Introduction

During the last half of the twentieth century, there have been outstanding advances in Newtonian scholarship because we have had universal access to major portions of Newton's papers and manuscripts that were previously available only to a few scholars. Among the major sources that have been published are the variorum edition of the *Principia* (1971) and the editions of Newton's correspondence (1959–1977), mathematical papers (1967–1981), and optical papers (1984).[1] Several recent contributions to the understanding of Newton's scientific work, based in part upon these sources, were presented by scholars at the symposium held in 1997 at the Royal Society in London. This book contains these contributions.

In the opening address, the distinguished Newtonian scholar I. Bernard Cohen reflected upon the foundations of Newtonian scholarship in a lecture entitled "Newton in Historical Perspective." He began with the following observation:

> "It was not until Rupert and Marie Hall published their volume entitled *Unpublished Scientific Papers of Isaac Newton* in 1962, that the scholarly world at large, the world of historians of science and of scientists and mathematicians interested in historical questions, became aware of some of the extraordinary new insights that were to be found by examining the unpublished Newton materials."

Most of these sources are now available in the seven-volume edition of *The Correspondence of Isaac Newton*, for which Rupert Hall was the major editor, and the monumental eight-volume edition of *The Mathematical Papers of Isaac Newton*, which was edited by D.T. Whiteside. In the latter edition, Whiteside not only provided a translation into English from Newton's original Latin text, but he also added scholarly annotations containing historical commentaries and mathematical clarifications. Both Rupert Hall and D.T. Whiteside participated as commentators at the Royal Society Symposium.

It is well-known that in the *Principia* and the *Opticks*, Newton established the foundations of dynamics and some elements of optics. He also solved and attempted to solve a number of very difficult problems in these two fields. Some of these problems remained outstanding during the centuries following Newton's death and became the major subject of scientific studies. In addition to his theoretical work, Newton also carried out remarkable but little known experiments on the diffraction of light and on the resistance of fluids; these experiments remained unmatched in accuracy for centuries, and stimulated further investigations. The lectures in this book contain a discussion of some of these problems, as well as the resolution of well-known puzzles related to the early evolution of the *Principia*. Examination of Newton's papers and correspondence with regard to his approach towards the solution of specific physical and mathematical problems, as well as to his early studies of dynamics and optics in general, provides an understanding of how the structure of his great masterpieces, the *Principia* and the *Opticks*, eventually emerged. One of the important new insights is an appreciation of the extent to which the organization of Newton's two books was guided by the major physical problems that he studied.

In his lecture "Newton's Experimental Investigation of Diffraction for the Opticks," Alan Shapiro described Newton's experiments on the diffraction of light from "slender obstacles", and his early attempts to explain the observations by his corpuscular theory of light. A hundred years after the first publication of the *Opticks*, Thomas Young made the following remark:[2]

> "The optical observations of Newton are yet unrivalled; and excepting some casual inaccuracies they only rise in our estimation as we compare them with later attempts to improve on them."

Some of Newton's unpublished optical papers[3] reveal the increasing care with which Newton carried out measurements of the position of diffraction fringes which led him to revise his optical models for diffraction. The remarkable accuracy of these measurements, which will be demonstrated here by a direct comparison of Newton's data with the modern theory of diffraction due to Fresnel,[4] led Newton to doubt his corpuscular models, and to resort ultimately to a set of Queries about the nature of light in the final section of his *Opticks*. In his review at the end of the symposium, Rupert Hall made the following comment:

> "Taken into the private laboratory, as it were, under Alan's guidance one meets a Newton who seems a good deal less dogmatic and confident than was the author of the *Opticks*, a book in which he had to face the public. This still uncertain Newton, seeking his way through a problem, who was exploring rather than legislating, choosing between multiple possibilities both mathematical and experimental that presented themselves to him, had to foresee and test his path leading towards the results he hoped to achieve."

In his lecture, "Fluid Resistance: Why Did Newton Change His Mind?" George Smith discussed the evolution of Newton's theoretical ideas and experiments on the motion of bodies in fluids like air and water. In 1926, a fellow of the Royal Society, R.G. Lunnon, repeated Newton's experiments on the resistance of fluids and made the following observation:[5]

> Newton's results are of much more than historical value, for two reasons. They are obtained from careful experiments which have never been repeated, and they contain a special value of the resistance constant which is a remarkably good one, well within the range of modern determinations.

