Directorship. The Royal Society was exposed to official censure and public odium in a way that had never occurred in the halcyon days of Huxleyism. Eighty Fellows had signed the 1898 Memorial so the implications of any substantiated charge of impropriety were great. The Society then tried to dissociate itself from the whole affair. Its chronicler states:

> "The attack on the Trustees by a letter to the 'Times' backed by the President and Council of the Royal Society and by scores of prestigous signatures as intended by Lankester and his supporters, had been defeated. It was to be nearly twenty years before another was launched from Burlington House."119

The controversy surrounding the policies of the Marine Biological Association in 1887 gave the Huxleyites one of their earliest experiences of the damaging effects of displaying public rancour and disunity. Later in the year the far more serious split occurred over the issue of Stokes' "political Presidency".[120] When the Natural History Museum crisis came to its full intensity, Huxley had been dead for five years and the climate in the scientific world in London had changed. The

attempts of the remaining "hard line" Huxleyites to govern scientific affairs in the old way were destined to be exposed and discredited.

The New Century

Natural Selection had gained widespread acceptance by the end of the century but the wider social and political programme of scientific naturalism was virtually defunct. The key scientific concepts which materialism had identified with were being left behind by modern science. By a cruel parallel irony, the world picture of the Cambridge mathematical physicists was also about to be overturned by the sudden outgrowth of atomic physics. The strictures which Lord Kelvin had successfully placed on the age of the earth in order to deny Darwinian evolution's full explanatory power were likewise swept away by discoveries in radioactivity. The physicist John Perry expressed the lack of social advancement which science had achieved during the previous 25 years when he stated in 1900 that: "all the most important, the most brilliant, the most expensively educated people in England" remained uninformed about both the principles and methods of science.[121] The survivors of the original Huxleyites suffered a predictably piquant anti-climax. Theirs was the superannuated revolutionary's sense of events having inexplicably run on beyond his knowledge and sympathy. In 1906 Hooker wrote to Joseph Larmor, the last of Cambridge's world leaders in mathematical ether theory.

> "I wish that I knew as much about starch as I did in 1878 - as it is I am far behind hand and read my weekly Nature with wonder and awe. My inability to attend the R.S. meetings is a great regret."122

A number of scientific naturalists received state honours, but in several important cases they were not conferred primarily for scientific eminence. Hooker felt hounded by the prospect of official honours for years because of the difficulty of preserving intact the public

image of scientific independence demanded by strict Huxleyism. In 1874 Hooker told Huxley that he didn't want a C.S.I. (Star of India) because it would have been conferred only for services to the Empire. He did not want to accept a mere knighthood (K.C.B) because it did not cite special services. Murchison and Lyell set off to see the Duke of Argyll to get him to confer a C.S.I. on Hooker but the Duke would not comply. According to Hooker, "the Duke might do it with the stroke of a pen, but he don't like my Darwinism and my [R.S.] Address and I am right proud of that."[123] Frankland received the K.C.B. in the Jubilee Honours list of 1897. At the time it was said to be largely for his contribution to the technical improvement of water supplies.[124] It was very difficult for the scientific naturalists to sustain the fiction that their official honours reflected the emergence of the scientific enterprise as the transfiguring blueprint of a new social and political order. Everybody knew that droves of mere party politicians and camp followers of Victorian high society continued to be honoured in the traditional fashion. The scientific naturalists were therefore more likely to be compromised than bolstered by official decorations. Towards the end of the century when the leading Huxleyites were passing into retirement it was not uncommon for them to experience serious difficulties in obtaining their pension rights. This happened to Frankland, Huxley, and later on to Lankester.[125] At the same time, the Huxleyites were well-aware of the extent to which the special sense in which they interpreted their own official honours was clouded in the public mind by the indiscriminate acceptance of honours for their own sake by the Cambridge Mathematical physicists. Hirst provides a double insight into this process by his description of Lockyer's report in Nature of the dinner given by Sir Frederick Bramwell at the Goldsmith's Hall in honour of William Flower in 1889. As has

been noted earlier the dinner was ostensibly for the departing President of the B.A.A.S. to display public deference for the new President-elect. Hooker, Huxley and Tyndall regarded this as a threatening piece of self-vaunting flummery on the part of Bramwell who was a leading exploiter of science for what the Huxleyites saw as petty commercial ends. Bramwell's scientific credentials were lowly. Hirst clearly distrusted Lockyer for his devotion to garnering social distinction regardless of its provenance.

> "Lockyer, who was present at it, wrote much twaddle about science being represented on the occasion of the Queen's (70th) Birthday Celebration. On the same page of Nature was an account of Stokes Baronetcy, which was regarded by the writer who daily grows cockier and cockyer, [an allusion to Tait's rhyming critique of Lockyer] less as an honour to science than a favour to the conservative party in the House of Commons; to which House and party Sir George now belongs."126

This incident is worth detailed consideration because it illustrates well the beginning of the sort of divisions which weakened scientific naturalism from the inside. Taken with the formidable resilience of the existing structure of power relations in late Victorian society these division provide an explanation of the virtual disappearance of Huxleyism as an effective force by the turn of the century. In retrospect the burial of Charles Darwin in Westminster Abbey turned out not to be the triumphal turning point in an ever upward march of the scientific naturalists. For the serious and extremely ambitious "new bearers of culture" the Abbey ceremony was the high water mark of a movement shortly on the wane.

A quite false significance has been given to the fact that Huxley was appointed to the Privy Council in June 1892. He had "always been dead against orders of merit and the like". For science and letters, Huxley approved of only the highly exclusive appointment to the Privy

Council. Donnelly's grounds for Huxley's recommendation were presumably based on his contributions to the development of scientific and technical education. Huxley himself pointed out that the accolade was not all that it seemed to be, in a reply to Frankland's good wishes.

> "Many thanks for your congratulations (and prayer). But I do not think the latter is likely to be answered as the only P.C.'s whose advice counts for anything are those in the cabinet. The G.O.M. has not offered me a seat on that body."127

In the end, none of the Huxleyite outposts was safe. This was to prove so for John Donnelly, procurer of the Privy Councillorship for Huxley. Many politicians and civil servants had for many years entertained suspicions of the Science and Art Department. During that time Donnelly had been secured by Huxley's pre-eminence and the staunch support of his political chief Sir Henry Acland. In 1895 Huxley died and Acland vacated his office with the fall of the Earl of Roseberry's Liberal Government. The incoming Conservative Ministry under Lord Salisbury enabled the grumbling hostility towards Donnelly to emerge into the open. Attacks were mounted on him resulting in his departure in unhappy circumstances in 1899.[128]

By this juncture, Huxleyite influence had diminished to the extent that even inside the Council Chamber of the Royal Society it counted for little. When John Evans retired from the office of Treasurer in 1898, a little-known Cambridge man called Alfred Kempe was quickly proposed to succeed him. As stated earlier, a faction led by Hooker, Lockyer, (P.356) and William Crookes signalled their intention to nominate Lubbock to contest the election. The group committed themselves to the notion that officers of the Society ought to posses authentic scientific eminence in their particular field. This requirement is somewhat ironical considering the moderate scientific standing of a number of Huxleyite appointments to the executive. In this category would

be placed Spottiswoode, Foster, and Evans. Lubbock, who refused to be put in for the Treasurership in 1898 because he would not take part in a contest, would have provided a further example had he been appointed. The Huxleyites then decided to run Lockyer as their candidate because he possessed the widest circle of connections. He was beaten in the election and withdrew in high dudgeon from active participation in the Royal Society's affairs.[129] Five years later, Foster was unsuccessful in his attempt to secure the succession of his Secretaryship. The sympathetic physiologist of his choice lost a contested election to Archibald Geikie, Director of the Geological Survey and bulwark of Kelvin's limited age of the earth. Geikie took the appointment by a large majority.[130]

Evidence of concerted opposition to Huxleyism during its hey day is hard to find in institutional intrigues. It is more evident in support for theoretical anti-Darwinian initiatives such as that launched by Sir William Thomson in respect of the age of the earth. This area is explored in the next chapter. The maverick person of James Dewar was suspected, by Michael Foster, of being behind the Times attacks of the early 1890's. His statements in private correspondence provide a rare, if singular, sidelight on the outsider's view of residual Huxleyite influence in eclipse. Confusingly, Dewar identified the new potency of Cambridge physical scientists with the guiding influence of Michael Foster. The following extract is from a letter sent by Dewar to Sir William Thomson then President of the Royal Society about a burning issue of the day now obscured.

> "It came about by the conduct of the late V.C. [Vice-Chancellor]. He behaved in the most extraordinary way to both Liveing and myself. The whole affair is - [?] and discredible to the University. The place is infected with a clique of scientific heavy rollers chiefly organised by "Michael" and his physiologists"131

Two years later (1893) Dewar corresponded with Frankland's pupil, H. E. Armstrong, about his suspicions of dark work by Lockyer in reinforcing Royal Society bias towards German scholarship in chemical work. Dewar accused Lockyer of being "at the bottom of the whole affair".[132]

In 1893, Thistleton-Dyer still envisaged a near future in which the structure and function of the Royal Society would change in full conformity with the radical precepts of scientific naturalism. Although he was given to mildly apocalyptic speculation, the direction of Dyer's thought is significant.

> "I attatch but little importance to the papers. I am convinced that the great function of the R.S. in the future is to take the initiative in matters concerning science. As the legislature gets more and more democratized the need of impartial and -[?] intelligence will become more and more necessary. And in the scientific field the R.S. in my opinion sh'd supply it. And this positively at the moment it does less and less. Even the British Association is a more effectively - body in public matters. The simple fact is that the R.S. from one cause or another is drying up. Who is to make the dry bones live?"133

Dyer did little to achieve the ends set out in this letter to Huxley. He retired at the age of 62 in 1905 to devote himself to an old interest in ancient botany. Ray Lankester, of whom so much was required after the early deaths of Francis Balfour and H. N. Moseley, ended his effective but acrimonious tenure as Director of the Natural History Museum at the end of 1907. He was forced to retire by the Standing Committee of the Trustees of the British Museum. To get rid of Lankester they insisted on enforcing a technicality in the Civil Service rules on a superannuation which enabled any head of department to call upon any of his officers to retire at the age of sixty. This was a great humiliation to Lankester and the coup de grâce to his original purpose in taking the post. The reduced pension of £300 per annum to which he was then entitled was a serious additional blow. In exquisitely

ironical fashion it was the Archbishop of Canterbury, as one of the British Museum's Trustees, who came to his assistance. The head of the Anglican Church urged that Lankester be granted a Civil List pension of £250 per annum and a knighthood in addition to his reduced pension.[134]

Michael Foster vacated the Senior Secretaryship of the Royal Society in 1903 after twenty-two years. Following his tenure, the ten-year limit was placed on the occupation of the Secretaryships. Foster died suddenly at the age of 73 in 1907 of a burst oesophagal ulcer. Curiously his range of major concerns about his personal health paralleled Huxley's.[135] His retirement in 1903 ended Huxleyite representation in the Society's executive. The advent of Joseph Larmor in 1903 and Geikie in 1903 meant that the Secretariat was firmly in the hands of the Cambridge physicists. Kempe had been Treasurer since 1898. Francis Darwin was made Foreign Secretary five years later.[136] Clear evidence has been set out (Note 107) which reveals that William Huggins, President from 1900-1905 was a Cambridge appointee. The power of the Huxleyites in the forefront of Royal Society affairs lasted from 1871 when Spottiswoode was elected to the Treasurership until Foster's retirement in 1903. Although this period is nearly concurrent with his stay as Secretary, the period of Huxleyite domination of the Society lasted a bare fifteen years. This began when Hooker took the chair in 1873 and effectively ended with the crisis over Stokes' "political Presidency" in 1887.

* * *

Notes

1. Rollo Appleyard, Pioneers of Electrical Communication, Macmillan, London, 1930, pp. 211-260.

2. F. M. Turner, "Public Science in Britain 1880-1919", Isis, 1980, 71, pp. 589-608.

3. Leonard Huxley, Life and Letters THH, vol. 2, pp. 340-341.

4. Huxley Papers (ICL) 2.297.

5. Leonard Huxley, ~~ibid~~., p. 54.

6. See Stokes Papers (CUL) for Stokes' scientific dealings with each of these individuals.

7. Robert John Strutt, Life of Lord Rayleigh, Arnold, London, 1924, p. 168.

8. Stokes Papers (CUL), F258.

9. Huxley Papers (ICL), 4.279.

10. Ibid., 4.325.

11. Ibid., 4.269.

12. Ibid., 3.272.

13. Leonard Huxley, Life and Letters THH, vol. 2, p. 174.

14. Huxley Papers (ICL), 2.293.

15. Stokes Papers (CUL), K8.

16. Huxley Papers (ICL), 2.295.

17. Ibid.

18. Ibid., 4.294.

19. Ibid., 2.297.

20. Ibid., 4.296.

21. Ibid. The memoir which Stokes' daughter contributed to Larmor's Memoirs and Correspondence of her father contained a curious detail. According to Isabel Humphry her father, whilst out walking: "gave me a most eloquent lecture on the Royal Society, its foundation, growth, position in England, position in the world, and spoke of the misfortune it would be to science if it ever departed from its isolated position, or got entangled socially, or politically . . . ." (Joseph Larmor, Memoir and Correspondence of George Gabriel Stokes, Cambridge, 1907, 2 vols., vol. 2, p. 36.

22. Hirst Journal, 2641. In 1880 Hooker told Stokes that Williamson was "the most tiresome expounder and verbose writer I know" (Stokes Papers CUL, H951).

23. Huxley Papers (ICL), 4.302. According to his daughter Isabel, Stokes' reign as P.R.S. was "not marked by any stirring or disturbing events" (Larmor, ~~ibid.~~, pp. 100-101).

24. Huxley Papers (ICL), 27.210.

25. Ibid., 3.179. In November 1872 Hooker wrote bitterly to Huxley: "Nature has not done well in the whole affair of Kew & Ayrton" (Ibid., 3.176).

26. Ibid., 3.307.

27. Ibid.

28. Ibid.

29. Ibid., 3.316.

30. Ibid., 22.100. There was a mixture of opinions among the X club members, but none of their serious enemies opposed Stokes' entry into the House of Commons. One particularly serious opponent St. George Mivart wrote to Stokes assuring him of personal support in addition to that of P. Scläter and William Flower. The three approved of Stokes' "double position" and promoted the idea the presidents of the leading scientific societies should be ex-officio M.P.'s (Stokes Papers CUL, M626).

31. Huxley Papers (ICL), 3.316.

32. Ibid., 16.272.

33. Ibid., 2.324. In 1887 Hirst described his "sad political history" to a huckster who chanced to call. Hirst recounted in his journal that he had been a lifelong liberal, as had his father before him. At the last election - during the previous year - Hirst had not voted. In 1887, Gladstone having "lost his head", Hirst had voted for the Tory candidate for Marylebone. The 1887 election saw Salisbury safely established for a five year term (Hirst Journal, 2442).

