Early Modern Mathematical Principles and Symmetry Arguments

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Mathematics is the home ground of principles. Since Euclid, mathematics has been the model of a body of knowledge organized as a deductive structure based on self-evident axioms. The prestige of that model was highest in early modern times, lying after the vast extension of the realm of mathematics in the Scientific Revolution but before the discovery of non-Euclidean geometries and the foundational crises of the late nineteenth century. When the Jesuit missionaries chose Euclid as the first book to be translated into Chinese (Engelfriet 1998) and when Spinoza offered to exhibit ethics as a system demonstrated *more geometrico* from definitions and simple self-evident axioms, he paid tribute to the place of the Euclidean model at the summit of intellectual achievement.

Seventeenth century thinkers who applied mathematics to physics – Stevin, Galileo, Descartes, Pascal, Huygens, Barrow, Newton, Leibniz, to name only the most prominent – were captivated by a model of applied mathematics, apparently realized in Euclid's *Optics* and Archimedes' mechanics, according to which pure thought could establish principles for empirical reality. Their ambition stood at the opposite extreme from the Baconian style of "experimental philosophy" which recommended generalization from carefully collected facts. (Dombski 2013)

Their hopes were not fully realized, because of such awkward facts as the contingency of the constant of gravitational attraction. But their vast optimism concerning the power of mathematical reasoning and the possibilities of understanding reality through pure symmetry arguments proved astonishingly successful and created modern physics.

The Background: Aristotle's *Posterior Analytics* and Euclid's *Elements*

The model of mathematics as a deductive structure of propositions proved from self-evident truths was backed by the two greatest possible authorities, Aristotle and Euclid, and was in any case open to inspection if one followed the proofs in Euclid oneself. Virtually all thinkers accepted it without question.

The essentials of the model were laid down in Aristotle's *Posterior Analytics.* A true or fully-developed science should demonstrate its truths by syllogistic deduction from self-evident first principles, explaining why the truths of the science must be as they are. The first principles should be simple enough to be evident to the pure light of reason, or *nous* (Latin *intellectus*), a divinely-granted faculty of the soul capable of grasping necessities.

If to modern ears that seems a promise too good to be true, for early modern thinkers it was confirmed by the existence and obvious success of Euclidean geometry, a science that seemed to conform exactly to Aristotle's model. (McKirahan 1992: ch. 12) Euclid's *Elements* begins with twenty-three definitions, such as:

> 1. A *point* is that which has no part …
>
> 15. A *circle* is a plane figure contained by one line such that all the straight lines falling upon it from one point among those lying within the figure equal one another …

These are meant to be not arbitrary stipulations but contentful truths about real geometrical objects. Though Euclid omits any philosophical commentary, modern writers rightly emphasize that to appreciate the definition of a circle, for example, one needs an act of insight to understand that something uniformly round does in fact consist of all the points equidistant from a centre. (Lonergan 1997: ch. 1) There follow five postulates or axioms, such as

1. To draw a straight line from any point to any point …
5. That, if a straight line falling on two straight lines makes the interior angles on the same side less than two right angles, the two straight lines, if produced indefinitely, meet on that side on which are the angles less than the two right angles.

The way in which the Fifth Postulate is posed is such as to make is appear as self-evident as possible, despite the worries, expressed since ancient times, as to whether anything involving lines "produced indefinitely" could be truly self-evident. Indeed, behind the scenes there were considerable doubts among the best professional mathematicians as to whether Euclid's logic was watertight (De Risi 1998), but from the perspective of outsiders the Euclidean structure was monolithic and impregnable.

There follow a large number of propositions or theorems derived logically from the postulates, beginning with:

I.1 To construct an equilateral triangle on a given finite straight line.

These truths were taken to be necessary truths about the real space we live in, even though Aristotle to some extent adopts the Platonist idea that the geometer's lines and circles are assumed perfect and hence are idealizations of the real shapes of rulers and wheels. Any doubts about the principles of geometry and about its direct applicability to the physical world were suppressed in the seventeenth century. Even as countersuggestible a mind as Hobbes, initially doubtful about Euclid, was convinced by the proofs. Virtually the only doubter was the Chevalier de Méré, a "man of the world" without intellectual standing. When he wrote to Pascal with some puzzles about probability which he took to show that mathematics was self-contradictory, Pascal wrote to Fermat that de Méré's incompetence in mathematics was clear from his belief that space was atomic (contrary to the infinite divisibility of space in Euclid). (Pascal, 1964-70, 2, 1142; embarrassingly, it turned out that whether space is continuous or atomic is not provable: see Franklin 1994)

To appreciate the early modern understanding of mathematical principles, it is essential to put to one side certain more recent philosophical views which make it appear implausible. Right or wrong, certain contemporary received ideas about mathematics and its relation to reality impede a clear view of early modern assumptions. We discuss briefly the two main ones, which both assume a divorce between mathematics and reality that was not part of, or even one of the possibilities considered by, the early modern view of mathematics.

Firstly, the legacy of four hundred years of experimental science is an assumption that substantial scientific knowledge is contingent and must be established by experiment; hence the role of mathematics is as a "theoretical juice extractor", a collection of methods to codify the generalizations arising from experiment and to enable predictions from them. Mathematics itself is not seen as directly about reality. As Einstein put it, "As far as the propositions of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality." (Einstein 1954: 233; argued against in Franklin 2014: ch. 5)

The second and related contemporary obstacle to understanding early modern views on mathematics is the oscillation in post-Fregean philosophy of mathematics between nominalism and Platonism. Nominalist philosophies that take mathematics to be not about anything but a language of science, or a collection of tautologies, or manipulation of formal symbols, or an investigation of what follows from arbitrary axioms, stand against Platonism, which regards mathematics as about "abstract objects" such as sets and numbers (or idealized geometrical objects like perfect circles). (The dichotomy explained and criticized in Franklin 2014, especially ch. 7; a textbook on mathematical proof from an Aristotelian point of view in Franklin and Daoud, 2011)

Nothing could be further from the early modern conception of mathematics, which was neither nominalist nor Platonist but Aristotelian (or "moderate") realist – mathematics was said to be the "science of quantity", with quantity understood as a property of extended physical things. (E.g. Barrow 1734: 10-15; further in Barrow 1664/1860: lectiones 4-8; Gillette 2009; *Encyclopaedia Britannica* 1771: article 'Mathematics', vol. III 30-1; Mancosu 1996: 16, 35-37, 56, 88; Jesseph 1993: ch. 1. Gassendi and Hobbes do have nominalist tendencies: Sepkoski 2005, Pycior 1987; Dear 1995 puts a constructivist interpretation on the topic; eighteenth century continuations in Franklin 2006) Quantity, one of the basic Aristotelian categories, comes in *discrete* (studied by arithmetic) and *continuous* (studied by geometry). (For the bridging of this gap in the 16th and 17th centuries see Neal 2002a, Malet 2006) As Barrow writes,

> It is plain the *Mathematics* is conversant about two things especially, viz *Quantity* strictly taken, and *Quotity*; or if you please, *Magnitude* and *Multitude*. By others they are called *Continued* and *Discontinued* Quantity … (Barrow 1734: 10)

There are also *subordinate* or *mixed* sciences, such as music (subordinate to arithmetic) and optics and astronomy (subordinate to geometry). Barrow says:

> But because both *Magnitude* and *Multitude* may be considered in a double respect; *viz.* either as they are mentally separated, or abstracted from all Matter, material circumstances and Accidents; … or as they inhere in some particular Subject, and are found conjoined with certain other physical Qualities, Actions and Circumstances: Hence arises the Division of *Mathematics* into *Pure* or *Abstract*, and *Mixed* or *Concrete*.

