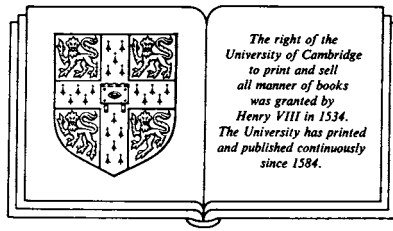


Promoting Experimental Learning

Experiment and the Royal Society
1660–1727

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Introductory

The business of the Society in their Ordinary Meetings shall be to order, take account, consider, and discourse of philosophical experiments and observations; to read, hear, and discourse upon letters, reports, and other papers concerning philosophical matters; as also to view, and discourse upon, rareties of nature and art; and thereupon to consider, what may be deduced from them, or any of them; and how far they or any of them, may be improved for use or discovery.

So run statutes of the Royal Society as passed in 1663 and so also those of 1939 and ever since. Moreover, in 1663 there was also a separate chapter of the statutes devoted to the making of, reporting on and financing of experiments.¹ But the statutes of 1847, which were intended to embody new reforms which should render the Royal Society more strictly scientific than it had become in earlier decades, read, starkly,

The business of the Society in their ordinary Meetings, shall be to read and hear letters, reports, and other papers, concerning Philosophical matters.

These mid-nineteenth-century statutes in fact recognised a situation which had existed throughout much of the previous hundred years, a period during which both experiment and discussion were slowly abandoned and the character of the Royal Society's meetings altered, changing from an atmosphere of lively discussion and debate and the frequent display of experiment to one which was determinedly formal and lifeless.

So, paradoxically, the Royal Society no longer reflected the practice of contemporary science which was certainly devoted to experiment. In 1839, and again nine years later, the physiologist Marshall Hall was to complain bitterly at the rejection of his papers by the Royal Society's Physiological Committee.² The burden of his complaint was that members of the Committee either misunderstood or disbelieved his claims for experimental proof of his discoveries and interpretation, and,

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worst of all, refused to let him show them the experiments he was interpreting. Yet the original 'designe' of the Royal Society, as its Statutes confirm, had been 'the promoting of Physico-Mathematicall Experimental Learning', which by no means excluded biological experiment, and in the seventeenth century it had been expressly stated that not only should its weekly meetings have for their purpose 'to consult and debate concerning experimental learning' of all kinds, but to perform experiments at the meetings. As a Fellow could say in the late 1660s, 'The businesse of the Society is to make experiments',³ yet by the early nineteenth century meetings were purely concerned with the reading of papers, which might be based upon experiment and even describe experiments performed, but which were never accompanied by experimental demonstration.

This was, obviously, not because the importance of experimental science had been lost sight of, for no one could describe nineteenth-century scientists as opposed to experiment. Nor was it that the Fellows were no longer good experimentalists (the regular award of the Copley Medal specifically for experimental science excludes this hypothesis), nor that nineteenth-century scientific experiment was unsuited to public demonstration (the dramatic experiments regularly performed at the Royal Institution lectures by Davy and Faraday show the contrary). Clearly, the conventions of the Royal Society meetings had changed in some way while their purpose had changed as well.

To discover how these changes came about it is necessary to trace the conduct of the meetings and the general activities of the Society from its inception into the eighteenth century; this will also give some precise idea of what the Royal Society actually did during its first three-quarters of a century. This is a task not previously fully attempted, although a number of studies in the past few years have examined particular aspects of the question. The most closely related to what is attempted here is Heilbron's analysis of *physical* experiment at the Society during Newton's Presidency (1703-27), a study which within the compass of its subject is very thorough, analysing both the content of the meetings and the publication of experimental papers in the *Philosophical Transactions* (by no means the same thing, since that journal was not yet the official organ of the Society).⁴ This is an excellent work, with the one flaw that it takes for granted that the only purpose of the Society was the advance of natural science (not learning in general) and physical science at that. A good many years ago now I discussed the way in which certain leading

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Fellows of the early Royal Society contributed to the advance of experimental philosophy, concluding that their private contributions were often more important than their public ones, since they tended to experiment elsewhere than at the Society's rooms to advance their main work; this study was not very inclusive and was limited to the period before about 1680.⁵ More recently, Hunter has discussed the institutional workings of the Society, while Hunter and Wood have published various seventeenth-century proposals for the reform of the Society (all of which stress the importance of experiment). Hunter's work supplements the many discussions by historians of science of the original aims of the Society as manifested in its charter, but does not tell us what was actually done by the Society at its meetings; indeed, much of what Hunter discusses is, ironically, a record of failure.⁶ More recently still, Shapin and others have surveyed experimental activities in England inside and outside the Society during its early years from a sociological point of view, with particular emphasis upon the location of experimental work and its public or private nature, especially during the 1660s and 1670s.⁷

However, no one so far has analysed the whole of the Society's day to day activities as they relate to experiment, clearly revealed in the Journal Books, not just for its first fifteen years of existence – admittedly some of its most important – but for the less familiar later years when the founders were aging or dead, the first enthusiasm had dampened, and, as the members themselves were aware, new methods for the conduct of meetings were needed. What the Society really did during the later decades of the seventeenth century has been little studied in recent years. To round off an investigation of this kind it has seemed advisable to include a briefer survey of the Society's activities as they relate to experiment during the first quarter of the eighteenth century, chiefly the period of Newton's Presidency.