34. Huxley Papers (ICL), 2.308.

35. Hirst Journal, 2735. Katherine O'Shea was divorced from Captain O'Shea and remarried to Parnell, The scandal caused a split between the Parnellists and the Gladstonites which seriously damaged the Home Rule cause.

36. Huxley Papers (ICL), 8.264.

37. Hall, ASN, p. 120.

38. Royal Society, NLB 1, no. 262-370. Foster expressed strong disapproval of this turn of events but he disapproved for very different reasons from those which motivated Stokes. The latter

saw his, and by implication the Society's, prestige being impaired. Foster and Huxley seem to have regarded it as a blow to their ambition to raise science to a new position within national life in which it would transcend conventional party politics.

39. Leonard Huxley, Life and Letters of THH, vol. 2, p. 212.

40. Royal Society, NLB 1, no. 353.

41. Huxley Papers (ICL), 4.305.

42. Terence Johnson, Professions and Power, Macmillan, London, 1972, p. 45.

43. Ibid., p. 46.

44. Hilary Rose and Steven Rose, Science and Society, Penguin, 1971, p. 38. D. S. L. Cardwell, "Science and the European Tragedies", Percival Lecture, p. 26. Cambridge's élitist esprit de corps covered science and classics. In 1862, William Thomson told Stokes that "Lushington was not senior wrangler because he was senior classic" (Stokes Papers CUL, K135).

45. Cyril Bibby, T. H. Huxley Scientist, Humanist, and Educator, Watts, London, 1959, p. 131.

46. Royal Society, NLB 1, no. 311.

47. Royal Society, NLB 5, no. 149. Thomson was P.R.S. at the time. Ayrton was one of his keenest supporters.

48. Huxley Papers (ICL), 4.316. Murray eventually got what he wanted: a knighthood and a substantial government honourarium for his work on the deap sea deposits (Ibid., 4.310).

49. Ibid. It may be recalled that by 1914 the treasury had recouped an amount equivalent to the whole cost of the "Challenger" expedition from tax revenues on the Christmas Island guano.

50. Ibid., 4.333. Only Sir Richard Temple bettered Stokes' attendance record at the House of Commons.

51. Stokes Papers (CUL), E 52.

52. Barton, "The X club", p. 157.

53. Huxley Papers (ICL), 3.413.

54. Ibid., 4.325.

55. Ibid., 16.268.

56. Ibid., 4.318.

57. Royal Society MM VIII-IX, no. 13.

58. Open University microfilm of Frankland Papers (RFC).

59. Huxley Papers (ICL), 4.345.

60. Hirst Journal, 2685.

61. Royal Society, NLB 7, no. 330. Rix could rarely keep in touch with Kelvin. In 1893 the harrassed Assistant Secretary wrote to Foster to say: "I discovered that Belgrave Square meant Edinburgh and telegraphed on Saturday to the President" (Royal Society, NLB 8, no. 269).

62. Huxley Papers (ICL), 4.357. During his 1888 Presidential Address, Stokes announced the decease of a Fellow - Gull - who was very much alive.

63. Royal Society, MM VIII-IX, no. 15.

63*. Royal Society, MM VIII-IX, no. 15.

64. Royal Society, ibid., no. 16.

65. Royal Society, ibid., no. 17.

66. Huxley Papers (ICL), 3.370.

67. Royal Society, NLB 12, no. 147.

68. Huxley Papers (ICL), 2.232.

69. Leonard Huxley, Life and Letters fo THH, vol. 2, pp. 386-387. The time-scale of such awards does not seem to reflect straightford recognition of scientific achievement. Frankland waited until 1894 for the Copley Medal.

70. Huxley Papers (ICL), 2.444.

71. The Times, 1st December 1892. Frankland mentioned in a letter to Huxley how the Council used to "vote solid" almost invariably (Huxley Papers ICL, 16.272).

72. Royal Society, NLB 7, no. 46.

73. Huxley Papers (ICL), 27.226.

74. A. J. Meadows, Science and Controversy a Biography of Sir Norman Lockyer, Macmillan, London, 1972, pp. 227-228. Howarth made FRS 1893.

75 Royal Society, NLB 12, no. 376. Rix noted that when Mrs. Kennedy, cleaner of the R.S. rooms in Burlington House, had entered her employment the total staff had been five persons. At the time of his letter in July 1894 the staff numbered fifteen. For Mrs. Kennedy the Catalogue Room (for work on the R.S. Catalogue of Scientific Papers) was the last straw (Royal Society, NLB 9, no. 356).

76. Royal Society, NLB 12. no. 376.

77. Royal Society, NLB 13, no. 11 and 23.

78. Huxley Papers (ICL), 3.413.

79. Huxley Papers (ICL), 3.414.

80. Huxley Papers (ICL), 4.363.

81. Loc. cit.

82. Huxley Papers (ICL), 4.369 and 4.370. G. E. Buckle was the formidable editor of The Times.

83. James Dewar was known for determination, irascibility, and his great expertise as an experimenter. Dewar suffered ruptures in his relationship with nearly everyone. Two years earlier Hirst had heard, following an ordinary meeting of the R.S. that the real cause of Dewar's well-known breach with his Cambridge assistant "was the arrival there of a barrow load of gutta perch tubing & c. which the latter was ordered to examine; but declined to do so!" (Hirst Journal, 2830).

84. Royal Society, NLB 7, no. 411.

85. Huxley Papers (ICL), 4.371.

86. Royal Society, Register of Papers.

87. Paul F. Cranefield (ed.), Two Great Scientists of the Nineteenth Century: Correspondence of Emil Du Bois-Reymond and Carl Ludwig, John Hopkins University Press, Baltimore, 1982, p. 73.

88. J. M. Cohen, The Life of Ludwig Mond, Methuen, 1956, p. 220.

89. Huxley Papers (ICL), 14.159.

90. Huxley Papers (ICL), 2.454.

91. Robert John Strutt, Life of Lord Rayleigh, Arnold, London, 1924, p. 173.

92. Ibid. The author states that his father had completed his ripost before getting Lister's letter.

93. Royal Society, NLB 7, 147. T. J. Parker was trained at South Kensington by Huxley, became his assistant, then emigrated to Otago in New Zealand as Professor. His father received in all £2,500 from the Government Grant - around £100,000 in modern monetary terms.

94. Stokes Papers (CUL), K 106.

95. Huxley Papers (ICL), 3.372.

96. Huxley Papers (ICL), 27.232. At the apogee of Huxleyite influence Hooker had had the best attended Anniversary meetings and soirées (preceded by more formal conversaziones) in memory.

97. Richard B. Fisher, Joseph Lister 1827-1912, Macdonald and Jane's, 1977, p. 298.

98. Ibid., p. 299. Lister had, like other eminent Victorians, lost his religious faith.

99. Dr. J. K. Franklin, "A short history of the International Congresses of Physiologists", Annals of Science, 1938, pp. 241-298.

100. Royal Society, NLB 12, no. 202. Rix described to Foster how Dyer went to take the chair as Soirée Committee chairman when the President (Stokes at this juncture in 1896) dropped into it "in all innocence under the Statute VII.I, which obliges him to do so whereupon Mr. Dyer went home in a huff."

101. Royal Society, MC 17, no. 47. Dyer was described by Hirst as being "captious, quarrelous, sententious" and as "speaking far too much" (Hirst Journal, 2578, 2666).

102. Royal Society, MC 17, no. 12.

103. Royal Society, NLB 14, no. 154. Ibid.
104. Royal Society, NLB 14, No. 154.
105. Stokes Papers (CUL), F 258. The scathing review appeared in Nature, Dec. 30th 1897, p. 195. A letter from Foster to Huxley twelve years earlier gives a clue to the explanation for this incident. A relative of Walter White was in the employ of the R.S. and was passed over for the appointment as clerk to the Society when Rix replaced Walter White as Assistant Secretary. Foster wrote to Huxley: "The man James who we have appointed in Rix's place, promises very well indeed. WW did not like our passing over his black nephew, who works on scientific catalogue; but we did right nevertheless." A grudge of long standing developed in one or more of the Whites which found expression in the publication of Walter White's Journal after his death (Huxley Papers, 4.250).

106. In January 1881 Foster was more or less "appointed" by Huxley to be his successor in the Secretaryship of the R.S. Foster wrote to him saying: "As to R.S. of course if things turn out and Flower won't stand - I will see if I can conscientiously follow your bidding (Huxley Papers ICL, 4.217).

107. Royal Society, La. 1731. Letter dated July 1898. Huggins received the Copley Medal in that same year, Rayleigh in the following year. Lockyer was defeated for the Secretaryship by Geikie, an erstwhile comrade who wrote leaders for Nature. Lockyer withdrew from R.S. affairs. Within a few years Larmor was a central figure in the take-over of Lockyer's Solar Physics Observatory by Cambridge University.

108. Meadows, ~~ibid.~~, p. 228.

109. Huxley Papers (ICL), 14.144. Sir Arthur Acland was Vice-president of the Committee of Council on Education. As a Liberal politician with a seat in the Cabinet, he was sympathetic towards Donnelly.

110. W. T. Stearn, The Natural History Museum at South Kensington, Heinemann, London, 1981, p. 69. The statue of Adam which had pleased Richard Owen was toppled in an air raid during the blitz.

111. Bibby, ~~ibid~~., p. 107.

112. Ibid. Lowe was himself a visitor to Australia, working in practice as a lawyer. He became Home Secretary 1873-1874.

113. Huxley Papers (ICL), 2.312.

114. Stearn, ~~ibid~~., p. 76. Flower's most prominent contribution was the confirmation of the simian origin of the human skull.

115. Ibid., p. 77.

116. The Times, 8th July 1898.

117. Stearn, ~~ibid~~., pp. 79-82.

118. Ibid. Foster was usually anxious to support Kew at the expense of the Natural History Museum.

119. Ibid., p. 84.

120. Huxley Papers (ICL), 15.218. Evans stated that only Huxley was capable of keeping Lankester's excesses in check.

121. Frank Turner, "Public Science in Britain 1888-1919", Isis, 1980, 71, p. 599.

122. Stokes Papers (CUL), L 118.

123. Leonard Huxley, Life and Letters JDH, vol. 2, p. 146.

124. Strong doubt is cast on this contention by a letter from Oliver Lodge to A. J. Balfour, in which it is implied that the considerations of water analysis formed an impediment to Frankland's chances of a knighthood (Papers of the third Marquis of Salisbury at Hatfield) Huxley's appointment to the Privy Council was, according to Donnelly, for Huxley's contributions to the development of technical education. Hooker told Huxley that he viewed the prospect of Sabine's death with trepidation because of the immediate danger that the ex-President's KCB would be conferred on him.

125. O.U. microfilm of Frankland Papers (RFC), Stearn, ibid., p. 88.

126. Hirst Journal, 2604. As a focus of influence, Lockyer was not really effective by this time. Hirst mentions this frequently as in the following incident of 1889 in connection with the twentieth anniversary edition of Nature which reviewed major scientific advances during the twenty-year period, "the article in question does not mention the name of Lockyer or allude to his sensational Cosmogony. And yet I could not help recalling an exquisitely shrewd remark of the late Henry Smith about Lockyer 'He fails too often to distinguish between Editor, and Author of Nature'."

127. O.U. microfilm of Frankland Papers (RFC).

128. Meadows, ~~ibid~~., p. 263.

129. Ibid., p. 228. Correspondence between Joseph Larmor and Arthur Rücker shows that the business of the Council, appointments, medals, and so forth were by this time being organised by leading representatives of the group known herein as the Cambridge mathematical physicists.

130. A. Geikie, A Long Life's Work, Macmillan, London, 1924, p. 336. Geikie served for five years until 1908. When Foster retired in 1903 the representatives of Cambridge physical science achieved a dominance in the government of the R.S. which was to last for thirty years.

131. Kelvin Papers (CUL), D 82.

132. Dewar Letters (ICL), no. 251 and 252. Dewar was typically unimpressed by the scientific credentials of the R.S. executive in 1899. He declared that "not one of the permanent officers has anything like privileged class rank as investigators (Ibid., no. 257). Dewar objected to the potentially corrupting privileged class elections.

133. Huxley Papers (ICL), 27.228.

134. Stearn, ~~ibid~~., pp. 88-89.

135. In October 1894 Huxley listed these as "a. Seediness b. Liver c. Doctor says influenza d. Neuralgia e. lumbago f. ?

136. Record of the Royal Society, 1948.

CHAPTER 10

ASPECTS OF THE CONFLICT BETWEEN THE SCIENTIFIC NATURALISTS AND THEIR OPPONENTS

During the period of increasing Huxleyite domination of the Royal society, it is inevitable that numerous skirmishes took place in other parts of the British scientific world. Many of these involved the opponents of Darwin's theory of natural selection. This new source of tension between men of science was felt within the Royal Society itself before the publication of the Origin. In April 1858 a paper entitled "On the poison apparatus in the Actinadae" by Philip Henry Gosse was marked down for the archives of the Royal Society. The referees were George Busk and Thomas Huxley.[1] Gosse at the time possessed a wide reputation for acute observation and skilful description. He had previously acquired an enviable reputation at the Royal Society, with a series of Phil. Trans. papers to his name. During the period since his last publication had appeared in the Phil. Trans. Gosse had rejected Joseph Hooker's informal overtures which were intended to prepare the ground for Darwin's theory. Later in the same year Gosse published his Omphalos which sought to establish that (in accordance with the Biblical timescale) a world was created that already contained evidences of great antiquity.[2]

The self-conscious intention, on the part of the participating Darwin supporters, to defy ecclesiastical interests is obvious even at this early stage of their activities. The crucial alliance of Hooker and Huxley showed a great interest in facilitating the election of men of their circle to the Athenaeum Club. At the beginning of 1858 Hooker was clearly taking the initiative in this matter, judging from the contents of a letter which he sent to Huxley at that time.

> "I want Busk to be the next man if Tyndall does not come forward - but the less we say of these matters the better - only if you have an opportunity of poking fun at Murchison or any other influential

> committee man, do not loose [sic] it but talk sagely and confidentially of Busk and Tyndall . . .."

Hooker went on to recommend that Huxley pay his Athenaeum subscription and "come along on Monday night and help to swamp the parsons"[3]. . . A considerable furore was caused by Huxley's aggressive reaction to the way in which Darwin's Copley Medal was presented in 1864 at the Royal Society's Anniversary meeting.[4] This incident made it clear to Huxley that opposition to Darwin's new doctrine remained very powerful at Burlington House. In 1865 Michael Foster wrote to Huxley on this subject.

> "By the bye talking of emancipation don't you think science wants a little heroic striking off of fetters? I mean of course you do but don't you think that something useful might be done by comparing in the Reader work done by furriners [sic] during the last 30 years, showing the influence of the fetters? In physiology at least we should look <u>very small</u>."5

In 1886 Francis Darwin did not know the details of the situation surrounding the presentation of his father's Copley Medal twenty- two years earlier. When Huxley sent him the correspondence he was able to take a very light-hearted view of the affair.

