HOW CAN WE ASSESS THE SCIENTIFIC VALUE OF HOOKE'S WORK?

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The assessment of scientific value was very evident in the technique of rational reconstruction (Christie 1990, 13), and was an essential part of the internalist view of the history of science (Porter 1990, 35). These approaches assumed that science could be defined, and how a person's work contributed to it could be measured, in scientific terms. They have been abandoned for many reasons: their view through the filter of present knowledge obscured or distorted the record, they were prone to partial appreciation of people and their relations, to premature demotion of 'lesser' figures, to ignorance of knowledge that did not fit their construction of modern science, and to writing a legend of heroes (and a few villains). It is understandable that evaluation, with all these bad associations, no longer figures strongly in the literature. The present paper attempts to face and deal with the difficulties of evaluation, to set out some examples of how it might be done, and arrive at productive outcomes for science and its history.

The breadth of Hooke's work means that to evaluate it we must deal with theory, experiment, technology and discovery. There is no agreed view that will do for this. The received view (or legend) of science (Gooding 1989, xiv, Kitcher 1993) asserted in principle the supreme power of combining experiment with theory, but placed theory firmly in the foreground. This has been observed and challenged (Hacking 1983, Gooding 1989), and the close study of experiment has been successfully established. Useful ideas about discovery have been put forward (Nickles 1990), but that area is essentially hard to grasp. In its simplest versions (Bronowski 1951, 29-35) the received view associated creativity with theoretical genius, from whose ratiocination all new knowledge flowed. This is unsound both in principle and in practice (Wartofsky 1980, 2-5), but even those with a better appreciation of discovery (Polanyi 1983, 66-68) remain centred on methodical and analytic approaches, and those with a better appreciation of the record (Hunter 1981, 64-65) still associate imagination with theory.

The received view commonly turned a blind eye to technology (Westfall 1971, 45). Where it was paid any attention, the standing of technology has risen over the years from being beneath contempt (Sarton according to Hall 1996, 2), through being worthy of contempt (Hall as reported by McKendrick 1973, 282), to being condescended to (Hall 1996, 6), to being the source of mild amusement (Dear 2001). This forceful distancing is surprising given that more considered assessments (McKendrick 1973, 292ff; Keller 1993, 85; Keller 1984, 165; Bennett 1986, 24-25) demonstrate the close relationship between technology and science. It may be due to the perceived threat to profundity (Ravetz 1971, 39) and the higher life of the mind (Polanyi 1983, 88) posed by applications of science: there are few things more forceful than a close neighbour who feels under threat.

The relationships between science, technology and discovery remain deeply debatable (Cunningham 1993, 431), but are central to our concern. Hooke has always had acknowledged strong points: his inventiveness and his instruments (Westfall 1983, 90). More recently, his experimental craft and scrupulous procedures, and his professional practice (Cooper 1997, 2001) have been recognised as setting new standards for the times. His supposed weak points have been a lack of focus, understandable given his busyness, and a lack of rigour to the degree that his work generally lacks scientific value (Westfall 1983, 96-104).

Hooke's celestial mechanics is discussed in depth elsewhere (Patterson 1949 & 1950, Nauenberg 1994, Gal 1996) and will not be treated in the present paper. In the following paragraphs six accessible examples of Hooke's published work on mechanics and materials are outlined in terms of their technical content. Each is followed by an analysis that covers experimental and theoretical demonstrations. We will see that previous technical assessments have to be substantially revised and extended.

In 'An attempt for the explication...' (Gunther vol 10, 1) and 'On small glass canes' (Hooke 1961, 10) the subject of the enquiries, being the rising column of liquid in a minute tube, fits well with Hooke's interests in air pressure and in microscopy. The greater part of the observations is taken up with a description of surface phenomena associated with the rise: immiscibility, drop formation, wetting, mixing, surface minimisation, sessile drops, intermediate suspension, menisci positive and negative, surface contamination, surface suspension and forces between bodies so suspended. He conceives of these as all being due to congruity between materials, and claims that they prove an unequal pressure upon the liquid, that evidently would cause the rise. He further invokes the apparent rigidity in the liquid surface and a plausible reduced pressure of the air in the tube in his explanations, and settles on the latter as the key, but admits to not being able to calculate the various effects. The treatment in Micrographia proposes in addition mechanical ideas of the states of matter and the nature of heat.

