Newton versus Leibniz: Geometry versus Analysis

Stephen Gaukroger

The origins of Newton's *Principia* can be traced back to November 1679, when Hooke wrote to him proposing an account of planetary orbits in which an orbit is the result of a rectilinear motion at a tangent to the orbit and an attractive force towards the sun.[1] Hooke had been toying with this idea from as early as 1666, but he encountered an insuperable mathematical obstacle: to work out the nature of the orbit required him to balance changing velocities and changing distances. Hooke had no idea how to do this, and neither did Wren nor Halley, who tried to help him solve the problem. More generally, what he did not know was how to determine the relationship between rates of change of continuously varying quantities. Newton was probably the only person in England with the mathematical resources to deal with such a problem,[2] and, prompted by Hooke's suggestion, these enabled him to pose and resolve a question that was central to the *Principia*, namely the 'Kepler problem' of determining the force required to maintain elliptical motion around a focal force centre. A fundamental question underlying the success of the Newtonian dynamical system set out in the *Principia* was that of the mathematical resources that it deployed, and the issues hinge on the use of the calculus. It was also on this question that the eventual dissatisfaction with the *Principia* was focussed.

Three sets of questions are entangled in the mathematical issues here: Newtonian versus Leibnizian notations, analytical versus

---

1 Hooke to Newton, 24 November 1679: Newton, *Correspondence*, i. 297. See Gaukroger, *Emergence*, 430-40.

2 Both Newton and Leibniz, in their development of the calculus, were indebted to the mathematical work of Isaac Barrow, who died in 1677, but Barrow himself does not take the crucial steps: see Michael S. Mahoney, 'Barrow's Mathematics: Between Ancients and Moderns', in Mordechai Feingold, ed., *Before Newton: The Life and Times of Isaac Barrow* (Cambridge, 1990), 179-249.

synthetic methods, and limit procedures versus differential equations. When, in the course of the 1740s, dissatisfaction with the *Principia* was such that it began to be significantly revised, reworked, and rewritten, the dissatisfaction sprang from a perceived need to replace the synthetic mode of working through problems and the use of limit procedures with an analytic way of proceeding which used differential and integral calculus, employing the Leibnizian notation.[3] The problems are compounded by the fact that much of the bitter dispute over whether Newton or Leibniz was the originator of calculus turned on the obscurity of the mathematical methods that Newton had employed in the 1660s and 1670s.[4] Identifying the procedures used by Leibniz is an easier task than identifying those used by Newton, and the Leibnizian programme was that which was followed in the most significant developments in eighteenth-century mechanics. I shall therefore begin with Leibniz and the development of Leibnizian calculus, and then deal with Newton only with regard to those respects in which he differs from Leibniz significantly.

With his 1684 paper, 'Nova methodus pro maximis et minimis', Leibniz was the first to deal explicitly with the rules of calculus, raising a profound and long-standing problem about the requirements of mathematical demonstration, a problem that turned on the question of whether a mathematical proof should just secure the conclusion on the basis of the premisses, or whether it should also reveal to us how the conclusion is generated. Descartes, for example, in his mathematical work of the 1620s, had extolled the virtues of

---

3 See Clifford Truesdell, 'A Program Towards Rediscovering the Rational Mechanics of the Age of Reason', *Archive for History of Exact Sciences* 1 (1960), 3-36. Newton himself was aware that his geometrical presentation might be taken as outdated as early as 1710, writing that 'To the mathematicians of the present century, however, versed almost wholly in algebra as they are, this synthetic style of writing is less pleasing, whether because it may seem too prolix and too akin to the method of the ancients, or because it is less revealing of the manner of discovery.' *The Mathematical Papers*, viii. 451. On the attitude to continental developments in calculus in Britain see Niccolò Guicciardini, *The Development of Newtonian Calculus in Britain* (Cambridge, 1989).

analysis over geometry[5] on the grounds of the transparency of algebraic proofs, which revealed the path by which the conclusion was generated, by contrast with geometrical proofs, which were generally obliged to follow a very indirect path, one which took us through all kinds of auxiliary constructions needed for the demonstration of the result.[6] Once infinitary mathematics was introduced into the picture, matters became significantly more complex, because infinitesmal quantities did not behave like ordinary arithmetical quantities, and their treatment in terms of ordinary arithmetical procedures generated paradoxes.

The issues go back to ancient mathematics. The Greek and Alexandrian mathematicians had employed various heuristic devices in their attempts to deal with mathematical problems, but had always sought to present their results in formal terms. In the course of the sixteenth and seventeenth centuries, some attention began to be paid to trying to reconcile heuristic and formal methods, and various forms of demonstration had been singled out as problematic. The formal geometrical legitimacy of proof by superposition—where the equality of two geometrical figures is shown by imagining one placed one over the other so that their boundaries exactly coincide—was questioned for example by Peletier in his 1557 edition of Euclid.[7] And (more plausibly) proofs by contradiction, where we assume the opposite of the proposition to be demonstrated and show that it leads to a contradiction, also began to be questioned. Closely connected with the

---

4 See the exemplary treatment of these questions in Niccolò Guicciardini, *Reading the Principia: The Debate on Newton's Mathematical Methods for Natural Philosophy from 1687-1736* (Cambridge, 1999).