Being concrete does not impede the certainty of the mixed mathematical sciences. They are again conceived as sciences of real but necessary aspects of the world. The search for self-evident principles for the subordinate sciences is the most interesting aspect of the topic and we will return to it.

The Aristotelian-Euclidean background to seventeenth-century mathematics means that mathematics does not fit at all into the old picture of the Scientific Revolution as a revolt against scholastic obfuscation in favour of empiricism and experimentalism. That model, even if over-simplified, does make some sense as applied to the natural sciences, especially chemistry, biology and the more empirical parts of physics. Mathematics, including applied mathematics, is otherwise. As Derek Whiteside put it after his extension investigation of early British mathematics,

> I have neglected a prevailing fashion which sees mathematics as a mere handmaiden of the sciences, and the 17th century scientific achievements as a revolution in which scientific thought was freed from the largely sterile dominance of scholastic authority under a guiding principle of the primacy of theory induced from observing instances in phenomena. (Whiteside 1961: 180)

The story of mathematics is more like an extension of scholasticism than a retreat from it. Where the old scholastics had been excessively modest about the possibilities of reducing the contingent physical world to quantitative order and demonstration, the mathematicians showed it could be done by doing it. Early modern applied mathematics is the pursuit of the scholastic vision by other means.

Mathematical Developments in the Seventeenth Century

Before the Scientific Revolution was a revolution in *science*, it was a mathematical revolution. Copernicus' astronomy was purely geometrical, an attempt to rearrange the geometry of the heavens without any considerations of the causes of celestial motions. The same is largely true of the successful parts of Kepler's astronomy, though he did add some speculations about magnetic attractions as a possible cause of orbits. John Dee's preface to the first English Euclid, of 1570, describes a culture of some thirty useful mathematical sciences like perspective, navigation, astrology and statics. (Euclid [1570]: Billingsley's Preface) The important mathematical achievements around 1600 of Viète, Stevin and Harriot in fields like algebra, statics, navigation and logarithms preceded the first new experimental results of Galileo and Bacon. (Franklin 2000)

Then the seventeenth century itself saw a flowering of mathematical genius – much more so than in the two adjacent centuries. Galileo, Descartes, Fermat, Pascal, Huygens, Newton and Leibniz were mathematicians of the first rank; Kepler, Cavalieri, Barrow, Wallis, Jacob Bernoulli and others important contributors. All were trained in Euclid's model of how to do mathematics. All had, so to speak, a Euclid baton in their knapsack; they harboured the ambition of becoming the new Euclid of their fields. In many but not all cases, as we will see, that involved explicitly laying down definitions and axioms for new fields and deriving theorems from them.

What they hoped for is expressed in a confident, even impudent, exchange about theory and experiment from Galileo's *Dialogue Concerning the Two Chief World Systems*:

SIMPLICIO: So you have not made a hundred tests, or even one?

SALVIATI [for Galileo]: Without experiment, I am sure that the effect will happen as I tell you, because it must happen that way. (Galileo [1953]: 2nd day, p 145)

The major mathematical developments in the seventeenth century included:

- Number theory, algebra and Cartesian geometry
- The science of the continuous: from Cavalieri's infinitesimals and Descartes' tangents to the calculus of Newton and Leibniz
- Astronomy: Kepler's and Newton's laws
- Mechanics, from Galileo's law of free fall to Newton's laws and gravity
- Laws of proportion in physics
- Mathematical probability theory

The seventeenth century plan was to organize these as far as possible as Euclidean structures founded on self-evident principles. But the principles were not to be found in Euclid. We will examine a number of these areas of mathematics and consider the attempts to provide them with principles. While success was not total, especially in exhibiting principles that were fully self-evident, the overall success of the project was remarkable.

Before proceeding, a word on terminology. Writers of the time used more or less interchangeably the Latin "Principles/principia", sometimes "postulates" and "laws/leges", and the Greek "Axioms/axiomata". Thus Newton has "Principia" in the title of his most celebrated book, but then "axiomata sive leges motus". There is however a tension between the traditional language of "principles" and "axioms", suggesting absolute necessity, and the new language of "laws", meant to suggest obedience to the commands of God. (Oakley 1961) If God has to command something, presumably it could be otherwise and hence cannot have the necessity that gives rise to self-evidence. A voluntarism with regard to divine action is a departure from Aristotelian orthodoxy. Issue was not directly joined (unlike in the Ockhamist voluntarism of earlier centuries), but in thinkers like Barrow and Newton, one must be aware of a tension that is suppressed. (Malet 1997)

Number Theory: Free of Principles?

It is something of a mystery why the central part of pure mathematics, number theory, has rarely been seen to need principles or axioms in the way that geometry does. Euclid does number theory, but mentions no axioms. Undoubtedly one reason is that the basic truths of arithmetic just look obvious, lacking, for example, the subtleties of continuous ratios and infinite divisibility that afflict geometry. That is the point of view even in the unusually detailed account of discrete quantity by Wallis in his *Mathesis Univeralis*. The basic truths about natural numbers can just be written down and are obvious. (Wallis 1657: ch. 10; Whiteside 1961: 186-8) It is arguable that number theory was seen as so general that it needed no axioms, while geometry's need for subject-specific axioms was comparatively a defect. (De Risi 1998, section 1.2.4)

In the late nineteenth century, Peano did successfully axiomatize arithmetic, revealing the crucial role of the principle of mathematical induction. Almost all truths about numbers need to be proved using this principle (which says that if a property is true of the number 1, and its being true of any number n implies its truth for $n + 1$, then it is true of all numbers).

While no such principle was thought of before Peano as an axiom of number theory, the use of mathematical induction as a method for rigorously demonstrating arithmetical truths did become established in the seventeenth century, after isolated earlier uses of lesser degrees of explicitness. Pascal uses the method fully explicitly in his *Treatise on the Arithmetical Triangle*, written around 1654. He writes of a complex proportion among consecutive numbers in any row ("base") of "Pascal's triangle" (as we now call it):

> The first, which is self-evident, that this proportion is found in the second base … The second, that if this proportion is found in any base, it will necessarily be found in the following base. Whence it is apparent that it is necessarily in all the bases. For it is in the second base by the first lemma; therefore by the second lemma it is in the third base, therefore in the fourth, and to infinity. (Pascal 1665: twelfth consequence)

Pascal plainly understands a very general template that is applicable to the proof of any arithmetic statement that can be expressed as a proposition about an arbitrary number n.

Fermat's Method of Infinite Descent, which is logically equivalent to the principle of mathematical induction, was also understood by Fermat as a very general technique that relies on the structure of the natural numbers to prove arithmetical statements. (Bussey 1918) While it is not usual to call a method a "principle", the reliance of the method on the essential linear structure of the numbers makes it close to an axiom.

While there was debate about the status of algebra, the normal perspective on it was as just a method, rather than a body of knowledge in its own right which might require principles. (Debates on algebraic reasoning with symbols in Pycior 1987)

Infinitesimals and Calculus

In the area of mathematics that deals with the continuous – involving limits, rates of change, tangents to curves, areas and volumes of curved figures – the seventeenth century had inherited the cumbersome "method of exhaustion" of Eudoxus and Archimedes. It permitted rigorous answers to be derived in a limited range of questions about areas and volumes, but was a barrier to progress because it ruled out as unrigorous methods that seemed intuitively fruitful and justified. Four centuries of difficult mathematics since then have shown that the field is a quagmire where unrigorous methods can easily lead to disaster, so the Greeks had a point in demanding absolute rigour. But in the meantime, it was natural to invest hope in intuitively attractive new "shortcut" methods and to look for new principles which would undergird them.