> "I forgot to thank you for the Stokes-Copley correspondence - which amused me very much - I did not know the history of the row tho' I gathered there was one."6

Despite the emergence of evolution by natural selection as a dominant research tradition by the time of Francis Darwin's letter to Huxley, its position did not seem by any means completely secure to the veteran Huxleyites. It is helpful to recall that Darwinism took several forms by 1886 and that its original Huxleyite promoters perceived their version of the doctrine as being threatened from various quarters. They felt that the challenge from physicists based on the age of the earth was as yet unresolved, as were various doubts raised from within the

ranks of the evolutionary biologists themselves. These reinterpretations of Darwin from within the the biological camp hinged around such matters as the mechanisms of variation and the possible special creation of man's higher powers.

The radical outriders of scientific naturalism were not averse to disputing with the representatives of the clerical authority in the most sensational way. Such issues as the debate over the efficacy of prayer were not dignified by any sort of direct association with the Royal Society.[7] At the forefront of the most sensational confrontations between traditional belief and the stark demands of materialism was John Tyndall. In 1865 Tyndall became involved in a typical exchange of views in the pages of The Pall Mall Gazette, concerning the place of prayer in obviating the worst effects of cholera.[8] At the end of the period under consideration (1850-1900) religiously motivated opponents of Darwin, such as the Duke of Argyll were still mounting vociferous, effective attacks on the Huxleyite position. To a great extent the Royal Society simply reacted to the outcome of campaigns which were conducted elsewhere. One of the most important alternative arenas for the playing out of the conflicts engendered by The Origin of Species was provided by the British Association. One major source of interest in considering the interplay of Huxleyites and their opponents at the B.A.A.S. is that it shows where the line was drawn regarding behaviour appropriate to the august setting of the Royal Society. Explicit propagandising often took place at the British Association because it was not acceptable within the portals of Burlington House.

Darwinism at the B.A.A.S. Meetings

Following the much cited 1860 Oxford meeting, the main issues brought up by The Origin of Species were taken to the B.A.A.S. meetings virtually every year. Relatively little attention has been paid to the

circumstances of these subsequent exchanges by modern historians. For a lengthy period the Darwinists were unable to increase the amount of scientific and public attention directed towards their cause because successive Presidents remained diplomatically silent on the subject. The Huxleyites wanted the debate to transcend the meetings of Section D by being projected through the authoritative medium of the Presidential Address. Although Lyell presided in 1864 it was not until 1868 that Hooker became the first Darwinist to head the Association. Hooker's year followed that of the highly traditional Duke of Buccleuch whose turn had come after William Grove. The lawyer-physicist had played a central role in the reform of the Royal Society in 1847. This loose pattern of alternation of the Presidency between the traditional and more youthful reforming outlooks was hardened by Hooker's broaching of the issue of natural selection at Presidential level in 1868. Over the next five years a strict alternation was followed between leading Darwinists and their most eminent opponents. Where attempts were made to subvert this mutual accommodation, bitter wrangling ensued. The period in question saw the intensity of the debate over the age of the earth at its highest. The importance of the B.A.A.S. as a forum for manufacturing publicity for Darwinism in particular and the developing cause of scientific naturalism in general was not appreciated by all of the Huxleyites. In October 1861 Frankland wrote to Huxley revealing a complete lack of sympathy with this aspect of the Association.

> "The fact is I make a point of not going to the British Association meetings when they occur in the vacation, as the proceedings there are merely a hash of what one has been listening to for the previous year at the London scientific societies, and consequently they merely break in upon one's vacation to no purpose whatever."9

In Hooker's Presidential year Sabine wrote to tell William Sharpey that neither he as President of the Royal Society nor any other of the Officers would be going to the Norwich meeting. The President noted that W. A Miller the Society's Treasurer was a possible exception.[10] The antipathy felt by the men of the Royal Society executive of the late 1860's has been documented in the previous secton of this work. Their absence from the Norwich meeting of 1868 is not surprising in view of their strong opposition to the strong version of the Darwinian thesis to which Hooker's name was widely associated. Hooker's stated position at Norwich nonetheless shocked many people. At this time he had been actively promoting the Darwinian mechanism of evolution for ten years.[11]

For a number of related reasons the end of the 1860's brought the evolution debate to a high pitch of intensity. Sir William Thomson's most powerful undermining attacks on uniformitarian geology were mounted between 1868 and 1871. The Presidency of George Stokes at Exeter in 1869 was therefore of considerable significance to Thomson, his close ally in matters spiritual and physical. At the time Darwin was working on the last two editions of the Origin. That there was no simple or compelling counter to the physicists' new position had been shown by Huxley earlier in the year. His dogged but uninspired Presidential Address to the Geological Society had highlighted this only too well. As Burchfield points out, the fifth edition of the Origin which appeared in 1871 bore no trace of Darwin's troublesome calculation of the time required for the denudation of the Weald. Darwin's embarrassment by the Scottish physicists led him to introduce various contingent devices which would speed up the historical process of evolution. These compromises included aspects of Lamarkian theory and contributed to a clouding of many of the issues in the Darwinian debate towards the end of the century.[12] Stokes was not the first choice of President for the

Exeter meeting of the British Association in 1869. The Joint General Secretary Francis Galton wrote to Stokes explaining that the usual policy was to alternate between a naturalist and a physicist. Wherever possible the man chosen would also have some sort of local connection with the venue. Galton related to Stokes how Professor Adams the Cambridge astronomer and Mr. Fox Talbot the pioneer of photography had been selected on these criteria. Adams had refused and Fox Talbot had pleaded ill-health. Stokes accepted the prestigious Presidency, but Huxleyites had found their way into a number of other significant positions of responsibility. George Busk was to be President of the Biological Section and Heinrich Debus headed Section B (chemistry). Debus was a close friend and confidante of Thomas Hirst and John Tyndall. Michael Foster and Ray Lankester were Secretaries to the Biological Section.[13] Stokes' Presidential Address did not disappoint the knowing individuals who were expecting him to express his view of the limitations of science.

> "Admitting to the fall as highly probable, though not completely demonstrated, the applicability to living beings of the laws which have been ascertained with reference to dead matter, I feel constrained at the same time to admit the existence of a mysterious something lying beyond - a something which I regard not as balancing and suspending the ordinary physical laws, but as working with them and through them to the attainment of a designed end. . . . If a thick darkness enshrouds all beyond, we have no right to assume it to be impossible that we should have reached even the last link of the chain, a stage where further progress is unattainable, and we can only refer the highest law at which we stopped to the fiat of an Almighty Power."[14]

Immediately prior to the delivery of the Presidential Address, Hirst had dined with the Spottiswoodes, Lubbocks, Hamiltons and Busks. They were all staying at the London Hotel. Hirst then accompanied the party to hear Stokes. Hirst's Journal predictably reports the Address as being "clear and scientific, [its] orthodox conclusion lame and 'clap-trappish'."[15]

The divide between the physical and mathematical sciences and the biological sciences was widening rapidly at this time. The special quality of the situation resulted from an odd aspect of the relationship between the leading practitioners of these two divisions of the scientific enterprise. Whereas the mathematical physicists were fully versed in precise mathematical methods, the biologists whose science had very recently been part of the rather vague concern known as natural history, were not. Nevertheless, the ideological outlook of scientific naturalism set great store by the boundless range of scientific and technical possibilities opened up by the mathematical sciences. To modern eyes it can therefore appear ironical that it was the most accomplished exponents of the exact physical sciences who should insist upon their limitations. It was this misalignment of ideological commitment and actual scientific expertise which lent the debate over the age of the earth its particular quality.[16]

In 1869 William Thomson's attack on the current conventions of geology was in its early, most vigorous form. The debate clearly signified much more than the innocuous operation of a predictable subject rivalry. The mathematical physicists took up Thompson's attack on uniformitarian geology with a will. They were all Christians and very much inclined to deny the full, unconditional working out of evolution by natural selection alone. The mathematical physicists used Thomson's foreshortened geochronology as a means of counter-attacking

the "full Darwinism" of the young and confident Huxleyites. Darwin's evolutionary mechanisms could not be demonstrated in the way that the imperious elegance and seeming finality of mathematical physics apparently could be. The attempts of the Darwinian geologists and biologists to concert their efforts in the defence of the Origin were not an unqualified success. Huxley had earlier described the book as "a veritable Whitworth gun in the fight for liberalism". Thomson's pronouncements on the age of the earth were the most effective attempt made from within scientific circles to spike this gun.[17] The power of the threat posed by Thomson's chronological out-flanking manoevres must have been sobering to the Huxleyites who were getting their first taste of power within the small scientific establishment. The jockeying for position produced by debate on matters bound up with the internal technical aspects of the age of the earth question had wider consequences. The Huxleyites were prompted to re-evaluate their previously wholehearted approval of the methods of the mathematised natural sciences in order to defend Darwin. Admiration for the methods of the physicist was axiomatic for the scientific naturalist of the time. The questioning of this precept as a means of discrediting William Thomson's estimate of the age of the earth was a significant development. It led to a wider rivalry in providing competitive definitions of the nature of the scientific enterprise. When the Oxford mathematician J. J. Sylvester spoke as President of Section A at the Exeter B.A.A.S. meeting, he was aiming to settle a score with Huxley. Oliver Lodge described the incident in his reminiscences of the British Association meetings.

> "In this address Sylvester eloquently depicts the soul of the pure mathematician and vigorously contests the statement attributed to Huxley in 'The Fortnightly Review' (though it might have been said by many people), that mathematics 'is that study which knows nothing of observation, nothing of induction, nothing of experiment, nothing of causation'."18

Lodge had studied under Frankland at Jermyn Street, but later became an admirer of both Thomson's physics and his view of the limitations of science. The optical glass maker W. V. Harcourt was another keen supporter of the line taken by the Cambridge mathematical physicists. He mentioned the exchanges during Section A meetings at Exeter in a contemporary letter to Stokes.

> "I should think the mathematical section had never had so lively a séance as when Sylvester fell foul of Huksley [sic] for his after dinner speech and derogatory description of mathematics! When you pay us your promised visit I shall know how you approved of ----'s Euclid and shall beg to learn more distinctly from you the resemblance between mathematics and poetry." 19

As usual the General Committee of the British Association met at the venue of the annual meeting in order to make preliminary arrangements for the following year. The Committee initially resolved to appoint Huxley as the next year's President and to hold the meeting at Liverpool. According to Hirst there followed "a great difference of opinion. Dr. Miller proposed Edinburgh with a view of following up his motion, I believe, by another that Sir William Thomson should be President".[20] Hirst goes on to detail the defeat of Miller's motion by 91 votes to 86 after which the opposition shifted allegiance in the customary way so that the new President could be elected unanimously. Huxley had been proposed by Sir Stafford Northcote, and seconded by John Lubbock. Lubbock had impressed Hooker at the meeting proper by his conduct of an exchange with the prominent anti-Darwinian Duke of Argyll.[21]

Liverpool: 1870

Ten years after creating a minor sensation as a little known zoologist at the 1860 Oxford meeting, Huxley became President of the Association. He treated the vexed question of biogenesis in his Presidential Address. Stokes had repudiated the "favourite hypothesis of manufacturing life out of chemical reactions and thought out of mechanical motions" at Exeter the year before.[22] The Huxleyites seem to have themselves opposed the notion of spontaneous generation for somewhat different reasons. Nevertheless, Huxley's Address appears to have been delivered as a deliberate anodyne. Hirst unsurprisingly described the speech as a success. The fate of W. A. Miller the Royal Society Treasurer was less happy.

> "A more harmless [Address] was never delivered. Poor Dr. Miller, Treasurer of the Royal Society came down with Lady Lubbock and exhibited strange wildness of demeanour. he is said to have stated that he considered himself ordained to come to Liverpool to combat the Heresy of Tyndall and Huxley. He grew wilder and wilder and his physician Dr. Inman found it necessary to have him put under restraint (at an asylum I believe). This circumstance cast a gloom over us all; here is a thoroughly upright and to a certain extent clear headed and able man of science completely wrecked by religious mania." 23

Miller died two months later. Ironically it was this unexpected vacancy for a Treasurer at Burlington House which provided the initial point of entry for the Huxleyites to the Royal Society's executive.

Sir William Thomson and the Meteoritic Hypothesis.

Edinburgh 1871

The meeting took place during the first week of August. Thomson (who had not been to Liverpool in 1870) was to Preside whilst at the same time his friend the lively controvertialist P. G. Tait was to be in charge of Section A. Thomson invited his friend to join him on his yacht "Lalla Rookh" for a cruise among the Hebrides afterwards. James

Clerk Maxwell, Tait, Tyndall, and Huxley were also invited.[24] The last two names are remarkable inclusions. Both men were principal targets for Thomson's redetermination of the age of the earth which was being debated in very lively fashion at this time. In addition both had a history of acrimonious controversy with the Scottish Natural Philosophers. Tyndall was the most wilfully abrasive of the original scientific naturalists. For years he had been embroiled in a grumbling series of exchanges with Tait, which from time to time broke out in renewed ferocity. The fundamental cause of their antipathy was the division between the groups to which each looked to forward his vital interests. That both men were physicists served only to define the occasions and extent of their rancour. Both men sought to mobilise the explanatory power and the prestige of physics to document their respective metaphysical standpoints.[25] Thomson's inclination to assimilate opponents who were his social inferiors is plainly displayed in the invitations which Huxley and Tyndall received for the post-B.A.A.S. cruise aboard the "Lalla Rookh" in 1871. Thomson and all the eminent cronies who were to make up the rest of the party were closely allied on the vital issues. All of them enjoyed great renown as mathematical physicists and maintained strong Christian beliefs which convinced them of a limited domain for the effectiveness of science. As has been noted, the age of the earth debate was in full spate at the time of the Edinburgh meeting. Thomson's strictures on this subject were admitted by the boat party, to a man. For the Christian mathematical physicists, Thomson's chronological stumbling block to the full and unconditioned working out of the Darwinian thesis was very welcome. It seemed to ground the denial of untrammelled natural selection in irrefutably "hard" scientific terms. At the time when the wider controversy about Huxleyite philosophy was a vital concern in some educated circles,

Thomson's guest list for the post-B.A.A.S. cruise takes on a particular significance.[26]

During the previous year both camps had scored successes. Huxley's supporters had secured the Presidency for him, after having swept aside W. A. Miller's attempt to break the pattern of alternation in favour of William Thomson. After Miller's bizarre demise Spottiswoode had been successfully installed as the first major Huxleyite within the Royal Society executive. On the other side of the account, the Copley Medal had been procured for Joule in advance of Mayer and the number of geologists defecting to Thomson's abbreviated age of the earth had increased. This trend was subversive of the theory of natural selection and had begun in earnest in 1869 with the defection of Archibald Geikie. Huxley had come to an uncomfortable accommodation with Thomson's impressive show of strength in the same year. The two most controversial London-based scientific naturalists did not accept Thomson's unusual invitation.