5 In the seventeenth and eighteenth centuries, the mathematical procedure of analysis was taken to be the resolution of mathematical problems by reducing them to equations.

6 See Stephen Gaukroger, *Descartes, An Intellectual Biography* (Oxford, 1995), 172-181.

7 Jacques Peletier, *In Euclidis Elementa Geometrica Demonstrationum Libri XV* (Basel, 1557), note appended to Proposition I.4. Hobbes was later to ask how on earth we could establish equality if not by superposition, which does seem a particularly compelling form of demonstration. On Peletier's conception of geometry see Giovanna Cifoletti, 'From Valla to Vièta: The Rhetorical Reform of Logic and its Use in the Early Modern Logic', *Early Science and Medicine* 11 (2006), 390-423: 398-410.

latter was the exhaustion method, which involves a double *reductio*: unable to prove that A=B directly, we prove that A cannot be less than B and that A cannot be greater than B.[8] The usefulness of this way of proceeding was not in doubt, but many mathematicians believed it did not have the formal rigour or perspicacity required of formal geometrical proof.

The problem was how one reconciled powerful heuristic techniques with the need for formal and genuinely revelatory demonstrations. It was common ground among seventeenth-century mathematicians that the ancient geometers had employed heuristic methods to discover various theorems which were quite different from the synthetic demonstrations by which they proved them. The exhaustion method is a case in point. In Proposition 8 of Book XII of Euclid's *Elements* there is a straightforward proof that a prism with a triangular base can be divided into three equal pyramids with triangular bases, from which it follows directly that any pyramid is a third part of the prism which has the same base as it and is of equal height. But when, in Proposition 10, Euclid turns to what is in effect the parallel problem of showing that any cone is a third part of the cylinder which has the same base as it and is of equal height, the fact we are dealing with curvilinear figures makes direct proof impossible, so we are supplied with an exhaustion proof, that is, a double *reductio*. Cavalieri, in his pioneering work on infinitary mathematics of the 1620s and 1630s, culminating in his *Geometria indivisibilibus* (1635),[9] sought a method of demonstration of the cone/cylinder theorem which, unlike the method of exhaustion, provided a direct proof, one in which we can understand how the theorem emerges from the premisses, rather than just being shown that the alternatives cannot hold. One could be reasonably certain that ancient mathematicians themselves had not come by the demonstration

8 On the use of exhaustion methods in seventeenth-century mathematics see D. T. Whiteside, 'Patterns of Mathematical Thought in the Later Seventeenth Century', *Archive for History of Exact Sciences* 1 (1960), 179-388: 331-48.

simply by testing every possibility (the cone is half a cylinder, a third, a quarter, and so on) against the method of exhaustion. Moreover, the procedure by which it was discovered was presumably direct, not a question of systematically dismissing alternatives. Cavalieri's solution was to construct a geometry of indivisibles in order to overcome the perceived difficulties with the method of exhaustion, in particular the formal inadequacies of proof by contradiction. Among the procedures he used, the most powerful consisted of summing infinite aggregates of lines to measure areas, and infinite aggregates of planes to measure volumes. This is the procedure he employed in the demonstration that the volume of a cone is a third of that of a cylinder of the same height with which it shares a base. The method of summing was genuinely revelatory in a way that exhuastion procedures are not. In particular, Cavalieri was able to show why it is that a right triangle whose sides are the base and height of a square and whose hypotenuse is the diagonal of the square stand in the ratio of 1 to 2, yet when we rotate the triangle and the square around the height of the square, the cone and cylinder generated stand in the ratio 1 to 3.[10]

The trouble was that the procedure also generated paradoxes. An infinitesmal quantity α behaves in a peculiar way in that $A+\alpha = A$, contrary to the addition of finite quantities, where, assuming positive numbers, $A+a > A$. Galileo rejected Cavalieri's method using a *reductio* in which a Cavalieri-type indivisible demonstration is used to prove that the area of a cone and a bowl have equal areas, but where, in the limiting case of the last 'indivisibles', it turns out that the circumference of a circle, which contains infinitely many points,

---

9 Bonaventura Cavalieri, *Geometria indivisibilibus continuorum nova quadam ratione promota* (Bologna, 1635).

10 See Paolo Mancosu, *Philosophy of Mathematics and Mathematical Practice in the Seventeenth Century* (New York, 1996), 39-44, to which I am indebted here. More generally, see Kirsti Andersen, 'Cavalieri's Method of Indivisibles,' *Archive for History of Exact Sciences* 31 (1985), 291-367.

equals a single point.[11] The case is one where B = A+α_1 and B = A+α_2 but where $\alpha_1 \neq \alpha_2$.