As all students of the early Royal Society are keenly aware, experimental activity at meetings after November 1663 depended very much for much of the century upon the work of its first Curator of Experiments, Robert Hooke. Hooke was an extremely able and inventive experimenter who gave experiment a very high place in his view of proper scientific method. It might therefore have been expected that when he became Secretary in 1677 and played a more dominant rôle in the Society's affairs, experiment in turn would have had a greater rôle in the meetings, in accordance with his frequent declarations of its

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importance for natural philosophy. The record of the Journal Books shows this not to have been the case. Hooke, like many others, was often happier to talk about experiments already performed or to suggest experiments that might be performed than to go through the time-consuming, costly and laborious business of performing experiments suitable to the topic in hand. And, again like many others, in old age his interests tended to turn from experimental to descriptive science and even to such subjects as mythology or archaeology, not directly connected with the study of nature as usually conceived. Newton, of course, also interested himself in what, to moderns, are peripheral subjects, such as biblical exegesis and chronology, but until the last years of his long life he was actively concerned with experimentation and effectively saw to it that the Royal Society was as well. In this connection it should be recalled that much brilliant and important work in the biological sciences was presented to meetings during the later years of his Presidency.

It has proved possible to show how discussion gradually dwindled and died at the Royal Society's meetings as the reading of papers replaced in very large part spontaneous discussion and the performance of experiment (which always provoked it), but during the period under discussion here comment upon the papers read was always welcomed, even if not always given. It has not proved possible to discover the reason why discussion of any sort disappeared so thoroughly as to be taken by the later eighteenth century to be counter to the statutes. It is only possible to observe that it did so. That the performance of experiment came to such a complete stop is equally surprising. In part this may be laid at the door of the last effective Curator of Experiments, J. T. Desaguliers, who performed fewer and fewer experiments once Newton ceased to direct him in them. He did occasionally perform experiments as late as the 1730s, mainly apparently in order to claim the reward of money from Copley's legacy (before it paid for a medal). Others too continued to produce experiments, but these became fewer and fewer as time went on. To this extent, the common historical opinion that the Royal Society was in decline in the early and mid-eighteenth century is justified.⁸ But for a rounded picture it is necessary to judge the Society not only in terms of the conduct of its meetings as revealed in the minutes but also in terms of the standing of its Fellows and its general reputation. Hence it has seemed right here to include some discussion of the aims that the Society held as seen by the members

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(Chapter 2), of the means by which the attitude of the Society was disseminated throughout the world (Chapters 4, 6 and 8), and of the way in which the Society was viewed by contemporaries (Chapter 9). But the main emphasis here must be on the detailed record of the minutes and an analysis of the place experiment held in the regular conduct of the meetings.

To assist the reader in understanding this analysis, I have, in accordance with modern historical usage, provided graphs to illustrate experimental activity in the three periods into which this study is divided. These attempt to show both the approximate number of experiments performed annually at meetings and, for the period after 1674, the number of experimental papers, discourses or letters read at meetings during each year. Clearly any such analysis must be highly subjective, and I make no claims to precise accuracy; yet I hope that my graphs will be a useful guide and show at least relative activity if they cannot show absolute numbers. They should provide some help in reading the verbal analysis which presents the facts and events upon which the figures are based. For convenience, I have also provided three lists (Figures 1, 5 and 6) which give the names of the Officers and principal employees during the period 1662 to 1727.

In both the graphs and the text I have tried to distinguish roughly between *experimental* learning and *observation*, but without attempting any exact definition of experiment. I have not counted the presentation of random empirical facts, fancies or thoughts as experiment, nor in consequence papers based upon them as experimental. I have not counted histories of trades, though these were clearly empirical. Nor have I counted the display of instruments or models as experiment, except where this involved their use for the presentation of experiment. Obviously I could have done these things and some will argue that I should have done. Seventeenth-century usage might seem to justify it, but I chose to take a slightly more rigorous attitude for reasons which will appear below. My justification for this is not mere simplicity, and it is, above all things, not for the passing of judgement. It is rather that, as one must always keep in mind, the Royal Society appointed Curators of *Experiment* (whether long or short term) and not Curators of *Observation*, of *Empirical Record* nor of *Instruments*. The Society of course welcomed the presentation of observational accounts or descriptions of instruments or the showing of models. And it is true that the paid Curators of Experiment, like Hooke, seem sometimes to have regarded

