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THE ORIGINS OF THE ROYAL SOCIETY

I

Trevor-Roper's introduction to Miss Purver's work * sums up the book adequately. There are two stories about the antecedents of the foundation of the Royal Society of London, overlapping yet different. Sprat's *History of the Royal Society* of 1667 declares the Oxford group to be its antecedent, whereas most other writers assume the London group to be the one. The prejudice against Sprat, namely that he was himself prejudiced in favour of Oxford, is dispelled by showing that his *History* was the semi-official one. The question, however, is ideological. The Royal Society was Baconian. So were both the Oxford and the London groups. But whereas the London group held vulgar Baconianism, the Oxford group and its successor the Royal Society were purist Baconian. Thus, the true predecessor is the Oxford group as Sprat has claimed, not the London group as his successors have claimed.

The London group's vulgar Baconianism, to continue Trevor-Roper's summary, was the weaving into Bacon the ideas of pantheism, social radicalism, millenarism; the nineteenth century followed Macaulay and read Utilitarianism into Bacon. Pure Baconianism ("the new philosophy") replaces the idols of the theatre and the market-place with the truth.

So much for Trevor-Roper's summary. Regardless of how well he represents Miss Purver's views, the question may be asked, is his summary acceptable prior to considering the new evidence? I start here because Trevor-Roper concludes his introduction frowning at those who, like myself, tend to resist Miss Purver's conclusion before examining her new evidence. (She had published her conclusions without the evidence some years before; see Appendix below.)

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* Margery Purver, *The Royal Society: Concept and Creation*, with an introduction by H. R. Trevor-Roper, Routledge and Kegen Paul, London, 1967, pp. xvii + 239.

It is trivially true that antecedents in intellectual history have to be judged ideologically in the first instance. But there are, or may be, other kinds of antecedents which may be of interest to a historian, even to a historian of ideas and ideologies. In the case at hand, in particular, the concern may be with a scientific organization. It was the seventeenth century which developed the very idea of scientific societies, and almost all important firsts, particularly the Paris, London, and Oxford groups, were inspired by Bacon, the inventor of the idea of the lay university, with its research laboratories—the non-monastic monastery. The Society was, in a way, a lame substitute for the lay university. To be more precise, the idea of the Society itself, as opposed to the groups, surely belongs to Evelyn, Boyle, and Wilkins, not to any group, the Oxford or the London. Evelyn was for a lay university, but not Boyle. The antecedent events leading to the formation of the Society, the various abortive efforts to organize something of a scientific institution, surely belong to the vulgar Baconians of the Paris and London groups. It will be interesting to see what Miss Purver has to say about the origins of the theory and practice of building a scientific community.

So much for organization as an additional dimension of the problem. Confining ourselves to ideology, then, we have one more problem. How Baconian were the London or the Oxford group? What exactly is true Baconianism and what is vulgar Baconianism? Trevor-Roper says, true Baconianism is the idea of replacing the idols of the theatre and the market-place by a "true model of the world". This is what philosophers call the (empirical) verification of scientific theory. Were the London group against it and the Oxford group for it? This is hardly conceivable. Amos Comenius, an idol of one London group (for there were two or three of those, naturally) regretted that Bacon had discovered the key to the secrets of Nature yet failed to use it. It is also true, of course, that various people had various millenarian ideas of social radicalism, and that the Society confined itself to intellectual radicalism (as described by Trevor-Roper) and tabooed all other radicalist ideas, social or religious. How much this exclusivenees belongs to the Oxford group, how much to the Restauration (as noticed by Macaulay), is an open question.

As to Bacon himself, it is hard to say *a priori* how much he was a pure Baconian, how much vulgar. The vulgarization of the seventeenth century may be in part an expression of immediate needs of the second quarter of the century which had nothing to do with Bacon, who wrote in the first quarter of the century. Alternatively the immediate successors of Bacon may have shared with him much background knowledge and so naturally read him nearer to his intention than we do. This second idea is not mine, but that of James Spedding whose own small odyssey is not without interest.

Spedding began his career as a Bacon scholar by responding to Mac-

aulay's *Essay on Bacon.* In that essay Macaulay debunked Bacon the philosopher to the extent that he gave rise to a problem: why had Bacon been so revered throughout the eighteenth century as a philosopher? This problem has engrossed most Bacon students since, and is the chief biographer of Newton and the one who stated the problem, explained C. D. Broad, and even more recent writings. Sir David Brewster, the biographer of Newton and the originator of the problem, explained Bacon's fame by reference to his political and literary career. Justus von Liebig, the greatest and most influential of Bacon's debunkers, later endorsed this solution; Macaulay, the utilitarian, was more charitable, and praised Bacon as the father of modern utilitarianism.

At this junction of Macaulay's essay, the historian recedes to the background and the propagandist takes over. Had Macaulay been challenged in his attribution of modern utilitarianism to Bacon, it seems he would have preferred to discuss the truth or falsity of the doctrine than the truth of falsity of his attribution of it to Bacon.

Spedding read Macaulay's book as debunking, mainly Bacon the politician, since it hardly treats Bacon the philosopher. Spedding wrote a very long defense of Bacon against Macaulay's attack. At about the same time Robert Leslie Ellis was working on a complete edition of Bacon's works. Ellis was a mathematician and a biologist, in addition to being somewhat of a classicist and a surprisingly well read scholar. He was a consumptive who died before he was forty. He asked Spedding to join him in his work, and died soon after, leaving it to Spedding to do with the uncompleted task whatever he found fit.