Huxley's Presidential Address in 1870 had expressed the broad agreement of the "materialist party" with the stance of the mathematical physicists in favour of the principal of biogenesis.[27] In 1871 at Edinburgh Thomson delivered a rather sensational address intended to explain the possible beginnings of life on earth. His speech did not seek to enshrine the doctrine of biogenesis (after all any original supernaturally conceived act of creation must have been abiogenetic) in absolute terms. What Thomson did was to endorse biogenesis for all terrestrial purposes and shift the scene of the original emergence of life. This was the basis of his meteoritic hypothesis and it formed the main body of the Edinburgh Address. Constrained by his own estimate of the age of the earth as being in the region of 100 million years Thomson hoped to explain how life might have begun on earth. He was obviously

not averse to disregarding the biblical account of creation in all its particulars. Hooker wrote to Darwin on August 5th 1871 giving his opinion of Thomson's purpose and performance. He was clearly convinced that Thomson meant to project the original creative act into astronomical regions which were inaccessible to scientific study by the Huxleyites. Hooker began:

> "I have been reading Sir W. Thomson's address and I am anxious to hear your opinion of it. What a bellyful it is and how Scotchy. . . . "
>
> "Does he suppose that God's breathing upon meteors or their progenitors is more philosophical than breathing upon the face of the earth?"28

Hooker told Darwin that he thought that the whole of Thomson's approach was fabricated with the one object of undermining the Origin of Species. He acknowledged personally the great prestige accorded to mathematical physics. Alluding once more to Thomson's recent speech Hooker declared:

> "it seems very able indeed, and what a good notion it gives of the gigantic achievements of mathematicians and physicists - it really makes one giddy to read of them."29

Rather more had been expected of the Edinburgh meeting in the way of vigorous controversy than was reflected in the general conditional approval of Thomson's hypothesis. Tyndall did not attend the meeting and wrote to Thomson from the Swiss Alps at the beginning of August. The fact of writing to his Scottish foes at all was most unusual for Tyndall. The bonhomous tenor of his letter perhaps reflects the regular urgings of his image-conscious fellow Huxleyites to desist from his habitual public blood-lettings. He reported having been in the vicinity of an earthquake.

> "and indeed the tremor was felt and the noise heard at treble this distance . . . so that no doubt can remain that the thing was a thud of those forces which will probably occupy some of the attention of the Edinburgh meeting.
>
> Again most heartily I wish I was near you. Were I as strong as Tait and equal like him to beer

> and tobacco ad infinitum, I should never have come to Switzerland at all but gone to Edinburgh instead." 30

The relatively amicable aspect of the 1871 B.A.A.S. meeting was shortly revised by events in London, of which Tait almost invariably disapproved. The decision bore directly on Tait's long running battle with Tyndall over the apportionement of credit for the Law of Conservation of Energy. At the end of October Tait wrote to Stokes objecting to the proposed award of the Copley Medal to Mayer rather than to Helmholtz.

> "Although I am not and probably never shall be a fellow of the [Royal] Society I should very much regret for the credit of British Science that what appears to me so fatal a mistake should be committed. . . . I suppose the accident of my having been President of Section A has led to my being asked to write this note - perhaps you will hear in a more forcible manner from the President of the Association." 31

This initiative represented a continuation of the controversy which has often been described elsewhere. Joule had duly received the Copley Medal in 1870. Tait was an irascible man at the best of times. He was outraged at the possibility of the Royal Society "legislating", as it were, a parity between the merits of Mayer and Joule at the incidental expense of Helmholtz. Knowing full well who Tyndall's supporters were in his attempt to secure the Copley Medal for Mayer, Tait could not accept the implied compromise of the opinion of thoroughgoing natural philosophers by a group of non-mathematical scientific naturalists. In common with the other leading mathematical physicists Tait reinforced his religious faith with a strong sense of scientific superiority. He felt that as the mathematical physicists were far and away the most acute interpreters of nature so they were vastly more fitted to elucidate any larger questions. At the head of such questions stood the matter of the limitations of science (if any) and the

elucidation of the order of being which transcended tangible nature (if any). On occasion Tait could not disguise the disdain that he felt for the scientific naturalists who in like fashion took upon themselves the exclusive right to declaim on these larger questions. Tait asserted that:

> "There is a numerous group, not in the slightest degree entitled to rank as physicists (though in general they assume the proud title of Philosophers) who assert that not merely Life, but even Volition and Conciousness are mere physical manifestations."32

The Huxleyites reacted vigorously to the physicists' assertion of their epistemological seniority. Mention has already been made of Huxley's anti-mathematical campaign of 1869. It was unfortunate for the scientific naturalists that their own beliefs tended to affirm the claims of the mathematical physicists. The susceptibility of Huxley and Hooker to the propaganda (however implicitly conveyed) of Cambridge physics in its special guise of Scottish Natural Philosophy has been noted in their responses to the B.A.A.S. Edinburgh Address. In a publication two years earlier Tait showed scant respect for Huxley and the loyal Darwinian geologists whom he took to be part of a rudimentary "beetle-hunting" and "crab-catching" stage in the development of their immature science. He continued his swingeing dismissal of Huxley thus:

> "Let us hear no more nonsense about the interference of mathematicians in matters with which they have no concern . . . rather let them be lauded for condescending from their proud pre-eminence to help out of a rut the too ponderous wagon of some scientific brother."33

The years 1868-1873 marked the arrival of the Huxleyites at the forefront of the leading British scientific institutions. Although their success heralded the domination of British scientific life by the qualified middle class professional, the particular beliefs of Huxleyite scientific naturalism faded from the scene at the turn of the century.

The short reign of scientific naturalism in its special radical form was followed by the much longer hegemony of Cambridge scientists. The irony of the situation lies in the fact that whereas Darwinian evolution was received by posterity, the dynamical world view of Thomson and Stokes was rapidly overtaken by the birth of atomic physics. Both men lived out their dotage in rather bewildered fashion, so far as their scientific sensibilities were concerned.

Tyndall at Belfast 1874

The damage which the mathematical physicists had done to Darwinism by the time of the Belfast meeting of the British Association was quite considerable. Thomson's 100 million year age limit for the earth had received very wide support from physical scientists and a large degree of resigned acceptance from leading figures within geological and biological science.[34] There can have been little leeway for complacency amongst the Huxleyites early in 1874 as they contemplated the prospect of supplying their leading Irish spokesman for the chairmanship of the forthcoming Belfast meeting. As has been described in an earlier chapter, John Tyndall was deliberately excluded from high office in the Royal Society by Huxley with the approval of Hooker.[35] They saw Tyndall as being far too impulsive and hot-headed to do other than damage their cause in the context of the Royal Society. The B.A.A.S. was considered a far more suitable setting for sensational announcements. In spite of this it was felt by some Huxleyites that Tyndall had gone too far in his 1874 Address. To a significant extent the B.A.A.S. at this time acted as a safety valve for the tensions existing between interested parties which could not be expressed at the Royal Society. The latter body's authority was so bound up with the appearance of philosophical unity, in the minds of the most influential contemporaries, that it had to be held aloof from the real conflicts of the time. Nevertheless, Tyndall's Address at Belfast was expressive of such an

aggressive materialism that many people thought he had violated the rules of circumspection appropriate to the Association. Such disapproval was focused by his enemies.

Although Tyndall himself was an experimental physicist of wide renown, the mathematical physicists considered the activities of most of the scientific naturalists as scarcely scientific at all in the "proper" sense of the term that they recognised. In April 1862 William Thomson wrote to Stokes regarding his use of certain terms in a paper which he had recently sent in to the Royal Society.

> "I hope "kinematics" and "naturalist" will not be objected to fatally. I know that pigeon fanciers and beetle collectors will be desperately offended at being classed with Newton and Faraday, but still I think propriety and convenience of language renders it necessary to disregard their feelings."36

Stokes himself had no doubt as to the discrepancy between physical and biological science in the epistemological hierachy:

> "Darwin's theory has been accepted by many eminent biologists with a readiness which is puzzling to an outsider, especially one accustomed to the severe demands for evidence that are required in the physical sciences."36A

Early in 1873 William Thomson found his wishes thwarted by forceful committee work by the Huxleyites over the matter of selecting the President for the next year's Belfast meeting. In a postscript to a letter sent to Stokes on the 9th of May 1873, Thomson remarked:

> "I go to Cambridge tonight or tomorrow morning from Newcastle. I would like to speak to you there about the B.A. Presidency. I don't mean that what has been done by the Council can be undone but want to hear from you how it came about . . .. "

Thomson was in Newcastle to see the cable laying ship "Hooper" under test. His consultations with Stokes obviously reflect "concerted opposition" to Huxleyite schemes within the British Association. As was pointed out earlier, Ruth Barton and others have written of the absence

of all such opposition. Thomson's highly mobile parallel courses in industry and science seem to have prevented him from becoming an effective force in the management of British science during this era. In the matter of the 1874 B.A. Presidency there had been some "changing of the guard" within the Council. Thomson's letter continued:

> " I yesterday received the enclosed from Spottiswoode and was much surprised as I thought the Council had determined to offer the Presidency to Andrews [Professor Thomas Andrews of Belfast]. At a meeting when I was President there seemed perfect unanimity as to Andrews being the right man for the Belfast meeting . . . "37

Thomson appears not to have realised the degree of commitment and internal solidarity of the group who had decisively baulked his intentions over the Presidency of Belfast. In the later passages of the letter quoted above he went on to assure Stokes that "Tyndall himself would see all this and would agree" with him. Both Stokes and Thomson were anxious to secure their own interests. However, Thomson made a point of not giving up any of the time required by his purely personal ambitions and projects to the demands of the scientific societies. Having tried and failed to draw Stokes away from the administrative treadmills of London and Cambridge, Thomson subsequently relied upon him as both informant and representative of their common interests. This reliance was seriously misplaced in view of Stokes' obtuse naïveté. The goings on prior to the 1874 B.A.A.S. meeting serve to illustrate how Stokes' simplicity in public affairs served to keep Thomson's influence in abeyance.

The writers of the approved biography of Tyndall note encouragingly that his tendency to indulge in wanton controversialism appeared to be diminishing in the early 70's. Tait tried to draw him out in March 1871 by sending him an inflammatory letter. Tait referred to the attack on

Tyndall's views made by Zöllner in his Treatise on Comets. Tyndall replied in an unusually stoical vein which turned out to be shortlived. Tyndall replied:

> "Hirst brought me intelligence about Zöllner's book from Paris. . . . Ten years ago I should have been at the throat of Zöllner, but not now. I would rather see you and Clausius friends than Zöllner and myself."38

Tyndall's magnanimity over the long running struggles for priority in connection with the mechanical theory of heat was similarly transient. He had cause for his brief complacency. He knew that Mayer was to be awarded the Copley Medal for 1871. Helmholtz was to wait two more years for the honour, much to the disgust of his friends and admirers in Scotland and Cambridge. Tyndall was again uncharacteristically meek later in the year. Thomson omitted to make any reference to his work on comets throughout the Edinburgh B.A.A.S. Address which presented his meteoritic hypothesis. Hooker remarked on the omission in a letter to Darwin, pointing out that there was not the slightest reference to Tyndall's work "even when comets are his [Thomson's] theme, seems strange to me."[39] Tyndall was well used to rebukes and cajolery from fellow Huxleyites. They saw in his vigorous career as a controversialist a threat to the magnanimous and statesmanlike public image which they cherished as a goal for scientific practice. The version of the scientific enterprise which they sought to project was one marked by disinterested technical expertise. What Huxley's leading caucus of scientific naturalists wanted to avoid at all costs was the diminishing appearance of entanglement in the selfish wiles of party and faction. To be seen to be taken up with the same destructive methods as the despised party politicians would have been to contradict the special meaning and purpose which the Huxleyites claimed for science in the context of Victorian society.

Tyndall could never keep to the path of forbearance for long. Ultimately this resulted in his friends and allies keeping him out of the chair of the Royal Society. Early in 1874, following his selection as President of the British Association for the forthcoming Belfast meeting, Tyndall became involved in another noisy controversy. This was a further installment of the very durable dispute about the theory of glaciers. The question of originality and formal priority as regards the glacier work of David Forbes, Rendu and Louis Agassiz had been rumbling on for years with Tait and Tyndall once more in the key opposing positions.[40] A little earlier in the run up to the Belfast meeting Hooker had intervened directly with Norman Lockyer in an attempt to silence yet another series of bitter exchanges involving Tyndall which was in progress. On October 5th 1873 Hooker wrote requesting a meeting with Huxley so that they could discuss the whole affair.

> "then about this cursed Tyndall affair I want to tell you how it came about. That I am mainly responsible for the article in last 'Nature' quashing the disputation - & this for Tyndall's good."[41]

The Huxleyites felt considerable trepidation at the prospect of Tyndall as their chosen man in the chair of the British Association at Belfast. Huxley and Hooker set great store by the relatively popular public showcase which the meetings could provide for their vision of the scientific enterprise. The aura of impartial technical respectability which they were at pains to generate was seen to be seriously at risk from Tyndall's well known capacity for indiscretion. The Bradford meeting under the Presidency of A.W. Williamson had not assisted the Huxleyites, despite the fact that Williamson as a Comtist might have been taken to be one of their obvious allies. The Bradford meeting should have been headed by Thomson's sometime experimental collaborator J P Joule. Joule's withdrawal meant that a physical scientist ought to

be found to replace him, according to the conventions surrounding the selection of the Association's Presidents. The Huxleyites were very short of eminent physical scientists. The selection of Tyndall in 1874 signified a deliberate break by the Huxleyites away from the alternation between pro- and anti- Darwinian Presidents which had taken place for six years. Although Thomson's nominee Professor Andrews was selected for the Presidency two years later, this form of accommodation came to an end with Tyndall's appearance "out of turn" in 1874. On paper he was certainly the front runner for the scientific naturalists for that year, if they were set on flouting the informal convention described above. Tyndall had easily the most substantial claims according to the combination of scientific eminence and local connection which he embodied. Nevertheless, by mid-October 1873 Hooker was becoming seriously worried and wrote to Huxley

> "Spottiswoode and I have had a good talk with Tyndall and we are in a quandary. My impression gathers strength that the Belfast meeting will be, under any circumstances as bad as the Bradford and that was simply miserable - & that if Tyndall is Pres. it may be worse still & that involves the consideration of his being taken at his word. Spottiswoode and I are agreed that it is impossible either officially or unofficially to advise Tyndall. The ugly fact is that had Tyndall's letter on Tait been published faster it is more than doubtful if the Council would have nominated him for so sticky a place as Belfast at any rate."42

The B.A.A.S. meetings were run as major civic events in the 1870's, particularly when the newly expanded industrial towns were given the opportunity to stage them. At Bradford the Town Council spent £3,000 in "receiving and entertaining the philosophers".[43] The importance of the meetings for the scientific naturalists was unmistakeable. Their campaign for a radical new relationship between science and society set a high value on the prestigous showcase which the meetings could provide. Scientific populism was a major precondition for the success

of the Huxleyite programme. As the time of the Belfast meeting approached Huxley tried to coax Tyndall into a dignified withrawal from the well-publicised glacier controversy. The Irishman was intending to issue his next corrective in a forthcoming book Glaciers of the Alps. In what was close to a wheedling tone Huxley tried to attune Tyndall's mind to the demands of group loyalty. Huxley's letter attests to the maverick nature of the address which was actually delivered at Belfast. It is quite obvious that the prominent public display of such sweeping factional arrogance was no part of the programme of the leading scientific naturalists.