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reports of observations made with instruments or description or models of instruments displayed as being at least partly equivalent to the showing of experiment, especially when the instrument itself was available for demonstration. Yet one often gets the impression that this was 'fudging', that is, that the Curators knew that they were in such cases not so much presenting an experiment as presenting a substitute or preliminary to experiment. Experiments, after all, were often difficult or tedious to present; one intended for a meeting might not work or might not be suitable for presentation to an audience. In such cases instruments and models were an acceptable and easy substitute and always attracted interest. It is true that, obviously, this all makes strict quantitative analysis impossible. Yet it does not negate the possibility of relative quantitative analysis such as is displayed in the graphs, where it is clear that in some years more experiments were presented to meetings than in others. And this is all that I have aimed at, endeavouring to present the material with as much consistency as is reasonably attainable. *Empiricism* by itself is too vague a term to permit any quantitative or qualitative analysis, since it includes hearsay, sense impressions, supernatural experience and statistics as well as genuine observation. Unless some restriction is to be made, the whole exercise is meaningless. Any reader who finds this too restrictive must make allowances. I do not believe that the absence of such empirical presentations seriously falsifies my general conclusions in any way. And on the whole it is fully in keeping with seventeenth- and early eighteenth-century usage largely to abstract the performance of experiment from other forms of empirical presentation, not to suggest that it was better, but to understand the genuine role of experimental learning. That this is as valid for the early eighteenth century as it was for the later seventeenth century is clear from the terms of the Copley Award (after 1731 the Copley Medal) which explicitly specify *experimental* work only as admissible for the award.

Thus I surely need make no apology for abstracting the performance of experiment from other activities at meetings, while always noting the reading of experimental papers. Such activities as the reading of papers and letters not concerned with experiment, conversation, random comment and recollection, general discussion and so on, although informative and often including observation, must therefore be excluded. It should be obvious that this must have been only a part of what went on at meetings, for not all the proper concerns of the Royal

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Society (that is, what was thought proper by the Fellows) by any means lent themselves to experiment. Mathematical learning clearly came within the Society's interests, but the reading of pure mathematics could hardly be popular and was seldom attempted. Papers on applied mathematics, if short, were sometimes read, but not often; publication in the *Philosophical Transactions*, when that was available, was usually deemed sufficient, sometimes after brief consideration. (It is a measure of the Fellow's extraordinary appreciation of Newton's *De motu corporum* that it was read at meetings.) Astronomical papers were read not infrequently when theoretical, but papers of astronomical observations were 'received' (that is, mentioned or described to a meeting) and then recommended for printing. Meteorological observations when detailed were similarly treated, although theoretical comments might be read. The same considerations applied to geological and medical reports. All these subjects might produce papers of a high empirical content, but equally might be mathematical or theoretical. Chemistry was clearly experimental. Alchemy, which appeared infrequently, normally was presented only when empirical rather than theoretical; even theoretical chemistry was avoided as inclining towards 'system building' and requiring a judgement of theory which the Society regarded itself as pledged not to give.

I do not intend anywhere to offer value judgements. When I say that experimental activity languished I mean just that: that there was much less of it than had been the case in other years and than the Fellows generally wished there to be. I do not mean to imply that experimental activity was in some way 'better' than other activity (in spite of the fact that some Fellows at every period under discussion did say so). In quantifying experimental activity I do not at all intend to suggest that only experimental activity was proper for presentation, although, as will become amply clear below, it was a repeated complaint of the Officers, Councils and many Fellows that experimental activity ought to be maintained at a high level, and they did not say this about the presentation of instruments, models or histories of trades. This is clearly shown by the way that Curators of Experiment were sought for and even paid out of the Society's meagre funds. The Society was prepared to go to considerable lengths to ensure the presentation of experiment and experimental papers at meetings. And, as I have tried to show in Chapter 2, most of the plans for reorganising the Society in this period revolved around increased experimental activity, while (Chapter 9)

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critics outside the Society, both at home and abroad, judged the Society by such activity. Foreign savants regarded such activity as the chief characteristic of the Society and what differentiated it from other learned bodies (exceptions were the short-lived *Accademia del Cimento* and the also short-lived *Académie de Physique* of Caen). This was such a universal opinion that I commit no solecism in choosing to explore only the experimental content of meetings.

This I have tried to do without regard to subject, not, as Heilbron ably did for the Newtonian period, concentrating on physical science, but endeavouring to consider all subjects and all experimental learning. Once again I must stress that I intend no value judgements, conscious of the views on this point of modern historians. But I cannot resist pointing out that this is to be ahistorical to the extent that the period I am considering was one in which value judgements were constantly made, being considered moral and necessary to a rightly ordered society. I have here attempted only to present as clearly as possible what the Fellows thought about the uses of experiment, what they did to promote experiment, especially its presentation at their meetings, and how the quantity of experiment changed over the years between 1660 and 1727.

Finally, I apologise to the reader for the fact that the word 'experiment' occurs so frequently in these pages. I have avoided it where possible, but obviously it insisted upon being used in most cases. I must ask for indulgence. Any who finds such indulgence difficult to grant may permit me to offer in mitigation the report of Dr William Aglionby (F.R.S. 1667) in the spring of 1684 upon a German medical work:⁹ he 'declared, that there was a great deal of reading in it, but little experiment.' At least I have avoided that error.