Spedding found the editorial work painful. He followed Ellis' change from admiration to puzzlement, and found even more puzzlement. He found, with Ellis, that Bacon had no scheme for a new philosophy, no idea about induction, no willingness to accept, even tentatively, induction by generalization, no suggestion as to how science can be built on solid foundations. In addition he found that Bacon's immediate successors were right in reading Bacon's mythological and utopian writings not as mere fables, but as serious works intended to be taken as serious.

Trevor-Roper says (p. xv) that "Miss Purver has recreated the 'new philosophy' of Bacon, redeeming it not only from the puritan vulgarization of Hartlib and his friends but from the Victorian vulgarization of Macaulay." He does not mention Ellis, nor Spedding, but implies that no Victorian commentator on Bacon is better than Macaulay. One cannot but consider this a bit below the dignity of a scholar—particularly so since a page later Trevor-Roper admits that Sprat's reading of Bacon, which is the same as the Oxford group's reading and as the semi-official reading of the Royal Society, is an "idealized" version; that is, he admits that Bacon himself is a bit vulgar. It is one thing to say that a reading

of a text is unhistorical and another to say that it is vulgar; as it happens, Spedding's reading is both vulgar and historical, and he also offered an idealized and frankly unhistorical reading akin to Sprat's, which he recommended should be tried out. It never was tried out. In his famous review of Spedding, William Whewell criticized this proposal quite sharply, and it was forgotten.

This much for extended comments on Trevor-Roper's seven page introduction. Let us examine the detail of the book, but with an increased pace.

II

The problem Miss Purver comes to solve is how reliable Sprat's *History* of 1667 is. The current view is that his claim that the Oxford group was the one which led to the foundation of the Royal Society in 1660 is suspect as he was the mouthpiece of Wilkins of Oxford; Boyle had spoken of "the invisible college" in London of the 1640's, and John Wallis, the mathematician from London, made a similar claim. Then there are accounts of activities of other personalities in London in the period in question, especially Samuel Hartlib whose invitation had brought Jan Amos Comenius to London. After 8 pages of thus presenting the problem, Miss Purver devotes about 150 pages to Part One, where she expounds Sprat's and her own view about the Oxonian origins, and about 80 pages to Part Two on the alternative views.

Part One, Chapter one, allegedly on the validity and significance of Sprat's *History,* but in fact largely (ample) evidence that Sprat's work was officially declared the semi-official view of the society. This explains the delay in publication from 1664, when we know it nearly went to press, to 1667. This also explains why, as Charles R. Weld, a laten at the same time, will now read, description and history, etc.) uninformative. (Incidentally "history" in Sprat's title may be read as in "natural history" or as in "the history of England." Antony Wood's *History and Antiquities of the University of Oxford,* for example, written at the same time, will now read, description and history, etc.)

Chapter two on Francis Bacon's philosophy, without which the Royal Society could not be as important as it was. Though a number of societies of similar character had come and gone, the Royal Society was a real first. Evidence: Sprat says it succeeded to bring about in six years more than others have in six thousand (i.e. since Creation).

This is a very strange thing. It is not easy to declare that Miss Purver agrees with Sprat and accepts his testimony as final. But I am afraid I could not find another reasonable reading of her text. This has something very nice and commendable—her taking Sprat's claims seriously

and literally—as well as something very partisan and intolerable—her uncritical acceptance of so much on his mere say-so.

Radicalism is quite a thing to contend with. When one reads Bacon's claim that he has followed no one's footsteps and is the very first of his kind, one cannot but be moved. Robert Leslie Ellis was so moved that he was determined to attribute some valuable idea to Bacon. He could not attribute to him any idea about induction, because he said little about the technique of induction, because what little he did say was contingent on the questionable assumption of a very high degree of simplicity and comprehensibility of nature, and because he explicitly declared induction by generalization childish. Ellis finally attributed to him a version of atomism and an important idea in the psychology of learning, both of which he found in Bacon's myth of Cupid. Ellis was an immensely learned man and so he could find the source of almost any idea in Bacon which he considered valuable. Yet somehow even his scholarship was not broad enough. He had overlooked Natalis Comes or Conti, though this student of myths was fairly well-known in the early seventeenth century (see Paolo Rossi, *Francis Bacon,* Chicago, 1968, p. 80). As C. W. Lemmi has shown (in his *Classical Deities in Bacon,* Baltimore, 1933), what little original material Ellis had found in Bacon was material lifted by Bacon from Comes.

In his by now classical *Ancients and Moderns,* R. F. Jones quotes one Hakewill, probably a disciple of Bacon, to say of himself that he is utterly original, in almost the same words as Bacon. Later on Lynn Thorndike, in an essay in *Isis,* quoted a long list of important Renaissance figures, all claiming utter originality in accord with what obviously was the fashionable formula of the day. In the light of this it is hard to take radicalist claims as seriously as Ellis did. Meanwhile the arch-conservative Michael Oakeshott in his *Rationalism in Politics* (Cambridge, 1959) has shown this to be a standard feature of radicalism (which he identifies with rationalism so as to arrive from conservativism to irrationalism), quoting even Bernal to say in our century that since by comparison all science prior to ours is microscopic we may well view science as more or less an ultra modern creation. And Imre Lakatos has quoted Bertrand Russell (*Mysticism and Logic*) to say that perhaps George Boole was the first mathematician, but more likely Russell himself was.