> "I wonder if that address is begun and if you are going to be as wise and prudent as I was at Liverpool. Let my example be a burning and a shining light to you. I declare I have horrid misgivings of your kicking over the traces."44

Prior to the opening of the Belfast meeting there was local political opposition to Tyndall's Presidency. His staunch Unionism was well known and it prompted the Mayor of Belfast to talk of setting up some sort of Home Rule agitation at the meeting. He received little backing however, and his plans came to nothing.[45] When Tyndall actually reached the lecturn to deliver his speech, he placed his notes face downward and extemporised for nearly two hours. According to Oliver Lodge: "the atmosphere [was] getting more sulphurous as people sat it out and the materialistic utterances went on. Huxley piled on the agony by expanding Descartes notion of animal automata to man at an evening lecture."[46] Tyndall's character was not such as to make him averse to becoming a cause célèbre. Although he roundly defied Huxley in the matter of his address Tyndall was able to pull off a thoroughly Huxleyite stunt during the meeting. A strike had been in progress since before the scientists arrival at Belfast. At the last gathering Tyndall was able to annouce that science had intervened to such an effect that

the men would be back at work the following morning. Lodge reported this incident but was not certain how much credit was actually due to members of the British Association. However, "the climax was dramatic."[47]

Eighteen years later Tyndall's obituary in the Times described the style of the Belfast Address as "almost fascinating". All regret at the unflinchingly partisan stance adopted by Tyndall in the cause of materialism appears to have passed away. The writer described the Address as:

> "one of the greatest landmarks in the History of Darwinism . . . at the time of its delivery it caused an immense sensation all over the civilised world . . . it may be taken as the first clear and unmistakeable utterance as to the aims of modern science, and as to the bearings of the doctrine of evolution on the beliefs that have influenced humanity from the beginning."[48]

The obituarist asserted that a speech of that nature could be delivered in 1893 (his time of writing) "without creating any excitement at all."

Scientific Naturalism in the Ascendant

The fifteen years of Huxleyite control of the Royal Society executive did not see the original promise of the Devonshire Commission come to fruition.[49] Beyond that, there was little to indicate that the wider social and political programme of scientific naturalism was getting underway. There is no distinct event which demarcates the final acknowledgement by society at large of the authority of scientific naturalism. The burial of scientists in Westminster Abbey has been taken to provide an indicator of the "social arrival" of the Huxleyites but the evidence on this score is not clear cut. Charles Lyell died in February 1875 and was buried in the Abbey following Hooker's firm representations to Dean Stanley.[50] Seven years later Darwin himself was placed in the nation's most sacred ground. The climactic import which has been attributed to that circumstance does not satisfactorily fit

with its sequel. Nearly a year after Darwin's burial many of the same individuals who had petitioned for Darwin did the same for William Spottiswoode. Spottiswoode was the second Huxleyite President of the Royal Society, a wealthy member of polite society in London, and possessed only moderate scientific credentials. It seems rather odd that the honour done to Darwin and by association to the worldview produced by his theories should be put in jeopardy by seeking merely the posthumous advancement of Spottiswoode.[51] This incident is dealt with in greater detail in a subsequent section.

The outlook of the Cambridge physicists on the question of evolution could well accommodate Sir William Thomson's radically shortened age of the earth. Thomson's stance facilitated a thorough outflanking of natural selection as the unassisted manufacturer of the living world. Divinely guided evolution could obviously be accomplished far more rapidly than the tortuous accumulation of competitive advantages which constituted the Darwinian scheme. In the 1870's men of science who were not constitutionally disposed to embrace unconditional Darwinism had to work out some sort of accommodation with the theory. In 1880 the prominent Sheffield amateur Henry Clifton Sorby looked to Stokes for help in this regard. Sorby was a constant believer and regular church goer throughout his adult life. The following extract from a letter to Stokes reveals the care and thought which many of the men of science put into what they considered to be a inevitable compromise with Darwinism. Sorby was concerned to learn Stokes' position on the issue so that he could formulate a 'correct' stance for the Sheffield contingent to a forthcoming religious conference. The defensive stance of Sorby is clear from his insistence on avoiding anything in the nature of "dogmatic theology".

> "We do not see our way to doing much more than upholding pure and simple theism and the general

> credibility of the bible, leaving a vast margin for diversity of opinion on subjects with which pure science can have no very direct concern.
>
> We must also think that we must fairly abandon some old lines of defence. We must not maintain the verbal inspiration of Scripture and many conclusions which passed current only a few years ago.
>
> I need hardly [say that] we should not uphold the biblical cosmogony - at all events in the popular sense - and we are fully prepared to adopt some form of evolution as having taken place under the direction of a controlling intelligence. In our opinion to oppose evolution as a whole would be very injudicious. Even looking on it in the most unfavourable light we would treat it as a very plausible hypothesis - not proved if you like but . . . not to be opposed as necessarily at variance with the most fundamental principles or such a religion as we could maintain from a scientific standpoint."

The tactical nature of Sorby's stance [and, seemingly, that of Stokes] becomes clear later in the same communication.

> "I do not give my own simple opinion but express what we all agreed would be the most likely base for our present operations. You will of course fully understand that our belief goes far beyond all this."52

This sort of dissembling behaviour is quite understandable if the Christian men of science perceived a significant threat to their interests. As Sorby's letter shows, he was keenly aware of the challenge of Darwinism in the hands of the scientific naturalists.

The mathematical physicists doggedly pursued their jealous defence of their own expertise and the prestige which they claimed as a consequence of their exclusive access to the higher realms of scientific truth. William Thomson's highly vocal lieutenant P.G. Tait was fond of deriding the successful populism of the Huxleyites. Nonetheless, their own lack of popular appeal wrankled with some of the mathematicians. They could not directly popularise the conceptual rigours of their domain. Its elegant inaccessibility was the foundation of its imperious prestige. The detached élitism which this public image bolstered was

quite satisfactory to dons with little interest in wordly matters. The physicists with a strong vested interest in the evolution debate resented Huxley's public stature. They naturally felt that as an agnostic non-mathematician he misrepresented science in the most pernicious way imaginable. Irvine has described how it was said that, "in England when people say 'science' they commonly mean an article by Professor Huxley in the 'Nineteenth Century'."[53] The underlying conflict between the scientific naturalists and the mathematical physicists rarely manifested itself in the surface appearance of their behaviour. After his fairly open criticism of mathematics at the Bradford B.A.A.S. in 1873, Huxley henceforth clothed his attacks on the physicists in high-sounding pretexts and the internal logic of scientific polemics. A careful study of the main participants' actions on the level of detailed particulars reveals them behaving quite consistently with the dictates of the underlying conflict. At the end of 1878 Hirst wrote to Stokes informing him that the people of Heilbronn were planning to make amends to Julius Mayer for their neglect of him by erecting a statue. Spottiswoode, Hooker, and Huxley had assured Hirst that they would show their support by making contributions. Hirst told Stokes that his name was the one really wanted to bring many others "under the standard". Normally a punctilious subscriber in such cases, Stokes refused.[54]

Darwinism and the Special Projects of the Royal Society

The physicists' shortened geochronology challenged the Lyellian geologists and thereby threatened Darwinian evolution. This situation contributed largely towards the impetus within the Royal Society which mustered a number of its expeditions in the second half of this century. These included the later phase of the Nile Delta borings, several deep

dredging expeditions, the coral boring endeavours in the South Pacific, and a large proportion of the work of the "Challenger".

The Nile Delta Borings

A prominent geological Fellow of the Royal Society called Leonard Horner led the early efforts to estimate the age of the deposits forming the Nile delta. His efforts took place between 1845 and 1860 and consisted of several series of borings across a chosen sedimented area. Time was calibrated with the rate of accumulation of material by reliance on the approximate date of construction of the statue of Rameses II at Memphis. Horner found debris of human activity 48 feet below the pedestal of the statue which was reputed to have been erected 3,200 years earlier. Horner described how his estimated rate of accumulation "could not be regarded as very precise."[55] Little more was done until Huxley drew up a memorandum to the Council of the Royal Society in 1882.[56]

The new attempt was to be far more ambitious than Horner's work thirty years earlier. The borings were planned to go down to 300 feet and were correspondingly expensive. The work went on for five years from 1883. When the results came to be worked out at South Kensington Huxley and his immediate supporters were dismayed at the lack of concern with them, shown by the younger men. Huxley clearly regarded the results as being very significant when he wrote of them to Michael Foster in September 1886.

> "I brought the Egyptian report down with me. It is very important and in itself justifies the expenditure. Any day next (that is to say this) week that you like I can see Col. Turner. If you and Evans can arrange a day I don't think we need mind the rest of the Committee. We must get at least two other borings ten or fifteen miles off, if possible on the same parallel, by hook or by crook. It will tell us more about the Nile valley than has ever been known."[57]

Despite the high priority which the leading Huxleyites gave to the work, the detailed analysis of the results was consistently neglected. Foster strove to galvanize those immediately involved into activity, but to little effect amongst the reluctant geologists. In April 1888 Foster wrote to Huxley bemoaning the lack of momentum about the Nile delta project.

> "[there isn't] quite the grit among the Gs that I could wish. Judd is not all things - I can't for instance get the Delta business pushed on very much."[58]

Nearly two years later the situation was unchanged.

> "I can't get Judd or any of the geologists to go on with the Nile business."[59]

This sequence of events provides further evidence of the generational division of interests which developed amongst the geological and biological Fellows during the last two decades of the century. The younger men were not so much in awe of the physicists as Huxley and his long serving supporters had always been. The younger men were more concerned to get on with their own scientific work (which often entailed modifying Darwin's work in a way which Huxley reviled) than to defend the basic premise of The Origin against a threat which had little meaning for them.

Darwin and the Vogue for Scientific Dredging

For a number of years during the 1860's and 1870's a strong belief grew up among biological scientists that very important advances could be made by studying the flora and fauna of the ocean floor. Charles Wyville Thomson determined to look for samples in depths over 400 fathoms in the Lofoten Islands.[60] It had previously been supposed that no life existed below 300 fathoms. Deep dredging was an expensive pursuit which at the outset could only be considered by wealthy individuals usually working from their yachts. In the 1860's the solicitor and

mollusc expert John Gwyn Jeffreys took the lead in this field. There were important contributions from H.N. Moseley and William B. Carpenter, Registrar of London University from 1856.

The rapid intensification of scientific interest in dredging as a means of investigating the development and distribution of life coincided with a quickening of official interest in the ocean floor in connection with the recent successes of submarine cable laying. The ready exploitation of this interest by the scientists was crucial in the funding of the "Challenger" expedition a few years later. August 1868 saw the departure from Oban of Thomson and Carpenter in H.M.S. "Lightning" which they described as "surely the oldest and crankiest paddle steamer in the Royal Navy".[61]

"The Great Lesson": Darwin, Murray, and Coral Islands

Under the title "The Great Lesson" in the September 1887 issue of the Nineteenth Century the Duke of Argyll sought to expose a Huxleyite cover-up of the flaws in the intellect of Charles Darwin. Argyll's method was to draw public attention to what he considered to be the suppression of John Murray's theory of the origin and growth of coral reefs. Murray's theory contradicted Darwin's interpretation of the same phenomena which was first published in the 1830's. Darwin's experience on the "Beagle" produced the flash of insight which led him to view every coral island as "a monument erected by corals to the memory of a buried island."[62] Darwin's theory explained a much greater range of phenomena than Lyell's rival volcanic explanation. It also provided a possible key to the past relative movement of land and sea. A year after the return of the "Challenger" with it's wealth of data John Murray began to question Darwin's theory. Murray had been a key member of the expedition, which had been at sea when a third edition of Darwin's Coral Reefs had been published. Opponents of Darwin's theory

of evolution later took up Murray's theory as evidence of Darwin's fallibility. They aimed to cast grave doubts on Darwin's capacity to elaborate scientifically the inner workings of processes which were not directly demonstrable. Obviously Darwin's credentials for solving the larger problem of evolution would be diminished if could be shown to be in error in dealing with the development of coral islands. Argyll, the politician, reasoned from the premises of legal advocacy in his approach to the question of discrediting Darwin.

Two years after the reading of his first paper on coral reefs at the Geological Society in 1837, Darwin's first major scientific paper was published in Philosophical Transactions of the Royal Society. The paper concerned the formation of the Parallel Roads of Glenroy which Darwin portrayed as opposing shorelines formed by marine erosion when the land mass was at a lower level. In 1861 Darwin reluctantly had to withdraw his explanation in favour of the glacier lake theory of Louis Agassiz.[63] Two years after the publication of the Origin of Species it was awkward for Darwin to give best to the naturalists' leading proponent of the fixity of species. A decade later, the physicists' claims to the determination of the age of the earth prompted a further Darwinian retreat. In succeeding editions of the Origin Darwin felt constrained to modify and finally delete his rough calculation of 300 million years for the denudation of the Weald.[64]

Argyll's attack on Darwin's scientific ability and the personal integrity of his supporters formed part of a pattern of endeavour aimed at outflanking the successful concept of natural selection. In his article in the Nineteenth Century Argyll depicted Darwinian orthodoxy as a pervasive conspiratorial force. He described how in 1880 the reading of one of Murray's papers at the Royal Society of Edinburgh had challenged this orthodoxy. The Duke maintained that no attempt at a

full reply had been made because Murray's argument was so compelling and so well evidenced.

> "the reluctance to admit to such an error in the great idol of the scientific world, the necessity of suddenly disbelieving all that had been believed and repeated in every form for upwards of forty years . . . has led to a slow and sulky acquiesence."

To Argyll this state of affairs pointed to the "Great Lesson":

> "It is that Darwin's theory is a dream, it is not only unsound but in many respects the reverse of the truth. With all his conscientiousness, with all his caution, with all his powers of observation, Darwin in these matters fell into errors as profound as the abysses of the Pacific."[65]

The well known geologist and Royal Society activist Reverend T.G. Bonney took Argyll to task in the pages of Nature. In response to the charge that Murray's theory had been received by a 'conspiracy of silence' Bonney described the public stance adopted by several leading figures. According to his account in Nature, Professor Dana was against Murray's new theory whilst Huxley had suspended his judgment. Bonney himself did not find Murray's case compelling despite the fact that his views enjoyed full currency at the "Challenger" office. As head of that temporary institution Murray wielded great influence with a corresponding degree of access to the normal channels of scientific communication. Bonney quoted Professor Judd's then well known distillation of Argyll's case for the suppression of Murray. "If this be 'a conspiracy of silence' where alas! can the geological speculator look for fame?"[66] Hooker greatly approved of Bonney's account of the Argyll attack. A few days after the Nature article he wrote to Asa Gray complaining that the Duke could not comprehend that Darwin's theory had sprung into being as an isolated act of genius.