The essential idea of these methods was to use "indivisibles" or "infinitesimals", that is, quantities that were larger than zero but could be considered less than any positive number. After a significant start by Kepler in calculating the volumes of wine barrels by thinking of them as made up of infinitesimal slices, (Kepler 1615; Struik 1969: 192-7; Galileo's beginning on infinitesimals in Bascelli 2014) the first of these methods that claimed rigour, and the first to show both the suggestiveness, calculatory power, and logical slipperiness of all such attempts, was Cavalieri's Principle. (Full account in Anderson 1985; on the century's appreciation of the logical problems with infinitesimals, see Jesseph 1989; full account in Jullien 2015)

Cavalieri's Principle applied to the classic problem of finding areas or volumes of curved figures. In the volume case, as in Fig 1,

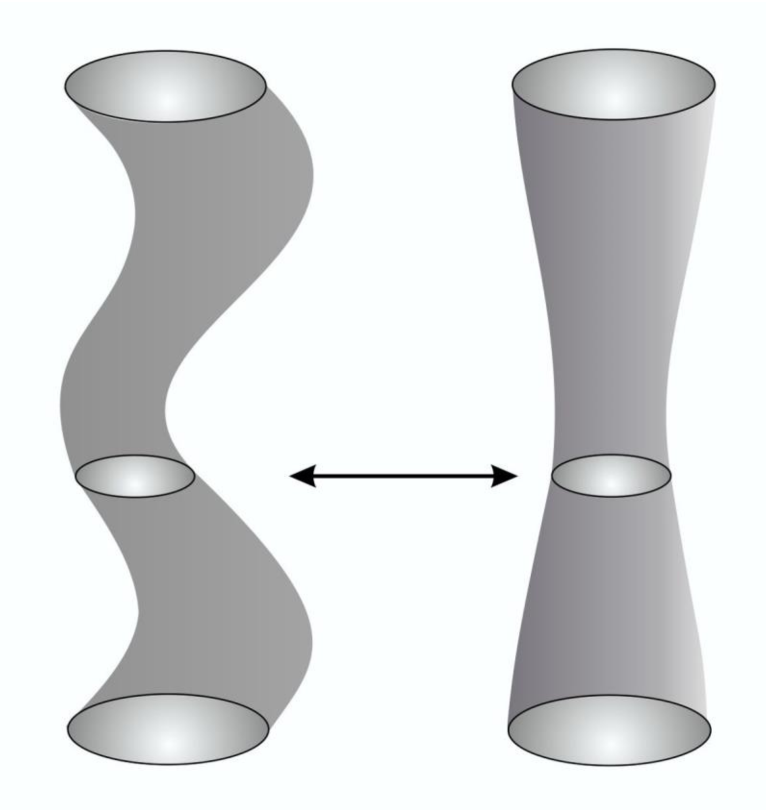

Fig 1 A modern explanation of the idea of Cavalieri's Principle

the Principle states that if two solids lie between two parallel planes (top and bottom in the Figure) and if every plane parallel to those two intersects the two solids in slices of equal area, then the solids have the same volume. One naturally thinks of the solid as made up of the infinitely many infinitely thin slices, which slide horizontally to make one figure from the other. However that creates a logical problem, since one has the dilemma of choosing between:

(a) having the volume actually made up of infinitely many zero-volume slices, in which case one has non-rigorous and dangerous ideas of "zero times infinity"; who knows how to multiply zero by infinity to obtain the "right" answer?

or

(b) having the volume made up of finitely many slices, which would each have a non-zero volume but when slid horizontally would not exactly fit the other volume (the edges of the slices cannot be exactly right to fit both curved volumes).

Option (a), which appears cleaner and more natural, is also more dangerous, as it can easily lead to wrong answers. For example, if one argued that in Fig 1 the surface areas of both figures were made up of the circumferences of the cross-sectional circles, and hence were equal, that would be wrong. The more curvy figure on the left has a larger surface area. It is not easy to explain in terms of infinitesimals why that is so.

Cavalieri himself understood the problem and attempted to straddle the two horns of the dilemma. What is normally called his "principle" is in his work not a principle in the sense of a beginning of demonstration, but a theorem which he attempts to demonstrate. He advances a notion of "all the planes" of a figure (that is, its infinitely many cross-sections), and argues that "all the planes" of one figure can, in the mass, have a finite ratio to "all the planes" of another. His fundamental postulate about these is:

"All the lines" of congruent figures … are congruent. (Andersen 1985: 316)

That has appeal as a symmetry principle, but there are difficulties applying it to more general cases by, as Cavalieri tries to do, dissecting and rearranging figures. On a modern understanding, Cavalieri's project is impossible, as no rigorous foundation of limits and continuity was discovered until Cauchy's work in the early nineteenth century explaining the concept of limit in terms of multiple quantification and separating safe from unsafe conclusions.

Cavalieri's ideas, reduced to a slightly simplified and easier form by Torricelli, became well-known. Roberval and Pascal, in making advances toward the integral calculus, saw themselves as applying Cavalieri's methods, but were in reality doing something substantially different at the foundational level. They found the areas of curved plane figures by cutting them into finitely many strips and taking a genuine limit as the number of strips increases (in the manner of the modern Riemann integral). That is unlike Cavalieri, whose principles avoid any transition from the finite to the infinite. (Andersen 1985: section X)

In developing the methods of the differential and integral calculus, Newton and Leibniz were not unaware of their lack of a rigorous foundation for limits. They reacted by such means as issuing magisterial lists of rules and keeping to algorithms for differentiating and integrating functions, rather than trying to lay down principles from which the results would follow. Both Leibniz and Newton found themselves manipulating "infinitesimals", quantities that were regarded as non-zero when convenient and zero when convenient – indeed, the calculus came to be commonly called "infinitesimal calculus". The obvious problem of regarding a quantity as both zero and non-zero – which was to attract Berkeley's entirely justified sarcasm that infinitesimals were "the ghosts of departed quantities" – provoked various attempted responses from the founders of calculus, including Wallis's attempt to regard infinitesimals as really zero, Johann Bernoulli's attempt to establish them as really positive, Leibniz's attempt to regard them as fictions, and Newton's hope of bypassing them by depending on intuitions of continuous movement to understand “fluxions”. (Jesseph 1998; Brown 2012: ch. 6)

A serious attempt to expound the calculus as a structure of theorems following from principles occurs in the first textbook on differential calculus, L’Hôpital’s *L’Analyse des Infiniment Petits pour l'Intelligence des Lignes Courbes* of 1696 (actually written in large part by Johann Bernoulli). It displays definitions and postulates in a Euclidean style, such as:

> Definition II: The infinitely small part whereby a variable quantity is continually increased or decreased, is called the differential of that quantity.
> Postulate I (literally “demande ou supposition”): Grant that two quantities, whose difference is an infinitely small quantity, may be taken (or used) indifferently for each other …
>
> Postulate II: Grant that a curved line may be considered as the assemblage of an infinite number of infinitely small right lines … (L’Hôpital 1696/1716: 2-3; discussion in Mancosu 1996: 151-2)

However, merely declaring as a principle that quantities that differ by an infinitesimal are equal does not render that statement meaningful.