All this takes us far afield from Miss Purver's study. She indicates her radicalism by approving of Bacon and of Sprat. Ellis is for her but a follower of Macaulay who distorted Bacon and presented him as a utilitarian. She makes no mention of R. F. Jones or of Lynn Thorndike. Since she begins her chapter by a survey of the history of scientific societies I had hoped to find a reference to Martha Ornstein's *The Role of Scientific Societies in the Seventeenth Century* (Chicago, 1920), one of

the earliest recognitions of the place of radicalism in the seventeenth century. But no. Miss Purver, says H. R. Trevor-Roper (p. xiv), "has presumed nothing. She starts from the beginning, and tests every piece of evidence before using it." He does not say what she does or ought to do with the evidence she does not use.

Bacon's philosophy begins with the maxim, presume nothing, collect all the available evidence. Let the evidence lead you to the formation of a theory. With enough labour and patience you will arrive at the true theory. The basis of this process is radicalism: assume nothing; first, destroy all error. Somehow, all of Bacon's debunkers, from Brewster to date, being radicalists like all debunkers, took radicalism for granted, took for granted the idea that it is best to take for granted no idea; they therefore could not see Bacon as the great innovator, as the inventor of radicalism. Miss Purver is a radicalist yet will not debunk him.

Miss Purver, at least, finds Bacon's greatness in his radicalism; and though not original, she is quite right. (Her view is expressed in Paolo Rossi's *Bacon*, opening of Chapter 6.) Also, this is for her the rationale of the founding of a society: the process of collecting vast data requires collaboration (Rossi) and hence organization. Also, she endorses Bacon's radicalism. It is unusual to endorse Bacon's radicalism yet praise him; for a radical it is hard to forgive Dacon's failure to abide by his own radicalism, his erroneous acceptance of magic, alchemy, and geocentricity, his calling Copernicus a charlatan, his poking crude fun at Gilbert while plagiarizing from him, his inability to understand or to take notice of Galileo's headline-making discoveries, etc. Miss Purver does not meet the difficulty: she says, "The celestial bodies were, as Bacon scornfully remarked, 'supposed to be fixed in their orbs like nails in a roof'." (p. 28), "Bacon was far from being the only one to see that such a concept of the natural order [the 'Aristotelian'], even if considerably modified, presented grave obstacles to scientific progress." (pp. 29-30), and even "In this context Bacon's own resistance to the Copernican hypothesis is not only reasonable, but scientifically impeccable." (p. 40)—but not a hint at Bacon's magic, alchemy, staunch geocentrism, and tirades against Copernicus. Miss Purver, says Trevor-Roper, "tests every piece of evidence before using it."

Miss Purver's chapter on Bacon comprises over forty pages, most of which are devoted to a general exposition of Bacon's works and thoughts. Ellis' classical summary is not much longer and C. D. Broad's (Cambridge, 1926) is shorter. Both are more accurate, more informative, more interesting. Neither is up-to-date, to be sure, but their errors are at least understandable within the terms of the accepted standards of scholarship.

In other words, the scholarly world, even when spouting the pure milk of Baconianism, practises a different, and non-radicalist, standard:

the scholarly world recognizes that certain errors are permissible within scholarship yet other errors disqualify their perpetrators as scholars. Miss Purver disqualifies Macaulay, whose *Essay on Bacon* is a paragon of beauty and of scholarship, because his presentation of Bacon's philosophy is (undoubtedly) scanty and erroneous. Standards much more lax than hers are violated by her, such as those which include the counsel to people who live in glass-houses to be sparing with the throwing of stones.

III

We now come to the chapter on the Royal Society's Baconianism. "That the movement originated in the University of Oxford is not very surprising" we are told (p. 63). "Its leaders, knowing that academically they were in hostile country, were conducting themselves with caution and tact, for nothing was to be gained by antagonizing the main body of their own university or of academic opinion elsewhere. So, when, in that year, Thomas Hobbes, in his *Leviathan,* attacked the whole range of Aristotelian learning in the universities, the club did not welcome his efforts." (p. 64) I like the juxtaposition of these two quotes—less than a page between them!

It may intrigue one that Miss Purver has chosen this line as an opener for the support of the thesis that the Oxford group comprised true Baconian radicalists, the followers of the one who—to date—is the severest critic of academic and Aristotelian practices. Miss Purver has an explanation: "In a witty rejoinder" she continues (this is a slip of her pen, "rejoinder" signifying a counter-offensive), the group critized Hobbes as one who wished to replace Aristotle. This is a variant of Bacon's attack on Copernicus which soon became traditional. Huygens said the same of Descartes, and Dr. Thomas. Thomson said something similar of Lavoisier: all radicalists must explain failure to implement the radicalist formula; the absence of pure intentions is the easiest available, and the one which Bacon had found in the Cabbalistic and alchemical literature (including the works of Comes) and expounded in his various works.

Another attack on Aristotle took place in the mid-fifties, this time by a frank Baconian (Hobbes, too, was influenced by Bacon, and even a personal friend; but not a disciple). It was, however, aberrant: its author believed in astrology and alchemy. Miss Purver forgets that even Boyle and Newton were aberrant. Another member of the group joins the comments this time, and with a "tactful statement" (p. 65) defends Aristotle's scholarship, "implicitly" endorsing some of the criticism (p. 65). The interesting part of the group's counter-attack on the poor frank Baconian was an expression of mixed feeling towards Oxford. Miss Purver quotes but does not comment. She does not say why the Oxford

group moved (1658-9) to London, even though Oxford was "Oxford of this enlightening and ameliorating influence". By "this" she means merely the Oxford group—now departing and taking the amelioration with them. Why did they depart? Was anything amiss? No answer.