> "whereas Murray's is a conclusion arrived at through the labour of most eminent fellows on the ocean - and a knowledge of all the facts and data they were collecting round him during the Challenger voyage.

> As you say 'the greater truth, the greater libel' - so we may say of Darwin's theory 'the greater error, the greater genius.'"[67]

Hooker told Gray that he believed both theories may operate in the formation of coral islands. Argyll's anti-Darwinian agitation prompted a good deal of research into the coral question in Europe and America during the succeeding decade. Alexander Agassiz, son of Darwin's vanquisher over the parallel roads of Glenroy, concluded from extensive ocean cruises that the subsidence central to Darwin's thesis in Coral Reefs was unnecessary to explain the phenomena. Murray's theory required no large scale changes. Argyll returned to the offensive by accusing Bonney of surpressing a Geological Society paper by a Mr. Guppy, one of Murray's followers. Bonney had been President of the Society at the time the incident was alleged to have taken place.[68] As has been described earlier, Murray became profoundly unpopular with the Huxleyites. Foster indulged in bitter, if abstruse, disparagement of his claims to a knighthood for his "Challenger" work.

The Funafuti Expeditions

Darwin had wished for a "doubly rich millionaire" to pay for the fieldwork that he thought would be necessary to settle the matter. He remained committed to his own theory right until his death in 1882. At that time no serious attempt had been made to accomplish the seemingly crucial experiment of drilling right through a coral island. According to Darwin's theory the coral should continue down to a great depth. Murray's theory supposed coral to grow in a thin layer on marine deposits. In 1896 Mrs. Eadith Walker a wealthy Sydney woman offered financial assistance which was to be augmented by the Government of New South Wales. The Royal Society became involved and, according to Bonney, eventually met most of the cost of the work. The leading lights at the Australian end of the long deferred project were Professors

Anderson Stuart and Edgeworth David. in May 1896 HMS. "Penguin" deposited the expedition on the atoll of Funafuti which formed part of the Ellice group in the South Pacific.[69] After initial failure, the endeavour succeeded only when a further expedition was sent out. The Funafuti specimens were examined at South Kensington under the supervision of Professor Judd, the reluctant analyst of the Nile Delta cores a decade earlier. Despite the great depth at which coral formations had been detected, both sides of the controversy interpreted the new information as the vindication of their views. Afterwards some unpleasantness developed between the Royal Society and the Australian academics over the latters' publication of a seperate report in advance of the Society's version.[70]

Later Huxleyite Campaigns

The Burial of William Spottiswoode

Charles Darwin's body was buried in Westminster Abbey in 1882. James Moore has described this as the means by which "the new leaders of English culture" were able to stage manage an unmistakeable demonstration of their power. He continues:

> "By appropriating it [Darwin's corpse which he and his family had wished to be buried at Downe] the new leaders of English culture were able to redeem its political value. Like the mind gone out within, the body now served them well, in a last symbolic rite testifying them to their authority, the extreme unction of a rising secularity."71

Just over a year later another prominent man of science was lowered into the same hallowed ground in circumstances which throw some new light on the meaning of Darwin's interment. The Huxleyites were without doubt still near the height of their powers in 1883 when the incumbent President of the Royal Society William Spottiswoode died. However, the manner in which the arrangements for his funeral were made suggests a surprising lack of cohesion and lack of common purpose amongst the

leading members of the "rising secularity". The incidents which took place in connection with the burial of Spottiswoode (in common with other incidents related in this chapter which are not directly connected with the Royal Society) reflect significantly on the crucial position which the scientific naturalists occupied within the R.S. at the time. The manoeuvrings which preceded the interment of Spottiswoode in Westminster Abbey show how scientific naturalism in Huxleyite hands was faltering at the very time that it has been portrayed as being in the ascendant. The somewhat magisterial disposition of the Huxleyites within the 'government' of the Royal Society cannot be evaluated accurately without reference to important events which took place away from Burlington House. The involvement of several individuals from backgrounds divorced from scientific naturalism in the petition for a place in the Abbey for Spottiswoode, presents a more complicated picture than that of a simple repetition of the previous year's triumph. Spottiswoode's upper class social background and unexceptional scientific accomplishments add further to the scope for misinterpretation. These factors, in the context of the controversy over Spottiswoode's burial, raise some doubts over the straightforward conception of Darwin's body as "the outward and visible sign of a profound but unfinished [social] transformation."[72]

Spottiswoode died of typhoid fever on the 27th of June 1883. Shortly afterwards Hirst went with Sir Frederick Pollock, George Busk and Moulton to see the Dean of Westminster. They wished formally to inquire as to his willingness to receive a memorial urging the burial of Spottiswoode in the Abbey. Dean Bradley was not to be found until the patient group of would-be memorialists ran into him at the Athenaeum. Hirst was asked about Spottiswoode's individual scientific standing.[73] Since the smooth reception of Hooker's initiatives which secured Lyell's

place in the Abbey in 1875, things had changed. At that time Arthur Stanley had been Dean of Westminster and had made a point of keeping up with the progressive spirit of the age. He was a member of the Metaphysical Society who had gone out of his way to establish a fresh climate of theological liberalism at Westminster. Bradley succeeded him at his death in 1881. The new incumbent was much more traditionally minded and so unlikely to be sympathetic towards petitions in favour of Huxley's friends. In 1882 Huxley had not signed Lubbock's petition to the Dean concerning Darwin. Almost as soon as the memorial putting Spottiswoode's case was circulated, it became clear that there was going to be trouble. Bradley had sagely acknowledged the claim made out by Darwin's surpassing scientific eminence in the previous year and telegraphed his "cordial acquiescence."[74] In Spottiswoode's case he immediately began to ask questions. For the more judicious of the memorialists this close scrutiny of the dead man's credentials was refusal enough. There then followed a period of unseemly wrangling against a backdrop of the Spottiswoode family's bewilderment. On the 28th of June Huxley wrote to Stokes.

> "I have just had a note from G. Spottiswoode withdrawing the letter he wrote me yesterday on the strength of which I have been telling everybody the Abbey was given up. It appears that the outside (non-scientific) world strongly supports the memorial."[75]

Huxley was most unhappy with the situation which he wanted to leave in the hands of the family. Stokes letter on the subject crossed Huxley's in the post. The Senior Secretary of the Royal Society remarked that he had seen Hirst at Burlington House, and supposed Huxley had heard that:

> "the idea of Westminster Abbey has been given up as the family did not wish it and told me he had just seen Moulton in the [Athenaeum] club who said it was going on very satisfactorily."[76]

Stokes surmised that the family had changed their minds and that Moulton was simply speaking of his collecion of signatures for the memorial. Clearly no-one was in charge of these proceedings which were adding nothing to the standing of science in either the official or popular mind. Hooker voiced his acute displeasure to Huxley in a letter written the day after the latter's exchange with Stokes.

> "I cannot acquiesce in all this touting for the Abbey graves in the name of science, it is to me a melancholy spectacle.
>
> In fact I am game for a counter movement, painful as it would be, if I were sure of adequate support. I have had a letter from the Dean telling me that the family had not withdrawn (as you also tell me) adding that it is proposed by all manner of people and that Pollock and Moulton had again been to see him - he told them that 'as a man of science, pure and simple he could not . . . that if considered it must be a union of that with official position, high character and great and beneficial empoyer of labour - as something cumulative.'
>
> Is not that enough to shake pooor Spottiswoode's bones in his coffin?
>
> Oh dear oh dear - that his case should have to be bolstered up so. . . . Dyer had signed regretfully before I saw him. . . . I am indignant at such men as Moulton and Pollock taking the reins in such a matter."77

Huxley's reply took the form of a quiet suggestion to sensibly limit the damage being done to the public image of science and the feelings of Spottiswoode's family. It is apparent that Huxley was not anxious for Spottiswoode to receive the dignity of a place in the Abbey. Huxley's alarm at the disarray of scientific lobby as a whole and his supporters in particular is very apparent in the following letter to Hooker.

> "My dear Hooker,
>
> Pray don't think of a counter-movement I am not usually accused of unwillingness to fight (in spite of being a man of peace) when any great interest is at stake but the circumstances of the present case can never recur and it would go to my heart to have any quarrelling amongst Spottiswoode's most intimate friends over his grave. It has long been too obvious to me that the relations of some of us at the X are getting very strained . . . . We shall smash the X completely if

> we get into public and open antagonism over this business - without doing any good that I can see - "[78]

Spottiswoode had occupied the chair of the Royal Society for five years. It is not difficult to see how the Huxleyites came to take it upon themselves to order the manner of his funeral. Their chagrin at finding themselves in disarray found its expression in sympathy for the Spottiswode family. The unity and power of scientific naturalism which has been portrayed as overwhelming in the accomplishment of Darwin's funeral and burial in Westminster Abbey in 1882, was nowhere in evidence a year later.[79]

The minor spate of scientific burials in Westminster Abbey ended with that of Spottiswoode. In later years small medallion portraits of Hooker and Lister found their way onto its hallowed walls. For Kelvin a plaque and a window were provided.[80] Six years after the death of Spottiswoode, Hirst was walking in the Abbey listening to the "beautiful music" when he saw the grave and was prompted to record in his journal:

> "It has been said that his eminence scarcely merited so marked a recognition. Perhaps not. But I do not regret having helped to the interment of him there. He was a noble and exceptionally high-minded man, at all events."[81]

* * *

Notes

1. Royal Society, Register of Papers.

2. Edmund Gosse, Father and Son, Oxford University Press, London, 1974, p. 60.

3. Huxley Papers (ICL), 3.28. Huxley's review of the Origin appeared in The Times a year later.

4. M. J. Bartholomew, "The Award of the Copley Medal to Charles Darwin", Notes and Records of the Royal Society, 1975, 30, no. 1, pp. 209 - 217. Stokes Papers (CUL), H 1381 - 1386.

5. Huxley Papers (ICL), 4.152.

6. Huxley Papers (ICL), 13.48.

7. Frank Turner, "Rainfall, Plagues, and the Prince of Wales", Journal of British Studies, 1973, 13, pp. 46-68. Isis, 1978, 69 pp.

8. Not surprisingly the power of divine intervention was aired in relation to the sensational news stories of the day. Hirst's journal quotes a part of Tyndall's reply to his opponents in the Pall Mall Gazette in October 1865. The "Great Sheffield Flood" was the worst man made disaster of the last century in which 250 lives were lost. "in reply to my question whether the suppliant voice of a whole nation would have altered the laws of hydraulic pressure in the case of the Bradfield reservoir, you reply "undoubtedly not" Why not? I would earnestly ask" (Hirst Journal, 1749). Geoffrey Amey, The Collapse of the Dale Dyke Dam 1864, London, 1974.

9. M.N.W. and S.J.C. (eds), Sketches from the Life of Sir Edward Frankland, London, 1902, p. 286.

10. Royal Society, MMXIX, no. 47. Sabine added that the B.A.A.S. would "do well to choose [Miller] for the Presidency on some early occasion". Frankland's lack of enthusiasm was not usually shared by the R.S. executive and the Huxleyites.

11. Leonard Huxley, LL JDH, vol. 2, p. 121

12. J.D. Burchfield, "Darwin and the Dilemma of Geological Time", Isis, 1975, 64, pp. 301-321

13. B.A.A.S. Report 1869, p. XXXVII.

14. Ibid, p. CIV.

15. Hirst Journal, 1852. The "x's and yv's" socialised in public on a regular basis without apparently giving away the X club's existence or significance. As has been mentioned earlier. Huxley made only one explicit reference to the X club in his letters to Michael Foster (Leonard Huxley, LL THH, vol. 2, p.55)

16. In view of the contention that the scientific naturalists can be distinguished philosophically from the mathematical physicists on the basis of the former's reductionism, it is ironical that the

most prestigious and powerful scientific techniques (maths and physics) were the preserve of men who set the greatest limitations on science as an explanatory agent.

17. Ruth Barton, The X Club, p. 73.

18. Oliver Lodge, Advancing Science, Earnest Benn Limited, 1931, p. 14

19. Stokes Papers (CUL), H 174

20. Hirst Journal, 1842. W. A. Miller and W. H. Miller were both committed Christians.

21. Huxley Papers (ICL), 3.126. Sir Stafford Northcote was a British Conservative statesman who became President of the Board of Trade in 1866 and Chancellor in 1874. He was Foreign Secretary in Salisbury's second Ministry.

22. Stokes Papers (CUL), H 203. Stokes seems to have been convinced that he had comprehensively typified and dismissed materialism in this way. For him the possibility of a contemporary means of creating life de novo seemed to deny the need for a creative force and a deliberate creative act.

23. Hirst Journal, 1884.

24. Silvanus P. Thompson, The Life of William Thomson Baron Kelvin of Largs, Macmillan, London, 1910, p. 587. The extent to which Kelvin's social ineptitude was affected is difficult to estimate. At times the patience of even his admirers was severely tested (Leo Königsberger, Herman von Helmholtz, Clarendon Press, Oxford, 1906, pp. 286-288).

25. Tait was a dualistic idealist, Tyndall a materialist (Science and Metaphysics in Victorian Britain, Open University, A381 Unit 3).

26. There is no other evidence of conviviality linking Huxley and Tyndall with William Thomson. Tyndall was a bitter enemy, leading one to think that the invitation was far from being simply a kind act.

27. C. Chant and J. Fauvel (eds), Darwin to Einstein: Historical Studies on Science and Belief, Longman/The Open University Press, 1980. The promoters of abiogenesis could fit the doctrine with a thoroughgoing Darwinism or with a view that denied significant parts of it. At the British Association Stokes opposed abiogenesis at Exeter and Huxley opposed it later at Liverpool.

28. Leonard Huxley, LL JDH, vol. 2, p. 127. The strands of polemic had become somewhat intertwined by this stage. Huxley was not too displeased with Thomson's 1871 B.A.A.S Presidential Address because it contained implicit acceptance of evolution.

29. Ibid. Huxley remarked of Thomson's 1871 B.A.A.S Presidential Address at Glasgow, in a letter to Hooker of 23rd August 1871, "I like what I have seen of Thomson much. He is, mentally like the scene which lies before my windows, grand and massive but much encumbered with mist - which adds to his picturesqueness but not to

his intelligibility. Tait worships him with the fidelity of a large dog - which noble beast he resembles in other ways (Ibid., vol. 2, p. 165). Thomson's speech, which Huxley described as "bright points in the midst of much nebulosity," was considerably jeered at in the press at the time (Lodge, ~~Ibid.~~, p.25). David B. Wilson, Kelvin and Stokes: A comparative study in Victorian Physics, Hilger, Bristol, 1987.

30. Kelvin Papers (CUL), T 627

31. Stokes Papers (CUL), T 64 Tait was to be disappointed. Forthright policy was not Stokes' way. A number of mathematical physicists (Stokes' natural constituency) had occasion to be surprised to derive so little advantage from Stokes' position as long term Secretary of the Royal Society.