The ability of Cavalieri's procedure to generate paradoxes was more than matched, however, by its heuristic power, developed in a number of new areas by his younger contemporary Torricelli. The most brilliant and striking extension of the procedure was Torricelli's 1641 demonstration, using the method of indivisibles, that an acute hyperbolic solid of infinite length had the same volume as a cylinder of finite length.[12] That a figure of infinite length could be shown to have a finite volume was puzzling, for it was generally assumed, *contra* Cavalieri, that one could not establish ratios that involved infinite quantities. Yet what Torricelli had done was to establish a ratio between a finite quantity and an infinite one. Moreover, he was able to supplement his analytic demonstration using indivisibles with a synthetic one, although he made it clear which procedure had done the real work:

> As for the method of demonstration, we shall prove a simple notable theorem in two ways, namely, with indivisibles and in the manner of the ancients. And this although, to tell the truth, it has been discovered with the geometry of indivisibles, which is a truly scientific method of demonstration which is direct and, so to say, natural. I feel pity for the ancient geometry which, not knowing or not allowing Indivisibles, discovered so few truths in the study of the measure of solids that a frightening paucity of ideas has continued until our times.[13]

Torricelli's demonstration opened up an intense discussion of the nature of indivisibles, a discussion which entered a new phase with the 1684 publication of Leibniz's exposition of the foundations of differential calculus in his 'Nova methodus',[14] and a companion piece

---

11 Galileo Galilei. *Two New Sciences: Including Centers of Gravity and Force of Percussion*, trans. S. Drake (Madison, 1974), 36-7.

12 Evangelista Torricelli, 'De Solido hyperbolico acuto' in *Opera Geometrica* (Florence, 1644). See Dominique Descotes, 'Espaces infinis égaux au fini', in A. Montandon, ed., *Le Grand et le Petit* (Clermont-Ferrand, 1990), 41-67.

13 Toricelli, 'De solido', translation (emended) from Mancosu, *Philosophy of Mathematics*, 131.

14 Leibniz, *Mathematischen Schriften*, ed. C. I Gerhardt (7 vols, Berlin and Halle, 1849-30), v. 220-6.

on the foundations of integral calculus, 'De geometria recondita et analysi indivisibilium atque infinitorum',[15] in 1686.

Leibniz's interest in mathematics dates from the mid-1660s, although his innovations began only with his stay in Paris in 1672, where Huygens offered him guidance on how to develop and refine his rudimentary mathematical skills.[16] His interest at that time was in numerical series, for example in the demonstration that the sum of consecutive odd numbers can be expressed as the difference between two squares. As he tells us in his 1714 autobiographical note (written in the third person), 'the application of numerical truths to geometry, and the study of infinite series, was at that time unknown to our young friend, and he was content with the satisfaction of having observed such things in series of numbers.'[17] Huygens set him the problem of finding the sum of the reciprocals of triangular numbers: $^1/_1 + {}^1/_3 + {}^1/_6 + {}^1/_{10} + {}^1/_{15} \ldots$ Writing each term as the sum of two fractions, he was able to show that the sum of the first t terms is:

$$\frac{2}{t} - \frac{2}{t+1}$$

so that in the case of an infinite sum up to n we have:

$$\sum_{t=1}^{n} \frac{2}{t(t-1)} = 2 - \frac{2}{n+1}$$

As n increases, $2/(n+1)$ becomes infinitely small or null, and hence the sum of the infinite series is 2. Examining other types of convergent and divergent infinite sequences, he developed the basis of a powerful and general method of summing.

On Huygens' advice, he also turned his attention to geometry, notably to Pascal's 1659 'Traité des sinus du quart de cercle'.[18] Traditional limit procedures had enabled one to determine tangents,

---

15 Ibid, 226-33.

16 Ibid, 404. See Aiton, *Leibniz*, 41-66.

17 Leibniz, *Math. Schriften*, v. 398.

18 Blaise Pascal, *Oeuvres Complètes*, ed. Henri Gouhier and Louis Lafuma (Paris, 1963), 155-8.

for example, by the use of chords of decreasing size. If we take two points, P and Q, on the circumference of a circle and join them then we determine a unique line, the chord PQ (Fig. 1). But there is no such unique line passing through a single point—there are infinitely many straight lines that pass through P alone—and the problem is to determine the unique one that is the tangent, i.e. the one that is at a right angle to the centre of

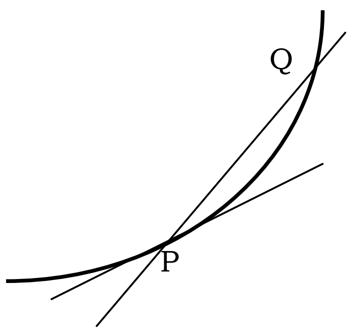
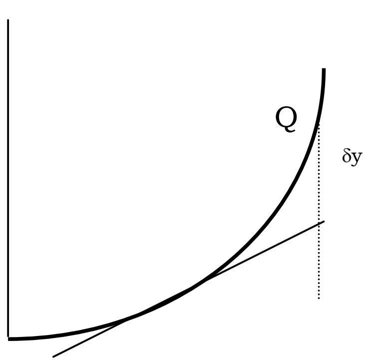


Fig. 1

the circle, or to the focus of a conic section. As Q approaches P, the chord PQ will provide a better and better approximation to the tangent at P. They cannot coincide, since then we would be back with the problem of determining a unique line from a single point, but if we make the distance infinitesmally small, we will generate the tangent from two points. Pascal associated with a point on the circumference of a circle a triangle with infinitesmal sides, and translating this idea into Cartesian co-ordinate geometry, as Leibniz now did (Fig. 2), he realised that the procedure could be applied to any curve, and proceeded to build up curves out of a polygonal figure having infinitely many sides. In short, he discovered that finding tangents to curves depended on the differences in the



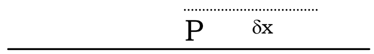


Fig 2

ordinates and abscissae, as these differences became infinitely small. At the same time, he realized that the problem of areas or quadratures was simply the inverse of this, so that the areas depended on the sum of the ordinates or infinitely thin rectangles making up the area. In other words, the techniques for dealing with the first question, which turned on the relationship between rates of change of continuously varying quantities, for which Leibniz provided rules for a differential calculus, were mirrored in an inverse set of techniques for dealing with the second, namely integral calculus.