In 1661 Glanvill attacks astrology. In 1665 he revises his work and dedicates it to the Royal Society. He is soon elected fellow of the Society. (The story of his being forced to revise his book is told by R. F. Jones. We find no explanation here as to why he revised it.) In 1667 Sprat's *History* appears, and in 1668 Glanvill's *Plus Ultra*, both apologies for the Society. Miss Purver gives the impression that Glanvill is a Baconian. Those interested in him may read Professor Richard Popkin's exposition of his skeptical philosophy.

In the fifties Oxford was the birthplace—a "not very surprising" (p. 63) fact since Oxford had "enlightening and ameliorating influence" (p. 67)—even if all this required some compromising. Things got better with the rise of the Society and its defense by Sprat and Granvill. So, in 1669, it all led to an open clash between Oxford University and the Royal Society. This may all be very clear to Miss Purver; for my part I wish she had explained the trend more clearly.

Anyway, clearly, the universities (for Cambridge joined Oxford) feared competition (p. 72–3), and competition not from a new university or its like, but from the new experimental Baconian ideology (p. 75–6). Is this "enlightening and ameliorating"?

The evidence is from Sprat's *History*. The history is of 1667, the quarrel from 1669. It is clear, however, that not Sprat was on the attack but the universities: clearly, when he said the Society did in six years more than the whole world since Creation, he was just stating the facts. What, however, has happened to tact? Was the Society so sure of the oncoming attack that it decided that 1667 was no time for niceties? Miss Purver does not say.

Nevertheless, Miss Purver is right on the major issue: the quarrel with the Universities was ideological: it was the Baconian radicalist ideology which made the Royal Society declare through Sprat that the universities were worse than nothing.

We are now in the midst of Miss Purver's exposition of Sprat's Baconian radicalism. The main point is Baconian indeed: the Society insisted on experimenting first, leaving theorizing to a later stage (so as to avoid error and dogma). Miss Purver admits, however (p. 84), that in a clever one. First, it is not to be doubted that Boyle's report is true. at least irrelevant. And she takes as a silly example Boyle's report that he had been informed that excessive coffee drinking causes palsy.

With so many silly examples around, Miss Purver had to choose a clever one. First, it is not to be doubted that Boyle's report is trus. Second, that suppressing it would have been irresponsible, since he could

not know *a priori* whether there was anything to it. Third, at least in view of the social unacceptability of coffee, possibly even the theory Boyle reports to have heard was also true (assuming the paralysis reported to be hysterical). But this only refers to incidentals and to unknown ones (was the paralysis hysterical?), where principle matters much more; to which I now revert.

A scientific fact must be repeatable. This was instituted by Boyle in 1661, in his essay on "The Unsuccessful Experiment" (the expression is Bacon's, but he meant it—in his *Advancement*—to denote an unsuccessful attempt to build a useful machine), under the influence of Galileo (in *The Assayer*). Boyle also suggested to declare any unrepeatable experiment unsuccessful rather than a distortion. Now a delicate matter, both politically and philosophically, is hidden in this suggestion and I wish to discuss both.

Henry Stubbe, the leading enemy of the Royal Society, attacked its radicalism most. In his attack on Sprat (*Legends, No Histories*) he says, the Society should make experiments instead of trying to remove all the rubbish of the past. Indeed, he adds, everyone knows that Bacon himself made a lot of mistakes, especially in gardening, as even fellows of the Society admits. The reference is to the fact that Bacon had transcribed from Pliny about gardening, especially roses, and forgetting that the climatic conditions in Italy and England are different, and to the fact that Boyle himself uses this as an example of an obstacle to repeatability though without explicity asserting that Bacon had transcribed what he had professed to report.

Bacon must have embarrassed his followers quite a bit. John Evelyn, for example, shows this in his letter to William Wotton on Boyle (Wotton was going to write Boyle's life but never did). Evelyn says there, Boyle always performed his experiments, unlike Bacon, though the fact about Bacon need not be broadcast. (Miss Purver quotes from this letter only the passage about the early days of the Society).

Bacon's most Baconian work was his *Sylva Sylvarum,* ten books of one hundred facts each, put at random and full of superstition. Boyle wanted to write a book to replace it, and called in *The Promiscous Experiment.* The fact that he advertized it yet never published it is quite remarkable since the man published voluminously and regularly. John Beale, his old school-mate from Eaton, regularly urged him to publish the book. He even reminded him how grateful they were to Bacon, how impressed they were when, for the first time Henry Wotton (the founder of Eaton and the father of the above mentioned William) placed Bacon's work in their hands (when they were in their teens; Wotton was the first Baconian who even performed experiments such as conceived by Bacon—see his posthumous *Reliquia Wottoniana*).

The reason for Boyle's reluctance to publish his promised *Promiscuous*

Experiment can be found in what seems to be the substitute for the *Promiscuous Experiment,* which is Boyle's posthumous *Experimenta et Observationes Physicae.* Boyle wrote in the preface to this work that he tried to describe the facts in it as circumstantially as possible, but he feared that nevertheless it is possible that in some descriptions some circumstances necessary for repetition were inadvertantly omitted; and he calls on his old friend Oldenburg (the secretary of the Society) to testify that he had performed even the experiments which the reader may find unrepeatable. When a dying man calls a dead man to testify in his favour he must be talking in earnest, and on a disturbing point.

This indicates how aware Boyle was of the philosophical difficulty involved in the philosophy of induction: we cannot decide what is an observed fact without deciding what of the observation is part of the observed fact and what is incidental to it. To decide this is to rely on theory, and to rely on theory prior to experiment may be a prejudice...

