

The Discovery of Boyle's Law, and the Concept of the Elasticity of Air in the Seventeenth Century

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Communicated by J. RAVETZ

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I. Introduction

The events which led to the formulation of BOYLE'S Law have been described frequently in histories of science, and the revolutionary features of the Law have caused its widespread use as an example in discussions about the nature of laws in science. However, there are notable deficiencies in our knowledge of certain aspects of the background to BOYLE'S work, and it is too readily assumed that BOYLE'S interpretation of the Law was similar to the twentieth-century view.

It has long been realised that the foundation for BOYLE'S work was laid by the experiments on air pressure which are popularly connected with TORRICELLI and PASCAL. Their work has been discussed in detail during the present century, and it is now realised that the concept of air pressure was of interest to many French and Italian natural philosophers in the mid-seventeenth century. PASCAL and TORRICELLI are consequently regarded as the major contributors to a co-

operative enterprise, in which the experimental and theoretical work of their contemporaries is of the greatest significance.¹

During this period, consideration of the physical properties of a air was inseparable from discussion of underlying assumptions about the physical construction of the Universe and the general principles operative in nature. Each development in the experimental aspect of the study of the nature of air had a profound impact on philosophical discussions, and the mutual interaction between these two aspects of the problem had an important influence on the eventual concept of the nature of air. Only slowly did the experimental investigation become divorced from its philosophical origins.

The initial impetus of the Continental work was largely lost by 1655. But the new generation brought the growth of experimental science in England, and the studies on air pressure culminated with the publication of BOYLE'S Law in 1662. This final stage has received less detailed treatment than has the Continental work, although the contributions of BOYLE, and his assistant, HOOKE, have been described on many occasions. In this article I will give a more detailed account of the English researches than has previously been attempted, giving particular emphasis to the problems which have been inconclusively considered.

I will begin by considering the origins of the concept of the elasticity of air, as distinct from its weight, by the European investigators, during the great burst of experimental investigation which occurred between 1640 and 1650, which resulted from the revival of the vacuist-plenist controversy. The introduction of these experiments into England will then be examined, followed by a detailed assessment of the work on air pressure of HENRY POWER and RICHARD TOWNELEY, who pioneered the English experimental studies. Finally, there will be a detailed discussion of the rôle of the various investigators who aided BOYLE in his search for a quantitative expression of the elasticity of air between 1658 and 1662. It was in this last stage that I derived most benefit from other accounts,² but I have been able to introduce supplementary material, particularly in respect to elucidating the rôles of HENRY POWER and RICHARD TOWNELEY in BOYLE'S work.

II. Philosophical Background: The Early Experiments Concerning Vacua

At the beginning of the seventeenth century physics was dominated by the Aristotelian principles as bequeathed by the neo-scholastic philosophers of the

¹ The major works dealing with PASCAL and TORRICELLI include: C. DE WAARD, *L'Expérience Barométrique, ses antécédents et ses explications*, Thuars, 1936. W. E. KNOWLES MIDDLETON, *The Place of Torricelli in the History of the Barometer*, *Isis*, 1963, 45, pp. 11—28; *The History of the Barometer*, London, 1964. C. THUROR, *Les Expériences de Pascal sur le vide et la pesanteur de l'air*, *Journal de Physique*, Ser. 1, 1872, 1, pp. 267—271. P. DUHEM, *Le Principe de Pascal, essai historique*, *Revue Générale des Sciences*, 1905, 16, pp. 599—610. F. MATHIEU, *Pascal et L'expérience du Puy de Dôme*, *Revue de Paris*, 1906, 13, Part I, pp. 565—589, 772—794; Part III, pp. 179—206; 1907, 14, Part II, pp. 176—224, 367—378, 835—876. G. MIHAUD, *Pascal et l'expérience du vide*, *Revue scientifique*, Ser. 5, 1907, 7, pp. 769—777.

² The following accounts are concerned with BOYLE'S work between 1660 and 1662. W. S. JAMES, *The Discovery of the Gas Laws; I, Boyle's Law*, *Science Progress*, 1928, 23, pp. 263—272. D. MCKIE, *Boyle's Law Endeavour*, 1948, 7, pp. 148—151. R. G. NEVILLE, *The Discovery of Boyle's Law, 1661—1662*, *Journal of Chemical Education*, 1962, 39, pp. 356—359.

previous half century,³ and the degree of acceptance (in academic circles at least) of hypotheses derived from the growing experimental science was largely determined by their consistency with these principles. Thus, the appearance of experiments which apparently supported the Democritean notion of vacuum, caused considerable debate, providing a powerful test of allegiance to the Aristotelian orthodoxy.