QuickTime™ and a
TIFF (LZW) decompressor
are needed to see this picture.

Fig. 3

These techniques take the form of simple rules, offered without demonstration, in the 'Nova methodus'. They are introduced geometrically: consider Fig. 3, with the axis AX, and the curves VV, WW, YY, ZZ, whose ordinates, perpendicular to the axis, namely VX, WX, YX, and ZX, are labelled v, w, y, and z respectively, and the segment AX taken on the axis is labelled x. Tangents VB, WC, YD, and ZE are drawn to the curves, cutting the axis AX at B, C, D, and E respectively. We let dx be a segment of an arbitrarily chosen line, and dv, which Leibniz defines as the 'differentia' of v, be a segment which is to dx as v is to XB. The rules of calculus are then set out:

> Let a be a given constant, da be equal to o, and $d\overline{ax}$ be equal to adx. If y is equal to v (i.e. every ordinate of the curve YY is equal to the corresponding ordinate of the curve VV), then dy will be equal to dv. Now *Addition and Subtraction*: if $z - y + w + x$ is equal to v, then $d\overline{z - y + w + x}$ or dv will equal $dz - dy + dw + dx$. *Multiplication*: $d\overline{xv}$ is equal to $xdv + vdx$, i.e. in making y equal to xv, one makes dy equal to $xdv + vdx$, for we may use the expression y as shorthand for xv. Note that in this calculus, x and dx are treated in the same way, as are y and dy, or any other variable letter and its differential. Note also that the inverse process, starting from the differential equation, is not always possible unless we take certain precautions which we mention elsewhere. Finally, *Division*: $d\frac{v}{y}$ or (making z equal to $\frac{v}{y}$) dz is equal to $\frac{\pm vdy \mp ydv}{yy}$.[19]

Having set out some rules for the manipulation of signs, depending on whether the ordinates increase or decrease, Leibniz moves to the behaviour of curves, leading him to introduce second-order differentials:

> if, when the ordinates v increase, the same occurs with the increments or differentiae dv (i.e. if we take dv to be positive then ddv, the differentiae of the differentiae, are equally positive, or equally negative if we take them to be negative), then the *convexity* of the curve, or in the opposite case its *concavity,* turns towards the axis. But in the case where the increment is a maximum or a minimum, i.e. when the increments of decrease become increments of increase, or vice versa, there is a point of inflexion; concavity and convexity change into one another …

---

19 Leibniz, *Math. Schriften*, v. 220.

This then leads to rules for two further operations:

Powers: $dx^a = a.x^{a-1}dx$, for example $dx^3 = 3x^2dx$; $d\frac{1}{x^a} = -\frac{adx}{x^{a+1}}$, for example if $w = \frac{1}{x^3}$, we have $dw = -\frac{3dx}{x^4}$. *Roots*: $d\sqrt[b]{x^a} = \frac{a}{b}dx\sqrt[b]{x^{a-b}}$, … $d\frac{1}{\sqrt[b]{x^a}} = \frac{-adx}{b\sqrt[b]{x^{a+b}}}$.

He concludes:

> When one knows the *Algorithm*, if I may call it such, of this calculus, which I term *differential*, one can, by means of ordinary calculation, find all the other differential equations, and maxima and minima as well as tangents, without having to rid oneself of the fractions, irrationals, or the other peculiarities that have been a feature of methods used to date. … As a result, one can write the differential equation of any given equation simply by replacing each term … by a differential quantity. For each of the other quantities (which are not themselves terms but which contribute to forming one another), this differential quantity must be introduced in order to obtain the differential quantity of the term itself, not by means of a simple substitution, but following the algorithm that I have set out.[20]

Nevertheless, it should be said that the programme advocated in 'Nova methodus' was obscurely formulated, and the paper was so cautious in its presentation that it hardly mentioned infinitesmals at all.[21] Two subsequent treatises—Guillaume de l'Hôpital's 1696 textbook *Analyse des infiniment petits*[22] and Jacob Hermann's 1700 *Responsio ad considerationes*[23]—set out what in its origins was the Leibnizian programme with greater clarity. Hermann and l'Hôpital were both closely associated with Jacob and Johann Bernoulli. Jacob had written to Leibniz as early as December 1687 seeking guidance on a number of points in his calculus,[24] and he and Johann quickly mastered and developed it beyond its Leibnizian origins. Hermann was from the Bernoullian stronghold of Basel, and was the author of one of

20 Ibid, 222-3.

21 See the discussion in Henck J. M. Bos, 'Differentials, Higher-Order Differentials and the Derivative in the Leibnizian Calculus', *Archive for History of Exact Sciences* 14 (1974), 1-90.