Back to Miss Purver, who accepts the maxim to begin with facts yet insists that they all be relevant. She is now recounting the list of the experimental projects reported by Sprat—with great approval. Those interested may be well advised to supplement her review of Sprat's history with L. L. Whyte's review of the facsimile edition of the work in the *British Journal for the Philosophy of Science.* Miss Purver quotes Glanvill's *Plus Ultra* and other sources to prove that the Society was orthodox Baconian. She then devotes a page to represent Sprat on language, concealing his chauvinism and his mention of Milton as the only English poet of any significance. She ends by declaring that the Royal Society alone put Bacon's vision into practice. The vision, we remember, is of removing past prejudices and of organizing a vast search of data. The vision was quite reasonable in the seventeenth century, when only geniuses like Boyle could criticize it. Centuries later, after criticisms by philosophers, psychologists, and Bacon scholars, Miss Purver endorses it with the same naïve freshness.

IV

Chapter 4 takes us back to Oxford—Sprat on Oxford, others on Oxford, some biographical data. The Oxford Club was founded in 1648; this is a bit of an exaggeration: there was no foundation and no club, only an informal colloquium. Anyway, the first public reference to it is by Ward in 1654, "declaring that Aristotelianism was being combatted" (p. 113). A footnote refers us back to pages 64–7, where all that we are told is on page 65 that Ward wrote a rejoinder to Hobbes's attack on Aristotle and on page 66 that Ward said in Oxford they were teaching not only Aristotelianism but also modern versions of Copernicanism "either as an opinion, or at leastwise, as the most intelligible and most convenient hy-

pothesis." This is not exactly evidence that "Aristotelianism was being combatted". Miss Purver takes some liberty with her own crucial point of evidence.

The chapter can hardly be summarized; at least I cannot summarize it. Again, no explanation of the movement to London, or to Gresham College. They referred to themselves as "the Society" or "the Company" or "the illustrious Company that meets at Gresham College". And soon they received the Royal Charter.

Chapter 5, The Royal Charter. The Society was founded in 1660 and ran into financial difficulties. C. R. Weld, a later historian of the Society (1848), discusses these difficulties; Sprat does not, nor does Miss Purver. The farthest she goes is to quote in a footnote Sprat and Birch to say that some fellows were researches others were financial contributors. On a previous page (109) she mentions that in 1654 Wilkins "had given 200 pounds towards a College of Experiments and Mechanics to be set up" in Oxford. Also, she quotes (p. 113) Seth Ward to speak then of "a conjunction of both purses and endeavours of several persons." It now seems that something had changed: Wilkins was broke (p. 130). There were people in better financial shape, especially Boyle. Weld complains. Even a biographer of Boyle, L. T. More, is not very approving. Miss Purver is reticent.

It is clear that Boyle's friends, particularly Oldenburg, tried hard to get Boyle to finance some scientific activity or another, preferably found a secular college on the Baconian line. But Boyle never did. Even when his friends procured for him some confiscated Irish land (1662) so as to enable him to support science without loss he was adamant: he said since his friends had not consulted him he was not bound by their intents and spent the money on charity and on missions. (This is the source of the complaints.) In his important early work, *The Spring of the Air* (1660), in the introductory part he says, a philosopher needs a purse as well as a brain (in obvious contrast to Ward's above quoted remark); in his will he bequeathed all his scientific materials to the Royal Society, including his stones but excluding the gems. All this was deliberate, it seems, and systematic. It even agrees with Boyle's philosophy of mind: whereas Descartes assumed the mind to possess reason alone, Boyle assumed it to possess reason and emotion. To reason he ascribed natural religion, which includes natural theology and experimental philosophy (as doctrine and ritual respectively); to emotion he ascribed Christianity (including revelations and miracles) as ancillary doctrine and as second chance for those who jettison reason. And so charity becomes religion but not science. Also, of course, science is rationally superior to religion as it is rational and so when science and faith clash science must win, and the Bible must be understood as a mere system of ethics, etc.

Without discussing Boyle's role in the Royal Society, we can take it for granted that he was a prominent member of the Society, and that he wanted it to function as a means of bringing amateur scientists together. If so, it cannot be suggested that he would oppose the idea of admitting to the Society people whose contribution is only financial, though he must have coveted their brains more than their purses. Nor could he have objected to the Royal Charter and such, and for similar reasons. We must remember that though he rejected peerage, bishopric, presidency of the Society and provostship of Eaton, though he was proud of not being a college fellow, he could not resist an Oxford degree as this eased the tension between the University and the Society. This last point was noticed in the thirties by J. F. Fulton, the renown Boyle bibliophile.

And so, Miss Purver's explanation of the foundation of the Society from Ward's viewpoint does not quite clash with any explanation from Boyle's viewpoint—on the condition that we notice that they differ, Ward liking better the idea of the man with a purse supporting the study of the man with the brain, and Boyle liking better the man with a purse pursuing his own researches.

Anyway, the story according to Miss Purver is sufficiently straight forward. The Royal Charter was given in 1662, allowing the Society a few privileges. It was revised and implemented in 1663; the revision did not offer new privileges (contrary to what historians say), but a coat of arms, the full name—the Royal Society for the Advancement of Natural Knowledge—and the statement that the King was its Founder and Patron. Some details about membership. I remember having read that one founding member was expelled. Miss Purver's talent could be put into use in search of the story; her disposition lies elsewhere.

