Qualitative novelty and the scientific revolution: The emergence of the concept of pressure

By: Alan Chalmers

In this paper I interpret the mechanical philosophy, not as an attempt to replace one, Aristotelian, account of the ultimate structure of the material world by another, mechanical, one but rather as an attempt to extend knowledge of such things as levers and clockworks that were archetypical mechanisms in the common sense of the term. At the dawn of the scientific revolution knowledge of that kind had been securely established and mathematically theorized in the area of statics, yielding a unified theory of such mechanisms as balances, levers and pulleys. The question I address is the extent to which extension of mechanical knowledge was capable of yielding in the seventeenth century the kind of novelty that might warrant the term 'revolution'. More specifically, I focus on the extension of statics to include hydrostatics via the introduction of the concept of pressure.

The fact that the issue calls for some finely-tuned historical and philosophical analysis is bought out by highlighting puzzling features of the relationship between two early versions of hydrostatics, *The Elements of Hydrostatic*'s published by Simon Stevin in 1586 and the treatise *On the Equilibrium of Liquids* composed by Blaise Pascal around 1653 and published posthumously in 1664. The former reads as a text modeled on Euclidean geometry. Theorems are derived from postulates with the aid of many geometrical diagrams. Applications of the theory to novel situations are treated by Stevin not as providing evidence for, but as applications of, it. By contrast, Pascal explains a range of hydrostatic phenomena, both novel and already familiar, in a way that is justified by an appeal to experiment. There is no explicit appeal to mathematics and there are no geometrical diagrams. Pascal's text would not be out of place in an introductory course on hydrostatics today. But the significance of these striking differences can be countered. All of the consequences of Pascal's theory are in fact consequences of Stevin's, or some modest extension of it. Many of the experiments appealed to by Pascal are modified versions of those described by Stevin (under the guise of practical applications). What is more, for all his emphasis on the experimental basis for his theory, there are reasons for doubting that Pascal actually performed the most significant of the experiments he describes! These latter points need to be dealt with if we are to read Pascal's hydrostatics as a significant early move in the revolutionary transformation of science in the seventeenth century.

Anyone wishing to develop a theory of hydrostatics late in the sixteenth century could take a mathematical science of weight for granted. They could also take for granted a common-sense distinction between solids and liquids, including some puzzling phenomena such as the balancing of unequal volumes of liquid communicating via a common vessel. Solids and liquids are alike insofar as they possess weight. What was needed was a characterization of the distinguishing feature of liquids that differentiates them from solids and which could be added to weight to yield foundations for a science of hydrostatics. The fact that that move was far from obvious is apparent from the shortcomings of hydrostatics as formulated by such able thinkers as Galileo and Descartes.

Stevin's hydrostatics can be challenged on the grounds that it appealed to questionable principles, such as his version of the impossibility of perpetual motion, and to arguments involving thought-experiments that lacked deductive rigor. But even if his derivations are conceded, there are some telling objections remaining. The additions to weight that need to be made to yield a hydrostatics were made by Stevin, not by way of an explicit and succinct characterization of the distinguishing feature of liquids, but by drawing on common sense knowledge of properties of liquids (such as the fact that they flow) in an opportunist and ad hoc way. Further, the reductio character of the arguments that he employed had the consequence that he failed to reveal the causality lying behind the phenomena described by his theorems, including novel phenomena described by Stevin as practical applications. If a mechanical explanation involves a grasp of the mechanism that links cause and effect, paradigmatically involved in the understanding of how clocks work, then Stevin did not supply a mechanical explanation of hydrostatic phenomena. (When Beeckman and Descartes evaluated Stevin's hydrostatics in 1618 they explicitly raised the latter objection, with Beeckman complaining that Stevin 'was too devoted to mathematics and dealt too rarely with physics'.)

In the first half of the seventeenth century figures such as Galileo and Descartes sought to identify fundamental principles on which to base their hydrostatics. These included the inverse proportionality principle, exhibited, for instance, by the movements about an equilibrium position of the weights on an unequalarmed balance, and the principle that a system moves spontaneously under gravity only if that motion involves a lowering of the centre of gravity of the system. These principles are restricted to the action of weights and the displacements involved are in a vertical direction only. For that reason attempts to extend application of the principles to the isotropic forces involved in hydrostatics were problematic and met with very partial success.

It is in Pascal's Treatise that we find the above deficiencies overcome. Pascal makes it clear that by virtue of their 'continuity and fluidity' liquids transform forces applied to them, whether stemming from their own weight or applied externally, into isotropic ones that are transmitted throughout the liquid in such a way that the force per unit area is conserved. In short, Pascal introduced the notion of pressure as a cause of hydrostatic phenomena in addition to weight. The adequacy of the theory was to be borne out by experiments, a number of which were identified by Pascal.

But what are we to make of the fact that Pascal may not have bothered to carry out those experiments? Here I appeal to a notion of theory confirmation that was mentioned by Descartes and which I believe can be taken as representing views that were intuitively held at the time. According to that view a claim is confirmed to the extent that it can be successfully applied to a diverse range of cases in a natural, rather than contrived, way. On this view, it makes no difference whether or not knowledge of the cases precedes or postdates knowledge of the claim and it also makes degree of confirmation a matter of degree. Adopting this view, Pascal's hydrostatics was significantly confirmed by virtue of the natural way that it could explain a wide range of phenomena, including puzzling phenomena, that had been known for many decades. That is why Pascal could be confident that the experiments he described would conform to his predictions. When Robert Boyle performed his own versions of Pascal's experiments he did in effect extend the degree to which Pascal's theory was confirmed, but in a way that would have come as no surprise to Pascal. What is more, Boyle's success shows that Pascal's theory was supportable by experiment in just the way he claimed it was.