22 Guillaume de l'Hôpital, *Analyse des infiniment petits pour l'intelligence des lignes courbes* (Paris, 1696).

23 Jacob Hermann, *Jacobi Hermanni responsio ad considerationes secundas Cl. Viri Bern. Nieuventiit* (Basel, 1700).

the most important early analytical treatments of dynamics.[25] The basic mathematical ideas in l'Hôpital's textbook, which were to form a statement of the approach to mathematics and mechanics associated with the Malebranche circle in Paris,[26] were in large part due to Johann Bernoulli, who had visited Paris in 1691, and whose lectures on calculus during that visit to Malebranche and the Oratorians was the formative event in the setting up of Malebranche's group of mathematicians.[27] In the wake of the lectures, l'Hôpital paid Bernoulli to send him his mathematical results on an exclusive basis.[28] The Preface to the *Analyse*, written by Fontenelle, was the manifesto of the group, and drew the line between ancient and modern methods sharply:

> What we have by the ancients on these matters, above all Archimedes, is certainly worthy of admiration. But apart from the fact that they deal with very few curves, and treat these somewhat

---

24 Leibniz, *Math. Schriften*, ii. 10-13.

25 Hermann, *Phoronomia*.

26 See, for example, Nicolas Malebranche, *De la recherche de la vérité*, ed. G. Rodis-Lewis (Paris, 1979), Book VI Part 1 ch 5: 'The invention of differential and integral calculus has made analysis limitless, as it were. For these new calculi have placed infinitely many mechanical figures and problems of physics under its jurisdiction.' As well as the works of l'Hôpital and Varignon, a number of textbooks issued from the Malebranche circle, notably Louis Carré, *Méthode pour la mesure des surfaces, la dimension des solides … par l'application di calcul intégral* (Paris, 1700); N. Guisnée, *Application de l'algebre à la geometrie, ov Methode de démonstrer par l'algebre, les theorêmes de geometrie, & d'en résoudre & construire tous les problêmes* (Paris, 1705); Charles Reyneau, *Analyse demontrée; ou, La methode de resoudre les problêmes des mathematiques, et d'apprendre facilement ces sciences* (Paris, 1708); idem, *La science du calcul, des grandeurs en general: ou, Les elemens des mathematiques* (Paris, 1714). Malebranche particularly recommended the works of Reyneau, and both Maupertuis and Clairaut used Guisnée as their initiation textbook.

27 See Pierre Costabel, 'Introduction', and André Robinet, 'Les académiciens des sciences malebranchists', in Malebranche, *Oeuvres complètes*, ed. André Robinet (20 vols, Paris 1958-78), xvii-2. 309-16 and xx. 162-74 respectively; André Robinet, 'Le groupe malebranchiste introducteur du calcul infinitésimal en France,' *Revue d'histoire des sciences* 13 (1960), 287-308; and idem, *Malebranche de l'Académie des sciences. L'oeuvre scientifique, 1674-1715* (Paris, 1970); J. O. Fleckenstein, 'Pierre Varignon und die mathematischen Wissenschaften im Zeitalter der Cartesianismus', *Archives Internationales d'Histoire des Sciences* 5 (1948), 76-138.

28 Bernoulli evidently dazzled the Oratorians showing how calculus techniques could be used to determine a catenary in a straightforward way. On this, and on the transaction between l'Hôpital and Bernoulli, see Lenore Feigenbaum, 'The Fragmentation of the European Mathematical Community', in P. M. Harman and Alan E. Shapiro, eds, *The Investigation of Difficult Things* (Cambridge, 1992), 383-98: 388-9.

> superficially, the propositions are almost entirely particular and lack order, failing to communicate any regular and applied method.[29]

It was the Bernoullis, he continues, who 'first perceived the beauty of the Method'.[30]

The Leibnizian approach, developed so fruitfully by the Bernoullis and their followers,[31] explicitly defends the idea of there being infinitesmal quantities whose addition to a given finite quantity does not change the value of the latter, so that two equal quantities remain equal when such a quantity is added to one of them but not the other: $A+\alpha = A$.[32] It also depends crucially on the use of such infinitesmal quantities in resolving curves into polygons with an infinite number of infinitesmal sides, so that infinitesmal methods can be applied to curves. The geometrical interpretation provides a way to envisage in what sense differentiation and integration are the inverses of one another: determining tangents to curves (differentiation) and computing the area between the axis and the curve (integration).

Guicciardini has shown that there were a number of basic questions on the nature of infinitesmals on which Leibniz and Newton were in agreement. They agreed that actual infinitesmals were useful fictions and did not actually exist; that they were best defined as varying quantities in a state of approaching zero; and that they can be

---

29 L'Hôpital, *Analyse des infiniment petits*, iv-v; also to be found in *Oeuvres de Monsieur de Fontenelle … nouvelle édition* (10 vols, Paris, 1762), x. 29-43: 31.

30 L'Hôpital, *Analyse*, ix; Fontenelle, *Oeuvres*, 36.

31 See, in particular, the comprehensive account of the development of integral calculus and its applications between 1690 (Leibniz) and 1741 (Clairaut's mature theory of the shape of the earth) in John L. Greenberg, *The Problem of the Earth's Shape from Newton to Clairaut: The Rise of Mathematical Science in Eighteenth-Century Paris and the Fall of 'Normal' Science* (Cambridge, 1995), 225-399.