Applied mathematics/Subordinate sciences/Mixed mathematics

The most interesting aspect of the topic of early modern mathematical principles concerns their appearance in (what we call) applied mathematics. As described in the introduction, the division of mathematics into pure and applied is an anachronism as applied to the seventeenth century. To think of mathematics as essentially a “pure” discipline whose results are then “applied” to a physical reality which it models imperfectly is a Platonist conception of mathematics, inapplicable to the Aristotelian conception that was dominant until at least the eighteenth century. (Franklin 2014, especially ch. 14)

The ancient division of mathematics, largely accepted in the seventeenth century, was into the science of discrete quantity (arithmetic) and the science of continuous quantity (geometry). In addition there were sciences “subordinate” to these, namely music or harmony (subordinate to arithmetic: really the study of discrete ratios of notes such as the octave and the fifth) and optics (subordinate to geometry) and astronomy (geometry in motion). The division of the mathematical sciences into the Quadrivium (arithmetic, geometry, music and astronomy) was old enough to be attributed vaguely to "the Pythagoreans", (e.g. Proclus [1970]: 29-30) while Aristotle said in the *Posterior Analytics* that optics is subordinate to geometry in the same way as harmonics is subordinate to arithmetic. (Aristotle, *Posterior Analytics* 75b16 and 78b37; McKirahan 1978; Lennox 1978)

That way of organizing the mathematical sciences suggests that the subordinate sciences inherit their principles from arithmetic and geometry, with the implication that those principles should be just as certain in the subordinate sciences as they are in the higher ones. That is a view opposite to the usual modern one (asserted by Einstein above) that there must be a model-reality gap that prevents any certainty of pure mathematics from transferring to the applications of mathematics.

As the successes of mathematics built up in the seventeenth century, high hopes were entertained of extending the certainty of mathematics to a wide range of areas of what we now call physics. (Generally on early seventeenth-century mixed mathematics in Brown 1991; Lennox 1986; Malet and Cozzoli 2010) Some of those areas are more promising for that approach than others. Astronomy, once the jewel in the crown of the mathematization of nature, was proving hard going, what with the debate about alternative "hypotheses", and with a resolution impeded for theological reasons. Mechanics (dynamics) was a prize not yet ready to fall. (Details on attempts in Meli 2006; Capecchi to appear) The two sciences that *prima facie* fit best the model of a subordinate science simply taking whole its principles from its higher science while retaining their certainty were the very old pair, optics and statics. As Barrow rightly said, in the course of extending mathematics to applications beyond the actual world – to worlds which are self-consistent and which God might create though he has chosen not to – statics, like optics, is a specially central and certain part of mixed mathematics:

> that part of mechanics dealing with centers of gravity and that part of optics called perspective are not unfitly numbered among the parts of geometry, because they scarce require any thing which is not granted and proved in that science, nor use any other principles or reasonings than what are strictly geometrical. (Barrow 1734: 27; Malet 1997)

17th Century Optics: A Wave of "Principles"

Optics is especially certain, it seems. To all appearances, it is just geometry. The seventeenth century was very familiar with the model of Euclid's *Optics*, which deals with the basic geometry of vision. It ignores all the physical and psychological aspects of vision, and starts with postulates that relate vision to straight-line geometry, such as:

1. That rectilinear rays proceeding from the eye diverge indefinitely
2. That the figure contained by a set of visual rays is a cone of which the vertex is at the eye and the base at the surface of the objects seen
3. That those things are seen upon which visuals rays fall and those things are not seen upon which visual rays do not fall
4. That things seen under a larger angle appear larger, those under a smaller angle appear smaller, and those under equal angles appear equal

and so on. (Lindberg 1976: 12; a complete translation in Euclid [1945]) (It does not have separate definitions, presumably on the grounds that the definitions of "line" and other terms in the *Elements* are sufficient.)

How are these postulates established? It seems that one can just look at experience and analyse it. We see what is before us in a straight line, if nothing closer in that direction obscures it. It is true that the "indefinitely" in postulate 1 is a big claim, given limited perceptual experience, but since the indefinitely distant stars are visible, it has a solid claim to be obvious from experience.

In any case, if the postulates in some way extend beyond experience, devotees of the "experimental philosophy" of the seventeenth century were in no position to complain, as experiments are particular and their results too are generalized by some semi-magical process to other times and places.

Euclid avoids discussing reflection and refraction, whose principles seem harder to establish a priori. How could the basic principle of equality of the angles of incidence and reflection be certain prior to observation? The difficult Snell's formula for the angle of refraction seems even less a priori. However the principles of Euclid are wide-ranging, being essentially sufficient for the science of perspective (as in painting), which was recognized in Renaissance times as one of the most useful areas of mathematics. Euclid's *Optics* provided a model of a substantial applied mathematical science with self-evident principles true of the real world.

But the great advance of optical theory in the seventeenth century – from Kepler's understanding that the eye focusses on the retina (Lindberg 1976; Gal and Chen-Morris 2010) to the achievements of Newton's *Opticks* – had another major contribution to make to the development of mathematical principles. Snell's (or Descartes') law of refraction, Fermat's principle of least action, and Huygens' Principle of wave propagation certainly explained optical phenomena in terms of very general mathematical principles. But they did not appear to be self-evident principles. As in Newton's principles for explaining planetary motions, but more perspicuously in the simpler and more geometrical field of optics, the principles need some degree of empirical support, and it is the combination of their inherent power, generality and simplicity with their ability to exhibit the phenomena as logical consequences, that gives them their credibility.

Why exactly it is a logical virtue in a theory to explain phenomena when it is not based on self-evident principles was unclear at the time, and still contested (although logical Bayesian philosophy of science has a credible in-principle explanation, it is still unknown, for example, how to quantify the strength of "arguments to the best explanation").

The mystery was deepened by the strange fact that the apparently contingent and less than intuitive law of reflection and Snell's law of refraction could both be explained in terms of two principles that were entirely different from each other, even if probably compatible.

Fermat's Principle of Least Time (or "Least Action") explains the equality of angles of a reflected ray of light as in Fig 2:

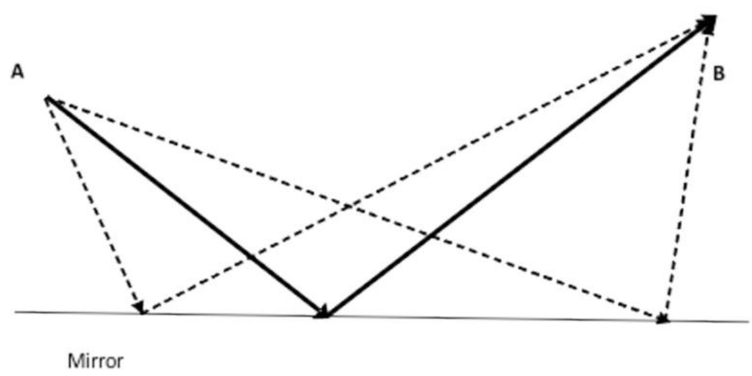

Fig 2 Fermat's Principle to explain reflection

Among all paths from A to B via the mirror, the solid path, with equal angles at the mirror, has shortest length

Similar but more difficult reasoning can be used to derive Snell's law of refraction from Fermat's Principle.

However, there was something unsatisfactory about regarding Fermat's Principle as a *principle*. Not only did it lack self-evidence, it appeared to require that light travel with finite speed, in order that the least distance from A to B should translate into least time. If light travelled instantaneously, why should the shortest path be preferred? The finite speed of light was suspected, but neither provable a priori nor able to be established by experimental evidence. Even worse, the form of Fermat's Principle seemed wrong for a free-standing physical principle, as it seemed to suggest that Nature somehow "knew" ahead of time which was the shortest path and directed light along it. As Clerselier objected to Fermat, "the principle which you take as foundation of your demonstration, viz. that nature acts always but the shortest and simplest route, is but a moral and not physical principle, which is not and cannot be the cause of any physical effect." (Mahoney 1994: 401)

Huygens' Principle explained, or purported to explain, the same phenomena (reflection and refraction) by a different supposition: that each point in propagating light acts as a source of a circular (or in 3D, spherical) wave, and the resultant light paths are the envelope of the waves. (Dijksterhuis 2005; Shapiro 1989; Animation at http://www.physics.ucdavis.edu/Classes/Physics9B_Animations/ReflRefr.html) The circular waves themselves may or may not be purely fictional.