Chapter 6 on the religious policy of the Society, and the end of Part One. "Bacon's view of new sciences was down to earth", it begins. Before one stops to gasp she adds, "the facts of nature were the subject of his study. Yet the impulse behind it was essentially a religious one". Before one stops to congratulate Miss Purver on her perceptive notice of the religion of science, she adds, "and the Royal Society, as a body, followed his percepts on religion in its relation to science." I have now quoted the whole first paragraph of Miss Purver's chapter on religion. I can only say I am at an utter loss.

That Bacon wanted people to study facts and find natural laws is uncontestable. Does this make him "down-to-earth"? Miss Purver analyzes Bacon's utopia, *The New Atlantis,* in detail. In particular she notices that *New Atlantis* is Christian but religiously tolerant. But this sounds more pedestrian than visionary. She does not state clearly enough to my taste that there was a revelation particularly for the benefit of the inhabitants of *New Atlantis* (wich is isolated from the outside world,

though somehow it has all sorts of immigrants, including Jews), and she hardly presents the place in all its true colors. In *New Atlantis* the lay college which engages in research has the power to decide which of its inventions to make public, which to make state secrets, and which to withhold (as too dangerous) even from the state. The Oppenheimer case shows that technocracy has not yet developed to the height of Bacon's vision. Neither Einstein nor Bohr, nor President Pusey of Harvard, ever entered town in a procession the way the College President in *New Atlantis* did. Bacon even tells us he found it impossible to get a ticket for the stand, and he only got one through his Jewish host. The host even arranges for an audience and the president tells Bacon all about the college, including their statues of discoverers and inventors and including the prayers to God to assist them in their researches.

Miss Purver is right: Bacon's view of science as a mode of worship separated from established religion, as well as Bacon's notion of religious tolerance, were central to the Society, which had Catholic members and somewhat low-Church (not really) protestants. This, says Miss Purver, disproves the thesis of Merton that the Royal Society was an expression of protestant ethics (in Weber's sense of the word). And since protestant ethics is utilitarian, she adds, surely the Society did not accept this ethics.

The interested reader may find a summary of the literature on the topic in Richard L. Greaves' "Puritanism and Science" in the *Journal of the History of Ideas*, 1969. Here let me only add this. In Weber's sense protestant ethics represents the idea of the virtue of work, and this certainly is something which Bacon had preached. Also Weber assumes that protestant ethics is Calvin's invention, which may be true for the business world (though this has been questioned too), but is certainly not true for learned world where good works and ritual were parts of purification processes of the mystic scholar, as expressed in the cabbalist and alchemical literature and echoed in Comes and in Bacon. Boyle, expounding similar views in his *Seraphick Love* of 1659, ascribes its origins to Philo Judeus! Unless we make clear what is new in "protestant ethics", we can scarcely decide its influence on the "new philosophy".

Miss Purver quotes some details about the toleration of the Royal Society which had even led some of its opponents to view it as an instrument in the hand of the Catholics. The presence of this typical intolerant argument might be expected *a priori*, though how weighty it was considered, or how large was the intolerant group amongst the intellectuals is very hard to assess. Miss Purver does not raise the question, how significant her evidence is. Miss Purver mentions that in the House of Lords Wilkins openly criticized the King's attempt to pass an intolerant law. But this has almost nothing to do with our topic. Macaulay has noted that the political significance of the foundation of the Society

is that it kept some important minds off politics; now religion was at the time a major political item. There is little doubt that the King could have his cake and eat it, allowing the Society to fight for tolerance and impose tolerance on its members, not on its founder and patron. I think Miss Purver should have told her readers clearly that we have ample evidence against the notion that Charles II had any weakness for either enlightenment or toleration.

V

We now come to Part II, on the London group, pp. 161–234 in four chapters. First John Wallis's account of the origins of the Royal Society, second on Gresham College, third on Boyle's account on the Invisible College, and fourth on Hartlib and his pansophia.

Wallis. He reports that before Wilkins went to Oxford he belonged, with Wallis, in a London group which was interested in the new philosophy. Now, first of all, we are told, Wallis was a plagiarist and a hot-head. True, but unimpressive. Secondly, Wallis uses the term "New Philosophy" for ideas preceding those of the Baconian radicalism of the Royal Society. He considers Harvey's views on the circulation of the blood, which he had studied in Cambridge, as an example. Now, clearly, absorbing a new idea into the old system is a sin by any radicalist standard, of Bacon or of Miss Purver (p. 169); and Wallis' acceptance of his Cambridge teacher's non-radicalist practice is really bad: "it seems to indicate", she quietly chafes, "that he never did fully appreciate the state of affairs which John Wilkins and... his group at Oxford sought to remedy" (168–9).

This is a tough spot for a critic, and I wish I had the tact which Miss Purver ascribes to Wilkins and the Oxford group when she explains why they did not act in Oxford as good radicals should. But let this ride. Wallis' list of sins is not here exhausted: he calls "the New Philosophy" not Bacon's ideas, but those "which, from the time of Galileo... and .... Bacon hath been much cultivated... abroad, as well as in England." He gives as examples a list of topics discussed by the London group. This is no evidence that the group made new discoveries. Indeed, the same list had been presented in Glanvill's *Plus Ultra* as examples of individual contributions, in contrast to the Baconian collective projects of the Society.