32 'I maintain that not only two quantities are equal whose difference is zero, but also that two quantities are equal whose difference is incomparably small': Leibniz, 'Responsio ad nonnullas difficultates a Dn. Bernardo Niewentiit circa methodum differentialem seu infinitesimalem motas', *Math. Schriften*, v. 322. Leibniz is replying here to three works by Bernard Nieuwentijdt: *Considerationes circa analyseos ad quantitates infinite parvas applicatae principia & Calculi differntialis usum in resolvendibus problematis geometricis* (Amsterdam, 1694); *Analysis infinitorum seu curvilineorum propietatis ex polygonorum natura deductae* (Amsterdam, 1695); *Considerationes secondae circa calculi differentialis principia & responsio ad Virum Noblissimum G. G. Leibnitium* (Amsterdam, 1696).

completely avoided in favour of limit-based proofs, which provide them with a mathematically rigorous formulation.[33] There is one thing that is very distinctive about Leibniz's approach, however, and which separates him markedly from Newton on foundational questions. This is the importance he attaches to a mechanical algorithm. Although both Leibniz and Newton accept that it is limit-based proofs that provide the foundation for differential calculus, for Leibniz such proofs are a means of establishing legitimacy for procedures which differ significantly from standard mathematical operations, but such legitimation is not required on internal grounds, and one can proceed without constant reference to limit-based proofs. Indeed, it is distinctive of the Leibnizian programme that the algorithms that make up calculus be applied without reflecting on the steps: the procedure is secure and, as in the case of Leibnizian logic, it is a matter of supplying the premises and letting the algorithm generate the conclusion by 'blind' reasoning. Once the general legitimacy of the procedure has been established by means of a theory of limits, the calculus takes on a life of its own, as it were. In September 1691, Leibniz writes to Huygens that 'what is better and more useful in my new calculus is that it yields truths by means of a kind of analysis, and without any effort of the imagination',[34] and in December of the same year, that 'what I love most about my calculus is that it gives us the same advantages over the Ancients in the geometry of Archimedes that Viète and Descartes have given us in the geometry of Euclid or Apollonius, in freeing us from having to work with the imagination.'[35] Guicciardini has argued that for Leibniz the calculus should be seen as an *ars inveniendi*, to be valued by its fruitfulness.[36] Certainly he set great store by its problem-solving power but, as I shall argue, it would

---

33 Guicciardini, *Reading the Principia*, 159-63. Cf. Douglas M. Jesseph, 'Leibniz on the Foundations of the Calculus: The Question of the Reality of Infinitesimal Magnitudes', *Perspectives on Science* 6 (1998), 6-40.

34 Leibniz to Huygens, 11/12 September 1691; *Math Schriften* ii. 104. Note that the Olms reprint of this edition includes volumes 1 and 2 of the edition in volume 1, whereas the two parts of volume 3 of the original is spread over volumes 2 and 3. I use the original division in references.

35 Leibniz to Huygens, 29 December 1691; *Math Schriften* ii. 123.

be a serious mistake to imagine that he thought of the power of calculus in purely pragmatic terms. The important point for present purposes is that, for Lebniz, its referential content has no bearing on the calculations that we are able to perform with it, and indeed it may operate with symbols devoid of reference, such as $\sqrt{-1}$, providing it is able to generate correct results.[37]

For Newton, by contrast, limit-based proofs were the essence of differential calculus, and the employment of calculus hinged on one's ability to articulate it in terms of a comprehensive theory of limits. Whereas Leibniz abandons geometrical interpretation once his calculus has proceeded past the legitimatory stage, and in fact handles differential equations as algebraic objects, Newton never (at least after the early 1670s) allows his calculus to transcend its original geometrical interpretation, insisting that this is what provides it with reference and meaning.

Between 1670 and 1671 Newton composed a treatise on the use of infinitely small quantities, 'De methodis serierum et fluxionum',[38] the central idea in which is that of the 'fluxion'. Consider a point moving at a variable speed and generating a line (e.g. a planet moving at variable speed around the sun and generating an elliptical orbit, although Newton did not realise this application in 1671).[39] The

---

36 Guicciardini, *Reading the Principia*, 166.

37 We are perhaps not so worried about square roots of negative numbers appearing in the proof as seventeenth-century mathematicians were, but in our own times there have been parallel concerns about Feynman's use of negative probabilities in calculations (an idea originally proposed by Dirac), justified on the grounds that they allow otherwise intractable calculations and do not appear in the solution: Paul Dirac, 'The Physical Interpretation of Quantum Mechanics', *Proceedings of the Royal Society of London*, A 180 (1942), 1–39; R. P. Feynman, 'Negative Probability', in F. David Peat, ed., *Quantum Implications: Essays in Honour of David Bohm* (London, 1987), 235–248. At the other end of the chronological spectrum, similar concerns about negative integers appearing in the process of calculation were expressed in pre-modern mathematics: see Jacob Klein, *Greek Mathematical Thought and the Origin of Algebra* (Cambridge, Mass., 1968), Part II.

38 The treatise was originally untitled: *Mathematical Papers* iii. 32-353. See Guicciardini, *Reading the Principia*, ch. 2, to which I am indebted here.