In the second edition of the *Principia*, Newton made radical changes to the presentation given in the first edition, but he did not reveal the reason why he undertook these changes. As Smith shows, Newton undertook new experimental investigations on the resistance of fluids because of criticisms from his protege, Fatio de Duillier, but "sorting out what went on between the first and second editions of Book II would have been hopeless without Newton's correspondence ..." Comparing Newton's measurements of the resistance of fluids with modern results shows that they were remarkably accurate, but again, as in the case of the diffraction of light, Newton was unable to develop an adequate theory for his observation. Indeed in 1752, d'Alembert proved the paradoxical result that a Newtonian inviscid fluid does not provide any resistance at all, while an adequate explanation for the resistance of fluids was not given until 1904 by L. Prandtl. As Smith pointed out, the "resistance forces result from intertwined inertial and viscous actions ... a full account of this intertwining remains one of the unsolved problems of physics."

In his lecture, "Newton's Dynamics: The Diagram as a Diagnostic Device," Bruce Brackenridge takes up the role of curvature in Newton's early dynamics[6] as revealed in Newton's diagrams. In addition to the analysis of the diagram for Lemma 11 of Book I, the curvature lemma, particular attention is paid to the the recent analysis by Michael Nauenberg of a diagram in the 1679 correspondence between Newton and Hooke.[7] It may well be asked as to what can possibly be new to discuss in a topic as well studied as Newton's dynamics. As recently as 1991, Whiteside responded to that question as follows:[8]

> "Surely there can be nothing profoundly new to be said about its (the *Principia's*) progress from first conception as an inchoate idea in it's author's mind to the maturity of its first publication in 1687? No and yes. Anyone not of the fraternity, however, would surely be surprised to

> see how much Newton scholars can still at times find to disagree upon in assessing what is now in itself known in such abundance, sometimes even at the most basic level of dating a manuscript."

In particular, Whiteside called attention to a "cryptic remark" made by Newton in 1664 to obtain the force acting on a body moving on an elliptic orbit.

> "If the body b moved in an Ellipsis, then its force in each point (if its motion in that point be given) may be found by a tangent circle of equal crookedness with that point of the Ellipsis."

As if to respond to the "no and yes" in his previous statement, Whiteside then took the following position concerning Newton's "cryptic remark":

> "Not only, however, was this remark original, but Newton's path to dynamical discovery might have been very different had he pursued it. In a Waste Book entry dated December 1664 he had already roughed out a method for constructing the centre of curvature, and so the 'quantity of crookedness' inverse to it, in an ellipse. (MP v.1 252–255) Six years later to jump ahead, he would in Problem 5 of his 1671 fluxion treatise derive the elegant result that the radius ρ of curvature at any point on a conic is proportional to the cube of the normal at the point down to the axis ... But I talk of a deduction that Newton never made till the 1690s."

Contrary to Whiteside's claim that Newton never made such a deduction until 1690, however, there is now considerable evidence based on Nauenberg's analysis of Newton's 1679–1680 correspondence with Robert Hooke, and on early manuscripts of the *Principia*, that Newton did pursue this "path to dynamical discovery" much earlier than scholars had previously thought.[7,9] Central to this path is the mathematical concept of curvature that Newton developed from 1664 to 1671 (and that Christiaan Huygens had developed somewhat earlier). Specifically, Newton used elements of the circle of curvature to represent elements of the curve generated by a given force.[10]

This curvature analysis of motion also clarifies the seminal role which Hooke played in the development of Newton's ideas on dynamics. In his early curvature approach to dynamics, Newton resolved a continuous central force acting on a body into tangential and *normal* components; the former is responsible for the changing magnitude of the velocity along the orbit, and the latter gives rise to the curvature or bending of the orbit. Hooke suggested, however, that the motion be resolved into tangential and *radial* components, and that the force be *impulsive* rather than continuous. Newton implemented Hooke's suggestion mathematically with the central force represented by a periodic sequence of impulses, and the motion resolved into tangential and radial components; the former is given by the tangential velocity, and the latter is due to the change in velocity "impressed" by the impulse. As a consequence, Newton shortly afterwards found the origin of Kepler's law of areas, which previously had been hidden in his curvature approach. Thus, he was able to implement the purely geometrical approach to orbital dynamics that is developed in the *Principia*, in which time is measured by the change of area swept out by the radial vector.