The correct association of all these phenomena and their effects, and the lack of incorrect associations, is astonishing: it could be transposed with minor changes to a modern textbook. His discussion of heat is similarly modern. His *congruity* is identical to *surface energy* on which our current understanding is based, and his claim that it proves unequal pressures is sound, but not demonstrated. His concerns with surface rigidity and reduced air pressure are legitimate: the liquid surface is today seen as having a tension in it rather than a rigidity, and the pressure is seen as reduced, not in the tube, but in the liquid by its surface. There is no obvious way that these multiplied causes could be disentangled at the time or that their effects could be calculated. The phenomena can be explained by simply invoking *congruity,* but perhaps he preferred not to rest the explanation entirely on a novel concept that was not yet part of an overall view of energy (Henry 1989, 164). His final attachment to reduced pressure in the tube as an explanation might be associated with his (and Boyle's) investment in that area of expertise.

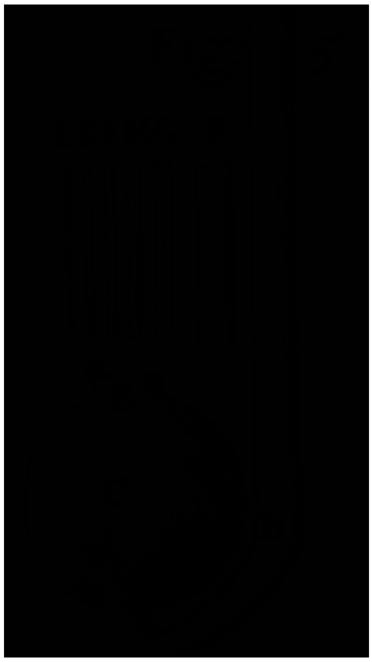

Figure 1 *This setup was not conducive to repeatable results*

His experimental proposals on pressure measurements (Hooke 1961, schem IIII, and Figure1 here) are not so impressive. The idea of using a J-tube to apply pressure to the air at one end of the small cane is good because it gives more control than the simple rising column configuration of the original phenomenon, but the manner of fixing the cane is not. His choice of this configuration may have been because it approached his theoretical picture, which was centred on the deformation of the liquid surface and the penetration of the air into the tube. In a reproduction of this configuration it proved possible to obtain a wide variety of pressures to force the air into the cane, the results being dependent on the amount of water in the cane, and on the conditions at its upper end. A simple rising column gave far superior results, showing the rise to be in reciprocal relation to the diameter of the tube. The tube diameters were measured using Hooke's technique for microscopic measurement, and his advice to wet the inside of the tube first was followed. This type of drawn tube is slightly tapered, so with just one tube carefully handled the reciprocal relation can be demonstrated.

In 'An account of how much descending bodies press upon the medium...' (Gunther vol 6, 91) an experiment is described that contrives a particularly sensitive verification of fundamental fluid mechanics by bringing the fluid medium into the balance (see Figure 2). It demonstrates that a steady state dynamic equilibrium can be maintained just as exactly as the familiar static equilibrium. Hooke reports no movement of the balance arm when the fibre suspending the weighted sphere is cut, and, concerned that this does not agree with the expected variability of resistance with speed, is tentative about the apparent conclusion that a moving body exerts its own weight on the fluid. His corollaries about the pressure of moving water would not have been novel to those who worked with it, but represent a sound line of reasoning from the experimental result.