Huygens, unlike Newton, was prepared to bite the bullet and conclude that hypotheses of this sort, from which phenomena follow naturally but which cannot be independently established, are probable – with "a degree of probability which very often is scarcely less than complete proof", (Huygens [1912]: Preface, vi-vii; Franklin 2001: 369) but nevertheless not complete proof. His choice was not a popular one.

Statics

After Euclid's *Optics*, the best-known science where it seemed that real-world certainties could easily be derived from self-evident geometrical principles was statics, especially the central part concerning the law of the balance. Pierre Duhem's original work on the late medieval influences on early modern physics concerned statics, (Duhem 1905-6/1991) because that science is very simple – much more so than dynamics – and is easily appreciated by those looking for mathematical principles in real-world examples. To the medieval contributions was added the powerful and clearly-expressed work of Archimedes in his *Equilibrium of Planes*.

The law of the balance states that two weights on a beam supported by a fulcrum balance each other if the ratios of the weights is the inverse of the ratios of their distances from the fulcrum. That is, lighter weights far away can balance heavy weights closer in. Archimedes does not regard this as a physical law needing experimental support but offers to demonstrate it from absolutely certain first principles. In the simplest example, where one weight is twice the other, his proof works like this:

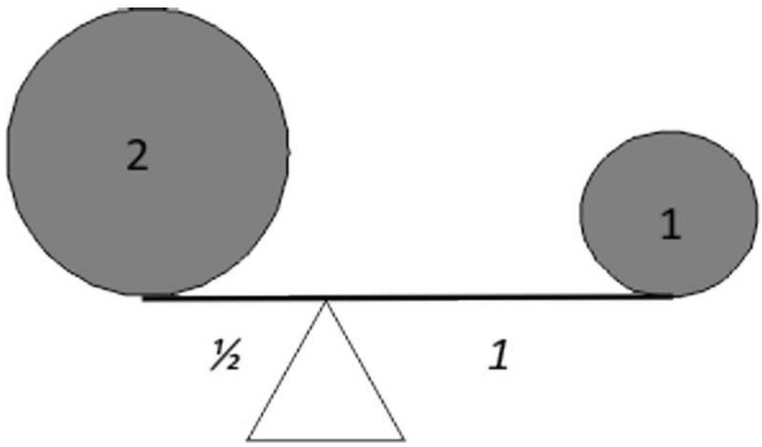

Fig 3a A weight of 2 close to the fulcrum balances a weight of 1 twice as far away

Archimedes first extends the (weightless) beam in each direction so that the fulcrum is now in the middle (the weights do not change position).

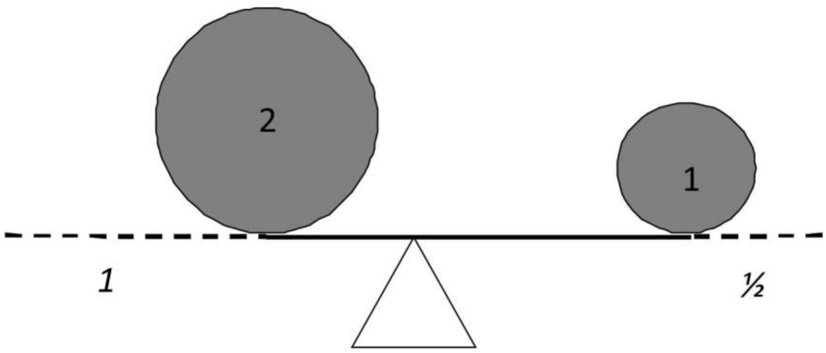

Fig 3b The beam is extended so that the fulcrum is in the middle

The weights are now imagined as malleable, like clay, and are gently patted down to rest with uniform thickness on the beam, as in Fig 3c.

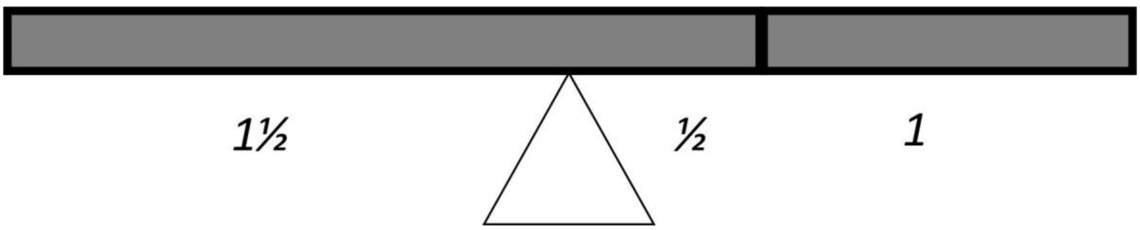

Fig 3c The weights are patted down to rest uniformly on the beam

We see that in the final state, weight is uniformly distributed, with equal amounts each side of the fulcrum. So by symmetry the weights must balance. Therefore the original pair of weights must balance.

Seventeenth-century thinkers were impressed, as well they might be. (Vampoulis 2010) The demonstration of what can be done by extending a pure symmetry is very powerful.

A well-known similar example, where pure symmetry considerations lead to a surprising and apparently empirical result, is Stevin's "wreath of spheres". Stevin printed the diagram as the title page of his 1586 *Elements of the Art of Weighing*, a work which imitates and extends Archimedes' deductive structure. (Stevin 1586, discussed in Devreese and Vanden Berghe 2008: 136-9)

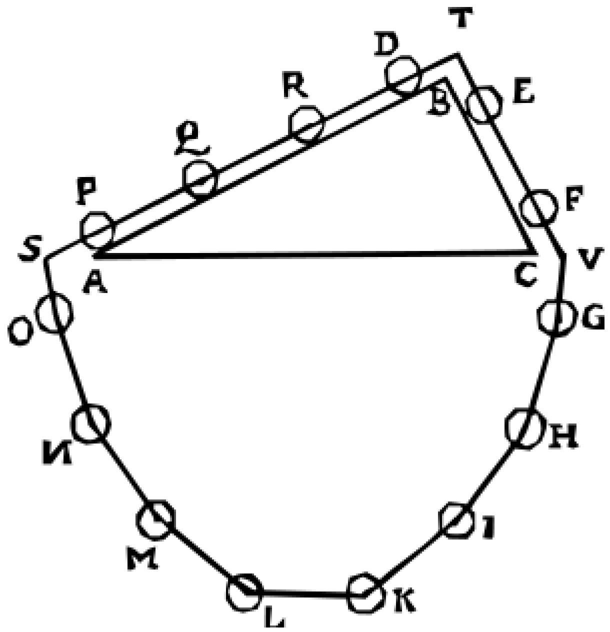

Fig 4 Stevin's "wreath of spheres" showing how weights on inclined planes balance each other according to the angle of inclination

(Public domain:

http://en.wikipedia.org/wiki/Simon_Stevin#mediaviewer/File:StevinEquilibrium.svg)

The text which decorates it says, approximately, "Wonderful, but not incomprehensible". That is correct. It is clear that the circle of balls does not tend to rotate either clockwise or anticlockwise (that would be perpetual motion, and in any case there is no reason to prefer clockwise to anticlockwise rotation). But the balls hanging below the horizontal line are in equilibrium. One could cut them in the middle, and allow them to hang down, or even remove them altogether, without disturbing the balls resting on the two inclined planes. These upper balls, then, are at rest, in balance: the many on the lightly inclined plane balancing the few on the steeper plane. The numbers of balls on each side of the apex are in inverse proportion to the sine of the angles at which the planes are inclined. One has derived, therefore, the law of the inclined plane, or, equivalently, the resolution of forces into components. It is significant that this, probably the only significant discovery of the early Scientific Revolution concerning forces, is expressed as pure geometry.