Thus, Wallis' evidence, though acceptable as factual, is rejected as an interpretation. Indeed, Wallis reports that Theodor Haak, a foreign resident, initiated these meetings. As Harcourt Brown has suggested (*Scien-*

tific Organizations in the Seventeenth Century France, Baltimore, 1934), Haak was influenced by Father Mersenne, the founder of the Paris group, with whom he corresponded. Mersenne was to a large extent a Baconian, who made even Descartes express approval of Bacon's attitude towards experiments. Miss Purver, however, has no difficulty showing that he was first an Aristotelian of sorts, then a Cartesian of sorts. (There is no difficulty showing this of Galileo, Bacon, Boyle, etc. etc.) She even sees (p. 174) in Mersenne's suggestion to found an academy of science in France an attempt "to ensure what he considered to be the proper intellectual control of knowledge, and no doubt to offset Bacon's proposal of colleges on an international scale". Even if her quotation from Bacon were in agreement with her statement, even if Mersenne's (Cartesian) mechanical philosophy were not the one also endorsed by the Oxford group and the Royal Society, even then, Miss Purver's reading of nasty motives in Mersenne may make me withdraw the wish that I could be tactful.

Miss Purver is right in dividing Wallis' account into the factual and the interpretative; she is right in saying he later withdrew his own interpretation and said, the Society had originated in Oxford; she is right in saying, the official version says Oxford, not London. She is even right in saying the official version is not a small matter since it is the radicalist version. There is only one snag; radicalism is false.

Up till now Sprat was the authority, not Glanvill. The Society even recognized, we are told (p. 14), differences between the two, and endorsed only the former. Some of Miss Purver's evidence against Wallis is from Glanvill. Of course, the reason is that both Glanvil and Wallis mention the same list of discoveries—Wallis to prove that the London group was the original one, Glanvill as a mere admission that some pre-Baconian individual discoveries—of Copernicus, Galileo, Harvey, etc.—are quite important. Perhaps we have here some clash between Glanvil and Sprat who said, we remember, that the Society, as a group of Baconians, did more in six years than the rest of the world in six thousand! If so, Glanvill must yield to Sprat; by Miss Purver's own standards.

Miss Purver's interpretation is not very convincing. She should not follow Glanvill and say, as she does, the discoveries Wallis mentions are not new; she should follow Sprat and say, as the good radicalist she is, the discoveries are not important! If they are important, as Glanvill but not Sprat admits, then those who met to discuss them may be seen as some beginning of the Society. Radicalism permits no ancestry to radicalism, as Bacon declared, as many others did (see above p. 121). Miss Purver's study of the evidence is somewhat coloured by her radicalism. Take away her radicalism and Wallis' reading may sound much more congenial.

The next hypothesis Miss Purver refutes in the second chapter is that Gresham College had anything to do with the foundation of the Society. I really find it too tedious to go into details, where the all-or-nothing attitude of Miss Purver leads her to an ever easier victory. It is a real pity. For, though not a real college, Gresham could have developed into a secular college proper, indeed in accord with Bacon's wishes, and the wishes of almost all its founders.

I do not wish to quarrel with Boyle's insistence that the Company found a Society, not a college. His idea of amateur scientists was also inspired by Bacon, and made better sense to him as the basis of a disinterested activity. There is evidence that when Evelyn, Boyle, and Wilkins, called the founding meeting, at least Evelyn, and probably also Wilkins, wished for a college, but Boyle was adamant and only he could afford the founding of a college. It was no doubt his privilege to refuse and his alternative idea did prove useful. Yet the connexion with Gresham for about half a century is some indication of the retention of some vestige of hope to establish a college. The University of London was formed only in 1830, partly because the scientific societies played a significant intellectual role amongst those debarred from Oxbridge and partly because even in the 19th century Oxbridge was not very tolerant and debarred nonconformists, Jews, and agnostics, not to mention the poor. As to Miss Purver's details of the weakness of Gresham College, they are misleading: the other universities were terrible at that time, and showed less hope. The hope, finally, fizzled out; but it could have materialized even after the college's demise—just as the Society could have disintegrated but did not, after Boyle died and before Newton revived it.

The third chapter deals with Boyle's report, with his famous "invisible college"—this is his label; theirs was "philosophical college". Though it had been identified with Theodor Haak's group, it is clearly the group of Samuel Hartlib—another foreign resident of London. (This was first noted by Miss R. H. Syfret; see note on p. 200.) Miss Purver also argues from the fact that Hartlib and his group wanted a college proper; but so did Evelyn, Oldenburg, Petty, and others.

There is also the question, how distinct were the three groups. From all we know the overlaps were small; but then this may be due to a division perceived more than practised. And Boyle may have felt the need to view all groups as essentially one.

Also Boyle, in 1646, at the age of 19, says the invisible college is in principle utilitarian. This, says Miss Purver (p. 194), shows it belonged to Hartlib. But again I am uneasy. I do not think anyone was a utilitarian then, not even Boyle. A private letter of a 19 year, old, even a genius, is not exactly clinching evidence.

Also, Hartlib's college was supposed to preach "reformation of church

and state" (p. 201). Now this is not a utilitarian reformationism; even Miss Purver notices that much. The whole adventure was one muddled turmoil, where Copernican and Cartesian and atomic theories mixed with Baconian condemnation of all speculations; where looking backward to antiquity mixed with looking forward to new horizons; where extreme radicalism in philosophy mixed finally with Restauration moderation and toleration in politics and religion.