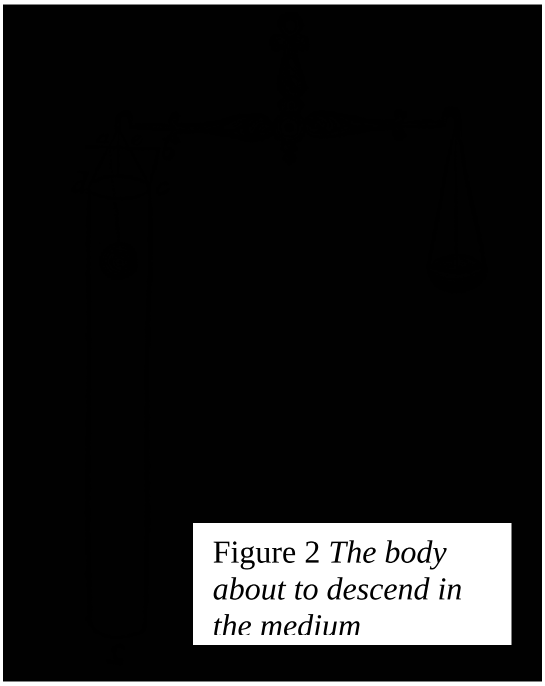

Figure 2 *The body about to descend in the medium*

The analysis of this experiment is a nice puzzle question in mechanics that has previously been incorrectly answered (Centore 1970). The lack of detectable movement was a product of the experimental conditions, which were set up to detect the smallest steady state disequilibrium through use of a sensitive balance and a small submerged weight (about 0.4gm) for the sphere. The downward motion of the sphere is actually accompanied by an upward excursion of the arm carrying the fluid container, but this excursion is at most 0.06 mm for the conditions, so not detectable. As Hooke surmised, the balance is only in equilibrium once the sphere has achieved its final speed. The excursion is given approximately by the distance the sphere travels multiplied by the ratio of the submerged weight to the total weights balanced. Had he experimented more freely with this apparatus, he might have had more food for thought. In a similar apparatus, with a sphere of submerged weight 50gm, an arm excursion of about 4mm was visible, coinciding with the descent of the sphere. With a smaller submerged weight the observed excursion is reduced, but its small size and duration makes quantitative results hard to obtain. Had he developed the apparatus, for example to use a longer descent, the size of the effect would have been amplified, and some measurements might have become possible. They could have been used to develop Wren's measure of proper motion, and extend it to fluids.

In 'Of glass drops' (Hooke 1961, 33 and schem IIII) Hooke takes a phenomenon that is striking, complex and commercially significant, and systematically investigates it. The resulting conceptual and experimental structure is magnificent, but can only be briefly narrated here: The sharp end was easily broken off the drop, which then shattered. Hooke carefully ground off the blunt end instead, leaving the rest of the drop intact. The pieces flew in all directions, so he developed a means to encase the drop and retain them. Despite the destruction, the drop could be reconstructed and the pattern of damage discerned. Annealing, familiar in the glass works, removed the effect. A plausible explanation based on differential cooling and thermal expansion was supported by reference to analogous situations, and an experiment constructed to test it. The explanation was applied to the pattern of damage, and shown to be consistent with familiar notions of the arch and of elastic energy (springiness). Experiments connecting the effect with the generality of expansion phenomena were described. His theory of heat was shown to be compatible with the observations. Applications to thermometry were given. Applications in casting and moulding processes remain today.

This account is a fine example to any student faced with an investigative project. The subject matter covers three dimensional heat flow, stress analysis and fracture mechanics, at a level which challenges modern analysis. The investigation shows how, even with only an elementary understanding of principles, theory and experiment can be deployed to elucidate a convincing explanation. It is perhaps the most transparent embodiment of Hooke's experimental philosophy.