Galileo: uniform acceleration of fall

Kinematics, the science of motion in itself without regard to its causes, is somewhat harder than statics to reduce to purely mathematical principles, but it can to some extent be done.

The necessary connections between acceleration, speed and distance travelled are laid out in the simplest case by the medieval Merton Mean Speed Theorem, repeated by Galileo, (Boyer 1959: 82-5) and cleared up in general by Newton and Leibniz's calculus. Feats such as Newton's derivation of Kepler's elliptical orbits from a central law of acceleration are purely mathematical and need only principles of calculus (though as we saw above the state of that subject left something to be desired at the foundational level).

It is otherwise with dynamics, which deals with the causes of motion such as gravity and other forces. It cannot be established a priori that the fall of heavy bodies is uniformly accelerated, much less what the constant of acceleration is. (That is why the contingency of the value of G and other constants of nature plays a crucial role in contemporary debates on the Anthropic Principle.) Galileo's observations were necessary to find the law of fall of heavy bodies.

Nevertheless, Galileo was at the same time responsible for one of the more remarkable demonstrations of the power of a priori mathematical reasoning in dynamics. When first considering what law should be followed by falling heavy bodies, once it is accepted that they go faster as they fall, he wondered about how to distinguish between the two simplest theories: the perhaps most natural one that speed is proportional to distance travelled from the start, and the equally simple but perhaps less natural one that speed is proportional to time from the start (that is, the body is uniformly accelerated, which is the correct answer).

Galileo realized, and was able to demonstrate, that the first theory needs no observations to refute it. It is absolutely impossible that acceleration should be proportional to the distance travelled. Galileo argues thus:

> When speeds have the same ratio as the spaces passed or to be passed, those spaces come to be passed in equal times; if therefore the speeds with which the falling body passed the space of four braccia were the doubles of the speeds with which it passed the first two braccia, as one space is double the other space, then the times of those passages are equal; but for the same moveable to pass the four braccia and the two in the same time cannot take place except in instantaneous motion. (Galileo [1974]: 160; Norton and Roberts 2012)

That reasoning is less than totally clear. But it suggests a diagram, surely present in some form in Galileo's mind, which clarifies it.

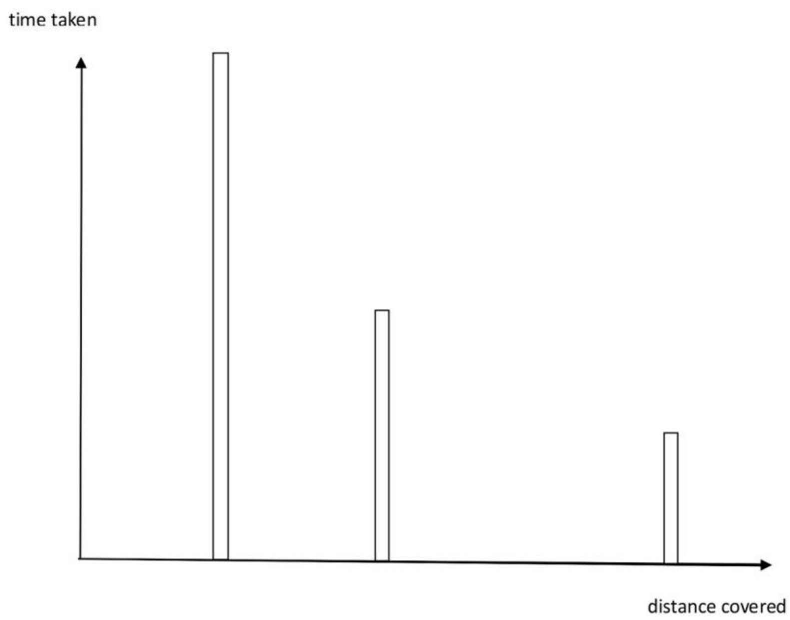

Fig 5 Galileo's proof that speed cannot be proportional to distance

The diagram should be read from right to left. If the time taken to cover a small distance is represented by the column of time above it (right column), then at half that distance, it needs twice the time, at a quarter the distance, four times, and so on. As distance approaches zero, it needs indefinitely more time to cover the same interval. So the motion can never get started.

From the falsity of the theory of the proportionality of speed to distance there does not follow, of course, the truth of the (true) alternative theory of the proportionality to time. But it leaves that theory as the natural simple alternative, guiding the effort of empirical confirmation.

A proper understanding of the matter required some principle to connect the cause of motion (a force) to the way the motion itself develops (the acceleration). Galileo did not attempt any such thing – indeed, he avoided thinking about forces in connection with motion at all, although he was familiar with them in statics. That was left to Newton. Before looking at Newton's mathematical principles, we consider the other area of physics in which early seventeenth century mathematical science had signal success, the physics of fluids.

Pressure/Hydrostatics

A priori symmetry arguments proved very successful in hydrostatics, the science of pressure in water. Stevin was again in the lead. There is something extraordinarily simple about how pressure in fluids works, sometimes called the "hydrostatic paradox".

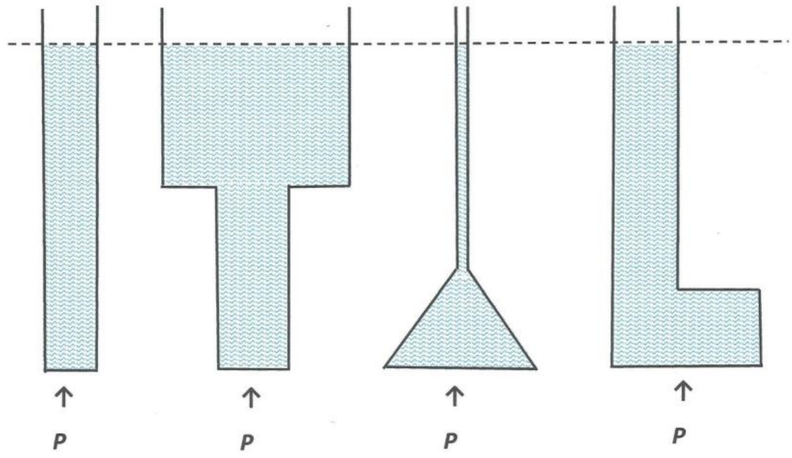

Fig 6 Modern illustration of the hydrostatic paradox: pressure at the bottom is equal, irrespective of the shape of the vessel above

As in Fig 6, one's initial intuition that different shaped vessels should support water differently is false. Pressure depends only on depth.

Why should such a simple law be true? As with the "wreath of spheres", Stevin offered a mathematical demonstration from first principles in place of empirical evidence.

