PHILOSOPHERS AT WAR

THE QUARREL BETWEEN NEWTON AND LEIBNIZ

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INTRODUCTION

I BECAME interested in the theme of this book while editing Newton's correspondence during the years of his controversy with Leibniz and Leibniz's supporters. Although the outline of its story has often been told, the great richness of materials bearing upon it that has appeared during recent years made a more detailed study seem worthwhile, and more than one scholar has asked that it should be made. Moreover, a historian of today can approach the calculus dispute with a more detached perspective than his Victorian predecessors could do. He will not be shocked to discover that even Leibniz and Newton could display human faults. Again, the historian who (like myself) has no intention of investigating in technical detail the origins, development, and applications of calculus methods in mathematics can safely rely on modern work devoted to precisely these questions. Although he will not overlook his debt to the pioneers, notably C. I. Gerhardt, he must be particularly grateful for the interpretations and especially the documentation provided by J. E. Hofmann, H. W. Turnbull, and D. T. Whiteside, not to mention other equally reliable scholars who have examined the lesser mathematicians contemporary with Leibniz, James Gregory, and Newton.[1] What happened, mathematically speaking, in the 1660s and 1670s is no longer in doubt – as it certainly was to many a century ago and perhaps to some more recently still – and therefore consideration of the quarrel between the two great rivals need be clouded by no hesitation as to the actual historical facts upon which the quarrel turned. It was certainly Isaac Newton who first devised a new infinitesimal calculus and elaborated it into a widely extensible algorithm, whose potentialities he fully understood; of equal certainty, the differential and integral calculus, the fount of great developments flowing continuously from 1684 to the present day, was created independently by Gottfried Wilhelm Leibniz. Whatever we may feel of the relations between these two men, we can-

not but admire their analogous creative achievements with as much impartiality as our emotions will admit.

Although quarrels and rivalries between painters, poets, and musicians have at best been taken to promote artistic skills or at worst been treated as subjects for comedy, the altercations of the learned have in the past, at any rate, been regarded as so reprehensible that they should be dismissed in silence. It is not easy to see why this should have been, because a philologist or a positivist is no less human than an artist and certainly no less prone to embrace absurd hypotheses with enthusiasm. Learning and science do not necessarily improve a man's judgment or his character. At all events, it is clear that grave doctors have very, very frequently taken the easy path from disagreement to dispute. Newton's own colleague (and opponent) in the Royal Society, John Woodward, engaged in an unpremeditated public sword fight with another distinguished physician, Richard Mead; if the unusual incidents of Woodward's life stamp him as a stormy petrel,[2] consider the battle-scarred career of Richard Bentley, another friend of Newton's, Master of Trinity, one of the most learned and one of the most hated men of Newton's era. Or recall – all within the same living experience – the "Battle of the Books," wherein Sir William Temple and his vitriolic protégé, Jonathan Swift, defended the ancients against the pretensions of modern learning and science, a battle that, of course, led Swift (like many lesser scribblers) to satirize the Royal Society, of which Newton was president, in his *Tale of a Tub* and *Gulliver's Travels*. And though Newton took part in no public conflict with any one but Leibniz, the German philosopher fought (both directly and indirectly) in many scholarly skirmishes, and Newton's own life was not free from struggles behind the scenes. And if all this suggests, not unjustly, the intellectual violence of the age in which the Newton-Leibniz dispute was set, that picture is surely confirmed by the concomitant political and religious excesses even of temperate England, where dissenters were sentenced to the pillory and nonjurors to the Tower.

If folly, self-love, and aggression are by no means incompatible with the highest intellectual powers (and few historians nowadays, perhaps, would claim a total exemption from such vices of pedantry on behalf of Isaac Newton and G. W. Leibniz), one should not forget either that, despite polite contrary conventions, success in the scholarly or academic world depended far more on a militant combativeness then than it does now. For most it began