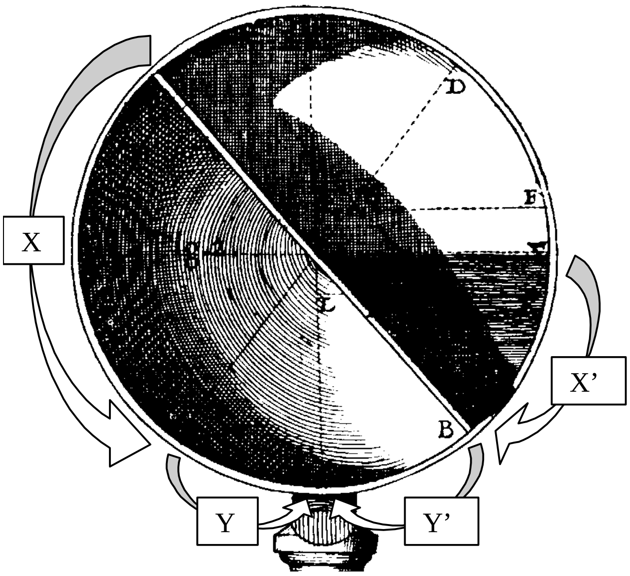

Figure 3. *The solid in* X *is half the density and double the space of the fluid in* X'*, so balances it.* Y *and* Y' *are equal and opposite.*

In 'Lampas' (Gunther vol 8, 155 and tab 1 p208) the themes of combustion and fluid mechanics are resumed, with demonstrations that both flame and flow yield their mysteries to experimental investigation. Earlier Cutlerian lectures applied invention to the instruments which extend our senses, but here it is applied to refine a laboratory tool, the oil lamp. At first sight, the rotating divided box device, which he proposes for maintaining a constant oil level (Bid p165), seems too ingenious to be true, but it is theoretically sound (see Figure 3).

The genesis of this device may have come from considering the familiar ascent of the arm of a balance when the weight on it is reduced. If the oil container is set on a balance, then as the oil is consumed its level will go down but the container will rise. If the two effects can be made equal and opposite a constant level will be maintained. For small angles of tilt, the desired outcome can be obtained by adjusting the sensitivity of the balance in the customary way, by adjusting its centre of gravity. For large angles of tilt, the liquid surface must not vary, and so Hooke's requirement that the box is a volume of revolution is obtained. Further, the condition that the level is correct when half

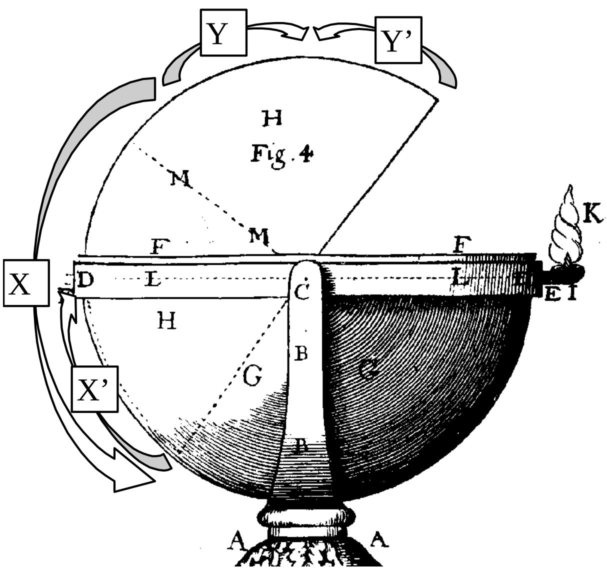

Figure 4. *The float* HH *balances whatever the container:* Y *and* Y' *balance as previously, and the solid in* X *has half the density and twice the space of the fluid displaced in* X'*, so is floated by it.*

full provides the weight and position of c.o.g. that, delightfully, ensures correct level at all times. The problem remains that the oil is in a box that moves, so it is not easily brought to the fixed flame. He proposed to feed it through a tapered gudgeon, which both seals and freely turns, at the axis of rotation.

His second device derives immediately from the first (see Figure 4). The dry half of the box becomes a float and the oil is placed in a stationary container. Hooke required that the container be a close fit with the float, which makes the geometry of the contained oil approximately the same as in the first device, so he can simply state that the same constant level effect will follow without demonstration. Hooke further proposes that the oil container be placed on gimbals, and the oil again fed through a rotating seal[1].

That simple statement is not valid for the second device, but a proper analysis (see Figure 4) shows that it does work, and better than claimed. The float works with equal precision in any shaped oil container, which makes it much more versatile, and easier to manufacture, than if a close fit were necessary. His freely rotating seal is a technical problem, however: the requirements for sealing and free passage of oil are quite opposed to those for lubrication and low resistance to rotation.