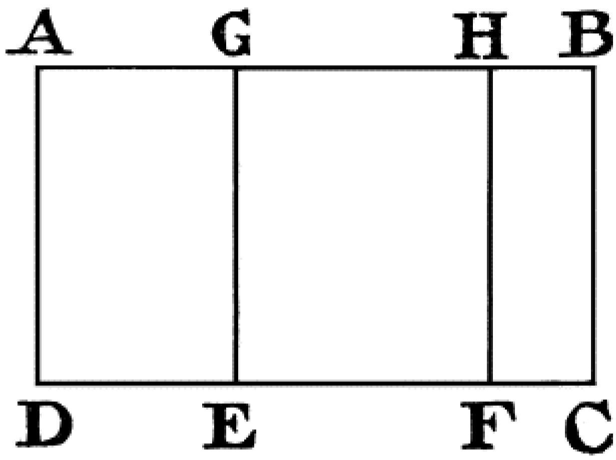

Fig 7a Stevin's diagram of water at rest on different vertical bottoms

(Gaukroger and Schuster 2002: Fig 1, reprinted with permission from Elsevier Science)

Stevin argues that in a rectangular tank ABCD as in Fig 7a, with imaginary vertical divisions GE and HF, there is no tendency of water to move across the vertical boundaries in either direction; hence, on each part of the bottom, such as EF, there rests the weight of the water directly above it. Next he considers a more complex figure of water resting on EF (Fig 7b).

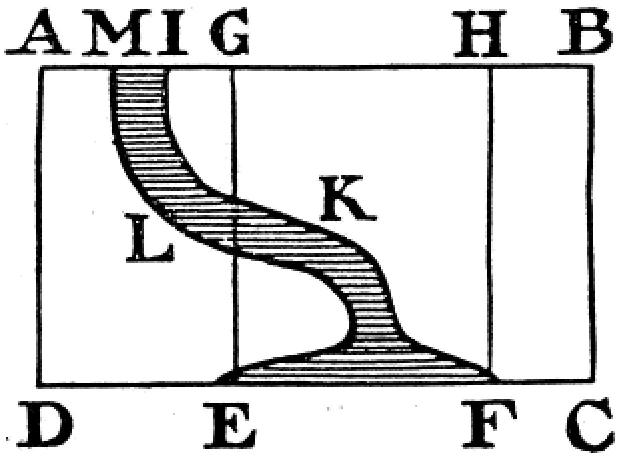

Fig 7b Stevin's diagram of a tube of water of complex shape resting on bottom EF

(Gaukroger and Schuster 2002: Fig 2, reprinted with permission from Elsevier Science)

He supposes that the parts of the water *not* in the shaded complex-shaped tube are replaced by a solid of the same density as water. That makes no difference to the forces in the water. So the pressure on the bottom EF, now caused solely by water in the complex shape above it, must be the same as before, that is, the weight of the column GHFE directly above EF. Therefore the pressure on the bottom depends only on the height of the column, not on its shape. (Gaukroger and Schuster 2002: 542-5)

Stevin's work impressed the next writers in the field such as Descartes and Pascal. Pascal put forward a demonstration of the proportionality of pressure to depth that is more local and calculus-like than Stevin's global symmetry argument. He also made sense of the brute empirical fact that there is a limit of 10.3 metres to the height to which water can be raised by a vacuum pump (again, irrespective of the shape of the column of water). Pascal explained it as an effect of air pressure – air being also a fluid with weight, and the differences between the pressures of air and water being due solely to the differences in their densities.

Practical Mathematics at Sea

Mathematics, as known in early modern times – and indeed today – was not just a theoretical study but in certain aspects a very practical and lucrative business. A snapshot of the state of mathematics during the "Mathematical Revolution", but at the very beginning of the Scientific Revolution, as usually calculated, can be found in Billingsley's Euclid, the first English translation, of 1570. It has a preface by John Dee, "specifying the chief Mathematicall Sciences, what they are, and wherunto commodious". He arranges in a tree the two principal sciences, arithmetic and geometry, and some thirty derivative sciences. (A reproduction of the diagram at http://www.math.ubc.ca/~cass/euclid/dee/dee.html) There are "vulgar" arithmetic and "vulgar" geometry, the latter divided into eleven sciences, concerned with surveying, in one, two and three dimensions, on both land and sea. There follow nineteen miscellaneous sciences, most with a strongly geometrical aspect (hence, these sciences are principled ones, not empirical ones like alchemy). Of these, perspective, astronomy, music, cosmography, astrology, statics and navigation are clear enough, but the remaining eleven have names now unrecognisable. "Anthropographie" is about the proportions in the human body; "Zographie" is something like the modern theory of rendering in computer graphics; "Trochilike" studies circular motions, simple and compound; "Hydragogie" "demonstrateth the possible leading of Water by Natures Law, and by artificiall helpe, from any head" and "Pneumatithmie" "demonstrateth by close hollow Geometrical figures (Regular and Irregular) the straunge properties (in motion or stay) of the Water, Ayre, Smoke and Fire." Some of these are plainly more commodious unto nascent capitalism than others, but the total picture is of a suite of mathematical, mostly geometrical, sciences, in common and successful use, with investors queuing. (Euclid [1570], Preface; survey of early modern practical mathematics in Bennett 1986) Two of the mathematical sciences that certainly did attract serious money, especially in England where it was already appreciated that naval superiority was the key to national survival, were navigation (Eden 1561; Dee 1577 – though more propaganda than technical navigation; Shirley 1985; Neal 2002) and the hydrostatics and hydrodynamics of hull design. Navigation is straightforwardly geometry on the sphere (plus the difficult task of finding where on the sphere one is located). Hydrodynamics is much harder. (Today fluid modelling and hull design are computer-intensive and lack simple principles.)

Hull design is heavily geometrical but with many complexities. For one thing, it has to take into account both hydrostatics (to make the ship float, high enough out of the water to ensure the gun ports do not take on water when the ship rolls, but stable enough so the ship does not roll over) and hydrodynamics (so it cuts through the water without excessive drag). Hydrodynamic and hydrostatic considerations are to some extent in conflict, since one can gain speed at the expense of stability. A famous picture by the Tudor shipwright Mathew Baker (fig 8) compares hull design to fish shape, but ships have different design requirements from fish and are not the same shape.

A nice image: Hull and fish shape compared, from Mathew Baker, *Fragments of Ancient Shipwrightry,* c. 1586

http://en.wikipedia.org/wiki/Mathew_Baker#mediaviewer/File:Matthew_Baker_-_Bionique.jpg

As was usual and natural, books such as Manoel Fernandez's *Livro de Traças de Carpintaria* (1616) (Fig 9) were highly geometrical but the text consists of recipes more than principles. There were attempts to apply to naval architecture the principles of the pseudo-Aristotelian *Mechanical Problems*, but success was limited. (Ferreiro 2010)

A nice image: Hull design from Manuel Fernandes, *Livro de Traças de Carpintaria,* 1616 (ms) (Lisboa, Biblioteca da Ajuda: BA 52-XIV-21, fl. 102, ©Biblioteca da Ajuda)

Deane's *Doctrine of Naval Architecture* (1670) is heavily mathematical and concentrates on hydrostatics. Problems were still experienced with buoyancy, as shown when the *Royal Katherine* at her launch in 1664 proved to have her gun ports only three feet out of the water before guns and provisions were loaded. (Deane 1670/1981: 16) Deane offers recipes for construction and formulas to calculate how far out of the water the resulting ship will be.

Ship design was typical of the more applied mathematical disciplines, in that the advantages of deriving results from basic mathematical principles were well appreciated, but the complexities of the subject-matter made it very difficult to achieve more than modest success. It was necessary to fall back on and cautiously adapt recipes that had proved successful for practitioners in the past.

The Scientific Revolution's Laws of Proportion

We will treat only lightly the high period of the Scientific Revolution when a large suite of scientific laws were discovered. These laws were mathematical in one sense, in that they ascribed to nature simple formulas – indeed, formulas in general of simple proportion. They were not *purely* mathematical, in the sense that (to the disappointment of some) they are not derivable solely from mathematical principle. They need some – however small – input of empirical and observationally-derived fact.