with the ability to put down opponents in university disputations and was confirmed by excellence of wit. The dull scholar, however learned, was not likely to get beyond a rural rectory. Patronage helped a few shy men to live productive scholarly lives in retirement, and some may see the secret of Newton's early success in Barrow's patronage, though it would be a misreading, in my view, to deny to the young man the toughness, energy, and determination so evident in the mature Newton; for most scholars, however, the lighting of their own brilliance required the dowsing of another's flame. In so tiny an intellectual world, where the highest rewards were so scarce (and often disposed of by those who appreciated an epigram better than a monograph), competition was inevitably unrelenting, and the more so for those, like Newton and Leibniz, endowed with no high social advantage in the first place. To put it crudely, an achievement in scholarship, science, mathematics, or medicine was a marketable commodity, a highly personal property: The recognition it conferred might be a first step toward attainment of a bishopric or an office of state. And the rules of the marketplace were both capricious and very different from those that now prevail. From the late nineteenth century, peer evaluation has been the rule of science and learning in the civilized world; and laymen have largely accepted the judgment of the internal experts. In the lifetimes of Newton and Leibniz what counted most was not the opinion of one's peers but the direct impression made upon princes and ministers, prelates and magnates, who exercised enormous personal powers of appointment.

Hence the competition, subtly weighted by all sorts of other considerations of family connection and personal character, was sharp between book and book, brain and brain, constituting (as Leibniz himself was to remark) almost a gladiatorial spectacle for the entertainment of the sophisticated. Philologists did not quite fight duels with Latin hexameters, as their successors were to try their skills on the Rosetta stone, but mathematicians fairly regularly battled over challenge problems, from the rivalry of Cardan and Tartaglia in the early sixteenth century through the celebrated cycloid puzzles of Pascal to the series of such duellos associated with the early development of Leibniz's calculus in the pages of the *Acta Eruditorum* of Leipzig. Peter Ramus's *Testamentum* (1576) had codified competition in the arrangements for the new chair of mathematics that he founded at the Collège Royale in Paris: Pro-

fessors were to be the winners of public competitions, and if any mathematician should challenge and defeat an incumbent professor, he was to be allowed to take his post. A similar spirit of naked, personal ascendancy permeated the Cambridge Mathematical Tripos until after the middle of the nineteenth century.

I do not mean to suggest that the quarrel between Newton and Leibniz was not shocking to contemporaries. Many clearly found it so. Charges of intellectual theft and personal dishonesty exchanged between two eminent graybeards, both quite close to the new Hanoverian crown of Britain, one the greatest of living philosophers (and hardly less a mathematician), the other the greatest living mathematician (whose philosophical views certainly commanded attention), could only reduce the dignity of learning. But I think the sheer egoism of the dispute, to us, perhaps, its most unpleasant characteristic, was less striking to contemporaries. In terms of the Augustan age, when matters came to a certain pass, it was right for a man to assert his intellectual property rights in a manner that would now be thought disgracefully self-assertive or self-regarding. Far from the development of scholarship, art, or science being a social phenomenon – of which an Einstein or a Picasso may be almost a passive vehicle – as some would have it now, any originality, any creative success, was judged to be the cause of uniquely personal merit, deserving personal reward, and therefore it was natural, rather than egotistical, to defend both merit and reward against rival claimants.

If the structure of society did not favor a sociological theory of success, its rather primitive psychological conceptions nevertheless strengthened an individualistic attitude toward achievement. For example, when ancients were compared with moderns, no one attempted to measure the literacy of Aristotle's Athens against that of Queen Anne's London; it was, rather, philosopher weighed against philosopher, physician against physician. If one man succeeded better than another it was because he had better natural endowments and a stronger character – though education (*formation*, as the French still significantly say) was not neglected as a factor, being, of course, the education of the individual child by an individual parent, not education as a social process. Our tendency to speak of a certain discovery or solution of a learned difficulty as "inevitable" was alien to Newton's era, still painfully conscious of the long noninevitability of progress, intellectual or material.