It has previously been stated (Westfall 1983) that the first device (and presumably *a fortiori* the second) does not work unless the dry half of the box contains a substance half as dense as the oil, and that Hooke is unable to distinguish the notions of weight and turning moment properly. Both statements are is quite untrue: so long as the weight and centre of gravity of the dry half coincides with that of a hemisphere of the specified substance, its action is the same, however it is constructed. As Hooke states (*Ibid.* p.166) "Let there be a counterpoise ... fixed somewhere in the line PO, so that the said upper hemisphere shall have half the gravity of the under hemisphere upon the centre of motion O". Hooke not only understands moments about a centre, but employs the concept of centre of gravity which was so lacking in Westfall's mistaken attempts at analysis. A proof of the second device has been obtained by constructing a working model according to the specifications in Lampas. Although simple in construction and of low precision, its operation was as claimed. In a test, 130ml of water was added to a vessel without the device, producing a rise of 13mm in surface level. With the device, the level remained constant to within ±1mm.

The rotating box, and its rotating float derivative, remain an inspiring example of creative mechanics. The logical development of the initial idea so as to deliver the final function demonstrates the highly intellectual nature of the technology. Hooke's final designs are also examples of how such inspirations can fail to be carried through into practical products. A further investigation and development of the rotating float would have led to a usable product, but to publish it would have meant rewriting Lampas. Perhaps Hooke preferred to publish rather than practice in this case.

In 'Of Spring' (Gunther vol 8, 331) we are taken shortly and easily through the proportionality of loading to extension for a few springy objects, and its relevance to applications. Next, a theory is presented in which an external menstruum is added to the vibrative theory of congruity so as to arrive at a quantitative analysis of the solid state and its elasticity. The applied force is seen to vary as the change of the inverse of the length from its natural state, and the analogous result for the pressure and volume of air is mentioned.

The analysis of a body attached to a linear spring is then attempted by consideration of the travel of the body from rest at a deflected position, back to the undeflected position.

[1] The earlier account of the lamp in Gunther vol 6, 295 relates to the second device, and applies the same argument to demonstrate it, but does not mention gimbals or gudgeons. The float is shown more than half submerged, which makes its analysis less simple than the one presented under Figure4 here.

Distance travelled is used as the base for the construction of several graphs, the first of which is a straight line depicting force. The square of the velocity is taken to vary as the area under this graph, and the graph of velocity is shown to form the arc of a circle. A graph of time is constructed as the ratio of distance to velocity, but indirectly, through construction of the parabola given by the square root of distance (CHHHF in Figure 5). The qualitative effects of more and less stiff springs are shown in this representation, and equated to the effects of inverse changes in mass. The consequence of release from a smaller deflection is depicted as the graph of velocity being a proportionally smaller arc of a circle, and as the measure of time scaling so that the total time of travel is unchanged. This measure is again constructed indirectly via the parabola, which does not appear to be scaled as the root of the initial deflection (AB_2 in Figure 5), instead it appears scaled as the root of the change in initial deflection (B_2C in Figure 5).

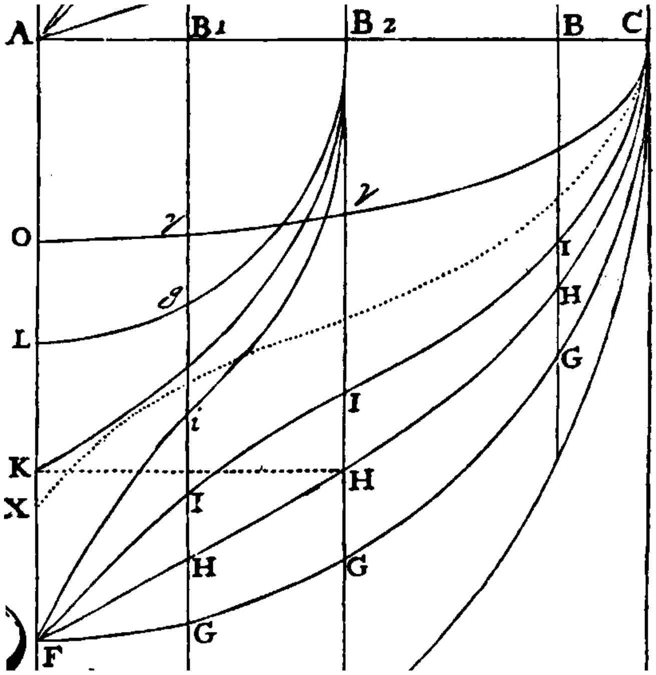

Figure 5. *Point* K *appears to be constructed by projecting down from* B_2 *to* H, *then across to* K. *This is wrong unless* $AB_2 = \frac{1}{2}AC$