32. Science and Metaphysics in Victorian Britain, Open University, A381 Unit 3, p. 54

33. Burchfield, ~~ibid.~~, p. 315

34. J. D. Burchfield, Lord Kelvin and the Age of the Earth, Macmillan, 1975, pp. 76-96.

35. Reference has already been made to Huxley's expectation that if elevated to the Presidency "Johnny would upset the coach in his first drive" (Huxley Papers ICL, 2.250).

36. Stokes Papers (CUL, K 135

36A. Quoted in David B. Wilson, "A Physicist's Alternative to Materialism: The Religious Thought of George Gabriel Stokes," Victorian Studies, 1984, Autumn, pp. 69-96, ff. p. 94

37. Stokes Papers (CUL), K 191

38. A. S. Eve and C. H. Creasey, Life of John Tyndall, Macmillan, London, 1945, p. 161.

39. Leonard Huxley, LL JDH, vol. 2, p. 126.

40. J. S. Rowlinson, "Tyndall's work on Glaciology and Geology", in John Tyndall, Essays on a Natural Philosopher.

41. Huxley Papers (ICL), 3.216

42. Huxley Papers (ICL), 3.212

43. Harvey Becher, Scientific London, Frank Cass and Co. Limited (First Ed. 1874) New impression 1968. p. 276.

44. Leonard Huxley, LL THH, vol. 1, p. 409

45. Ibid., p. 401. Huxley Papers (ICL), 8. 158.

46. Lodge, ~~ibid.~~, p. 35.

47. Ibid.

48. The Times, Dec. 5th 1895, p. 11.

49. D. S. L. Cardwell, The Organisation of Science in England, Heinemann, London, 1972. Frank Turner, "Public Science in Britain", Isis, 1980, 71, pp. 591-592.

50. William Irvine, "Thomson Henry Huxley", in British Writers and Their Work, no. 2, University of Nebraska Press, Lincoln Nebraska, 1963, p. 96.

51. Stokes Papers (CUL), S 1190 and S 1193.

52. Stokes Papers (CUL), S 1192.

53. Irvine, ~~ibid~~.

54. Stokes Papers (CUL), H 845.

55. T. G. Bonney, Annals of the Philosophical Club of the Royal Society London, 1919, pp. 136-137.

56. Royal Society, PLB 427, no.53. Horner had found pottery. He deduced the age of the Tertiary as thirteen and a half thousand years.

57. Leonard Huxley, LL THH, vol.2, p. 143

58. Huxley Papers (ICL), 4.325.

59. Huxley Papers (ICL), 4.351. John Wesley Judd was educated at the Royal School of Mines where he became Professor in 1876.

60. Professor Sars of Christiania had encountered living matter at 450 fathoms at the same location. At about the same time living creatures were found on a cable which had been to 1200 fathoms.

61. Margaret Deacon, Scientists and the Sea 1650-1900, Academic Press, London, 1971, p. 306. Royal Society, MC 8, no. 222. Carpenter had constructed an all encompassing theory of a global deep sea water circulation. Charles Wyville Thomson did not follow Carpenter in this "magnificent generalisation'. In March 1872 Britain, Norway, the United States and Sweden were all involved at a governmental level. Somewhat later Huxley's bathybius faux pas gave further significance to the deep sea as a possible key to the problems of evolutionary theory. The R.S. felt some of the reverberations of the bathybius affair. In early 1878 Mr. A. Wallich objected to Huxley as Secretary of the Royal Society on the grounds of his evident incompetence in connection with bathybius. Wallich was in dispute with Carpenter and Wyville Thomson who had kept his papers out of the Proceedings. Wallich, perhaps rightly thought that he was muzzled to protect the reputation of a far more eminent man. He wrote to those in power at Burlington House (who included Huxley of course) "pioneers in any special department of research can thus flagrantly be ignored, simply because embodied in less powerful and less widely distributed journals than the Proceedings". (Royal Society, MC 11, no. 146). Wallich showed that Huxley had not been ignorant of his early papers, and had actually signed the receipt for one of them. Nearly two years later Huxley recanted his belief in bathybius at the Sheffield meeting of the B.A.A.S.

62. T. G. Bonney in the "Scientific Postscript" to Mrs. Edgeworth David, Three Months on a Coral Island:an unscientific account of a scientific expedition, John Murray, London, 1899, p. 305.

63. M. Rudwick, "Darwin and Glenroy: A "Great Failure" in Scientific Method?" Studies in the History and Philosophy of Science, 1974, 5, pp. 97-185.

64. Burchfield, "Darwin and the Dilemma of Geological Time", Isis, 1975, 64, pp. 301-321, ff. p. 302.

65. Nature, 10th November 1887, p 25. The eighth Duke of Argyll (George Douglas Campbell) 1823-1900 was a prominent Whig and well publicised opponent of Darwin

66. Ibid. Geikie was an early convert to the foreshortened age of the earth prescribed by the mathematical physicists, he was also an early expounder of Murray's coral reef theory.

67. Leonard Huxley, LL JDH, vol. 2, p. 342. Charles Darwin spent just five days on a coral island. Murray's theory of atoll formation did not depend on the relative motion of land and sea levels. His theory held that coral grew on marine deposits.

68. Nature, ~~ibid.~~ Darwin's theory on this subject is still accepted today.

69. T. G. Bonney in Edgeworth David, ~~ibid.~~, p. 306. The authoress of this work did not have a scientific outlook as her title for the book suggests. She wrote: "Whether Darwin's theory , or Murray's or Wharton's is the true one, is a secret contained in that mysterious core, now in the hands of the scientific experts in London, the restless men who "want to know'". (Ibid., p. 298).

70. Royal Society, NLB 14, no. 110 and 148. Professor Stewart travelled to England to sort this out.

71. J. R. Moore, "Charles Darwin lies in Westminster Abbey", Biological Journal of the Linnean Society, 1982, 17, pp. 97-113, ff. p. 111.

72. Ibid.

73. Hirst Journal, 2130. Dean Stanley was brother to Owen Stanley who captained H.M.S. Rattlesnake. Dean Stanley it was that performed the marriage ceremony for Tyndall.

74. Francis Darwin (ed), Life and Letters of Charles Darwin, John Murray, London, 1887, vol 2, p.531.

75. Stokes Papers (CUL), H 1417. The confusion and diversity of purpose among the broad constituency and ruling group of scientific naturalism signify how far short its objectives the movement was falling, even at a time when it has been portrayed as in the ascendant.

76. Huxley Papers (ICL), 27.95.

77. Huxley Papers (ICL), 3.268.

78. Huxley Papers (ICL), 2.250.

79. The portrayals referred to are those of Barton, Jensen, and Moore. Leading Huxleyites had been involved in other claims for the honour of a "national" funeral. When George Eliot died in 1880 Tyndall pressed her claims for burial in the Abbey. Huxley opposed the movement. There had been rumours of an amorous entanglement between Mrs. Cross, as Eliot had latterly become, and Herbert Spencer. Faced by the "clatter of tongues" the movement was abandoned and she was buried in Highgate Cemetery (David Duncan, Lives and Letters of Herbert Spencer, D. Appleton and Co. New York, 1908, 2 vols., pp. 214-215.

80. A. Geikie, A Long Life's Work, Macmillan, London, 1924, p. 336.

81. Hirst Journal, 2622. Hirst had acted in this, as in numerous other connections, primarily with his mathematical dining group from the Athenaeum. When Huxley died in the early summer of 1895, a commemorative tablet was suggested for Westminster Abbey. Other counsel held that Dean Bradley might not take to the idea. The scheme was dropped.

CHAPTER 11

THE REARGUARD STRUGGLE FOR "PURE" DARWINIAN THEORY IN THE TWILIGHT OF HUXLEYITE POWER

Twenty years after the publication of the Origin of the Species the inclination of a new generation of biologists to make their mark by revising natural selection was strong. At the same time, the physicist's attempt to stultify Darwinian evolution (for want of time for its accomplishment) was in its full vigour in the 1880's.[1] In October 1883 one of Huxley's immediate concerns was the modification of Darwinism in such a manner that it could be incorporated into Christian belief. He wrote to Foster bemoaning this trend.

> "Do you see how Evolution is getting made into a bolus and oiled on the outside for the ecclesiastical swallow?"[2]

In the same year Huxley gave the Rede lecture at Cambridge. He took as his subject the Nautilus. Of that creature's 100 species all but two were extinct, providing a stark illustration of the intrinsic plausibility of the transmutation of species. Huxley's choice also contained an obscure irony. Fifty years earlier Richard Owen, his scientific arch-opponent and champion of the fixity of species, had first come to prominence as a result of his work on the Pearly Nautilus under Cuvier's supervision at Paris.[3]

Spottiswoode's death occured in 1883 and, as has been described in an earlier chapter, Huxley felt compelled to adopt the personally inconvenient expedient of occupying the chair himself. Huxley and Hooker clearly felt that the Office was threatened by "noblemen and traders" such as the Duke of Devonshire and Sir Frederick Bramwell. If anything, Huxleyite misgivings about such men seem to have outweighed their objections to the occupation of the chair by one of the Christian physicists. Of this latter group, Sir William Thomson and George Stokes

were the leading contenders. From the time of Spottiswoode's death the Huxleyites were in retreat. They were approaching the end of their careers and in a somewhat alarmed manner devoted considerable effort to shoring up a position which had until a short time earlier seemed one of invulnerable influence.

By the mid-1880's the wider socio-political programme of scientific naturalism had not progressed at all. The Treasury still regarded science as expensive gimmickry, encouraged by a government which took no pains to disguise its indifference.[3] A number of Huxley's skirmishes with rival authorities on evolution and man's place in nature and society have been documented elsewhere. In his introduction to the catalogue of the Huxley Papers Warren Dawson mistakes the basic character of Huxley's intentions. Dawson deprecated his involvement in causes and controversies outside the specialised domain of biological science.[4] Huxley's exchanges with the Duke of Argyll over the latter's book The Reign of Law were far from irrelevant to the proper concerns of a man who aimed at the scientific leadership of a transformed society.[5] Argyll's attempt to break Huxley's "scientific reign of terror" reflected a hostile response to scientific naturalism at its zenith. As it transpired, that specific effort was unnecessary. The rise of secularity would not be reversed but the leading position of the Huxleyites was already crumbling. In the same year of 1887, the President of the Royal Society entered the House of Commons and Huxley was unable to do anything effective about it. He continued to make occasional forays into public life after 1890 but frequently his most prominent followers were left to try their own mettle. This was the case when Donnelly and Lankester pursuaded Dyer to counter an attack from Argyll which appeared in Nature in the first week of 1890.[6]

The novelty of Darwinism was inevitably dulled by its incorporation into the established order of biological science. The problem of the

source of variations still loomed large. Darwin himself still indulged in the widespread activity of compromising natural selection by the introduction of ancillary processes into its working. As the century neared its close there was an increasing tendency for Darwin's revisers to be accorded great renown. In 1894 Dyer wrote to Huxley bemoaning the extent of disloyalty to "pure" Darwinism within biological science itself. Dyer maintained that there were only six men in Britain worth listening to on the subject of evolution. He was depressed by the way Darwinism was "being messed about" and how Darwin's works were no longer read or studied. The prospect of a new society being set up to promote Darwin's ideas made scant appeal to Dyer.

> "This Darwinian Society would only be a platform for drivel like the Victoria Institute. . . .And yet people like that old overrated Saint Flower [William Henry Flower] go about saying that it is inadequate. They don't understand it [the original version of Darwinian evolution]. Not even Wallace does thoroughly. . .I am tired of the gibes of Kelvin and Oliver Lodge on the time limit to evolution. Why should we assume that evolution has always gone at its present pace?"[7]

The letters of Huxley and Dyer at this time evoke a sharp sense of their anxiety about the careless rejection of the original unalloyed version of Darwinism. They fiercely resented this trend as the outcome of voguish mediocrity among the new generation of biological scientists. One of the chief targets of Huxleyite wrath was George Romanes. Probably the most thorough of Darwin's "enhancers", Romanes was a wealthy Canadian who had worked under Foster as a private student at Cambridge. According to Moore, Romanes' main purpose was "to vindicate Darwin's judgement that the minds of animals and mankind, like their bodies, had a common origin".[8] Ray Lankester was not impressed by the allusions which the Canadian made to a "Darwin-Romanes theory". Nor did he like the close identification with the original <u>Origin</u> which Romanes implied with statements of the

"Darwin and I" sort. At one point Norman Lockyer showed Romanes a letter about him which Lankester had sent in to the Nature office. Lockyer would not publish the letter but Lankester was pleased that he had shown it to Romanes. Lankester told Lockyer that: "it is time he knew that I consider him a windbag".[9] During 1888, the year in which Romanes' Mental Evolution in Man was published, Huxley expressed his lack of patience with his recent public statements in a letter to Hooker.

> "Romanes has spilt himself over four columns of "Nature" and does not seem to understand Darwin even yet." 10

The leading articles in Nature during the 1880's were more often written by Romanes and Archibald Geikie than by Lockyer himself.[11] This situation provides a clear instance of the serious divisions which existed between the leading scientific naturalists of the time. Geikie's religious commitment and the ardent endorsement of William Thomson's estimate of the age of the earth have already been detailed. The distrust with which Hooker and Huxley regarded Lockyer has also been touched upon. In this complex situation however, all of those mentioned above could agree on some issues such as Alfred Wallace's "deplorable weakness as a philosopher" as it was described by Romanes. The veteran Huxleyites were supplied plentifully with valedictory honours by the new men in their field. These awards lend a deceptive air of completeness to the enterprise upon which they had been engaged for 35 years. In fact such an appearance could scarcely be further from the truth. Not only was the grand object of social progress through scientific government as far away as ever, but also the Darwinism which had been the springboard of the Huxleyites' prominence was being "messed about" with impunity. Hooker received the Darwin medal in 1892 for his "association with Mr. Darwin in the studies preliminary to the 'Origin of Species'" and additionally for his own worksFlora Indica and Genera Plantarum.[12] Two years later Michael

Foster was responsible for the award of the Darwin medal to Huxley. The grounds given were his work in comparative anatomy and long term efforts in support of Darwin. In reply to Huxley's amiable reproof for the award of medals to "useless old extinct volcanoes", Foster told him "it was inevitable".[13] The generally observable move away from Darwinism in its original form in the last twenty years of this century was not caused simply by the enfeeblement of the Huxleyites and the ambitious drive of younger scientific men to recast old problems. The wide latitude for individual interpretation within evolutionary theory was largely due to the lack of knowledge of the source of variations. Out of this lack developed the controversy which has become known as the Biometrical-Mendelian debate. Prior to the rediscovery of Mendel's work and the maturity of the biometrical approach of Weldon and Pearson many leading figures experienced crises in their loyalty to the originator of natural selection. In 1894 Dyer assured Huxley of his commitment to natural selection, asserting that while the mechanism itself did not need to be talked about "variation, its laws and causes, is another matter".[14]

Hooker's personal lack of effectiveness by this stage is all too clearly revealed in a letter to Huxley in November 1890. Hooker wrote: "Darwinism is all a dream to me now. Please enlighten me."[15] The leading figures in biological circles in the 1890's were willing to endorse the backward-looking award of medals to their superannuated heroes. They were not prepared to defer to the contemporary scientific authority of these men. For his part Huxley was quite prepared to strike a jarring note on ceremonial occasions intending to celebrate his past glories. Hooker urged him to secure wider publicity for the defence of Darwin's original theory which formed Huxley's speech at the banquet commemorating the first 25 years of Nature.