In his lecture "From Kepler to Newton: Telling the Tale," Curtis Wilson recounted that Newton regarded Kepler's laws as empirical rules, which Kepler had only "guessed" but which he, Newton, had demonstrated as a consequence of the fundamental laws of nature. Wilson described how Kepler derived his rules from Tycho Brahe's planetary observations, and how these rules were thought to be only empirical, even by Voltaire. Newton realized that these rules are strictly valid only for the ideal orbital motion about a fixed center of force, and that they are only approximate for the case of planetary motion around a free central body with mutual gravitational interactions among a number of planets. In 1684, he wrote the following in the *De Motu*, the first draft of a manuscript which later became the *Principia*:

> "It may happen that the centripetal force does not always tend towards that immobile center (the sun), and thence that the planets neither revolve exactly in ellipses nor revolve twice in the same orbit. Each time a planet revolves it traces a fresh orbit as happens also with the motion of the Moon, and each orbit is dependent upon the combined motions of all the planets, not to mention their action upon each other."

On the continuing controversy concerning the nature of Newton's Moon test for the inverse square law of gravity in the 1660s, Wilson reported that there is clear evidence in a manuscript, which was first published by Rupert Hall and dated by him before 1669, that Newton did make the test. However, some of Newton's central arguments for the universal law of gravitation (i.e. that the gravitational force acts between all matter and is proportional to the masses of the interacting bodies) became clear to him only sometime during 1685, while he was in the midst of writing the *Principia*.

John Fauvel elucidated the development and impact of Newton's calculus in his lecture on "Newton's Mathematical Language." Quoting John Maynard

Keynes, who thought that "The proofs (in the *Principia*), for what they are worth, were, as I have said, dressed up afterwards — they were not the instrument of discovery," Fauvel raised the thorny question of whether there is a gap between Newton's private mathematical language and his exposition in the *Principia*. Much light has been shed on this issue by the availability of Newton's mathematical papers. These papers reveal, as Whiteside puts it, "... that he (Newton) made mistakes, that he learned from them and that with unwearying application he steadily enlarged his grasp as he constructed the mature fluxional calculus." Newton also gave a careful analysis of the transition between natural everyday language and mathematical language, and emphasized the importance of introducing mathematical definitions carefully. Inadequate attention to this point has led to unnecessary confusion among Newtonian scholars.[11] Fauvel pointed out the little known fact that Newton first discovered infinite power series expansion of certain algebraic expressions by transforming age old arithmetic rules into an algebraic language:[12]

> "I am amazed that it has occurred to no one (if you except N. Mercator with his quadrature of the hyperbola) to fit the doctrine recently established for decimal numbers in similar fashion to variables, especially since the way is then open to more striking consequences. For since this doctrine in species has the same relationship to Algebra that the doctrine in decimal numbers has to common Arithmetic, its operations of Addition, Subtraction, Multiplication, Division and Root-Extraction may easily be learned from the latter's provided the reader be skilled in each, both Arithmetic and Algebra."

One of the most difficult questions that has challenged historians of science has been Newton's application of his perturbation methods celestial dynamics to the motion of the moon. In an article entitled "From High Hope to Disenchantment," Whiteside[13] concluded that Newton's deduction of the lunar inequalities "was a retrogressive step back to an earlier kinematic tradition which he had once hoped to transcend." However, as Michael Nauenberg showed in his contribution to the symposium, "Newton's Portsmouth Method and its Application to Lunar Theory," it is misleading to see Newton's lunar theory as a failure. In fact, Newton's methods gave remarkably successful approximate solutions to the notoriously difficult three-body problem. By these methods, he calculated the inequalities of the lunar motion (some known from antiquity) that are due to the gravitational perturbation of the Sun. Even the most striking shortcoming, the treatment of the rotation of the line of apsis as it appears in the *Principia*, is discussed by Newton in a remarkably profound manner in one of his previously unpublished manuscripts, which was edited by Whiteside.[14] Apparently, Newton was dissatisfied with his results; they never appeared in any of the three editions of the *Principia*.