The advanced status of much of the analysis has been pointed out (Patterson 1948): the relation between the square of the velocity and the area under the force-displacement curve is the integral form of the second law of motion. The smaller motion is calculated by scaling the circular arc of velocity, so that isochrony could be claimed immediately: if a velocity-distance relation scales, then the time of travel is unchanged. Hooke's ratio of distance to velocity is not actually time, but this ratio is nonetheless proportional to the period of a given phase of the motion, and is invariant with amplitude as Hooke asserts. His use of the parabola to construct that graph is hard to understand, since the ratio could easily be constructed directly. It is also the site of an unclarity, such that the construction for the smaller motion might easily be mistaken, whereas if an intermediate amplitude other than half that of the full motion were illustrated, the ambiguity would be removed.

Why did Hooke not employ the view of vibration as a projection of motion around a circle in 'On Spring'? His earlier extensive work on pendulums included the identification of a circular pendulum with two perpendicular linear ones (Gunther vol 6, 267-9, 285-6, 352). Later in life he published the circular viewpoint (Hooke 1971, 549), and stated correctly that time was distance around the arc. An answer might be that the proper integration of these kinematics into the mechanical analysis would require a view of differentials and functions that we associate with Leibniz.

Some previous commentary (explicitly Westfall 1983, 103, and implicitly Hesse 1966) fixed on Hooke's unsupportable claim for a complete graph of time against displacement, ignored Patterson's points, and concluded that the entire work was invalid. Such selectivity rather invalidates those commentaries. A similar focus on simple theoretical issues is found elsewhere: Moyer infers from 'On Spring' that Hooke claimed that his proportional law applied to air, and concludes that, since the inverse relation between pressure and volume that holds for air is mathematically quite different from the proportional relation, Hooke's mathematics must have been deficient (Moyer, 270) . Could the professor of geometry really be unaware of the distinction between a direct and a reciprocal relation? A careful reading shows that Hooke makes no such claim, but does rather fail to point out the limitations of his law in relation to air. More importantly, physical reality is closer to Hooke's presentation than to Moyer's: air is more linear over a larger deformation than any solid material, and Hooke's enthusiasm for it as a spring (Gunther vol 8, 35) is well placed.

In 'The preference of strait to bunting sails' (Hooke 1971, 563) we see one of few examples of mechanics from the later publications. Hooke sets up the analysis of fluid drag by analysing the change in motion of the fluid, as it is done today. The incident column of fluid, its density and velocity, and the course of its motion are all realistically considered. The fact that in making way the ship is effectively in a counter current of water, which can be analysed just like the current of air on a sail, is then pointed out. He further demonstrates that the power of the air will be perpendicular to the sail. The case where the way is perpendicular to the sail is considered, and the relative power of a direct and an oblique wind are then correctly deduced, from the projected area and the component of the change in motion perpendicular to the sail. The case where the sail is set at an arbitrary angle to the way is considered, but the analysis is referred. The demonstration that a straight sail is superior is obtained by consideration that if there is a preferred angle to the wind then it will hold all over a straight sail, but only over part of a bunting one.

The correctness of Hooke's conclusion, that power of the wind is proportional to the square of the velocity, is perhaps more due to empirical knowledge than to an exact analysis, since the latter could not be started without clear distinctions of force, momentum and energy, and has not been finished to this day. His avoidance of the problem of the sail set at an arbitrary angle shows him by now cautious: he might well have realised that a best angle should be obtained by a compromise between squareness to the wind and squareness to the way, but the maths of an optimisation was out of reach. His analysis of a curved sail omits consideration that the final direction of the wind off it is more advantageous than off the straight, but his conclusion that a straight sail performs better is one shared by yachtspeople today. The forces exerted on the rigging by a straight sail are disproportionately large, however, which was perhaps why the mariners of his day were less enthusiastic.