> "Dyer tells me that your address at the 'Nature' banquet was exceedingly good in substance and manner,

> and ought to be printed for its worth as a warning voice. Do think of it." 16

There was no clear division between the youthful, progressive element within botany and a more senior, reactionary counterpoint at this time. Although Dyer heartily disapproved of the modifiers of Darwinian precepts, he was all in favour of the new protoplasmic theory of plant life. The contemporary leaders of opinion within British botany were not so impressed. In 1887 Dyer reported to Huxley the hostility of the Royal Society's referees to a "very important communication" from Walter Gardiner, one of his prôtegées. Gardiner was at the forefront of an assault on the predominantly German theory of plant life founded on osmosis. Dyer expressed his relief at having plucked Gardiner's "brand from the burners."[17]

The ad hoc establishment of subsidiary evolutionary mechanisms to augment the seemingly disabled natural selection had been conducted by Darwin himself among others. Driven by Thomson's reduction of the earth's age and the mysterious cause of variations Darwin was one of the earliest tamperers with the stark elegance of his thesis. For the Huxleyites, "loyalty to Darwin" became fraught with pitfalls and niceties from thenceforth. In the later decades of the century they came to see a reversion to Darwin's original position prior to his construction of the doctrine of pangenesis as heretical. Such was their view of August Weismann. His essay <u>On Heredity</u> of 1883 presented the basic premise of the Neo-Darwinian school. This amounted to the assertion that Darwin's later excursions into the Lamarkian realm were misconceived.[18] In the same year that Huxley received the Darwin medal Foster wrote to him, remarking archly that: "A certain school seems to think Weismann a second Darwin." Foster encouraged Huxley with the reminder that his forthcoming book would be a suitable place for Huxley to put this right.[19] For Huxley the time

available for setting things right was running short. His last public performance in the Darwinian cause was the B.A.A.S. meeting in 1894. Although the exchanges which took place there have been held up as exemplifying the true spirit of scientific magnanimity Edward Frankland's contemporary reservations portray a different situation. He wrote to Huxley after the meeting.

> "I hope you have got over the British Association. It is evidently rather a risky thing to entrust the Presidentship to a layman of great eminence; but perhaps it is useful to be sometimes reminded how much still remains to be done before the darkness even of the intellectual non-scientific mind will be enlightened." 20

Just a few months before his death Huxley was keenly attuned to the need to find new objective evidence in favour of Darwin. On February the 14th 1895 he wrote to Hooker excitedly about a recent study which seemed "to have turned up something like the 'missing link' in Java according to a paper I have just received from Marsh."[21]

As the dominance of the physicists regarding the age of the earth diminished the Darwinian camp might have been expected to lay triumphant claim to its old position. However the years of doubt had altered the situation irrevocably. The focus of the debate shifted to give central consideration to the causes and procedures of variation. Schism within the Darwinian faction had originally represented no more than a range of responses to the challenge of limited time. As both the seeming salience and intrinsic cogency of that challenge faded, Darwinism's internal divisions developed an autonomous momentum.

Huxley's last actions in connection with his life's work were typically polemical. The Conservative politician Arthur Balfour's Foundations of Belief was published in February 1885. The work approached metaphysics lucidly from an unmistakably political background of

assumptions. William Irvine describes the book in the following picturesque fashion.

> "like Lord Salisbury's effort at the British Association a kind of prolonged speech from the Opposition benches attacking the fashionable scientific-utilitarian universe and urging the Tory universe in its stead."22

One of the fundamental reasons for the atrophy of the radical strain of scientific naturalism is suggested by the situations of the participants in this incident. Huxley and several of his closest associates were from humble social origins yet in the year of his death he had been made a Privy Councillor and was bandying words authoritatively with a future Prime Minister. Balfour's brother Francis who had perished in the Alps a decade earlier had been Huxley's leading scientific prôtegée. One of Balfour's sisters was married to the leading Cambridge physicist Lord Rayleigh whilst another was the wife of Henry Sidgwick, Professor of Moral Philosophy at Cambridge. By the 1880's Huxley's political outlook had long been Conservative. He was against Gladstone's Home Rule policy at the same time maintaining a vestigial (and private) sympathy for the Parnellites. Huxley's original companions in London scientific circles had been Hooker, Tyndall, and Frankland. They had each undergone a similar metamorphosis. Tyndall forgot his differences with Joseph Chamberlain over the working class fellow Irishman Charles Wigham. Tyndall had been deeply incensed at what he saw as the suppression of Wigham's ambition to improve lighthouse illuminants due to the vested interests of the wealthy and powerful. Ten years later in 1890 Tyndall's Conservatism led him to announce his support for Chamberlain. At a political meeting at Guildford he declared:

> "We need that strength of character and steadfastness of purpose which are best exemplified by Arthur Balfour and Joseph Chamberlain. (Cheers)." 23

Huxley was invited to reply to Foundations of Belief by Sir James Knowles,

founder of the Nineteenth Century. Knowles was a man of considerable influence and spurred Huxley into action by telling him that:

> "Since you have foresaken the Constable's beat the loose characters of thought have plucked up too much courage."[24]

Knowles printed the first part of Huxley's article "Mr. Balfour's attack on Agnosticism" in the March number of the Nineteenth Century. Before the next instalment of what Huxley described to his daughter as a "cavalry charge" could appear he was dead.[25]

Frank Turner has suggested that the scientific naturalists closely identified the nature and purpose of their enterprise with theories they learned in the third quarter of the nineteenth century. Daltonian atomic theory, the conservation of energy, and evolution provided the technical structure of a worldview committed to "interpret the detailed phenomena of Life and Mind and Society in terms of Matter, Motion and Force."[26] As the century wore on these theories were re-evaluated and adapted to the shifting theoretical requirements of the physical scientists. The Daltonian atomic model was abandoned by many of the latter, but the scientific naturalists did not take up the theory of vortex atoms. They were neither equipped to make sense of it mathematically nor willing to incorporate it in their didactic scheme. At the point where the Huxleyites were retiring from their prominent institutional positions the underlying impetus of scientific naturalism emerges somewhat further into the open. By the early 1890's the prominent Huxleyites were all living in relatively opulent seclusion outside London. From their impoverished backgrounds in Leighlinbridge, Caterall and Ealing Tyndall, Frankland and Huxley had found their respective ways to Hindhead, Reigate, and Eastbourne. Viewing the biographies of the three men closely, social mobility might as well be seen as a cause as much as an effect of the movement known as scientific naturalism. The wider social and political

meaning of the Huxleyites wider ambitions went largely unregarded in their public lives. An ironical juxtaposition of the thwarted political meaning of scientific naturalism and the public ignorance of it was unwittingly made at the Anniversary Meeting of the Royal Society in 1892. The politician Mr. Shaw Lefevre added a teasing inquiry about geological time to his remarks. Huxley replied characteristically to the effect that:

> "so far as I understand myself, my faculties are so entirely confined to the discovery of truth that I have no sort of power of obscuring it. (Laughter) With regards to political life, the absolute contradictions that were made by politicians of opposite sides upon matters of fact were absolutely fatal to his chances in a political career. (Renewed laughter)"27

In proposing Rudolph Virchow's toast as Copley medallist Huxley was given a very narrow path to tread, because Virchow's career was well bound up with the German State. The Times reported Huxley as follows:

> "Without venturing on the dangerous field of politics he would like to add that these (Virchow's) merits were, to his mind, greatly enhanced by the fact that Virchow had never merged the citizen in the philosopher but amidst great difficulties and with undaunted courage, he had taken an active, a disinterested and a thoroughly independent course in the legislature of his country."28

There was no gradual movement towards anything in the nature of mellowed resignation or an elegaic tone in the speeches made by Huxley towards the end of his life. The "political Presidency" of George Stokes which he had regarded as disastrous was only two years past at the time of Huxley's Anniversary dinner reply to Shaw Lefevre. The Huxleyites intensified their disdain of what they believed to be "the inefficient principle of democracy" just as its bastions continued to ignore and resist by turns their scientific panacea. A leading article in Nature early in 1880 gives some idea of the breadth of the optimistic outlook with which the scientific naturalists had earlier clothed the political meaning of their endeavour.

> "Yet there is surely no reason why political action, the conduct of the State should not be guided by scientific method quite as much as the conduct of a scientific exploring expedition such as that which has so recently sailed over the North-East Passage."

The writer then recomended the direct application of the scientific method to the conduct of political life:

> "to elevate it [politics] into something like a science of national life and progress."

This was most likely to be accomplished by a generalization of Darwin's methods:

> "if he really desires to arrive at the true principles of scientific statesmanship. if scientific statesmanship, and not mere party prejudice, were the guiding principle in the conduct of public affairs, this nation would be more fitted than ever to survive and play the leading part in the affairs of the world."29

Nearly twenty years later Michael Foster, as President of the B.A.A.S. at Dover, portrayed a far more limited and less tangible political rôle for science.

> "in science there is no falling back. In respect to other things there may be times of darkness and times of light, there may be risings, decadences, and revivals. In science there is only progress. . . . The growth of science is that of a living being. . . In that broad field of human life which we call politics . . . science works for good."

Foster goes on to admit that science was enhancing the destructive power of military weapons but concludes that such efficiency would shortly put an end to war as a practical possibility. He took the preliminaries of the formation of the International Association of Academies to be:

> "one of the many signs that science, though she works in a silent manner and in ways unseen by many, is steadily making for peace."30

Foster's retreat to a general faith in the inexorable improving effect of science in an almost subliminal way was a far cry from "scientific

statesmanship". In 1901 Ray Lankester boldly stated in Nature that: "It is useless to address the democracy."[31]

Huxley's obvious frustration and disappointment distracts attention from the fact that the scientific and intellectual scene in London was greatly changed by his passage through it. The change was so great that it seems that those present at the time were also in danger of losing sight of one of its main authors. In the New Review number for August 1895 P. C. Mitchell described an incident at the Royal Society Soirée which took place that year during Huxley's final illness. A group of biologists drifted together and one said: "Remember, that it was Huxley who made all of us possible." The truth of that statement is debatable but it is certain that Huxley's misfortune was to live long enough to observe the decline and gradual replacement of his network of influence in London science.

* * *

Notes

1. James R. Moore, The Post-Darwinian Controversies: A Study of the Protestant Struggle to Come to Terms With Darwin in Great Britain and America 1870 - 1900, Cambridge University Press, Cambridge, 1979, pp. 134-136.

2. Leonard Huxley, LL THH, vol. 2, p. 56n. This denunciation was prompted by two papers read to the Church Congress by W. H. Flower and Legros Clarke.

3. T. G. Bonney, Annals of the Philosophical Club of the Royal Society, London, 1919, p. 18.

4. Warren R. Dawson, Catalogue of the Huxley Papers.

5. William Irvine, Apes Angels and Victorians, Weidenfeld and Nicholson, London, 1955, pp. 316-317. Cyril Bibby, T. H. Huxley Scientist, Humanist, Educator, Watts, London, 1959, pp. 225.

6. Huxley Papers (ICL), 27.222.

7. Huxley Papers (ICL), 27.232.

8. Moore, ~~ibid.~~, p. 187.

9. A. J. Meadows, Science and Controversy a biography of Sir Norman Lockyer, Macmillan, London, 1972, p. 219.

10. Huxley Papers (ICL), 2.329.

11. Meadows, ~~ibid.~~, p. 221. Geikie later opposed Lockyer for office in the RS. Geikie was too pragmatic to be a central figure in the cause of the Huxleyites. Geikie's religious disposition reflected the normal natural theology which invested the wider society of his day (D. R. Olroyd "Geikie and Whig historiography", Annals of Science, 1980, 37, 441-461, ff. 451).

12. Royal Society NLB 6, no. 479. Hooker was proposed for the medal by Sidney Vines, an ex-pupil of Huxley who became a leading Cambridge biologist.

13. Leonard Huxley, LL THH, vol. 2, pp. 386-387. Huxley Papers (ICL), 4.384. Foster was still powerful at this time but unpopular with most of the younger biologists. Amongst the latter the award of elegaic medals was not generally well received.

14. Huxley Papers (ICL), 27.232. In general the Huxleyites supported the biometrical approach in the Biometrical-Mendelian debate. Hirst disapproved of Galton's work. In 1894 Dyer mentioned in the same letter to Huxley that "Weldon and Pearson's statistical stuff is the only thing worth discussion here".

15. Huxley Papers (ICL), 3.365.

16. Leonard Huxley, LL THH, vol. 2, Hooker to Huxley Dec. 2. 1894.

17. Huxley Papers (ICL), 27.210. Dewar maintained particularly strong objections to what he saw as the subjection of British biological science to German expertise (Dewar letters ICL, A 251).

18. Moore, ~~ibid~~., p. 182.

19. Huxley Papers (ICL), 4. 371.

20. Huxley Papers (ICL), 16.276. Frankland received the Copley Medal in 1894, the same year that Huxley received the Darwin Medal.

21. Huxley Papers (ICL) 2.460. Huxley was referring to the discovery in Java of Pithecanthropus erectus by Dr. Eugene Dubois (Harry L. Shapiro, Peking Man, George Allen and Unwin, London, 1974, pp. 29-30).

22. Irvine, ~~ibid~~., p. 355.

23. Hirst Journal, 2689. Lord Kelvin was delighted with the Unionist victories in the July General Election of 1895 (Stokes Papers CUL, K 304).

24. Bibby, ~~ibid~~., p.87. Sir James Knowles was editor of the Nineteenth Century and in addition to encouraging the enfeebled Huxley was one of that ubiquitous class of minor Victorian notables who "dined everywhere".

25. Leonard Huxley, LL THH, vol. 2, p. 398.

26. Frank M. Turner, "Victorian Scientific Naturalism", in C. Chant and J. Fauvel (eds), Darwin to Einstein: Historical Studies on Science and Belief, Longman/The Open University Press, 1980, pp. 59-60.

27. The Times, December 1st 1892.

28. Ibid.

29. Nature, 29th January 1880, p. 295. In general the leading Huxleyites regarded contemporary political life as "a chaos of party prejudice and personal invective".

30. BAAS Report, 1899, pp. 14-22. Foster concluded: "if the intellectual, if the moral influences of science are no less marked than her material benefits, if, moreover that which she has done is but the earnest of that which she shall do, such men [those who despair within the political status quo] may pluck up courage and gather strength by laying hold of her garment. Our feet are set not on the shifting sands of the opinions and of the fancies of the day, but on a solid foundation of verified truth; which by the labours of each succeeding age is made broader and more firm . . . the golden age is in front of us not behind us".

31. E. Ray Lankester, "Darwinism and Statecraft," Nature, March 21st, 1901, No. 1638, vol. 63, supplement iii, (follows p. 508).

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