ARISTOTLE⁴ had elaborately refuted the views of the Democritean atomists, who had accepted that a space could be completely deprived of body to produce a macroscopic void, or *vacuum coacervatum*. They had also argued that motion could not occur without the existence of such a vacuum, and that light was a phenomenon caused by the rapid motion of corpuscles in the void. Expansion and differences in density were caused by void in its subdivided state, or *vacuum disseminatum*, and decrease in density was caused by the increase of the dispersed vacuum.

ARISTOTLE replied that there was no void separate from bodies, and that there could be no void occupied by any body, or existing in a body.⁵ For this negation of the idea of vacuum he relied upon the concept of "natural movement", believing that a void could not engender the natural upward or downward motion of simple bodies, or decide their direction or mechanism of propulsion. The velocity of movement of a body through space was determined by its weight and the resistance of the medium. Thus a body would pass through a void instantaneously, also there would be no difference in the velocity of a light and heavy body in a void.

Neither was the space occupied by a body a void since, even if the body could be separated from its attributes, such as heaviness and lightness, it would still occupy the same volume; thus the suggestion that a space containing matter was a vacuum was superfluous.

In denying the interstitial vacuum he reached the conclusion that change of volume occurred by condensation and rarefaction. Thus, when water was turned into "air", it did not add external matter but the water actualised its potentiality for becoming air. Change in volume was thus assimilated into the category of qualitative changes. He illustrated this by the example of the interconversion of air and water, which showed that, by rarefaction and condensation, the same quantity of matter could alter its bulk.

Similarly the matter of a body may also remain identical when it becomes greater or smaller in bulk. This is manifestly the case; for when water is transformed into air, the same matter, without taking on anything additional, is transformed from what it was, by passing into the actuality of that which before was only a potentiality to it. And it is just the same when air is transformed into water, the transition being from smaller to greater bulk, and the other from greater to smaller.⁶

³ The most notable works written in this tradition were the series of commentaries published by the Jesuit College of Coimbra; the importance of the influence of these works in the seventeenth century has been noted by É. GILSON, *Études sur Le Rôle de la Pensée Médiévale dans la formation du système Cartésien*, Paris, 1951.

⁴ ARISTOTLE, *Physics*, 213b, 20—214b11.

⁵ *Ibid.*, 214b 12—217b 28.

⁶ *Ibid.*, 217a 26—217a 31. Translation from ARISTOTLE'S *Physics*, by P. H. WICKSTEAD & F. M. CORNFORD, London, 1929, pp. 366—369.

Thus the expansion and contraction of matter involved no break in the continuity or plenum of nature.

The Aristotelian explanation of light also was relevant to the seventeenth century discussions of the vacuum. The Democritean theory of light did not preclude its transmission through a completely empty space, but Aristotle proposed that light was the actualisation of the potentially transparent medium, such as air or water. Thus, the transmission of light required a continuous medium, and any space lacking that medium would be invisible.⁷

The revival of the atomist philosophies during the renaissance brought increasing criticism of Aristotelian plenism, particularly with the popularity of LUCRETIUS' *De rerum natura*, which had been rediscovered in the fifteenth century. Interest in atomism led to the important Italian experiments which attempted to produce an artificial vacuum, this development being certainly stimulated by GALILEO'S contemplation of the Scholastic principle of nature's abhorrence of vacuum.

He concluded that *vacuum disseminatum*⁸ was an essential aspect of matter; he possibly reaching this conclusion during his consideration of the problem of the cause of cohesion of bodies, and he reinforced it with an explanation of the paradox of the *rota Aristotelis*. He believed that it was possible to discover an infinite number of vacua within a finite space, and since it was possible to divide a line by an infinite number of indivisible spaces, so it was possible to divide a three-dimensional body into an infinity of atoms, interposed between an infinite number of empty spaces.

However, GALILEO, like DESCARTES, avoided building a theory of matter on the assumption of a Democritean vacuum. In a passage in the *Discorsi* he hinted that he would have done so if it were not for the unacceptability of this hypothesis on general philosophical grounds.⁹ This was written at a time when Democritean atomism was universally considered atheistic. This attitude was only reversed later in the seventeenth century, although atomism had been tolerated during Islamic times.

He built his theory of cohesion in bodies (for which he had great hopes of practical application)¹⁰ on the assumption that every sort of matter had a characteristic upper limit of resistance to rupture.¹¹ He estimated this in the case of solids by computing from experimental values, the height of a column of the substance which would be on the point of breaking under its own weight if held at the top. Copper yielded the value of 4801 cubits.¹²

Liquids, including water, were considered as bodies whose cohesion due to micro-structure was zero, and whose breaking strength, as measured by the maximum height of a column, gave a true measure of the "force of the vacuum".

⁷ ARISTOTLE, *De Anima*, 418a 31—418b 12.