Previous technical commentary on this work has been so superficial (Westfall 1983) as to be of little interest. Its author, confused by the compactness of Hooke's treatment of the effect of water on the ship, chose to believe that the confusion lay with Hooke and found this reason enough to break off and mock the entire work. Unfortunately, this commentary has been echoed in a recent popular biography (Inwood, 2002).

The present study provides something towards the reassessment of Hooke's contribution. His work on surface phenomena and thermal stress should be better known. This work beautifully embodies the gathering and correct association of evidence, and the construction of an experimental programme, in the form in which they are to be found at the heart of modern scientific and technical investigation. The analytic-synthetic unity which marks out modern engineering is seen in 'Lampas' with particular clarity amongst his other architectural, design and instrumentation work. His ability to frame relevant problems and rationally analyse them, despite their being beyond contemporary science and maths, is an example to technologists who are in the same position today. The present study also reveals some limitations in Hooke's supposed strong points: experimental configurations that are restricted or become unworkable due to theoretical preconceptions, designs that are flawed in vital detail or not carried through to a finished product.

We have also seen that some established technical commentary on Hooke’s work has either been grossly mistaken or been too narrow in its approach. Such inadequacies in technical evaluation are not exceptional, and call for explanation. They may well arise from the following methodological problem: The scientific training that is prerequisite for evaluation incorporates the ‘simple linear model’ (Ziman 2000), used by laymen and many scientists (Physics World 2001 p27-48), that science first originates, and then technology utilises. Historians are agreed, with some qualifications (Jacob, 320), that this lay view is not valid for the seventeenth century, and push its period of validity forward as far as 1880 (Hall 1978, 99) although by this time there is widespread scientific activity that is explicitly driven by technology, so its validity remains questionable even then. Thus the scientific training that was used for evaluation embodied historical viewpoints that were not accepted by its users.

This methodological problem has led to various contortions. The received view replaced the lay view that it is science that originates, by the view that although technology does originate, its output is not science. This invokes other simplistic views, that a truth dimly grasped is separable from a truth demonstrated, and that the former is not part of science, views that do not stand up to the most cursory study of discovery. In this way the status of historical figures who elaborated and refined was elevated at the expense of those who formulated the initial concepts. Meanwhile, the teachers and textbooks from which scientists (and, presumably, historians) learn (Hutchings 1991, 121; Dobson et al 1997, 31) portray the formulation of the initial concept as more praiseworthy than the working out of the mathematical detail. They also convey that, historically, both were done by the same theorist.

This belief is, of course, easily dispelled by better acquaintance with the record. What lingers, however, is the structure of the subject as a series of precise laws to be learned and produced at the appropriate stimulus. In the present paper I have attempted to go beyond this textbook view, and show that technical assessment can provide deeper understanding of the accounts, and can produce questions as much as judgements.

A view arises from the present study that might be more widely applicable. Science appears as technology carried out with greater depth. Thus technology is first to achieve a dim understanding, and science is first to achieve a full one. Chronologically and causally, technology gets there first, and drives discovery forwards. Technology has to make do and mend, until science clarifies and organises. These strands remain distinguishable even in the close integration of the modern era. This view retains the need to defend resources for science to go deep, but does not support the notion of science for its own sake.

This view calls into question how physical science is presented, both to specialists and others. The laws of motion, for example, are learned right at the beginning of a physics course although historically they were not formulated until long after mechanical devices were being designed that were far too complex to be described by them. Elementary science students are baffled and disheartened by the “laws first” textbook presentations (Arons 1990, 17) and find it inimical to clear thinking and inspiration (Orton and Roper 2000, 128). It appears that the current syllabus is without pedagogical, conceptual or historical justification. It is still the common basis for the assessment of scientific value in historical studies.

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