In his concluding comments Rupert Hall aptly summed up the challenge and response of the symposium:

> "It is most illuminating that recent studies have begun to penetrate beneath the polished, admantine surface of Newton's great printed works to the foundations of his public formulations, to reveal the possible alternative arguments or positions that Newton considered and rejected. We now know that *Principia* and *Opticks* did not spring like Minerva from the head of Jove: they are a palimpsest of investigation and tentative endeavors."

Notes and References

1. Following Newton's death in 1727, the collection was secured by John Conduitt, passed into the possession of the Portsmouth family in 1740, and for over two centuries was virtually inaccessible to scholars save for a few determined individuals such as David Brewster and W.W. Rouse Ball. In 1872, the fifth Earl of Portsmouth, Isaac Newton Wallop, transferred to Cambridge University the portion of the collection judged to be concerned with scientific and mathematical topics. In 1936, essentially all the remaining papers were sold by Sotheby at public auction. The bulk of the theological manuscripts now reside in the Jewish National and University Library in Jerusalem, a few other lots of papers reside in private hands, but the vast majority are available to scholars in England who have the time and resources to examine them under the careful eye of university librarians. A new dimension to Newtonian scholarship was added, however, with the publication of *Isaac Newton's Philosophiae Naturalis Principia* (3rd ed. (1726) with variant readings (1972, eds. I.B. Cohen and A. Koyre, assisted by A. Whitman), *The Correspondence of Isaac Newton* (1959–1977, seven volumes, eds. Hall, Scott, Tilling, Turnbull), *The Mathematical Papers of Isaac Newton* (1967–1981, eight volumes, ed. D.T. Whiteside) and *The Optical Papers of Isaac Newton* (1984, Vol. 1, ed. A. Shapiro and Vol. 2 now being prepared by Shapiro).
2. Thomas Young, "The Bakerian Lecture: On the theory of light and colours," *Philosophical Transactions* **92** (1802): 12–48.

3. "The optical papers of Isaac Newton," in *The Optical Lectures* 1670–1672 (ed.) Alan Shapiro (Cambridge University Press, 1984), Vol. 1. Shapiro is currently editing Vol. 2.
4. M. Nauenberg, *Comparison of Newton's diffraction experiments with Fresnel's theory*, in this volume.
5. R.G. Lunnon, "Fluid resistance to moving spheres," *Proceedings of the Royal Society of London*, Series A, Vol. 110, 302–326 (1926).
6. J. Bruce Brackenridge, "The critical role of curvature in Newton's dynamics." in *An Investigation of Difficult Things: Essays on Newton and the History of the Exact Sciences* (eds.) P.M. Harman and Alan E. Shapiro (Cambridge University Press, 1992).
7. M. Nauenberg, "Newton's early computational method for dynamics," *Archive for History of Exact Sciences* **46** (1994): 221–252.
8. D.T. Whiteside, "The prehistory of the *Principia*: from 1664 to 1686," *Notes and Records of the Royal Society of London* **45** (1991): 11–61.
9. J. Bruce Brackenridge *The Key to Newton's Dynamics: The Kepler Problem and the Principia* (University of California Press, 1995).
10. Following Brackenridge's paper, Whiteside rose to express his continued opposition to the curvature analysis of Newton's 1679 diagram. Unfortunately, he has decided not to present these remarks in this volume, while stating his opinion that alternate interpretations are still possible.
11. An example is Newton's apparent change over time of the *meaning* of the term "centrifugal" force, although he was always consistent in his *mathematical application* of it to dynamical problems.
12. *The Mathematical Papers of Isaac Newton*, 1670–1673 (ed.) D.T. Whiteside (Cambridge University Press, 1969) Vol. 3, pp. 33–35.
13. D.T. Whiteside, "Newton's lunar theory: from high hope to disenchantment," *Vistas in Astronomy* **19** (1976): 317–328.
14. *The Mathematical Papers of Isaac Newton*, 1684–1691 (ed.) D.T. Whiteside (Cambridge University Press), Vol. 6, pp. 508–535.