⁸ GALILEO, *Discorsi e Dimonstrazioni Matematiche, intorno à due nuove Scienze*, Leyden, 1638. English translation, *Dialogues concerning Two New Sciences*, by H. CREW & A. DE SALVIO, New York, 1914; pp. 20—26.

⁹ *Ibid.*, p. 12.

¹⁰ J. J. FAHIE in *Studies in the History and Method of Science*, ed. C. SINGER, 2 vols., Oxford, 1921; vol. 2, pp. 207—220.

¹¹ GALILEO, 1638, *op. cit.*, pp. 16—17.

¹² *Ibid.*, p. 17.

The craft experience of the limited effective height of lift-pumps may have helped him towards this conclusion, but such evidence was confused and unreliable, since real or imaginary pumps of other designs were in principle capable of lifting more than the 18 cubits of water.¹³

Working from the above argument, GALILEO assimilated the phenomena of pneumatics to those of cohesion in the solid state, and was consequently led away from the explanation which, soon after his death, became the most fruitful and essentially correct one.

However, he had fully emancipated himself from the idea of nature's complete abhorrence of vacuum, opening the way to the experimental production of vacuum by suggesting that a column of liquid had only a limited resistance to vacuum. In the same work he confirmed ARISTOTLE'S experiment to show that air had weight, concluding that water was 400 times as dense as air.¹⁴

GALILEO had certainly been influenced by his discussions with GIOVANNI BALIANI (1582—1666), who was one of the most influential Italian proponents of vacuum. He too had reached the conclusion that the atmosphere exerted a pressure on the earth as a result of the failure of the siphon experiment on high hills. A similar idea had also occurred to ISAAC BEECKMAN (1588—1637) in Holland, who evolved the model of air which was supposed to be like a large sponge surrounding the earth. By using this analogy he introduced the valuable concepts of the compressibility of the lower layers and the weight *and* elasticity of the whole mass of air.

It cannot be denied that the lower part of water or air is more strongly compressed than the upper part, since it is compressed by its own weight, as would happen to an immense sponge, its lower part lying on the earth is packed more tightly than the upper part. But this cannot be of great importance in the case of air which by its nature cannot easily be overcompressed and which is not very weighty. Nevertheless, it is necessary to believe that the lower part is as compressed as it could be, by the upper air, and consequently there exists a greater compression at its base.¹⁵

It is probable that BEECKMAN'S theory of air pressure, although it was expressed in his private *Journal*, was introduced to his friend DESCARTÈS, who soon proposed that air was analogous to a pile of wool fleeces, and this same analogy occurring independently to TORRICELLI. Thus from the inception of the seventeenth century investigations of air pressure, there appeared a model which favoured the concept of the elasticity of air.

Soon after the publication of GALILEO'S *Discorsi* in 1638, the Italian investigators turned their attention to the experimental production of a vacuum. The first successful apparatus was probably devised by GASPARO BERTI (b. 16 . . d. 1643 ?) in Rome, the experiment being performed by him and his distinguished collaborators before 1642.¹⁶ The apparatus was a long glass tube, expanded into a small globe at the upper end, and the openings at either end could be closed by brass screws. The tube was probably about 33 feet long. The whole tube was filled with water and the lower end immersed in a tub of the same liquid.

¹³ C. DE WAARD, 1936, op. cit., pp. 73—74.

¹⁴ GALILEO, 1638, op. cit., pp. 79—80.

¹⁵ ISAAC BEECKMAN, *Journal*, 24 December 1620—19 February, 1621, quoted from C. DE WAARD, 1936, op. cit., pp. 155—156. Translated from the French.

¹⁶ *Ibid.*, 101—109. — W. E. KNOWLES MIDDLETON, 1963, op. cit., pp. 14—16.

Upon the lower tap being opened, the water descended from the globe, to stand in the tube at about 18 cubits over the surface of the water in the tub.

The experiment was performed privately, and publications relating to it were delayed and contradictory. Also, as an experiment conceived as an *experimentum crucis* between plenists and vacuists it showed many confusing features, characteristic of a first trial. The supposed vacuum in the globe transmitted light, magnetism and sound (this last because of conduction by the wooden frame supporting the bell). Moreover, strange sounds were heard as the liquid descended (which were due to the release of dissolved air). Also the height of the water changed overnight.

Further, the question of Democritean atomism was, at this time, as delicate as that of the Copernican hypothesis, and this discouraged discussion and publication of the experiment.

III. The Torricellian Experiment and Its Conflicting Interpretations in France, 1645—1648

In two letters of June, 1644, EVANGELISTA TORRICELLI (1608—1647)¹⁷ proposed a more elegant form of this experiment, using mercury and a tube only three feet long. The experiment was performed for him by VINCENZO VIVIANI (1622—1703), and it was found that the mercury was supported to a height of 29 inches. Further, he had designed this experiment to test the "ocean of air" hypothesis, predicting that under this hypothesis, water would be supported to a height of 32 feet. He had no inhibitions about adopting the concept of vacuum; the absence of matter in the space above the mercury could be shown by replacing the mercury by water, which completely filled the tube.