39 In his entry on 'fluxion' in the *Encyclopèdie,* d'Alembert criticised Newton's idea of defining mathematical entities in terms of motions as introducing unnecessary extraneous considerations: Denis Diderot and Jean le Rond

distance covered in a time *t* is called a 'fluent', the instantaneous speed is the 'fluxion', and the infinitely small speed acquired after an infinitely small increment of time is called the 'moment'. 'De methodis' provides an algorithm for calculating fluxions, in which it is assumed that motion is uniform during equal intervals of time, and in which infinitesmals, once they have done their work, can be cancelled (following the principle $A+\alpha = A$). The algorithm construes all quantities as continuous flows, and enables him to reduce a vast range of particularly intractable mathematical problems to two classes: given the space traversed, to find the speed at any time (corresponding to Leibnizian differentiation); and given the speed, to find the space traversed at any time (corresponding to Leibnizian integration). The problems of finding tangents, extremal points, and curvatures can be reduced to the first. The second, the class of 'inverse' problems, which includes the problem of determining the area under a curve, is more problematic, and Newton employed two procedures: either he changed the variable in order to reduce it to one of a catalogue of known fluents which he had built up, or he used series expansion techniques.[40]

This part of the exercise comes under problem-solving, that is, under what was traditionally known as analysis. The demonstration of results from first principles, synthesis, was traditionally a different kind of exercise, and was resolutely geometrical. Proponents of the 'new analysis' such as Descartes and Leibniz were inclined to diminish the importance of synthesis, as an unnecessary and artificial process. Descartes believed that geometrical proofs, which routinely involved detours via demonstrations of supplementary theorems needed for the final result but not part of the actual proof, were often obscure and meandering, whereas algebraic proofs were always direct: it is simply

---

d'Alembert, *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers* (40 vols., Geneva, 1777-9), xvi. 726 col. 1.

40 Expansion techniques played a central role in the development of Newton's mathematical thinking, and his first significant mathematical discovery, in the winter of 1664-5, was of the binomial theorem. See Guicciardini, *Reading the Principia*, 18-20.

a matter of assigning symbols to known and unknown quantities, and manipulating equations that connect them so as to reveal the required relationship, a manipulation which is always completely transparent.[41] Newton was committed to the virtues of the geometrical methods of the ancients as far as demonstration was concerned, however, and his model in mechanics was the rigorously geometrical *Horologium* of Huygens. It might seem, then, that Newtonian analysis, with its algorithm of fluxions, and Newtonian synthesis, with its rigorous geometrical demonstrations, are two complementary features of the Newtonian project, one a method of discovery, the other a method of presentation. As Newton himself put it in an unpublished draft preface to the second edition of the *Principia*:

> The ancient geometers investigated by analysis what was sought [i.e. found their solutions to problems by the method of analysis], demonstrating by synthesis what had been found, and published what had been demonstrated so that it might be received into geometry. What was resolved was not immediately received into geometry: a solution by means of the composition of demonstrations was required. For all the power and glory of geometry consisted in certainty of things, and certainty consisted in demonstration clearly composed [i.e., demonstrations according to the method of synthesis, or composition]. In this science, what counts is not so much brevity as certainty. And accordingly, in the following treatise I have demonstrated by synthesis the propositions found by analysis.[42]

But matters are not so straightforward, for Newton was in fact unable to accept two different canons of mathematical procedure. In contrast to Leibniz, who avoided geometrical demonstration, even though he was committed to the idea that his calculus required a geometrical grounding, Newton took the other direction. In the course of the 1670s, as Guicciardini points out, he began to distance himself from his early mathematical researches, abandoning the calculus of fluxions in favour of a geometry of fluxions in which infinitesimal

---

41 See, for example, the comparison of geometrical and algebraic solutions to the problem in Euclid's Elements II.11 in Gaukroger, *Descartes, An Intellectual Biography*, 175-6.

42 Translated (with interpolations) by Cohen, 'A Guide', 49-50; cf. Newton, *Mathematical Papers*, viii. 442-59. See the discussion in Cohen, op. cit., 122-7.

quantities were not employed.[43] He began criticizing modern mathematical practices, and took Descartes to task, for example, for his algebraic solution to a problem that had defied the attempts of ancient geometers, Pappus' four-line locus problem, showing that the algebraic solution did *not* demonstrate the unique power of analysis, as Descartes claimed, since it did in fact have a geometrical solution, which he went on to provide. What Newton appreciated in the ancient geometrical techniques (his model was Apollonius) was that they always had a definite interpretation in a geometrical construction. At no stage of a demonstration did they ever stop referring to anything. The techniques of the new analysis, by contrast, especially as developed by Leibniz and his followers, left the realm of the concrete once the problem has been presented, only to return to it at the solution, having proceeded via processes that eschewed all reference to the entities that the original problem (and the solution) dealt with.