A list of the Scientific Revolution's laws of proportion, with approximate dates, includes:

- Kepler's Second Law: The area swept out by a radius from the sun to a planet is proportional to the time taken (1609)
- Snell's Law: When light is refracted at a surface, the sine of the angle of refraction is proportional to the sine of the angle of incidence (1602, 1621, 1637)
- Galileo's Law of Uniform Acceleration: The speed of a heavy body falling from rest is proportional to the time from dropping (1638)
- Pascal's Law: The pressure in an incompressible fluid is proportional to depth (1647)
- Hooke's Law: The extension of a spring is proportional to the force exerted to stretch it (1660)
- Boyle's Law: For a fixed quantity of gas at constant temperature, pressure is inversely proportional to volume (1662)
- Newton's proposition on the prism: there is some kind of proportionality between refrangibility and colour of light (1672)
- Newton's Second Law of Motion: The acceleration of a body is proportional to the total force acting on it (1687)
- Newton's Law of Gravity: The force of gravity exerted by one body on another is proportional to the masses of each and inversely proportional to the square of the distance between them (1687)
- Newton's Law of Cooling: The rate of temperature loss from a body is proportional to the difference in temperature between the body and its surroundings (1701)

Debate still proceeds on the relationship between empirical evidence and these very general proportionalities. It is the inherent logical difficulty of putting together the Euclidean model with the establishment of basic laws by experiment that gave rise to the endless problems over Newton's claim to have not "feigned hypotheses" – already in his first paper on the prism, which plainly rests on experiments, he insists that everything is "not an Hypothesis but the most rigid consequence". (Discussion in Walsh 2012) He kept to that view, creating a conundrum whose irresolvability provokes debate that shows no sign of ending.

Let us look briefly at the most celebrated case, Newton's laws of motion.

Newton's Mathematical Principles

Newton's *Mathematical Principles of Natural Philosophy* opens in the Euclidean style, with definitions and axioms. The definitions include:

> 1. The Quantity of Matter is the measure of the same, arising from its density and bulk conjunctly …
>
> 4. An impress'd force is an action exerted upon a body, in order to change its state, either of rest, or of moving uniformly forward in a right line.

The axioms or laws (*axiomata sive leges*) of motion are:

> 1. Every body perseveres in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impress'd thereon.
> 2. The alteration of motion is ever proportional to the motive force impress'd; and is made in the direction of the right line in which that force is impress'd.
> 3. To every Action there is always opposed an equal Reaction …

Deduction then proceeds:

Corollary 1: A body by two forces conjoined will describe the diagonal of a parallelogram … (Newton 1729: 1-3, 19-21; clarifications of Newton's use of the geometrical model in Dunlop 2012)

These principles have a number of strange features, both from the point of view of what has gone before and from the purely logical perspective.

First, if it is asked whether these are *mathematical* principles, in the self-evident sense of Euclid's geometry, or principles that need to be established by observation, then prima facie they are neither. They appear natural yet contingent, so not totally self-evident. Yet the observations that would establish them are hard to pin down, mainly because of the difficult logical and observational status of Newton's primitive, "action" or "force".

The notion of "Action" is primitive and its observational meaning unclear. That is what has led to modern physicists' persistent worrying whether the law $F = ma$ (the modern formulation of Newton's Second Law) is merely a definition of force and lacks real content. (e.g. Wilczek 2004) Plainly that was not Newton's view, as he thinks of action as a real cause "exerted on a body"; nor was it the view of philosophers like Reid who pointed to the possibility of direct perception of force as pressure on one's body. But Newton does not make clear how force may be measured other than through observing the acceleration it causes.

He hardly helps clarify matters with his gnomic Scholium: "Hitherto I have laid down such principles *(principia)* as have been received by mathematicians, and are confirmed by abundance of experiment." (Newton 1729: 13)

That is doubly false. They are *Newton*'s laws because they had not been received by earlier mathematicians, nor does Newton make clear how they may be confirmed directly by abundance of experiment. He merely refers to some phenomena explained by them, such as Galileo's law of fall and the period of pendulums. Only the success of the global project of explaining planetary motion in terms of the principles gives them, in some overall sense, confirmation. The view of his contemporaries is understandable, that Newton simply communed with Nature and understood her principles. (Except, that is, for a few such as Huygens, who remained completely unconvinced by Newton's principle of gravitation. Guicciardini 1999: ch. 5)

Probability

Probability is surely among the hardest of quantities to establish principles for. It was far from clear even that it was a quantity, in the sense of something to which a precise number could be assigned: surely the realm of uncertainty and chance of its nature resists pinning down with precision? And if we consider just the phenomena of games with dice and coins, the subject of Fermat and Pascal's 1654 correspondence that began the mathematical theory of probability, it is quite unclear what the quantity is to which a number should be assigned – the modern answer, the long-run relative frequency of outcomes, was not considered in Fermat and Pascal's work nor in any other of that century.

Thus the initial problem considered, very unnaturally from a later point of view, was a moral one: that of the just division of the stake in an interrupted game of chance. The quantity to be calculated is a moral right. Initially, there was a complete lack of principles or calculations, and no apparent place to start in finding principles. Only Pascal realised that one might establish a realm of mathematical certainties in a new and unlikely field, the fluctuating realm of uncertainty. He wrote:

> … the matter has hitherto wandered in uncertainty; but now what has been rebellious to experiment has not been able to escape the dominion of reason. For we have reduced it to art so securely, through Geometry, that, participating in [Geometry's] certainty, it now goes forth daringly, and, by thus uniting the demonstrations of mathematics to the uncertainty of chance, and reconciling what seem contraries, it can take its name from both sides, and rightly claim the astonishing title: the Geometry of chance (*aleae Geometria*). (Pascal 1654; Franklin 2001: 312)

Where might the principles be found? Pascal reaches for that old staple that had been so successful for Archimedes and Stevin, among so many others, in introducing a priori reasoning into applied mathematics, symmetry:

> The first principle for discovering how one should make the division is this:
>
> If one of the players is in a situation such that, whatever happens, a certain sum will belong to him in case of either a loss or a win, without chance being able to take it away from him, then he should make no division of it, but take it all, as assured . . .
>
> The second is this. If two players are in a situation such that, if one wins, a certain sum belongs to him, and if he loses, it belongs to the other; and if the game is of pure chance and there are as many chances for one as for the other, and consequently no more reason why one should win rather than the other, then if they wish to separate without playing, and take what legitimately belongs to each, the division is that they divide the sum that is at stake in halves, and take one each. (Pascal 1665; Franklin 2001: 311)

Soon Huygens, writing the first book on probability and establishing it as a coherent body of theory, also laid down a symmetry principle as the basis for all calculations:

> Although in games determined solely by chance the outcomes are uncertain, there is always a fixed value for how much one has for winning over losing … I take it as a foundation that in a game the chance (*sortem seu expectation*/*kansse*) that one has towards something is to be estimated as such that, if one had it, one could procure the same chance in an equitable game (*aequo conditione certans*). For example, if someone hides from me 3 shillings in one hand and 7 in the other, and gives me the choice of taking either hand, I say this is worth the same to me as if I were given 5 shillings. For if I have 5 shillings, I can again arrive at having an equal chance (*aequam expectationem*) of getting 3 or 7 shillings, and that by an equitable (*aequo*) game. (Huygens 1657; Franklin 2001: 314)

The ability of a priori symmetry arguments to establish absolutely certain principles even in such an unlikely subject-matter as pure chance was the most perfect demonstration of their power.

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