"I stood upon the shoulders of giants" wrote Newton in an oft-quoted phrase,[3] carrying the implication that the ability to see farther depended on one's ability to scramble to the top of the human pyramid created by our ancestors. Obviously Newton would have understood, as we do, that the scramble was open to all, but the intense individualism of his age prevented him, and his contemporaries generally, from understanding also the correlative, so obvious to us, that many scramblers, more or less successful, more or less sharp-sighted, must glimpse the same new prospect. No one yet spoke of "movements" or "schools," still less of "research programs," all concepts that link intellectual innovation with a sociological interpretation of the evolution of learning or art, and hence the idea that within a given context, and on the basis of a common past experience, the thoughts and experiments of several men must necessarily converge upon the same innovation did not present itself; and it was the less likely to do so when truly creative individuals were (in absolute, not relative, terms) very few and, consequently, disparate in their environment – one educated, let us say, by the Jesuits of La Flèche, another in Cambridge and the Inns of Court, a third in Presbyterian St. Andrews. The phenomenon of convergence, the independent solution by more than one individual of the same problem in identical or closely similar ways, is in historical fact extremely common in seventeenth-century science: Galileo Galilei, Thomas Harriot, and (very possibly) Simon Mayr all turned the newly popularized telescope to the heavens in the same year, 1609; John Napier and Jost Bürgi both invented the idea of calculating by the use of logarithms; Galileo and Christiaan Huygens independently (and successively) devised ways of regulating the mechanical clock by the oscillations of a pendulum; Marcello Malpighi and Jan Swammerdam began the microscopic exploration of the same insects at about the same time, and Malpighi and Nehemiah Grew independently took up the microscopic histology of plants. Such examples are almost innumerable, and it is well known that Newton's own work in mechanics converged closely with that of Huygens and of Robert Hooke, older contemporaries who published long before Newton. Because convergence occurred without its being recognized as a necessary phenomenon of discovery, priority squabbles like those studding Galileo's career were far from rare.

The fact that convergence went unrecognized as a necessary so-

cial consequence of an active "research program" – a necessary evil, perhaps, from the point of view of some researchers, like the almost forgotten V. Hensen, who effected the *second* isolation of glycogen from liver tissue – of course did not prevent some examples of it from being perceived. Newton, for example, admitted that Huygens had preceded him in the discovery of the laws of centrifugal force, and it was well known that Wren and Huygens had produced identical laws of collision. Convergence was still more evident in the experimental study of pneumatics during the late 1650s where, however, the individualism of the age is clearly manifest in the universal and eponymous linking of Robert Boyle's name with the fruition of this line of research: not that Boyle's fame was unmerited or that anything much is to be gained by renaming Boyle's Law the Towneley-Power-Hooke-Boyle Law. Because, rightly, it was believed (though perhaps not always as a result of a very judicious examination) that *one* statement only of a truth becoming manifest to several inquirers about the same time was complete and general and richly based on argument and evidence, it was taken not to be unjust to assign credit to this one superior enunciation. So Newton maintained the justice of his claiming the universal theory of gravitation for himself against the pretensions of Robert Hooke.[4]

These various factors – the great value attached to personal merit, the emphasis on innovation as the creation of an individual talent, and the absence of a sociological theory of the growth of knowledge, which are rightly regarded as of a social character – may well suffice to account for the frequency and bitterness of priority disputes in the past, especially when combined with the lack of formalized conventions about behavior in the learned world, conventions that only became settled (and enforced by ostracism) in the nineteenth century. Mathematics, because it readily defines "research fronts," because it offers the possibility of attaining results equally rigorously by different means, and because its logical character virtually necessitates the occurrence of convergence, was peculiarly likely to be troubled by quarrels and priority disputes, just as, at the opposite extreme, natural history was almost completely free of such disagreeable incidents. One might almost venture the generalization that the life of no major mathematician of the seventeenth century was wholly free of such wrangles, except that of John Napier, perhaps. Accordingly, though the dispute between Newton and Leibniz was grander, more dra-

matic, and more interesting than most, it was far from unprecedented and is merely (as a sociologist might say) indicative in a striking way of those faults in the "reward system" of the period, which were almost every day manifested in lesser quarrels by claim and counterclaim.

It is obvious in particular that the discovery of the methods of the differential and integral calculus was a natural occasion for strife. If we limit the formal honor of discovery to Newton first and Leibniz later, we have to admit that (at the least) very close approximations to discovery were made by Christiaan Huygens, James Gregory, Nicolas Fatio de Duillier, and probably others. Limited mastery of parts of the whole that was to be the calculus had been attained not only by these but by others still – René François de Sluse, Nikolaus Mercator, Isaac Barrow, and again others. The discovery of the calculus was more than a synthesis of previously distinct pieces of mathematical technique, but it was certainly this in part; interest in special cases later subsumed under the calculus – such as the general method of tangents and the quadrature of particular curvilinear areas – had lasted for a generation. We can now see, although this was quite obscure at the time, that what appeared in the 1650s and 1660s as a series of independent problems actually constituted, in fact, a single "research front," and that those who succeeded in making an advance in the solution of any one of these problems were converging upon the concepts of the calculus. Although there were areas of pure mathematics, like the projective geometry of Girard Desargues (himself, incidentally, a victim of charges of plagiarism by one Curabelle), that had no relation to the development of the calculus, one might guess that perhaps a half of all the mathematical activity of the first seventy years of the seventeenth century was more or less contributory to it. So much talent was devoted to this research front that, in relation to particular successes, duplication was commonplace, as with Sluse's and Newton's methods of tangents, Mercator's, Gregory's, and Newton's methods of quadrature by infinite series, the particular series for the circle obtained by Gregory, Newton, and Leibniz, and Newton's and Gregory's formulations of the binomial expansion. With so many men doing similar things successfully, it was not easy for any one mathematician to set his accomplishment apart from that of others. In Hofmann's words:

> Infinitesimal problems were being hotly pursued simultane-

> ously in France, Italy and England; the improved concept of indivisibles was being used as a guiding principle by Fermat, Pascal and Huygens equally as well as by Torricelli, Ricci, Angeli and Sluse . . . The characteristic triangle – to take up a particular point – was known already [before Leibniz] to Fermat, Torricelli, Huygens, Hudde, Heuraet, Wren, Neil, Wallis and Gregory long before it was made public by Barrow. Each of these predecessors had used it, *but nobody wanted to expose the jealously guarded secret by which he had found his results.*[5] [My italics]

Even today, reevaluation by historians of the achievements of these various mathematicians is occurring: James Gregory was not recognized for the powerful innovator he was before Turnbull's researches of the 1930s, and yet more recently, Isaac Barrow's reputation, once so high (at least among English speakers), seems to be descending toward the status of an elegant codifier. As Hofmann's expressions also indicate, competitiveness produced secrecy and envy, obstructing the open and cumulative development of new methods. Was it likely, therefore, that one man could stand out from all others as "discoverer of a new infinitesimal calculus" in that decade from 1660 to 1670 of enormously rapid progress on the basis of the foundations laid by Cavalieri, Descartes, Wallis, and so many more? Only if, like Boyle in pneumatics, he could produce a powerful and persuasive treatise.

Of this, I think, Newton may have been conscious. He put his new mathematical methods on paper clearly enough, but he printed nothing, only circulating a part of his work to friends. In 1668 he found himself (as he judged) partly anticipated by Nicolaus Mercator's *Logarithmotechnia.* Three years later, and one year after the writing of his 1671 treatise on fluxions, Newton received – with respect to his novel ideas about light and color – a severe lesson as to the trouble and annoyance springing from ill-considered and incomplete publication of his own work. He toyed at this time with the idea of an extensive mathematical book, but it made no progress and in the end was abandoned. If proper publication of one's work required such a vast effort, and hasty publication caused such disasters to one's time and temper, better give it up altogether.

We cannot, in fact, be confident that the printing of one or more of the tracts about fluxions and infinite series that Newton had composed before he put mathematics aside would have guaran-

teed him a swift and easy immortality as "discoverer of the calculus." Newton's friends then and later admired these tracts for their wonderful originality, and they have remained as the public basis of Newton's achievement in the calculus since the early eighteenth century. But who can say that, printed in 1673, they would not have raised up for Newton again those dust devils of incomprehension, misunderstanding, skepticism, and hostility that danced over the field of optics in the 1670s?

It is true that Leibniz in 1684, more than ten years after Newton had renounced pure mathematics for other studies, set his seal upon the differential calculus with only one short paper. But the situation in which that appeared was quite different from that of the early 1670s. Some of the mathematicans in whose shadow Newton had worked were (like Collins and Gregory) dead, and others were no longer interested. The threads so actively spinning and intertwining then had broken off short; Leibniz's own paper was the exposition of ideas he had formed and tested nine years before and then put on the shelf. In violent contrast to the turmoil aroused by Newton's optical paper of 1672, no one now in 1684 challenged Leibniz, or set his paper in its ten-year-old context, or indeed in this flat calm commented at all (for several years) on this contribution to a new and not well-known Leipzig periodical. Several years passed before commentators and expositors appeared, and then fortunately they were friendly and respectful. Newton, when so many voices were shouting against him already, had kept silent; Leibniz had the luck to speak when all else was quiet, to be heard, and to be marked. Hence these tears.