The success of the Leibnizian approach was palpable. As a direct result of this way of proceeding, continental mathematicians were soon exploring equations in several independent variables as well as partial differentiation, equipping them by mid-century to move mathematically, if not with ease, then at least with a great degree of facility, between rigid bodies, flexible bodies, elastic bodies, and fluids. But this in itself does not, and could not, confer legitimacy on them: the issues are too deep to be resolved in this way. Leibniz's investment in 'blind reasoning' is not a pragmatic one: it goes to the heart of his understanding of what it means to be a philosopher. It is important here to understand in what respect Leibniz's views emerge from a reflection on Pascal.[44] Pascal was obsessed with the fallen nature of human beings, and with the limits that this placed on their capacity to understand: limits which he believed it was irresponsible to ignore, as he considered his contemporaries, particularly those active in

---

43 Guicciardini, *Reading the Principia*, 28.

44 See Matthew L. Jones, *The Good Life in the Scientific Revolution: Descartes, Pascal, Leibniz, and the Cultivation of Virtue* (Chicago, 2006), Parts 2 and 3,

natural philosophy and mathematics, were doing. The human condition, he argued, was characterized by worthlessness and despair, not egoism and optimism. The value of the true philosophy and the true religion lay in their ability to teach us the remedies for our inabilities.[45] The value of mathematics lay in its ability to reveal this to us clearly, especially when the infinitely large and the infinitely small were considered.[46] In pursuing mathematics in a serious way we are forced to recognize that infinities exist, Pascal believes, but at the same time we also have to admit to our incomprehension of such infinities, thereby forcing us to take stock of our limitations.

Newton's approach to infinitesimals, as I have indicated, was to insist that they must remain anchored in limit procedures, and his preference was to replace algorithms employing infinitesimals with geometrical demonstrations. Pascal, in sharp contrast, takes infinitesimals at face value, stressing the combination of their legitimacy and their lack of intelligibility, and drawing from this a general lesson about the nature and limits of human knowledge. This lesson is one that bears directly on the *persona* of the philosopher, in that recognition of our cognitive limitations is an integral part of the morality of the philosopher, who must struggle against optimism and egoism, along with various contrasting faults such as scepticism and relativism. Leibniz agreed with Pascal that our unaided faculties were insufficient for the kind of knowledge to which we naturally aspire, although this in itself was a widespread view motivated either by a sense of the fallen nature of human beings, or because of the kind of systematic criticism of sense perception that Descartes, and Cartesians such as Malebranche and Arnauld, among others, had mounted. Unlike those accounts that saw limitations to knowledge as a result of the Fall, the Cartesian approach had set out to undermine

---

on Pascal and Leibniz respectively. I am indebted to Jones' discussion in what follows.

45 See *Pensées* frag. 149 (Lafuma numbering): Pascal, *Oeuvres*, 520. On Pascal's conception of philosophy and the philosopher see Vincent Carraud, *Pascal et la philosophie* (Paris, 1992).

46 See, in particular, his 'De l'esprit géométrique', *Oeuvres*, 348-55.

the reliability of sensation only to replace it with the reliability of properly tutored reason, that is, reason that could issue in 'clear and distinct' judgements. Leibniz is very much in this latter tradition, and mathematics acts for him very much as a model of what knowledge could be, although he believes that the limitation of knowledge claims to what we grasp clearly and distinctly is mistaken, for, as he points out to Conring in a letter of 1678, there are symbolic operations which we cannot grasp clearly and distinctly but which nevertheless manifestly yield the kind of knowledge that mathematicians should be seeking.[47] This is clear from his attitude to one of his first discoveries, one that would be important in his subsequent invention of the calculus, namely his quadrature of the circle. What he discovered was a means of providing the area of a circle by means of an infinite series:

$$\frac{\pi}{4} = \frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} \dots$$

It is impossible, he points out to Conring, to express the ratio between a square and a circle by a single number, but we can express the ratio in an infinite series of numbers. As Jones notes, for Leibniz, series such as this offer 'the only exact knowledge of the quadrature of a circle available to embodied human beings without divine intervention.'[48] They offer a way of transcending traditional limitations which restrict mathematical knowledge to quantities and geometrical constructions, so that 'a value can be expressed exactly, either by a quantity or by a progression of quantities whose nature and way of continuing are known.'[49]

For Leibniz, our native abilities do not match our native capacities, and the way to realize our full capacities is through procedures that go beyond our unaided faculties. These procedures are, paradigmatically, algebraic analysis and infinitesimal algorithms, for these take us beyond the kind of pictorial geometrical representation

---

47 Leibniz to Conring, 19 March 1678: *phil. Schriften* i. 199.
48 Jones, *The Good Life in the Scientific Revolution*, 169-70.
49 Leibniz, *Math Schriften* ii. 96.

on which our unaided faculties rely, to new and far more powerful forms of cognition, where this new power is manifested in a clear and explicit way in the novel and general results that they yield. On this view, then, it is not a question of appealing to unaided faculties to secure a legitimation of infinitesimals (Newton), or to use their inability to provide such a legitimation as a means of criticizing unaided faculties (Pascal), but rather of using them as an extension of our reason in much the same way that we might use a microscope or telescope as an extension of our vision. We don't use microscopes or telescopes to show that our natural vision is fundamentally lacking, nor do we insist that their use be limited to things we can see with unaided vision. Rather, we use them to take us beyond our natural faculties. It is the fact that infinitesimal algorithms cannot be grounded in 'natural' mathematical reasoning that means that our use of them *must* be 'blind': it is they that guide us, we do not guide them.