There is no need to go into much detail of the discussion in the next chapter on Hartlib, Comenius, and "pansophia". Of course Comenius admired Bacon most and was influenced by him both as a philosopher and as an educationist. Of course "pansophia" averts to the fact that Bacon had taken the whole of Nature as his province. Yet Miss Purver sees only "a superficial resemblance" (p. 210) between his aphorisms and Bacon's. This is a superficial impressionism, and of an apologetic brand. Of course, Comenius was also influenced by others, including one Johann Valentin Andreae, the inventor of the word "pansophia" who, too, was a Baconian, though even less than Comenius (p. 211). True, Andreae was much influenced by other utopians; so was Bacon, to be sure. To me, clearly, Bacon's dream of a technocratic society is part and parcel of contemporary utopianism and an offshoot of a remark of More, perhaps indirectly related to the dreams of Roger Bacon. Hartlib even saw both More's and Bacon's utopianism as a symptom of the period's (neo-neo-) Platonism (p. 218). Miss Purver, however, puts a wedge here: Plato in his *Atlantis*, More in his *Utopia*, etc., "saw his ideal society as an end in itself. Bacon, on the other hand, had a specific aim in his *New Atlantis*. Although his society was to have the spiritual and social virtues which he considered desirable, the actual purpose of his proposed institution was to build up a new system of natural sciences" (pp. 225–6). This hot air should read, More wanted mainly justice, Bacon wanted justice too but stressed efficient technocracy. For Miss Purver that sets him apart, for me that sets him well within, the group of utopians—though admittedly with the merit of an added variant which proved very ingenious indeed.

In the last pages of her book, though, Miss Purver offers a pleasant surprise, a hitherto unpublished letter from John Beale (Boyle's schoolmate mentioned above) to Samuel Hartlib, concerning a hitherto unpublished plan. The same Andreae who had influenced Comenius has also influenced a Swedish nobleman who developed a plan about a Royal Society which made Beale suggest that King Charles II should be the patron and founder of the Royal Society. (See p. 228 and p. 229 and notes there). And so, Andreae, Hartlib, and others, somehow managed to enter into the act. This should warm the heart of an anti-radical like me, but I am not so much at home in the Establishment either, and find the whole business of "Royal" in the Royal Society not over-exciting.

VI

In conclusion, Miss Purver has rendered us a service she has not intended to, and at least I am glad I have studied her book—though this is only a retrospective feeling. She did force me to reexamine the known documents, and she did impose on me an image of a radicalist group which knowingly suppressed their origins in earlier groups which had understood the term "the new philosophy" in a less radical sense; a group which was embarrassed by the fact that it had to pay homage to some thinkers of previous generations other than Bacon; a group which functioned as a group and with a radicalist ideology which justified just this new facet.

Yet science is not a group activity or a collective activity. Contrary to the Baconian ideology of the founders of the Royal Society, we still see the foundation of their Society as no more than a landmark: Miss Purver is quite right in the factual part of her complaint (p. 3 *et passim*), though I do not see that we need change our appraisal.

This being so, one may wonder how the Society could function and contribute so much to the advancement of learning. That it contributed to human welfare in general is neither problematic nor questionable: in addition to their Baconian toleration and drive for enlightenment in general, their stress on the mechanical and agricultural practices, from shipbuilding and gunpowder to milking and gardening, this had a lasting democratizing effect; and their anti-Baconian stress on natural knowledge in a period renowned for its witch-hunts is of supreme significance too. Even within the commonwealth of learning their influence in the arousal of interest and hopes, as well as their offering a platform for scientific encounter, publications, and the like, could not but be beneficial. Yet the Society did more as an instrument for the advancement of learning: contrary to its own ideology, it encouraged the development of hypotheses and controversies, and contributions of individual thinkers as individuals head and shoulder above their colleagues.

There it little doubt that the Society's ideology was somewhat tempered with common sense from the start. The only staunch anti-radicalist in the group was Robert Boyle. His *Seraphick Love* of 1659, which moved Evelyn to tears and sent him first to Boyle and then to Wilkins and thus to the foundation of the Society which soon became Royal, spoke of natural religion as encompassing experimental philosophy as a ritual and as sublimation of unrequited love. His "Proemial Essay" to *Certain Physiological Essays* does endorse a quasi-Baconian philosophy, but staunchly rejects all radicalism and all hostility to hypotheses (such as preached by writers from Bacon to Miss Purver). This "Proemial Essay" is well reflected in the constitution of the Society which was

proposed by Lord Brouncker, the light-weight first president of the Society, and seconded by Boyle.

The tradition of science still reflects a double-standard, a shopwindow image which is radicalist and neat and devoid of all problem, and a workshop image where all is in constant mess. In this century, for some time, men of science tried to break away from this tradition and expose the workshop to lay inspection; but old traditions die hard, and Miss Purver's volume is but an instance of this.

APPENDIX

The Beginning of the Royal Society, Oxford, 1960, by Margery Purver and E. J. Bowen, F. R. S., 16 pages, recounts Sprats story in brief, supplements it, adds some biographical data, and such. It is a publication devoid of any merit except that it announces Miss Purver's conclusions to the world. In view of this it is hardly surprising that, as Trevor-Roper complains, her conclusions were resisted before her evidence was heard. Why there should be no resistance to the conclusions when they are not argued for? But Trevor-Roper finds comfort in the fact that already in 1638 Wilkins said it is the fate of new truths to be derided by the ignorant and rejected by others who are perverse. This is nice: if I endorse your view it is because you are right, if I reject it, it is because new truths are resisted. Is it possible that some allegedly new ideas are resisted because they are old hat? The combination of scientific radicalism with Establishment social attitudes is one we may call *passé*. For my own part, I neither accept nor reject Miss Purver's solution, as I reject the presuppositions of her problem. Antecedents are never as clear-cut as to allow us to pose the question. Those who did pose it in the 17th century wanted a neat and true shop-window picture of the antecedents of the Royal Society. This is neither possible nor interesting.