TORRICELLI'S work thus assimilated the various fragmentary ideas and experiments of his contemporaries into a hypothetico-deductive framework, and his letters, which were addressed to MICHELANGELO RICCI (1619—1682), became the effective propagators of the concepts of vacuum and the weight of air, the contributions of BERTI and his collaborators in Rome being overlooked. RICCI was in contact with MARIN MERSENNE (1586—1648) of Paris, who was Europe's leading publicist of scientific information, and it was in the more tolerant atmosphere of France that the unorthodox idea of vacuum was subjected to detailed scrutiny.

In 1644, RICCI sent MERSENNE extracts of TORRICELLI'S letters, but there was little practical response in France until after MERSENNE had visited Italy in 1644 and 1645.¹⁸ In 1645 he visited Florence and attended a demonstration of the experiment, and during his stay in Florence and Rome he had scientific discussions with the major participants in the Italian researches.

¹⁷ EVANGELISTA TORRICELLI; the letters were written on 11th and 28th June, 1644, but they were not published until 1663; CARLO DATI, *Lettera a Filaleti di Timauvo Antiate de la vera storia della Cicloidè famosissima esperienza dell'argento vivo*, Florence, 1663; also in *Opere di Torricelli*, ed. G. LORIA & G. VASSURA, vol. 3, 1919, pp. 189—190, 198—201. English translation in *The Physical Treatises of Pascal*, ed. J. H. B. SPIERS & A. G. H. SPIERS, New York, 1937, pp. 163—170.

¹⁸ C. DE WAARD, 1936, *op. cit.*, pp. 117—119. — P. LENOBLE, *Mersenne, ou la naissance du mécanisme*, Paris, 1943, pp. 118—119.

Upon his return to France in March, 1645, he publicised information about TORRICELLI's experiment, while losing sight of the contributions of BERTI and other Italians. Since the course of the ensuing experiments in France has been well documented, I will concentrate on the growth of the idea of the elasticity of air.

The Torricellian experiment was first performed in France by PIERRE PETIT (1589—1677), the engineer in charge of the fortifications at Rouen.¹⁹ This was at Rouen in October, 1646. It marked the inception of an intensive series of investigations of the physical properties of air, which provides one of the pioneer examples of the collaborative approach to experimental research and which was symptomatic of the trend which resulted in the foundation of the scientific academies in this century.

Although the French experiments are usually associated with BLAISE PASCAL (1623—1662) and his brother-in-law, FLORIN PERRIER, an important part was also played by MERSENNE, ETIENNE NOËL and ROBERVAL, while contributions were made by ADRIEN AUZOULT, PETIT, JACQUES PIERIUS, JEAN PECQUET, as well as DESCARTES and GASSENDI who utilized the experimental evidence in their rival philosophies of nature. Through MERSENNE, the discussions were carried far afield, to England, Holland, Italy and Poland.

The initial French experiments were concerned with the repetition of TORRICELLI's work and the verification of the hypothesis of the weight of air. In 1647 PASCAL confirmed TORRICELLI's prediction by showing that air supported a 33 foot column of water or wine,²⁰ and MERSENNE compared the densities of water and air, concluding that water was 1,356 times as dense as air.²¹

There was far less agreement over the interpretation of the space above the mercury or "apparent vacuum", in view of the powerful Aristotelian arguments against absolute void. In 1647, even PASCAL was unwilling to adopt the vacuum theory, although he noted that the "experimental and Democritean" vacua had many features in common, but against these similarities he placed the traditional arguments against vacuum.

MERSENNE, who was otherwise one of ARISTOTLE's most stringent critics also became increasingly doubtful about the idea of vacuum, during 1647 and 1648.

The opposition to the idea of vacuum came from two distinct quarters, the one proposed that the space was filled by a *materia subtilis* or aether, the other, that it was filled with rarefied air. The former view was adopted by DESCARTES and ETIENNE NOËL, and it was widely influential in England upon such diverse authors as HENRY MORE, POWER, and NEWTON. DESCARTES and NOËL proposed that the descent of mercury was associated with the entry of a subtle matter into the space, which passed either from the walls of the tube,

¹⁹ C. DE WAARD, 1936 op. cit., p. 119. — P. PETIT, *Observation touchant le Vuide faite pour la premier Fois en France contenue en une lettre écrite à Monsieur Chanut Resident pour sa Majesté en Suede, par Monsieur Petit, Intendant des fortifications, le 10 novembre 1646*, Paris, 1647. This is given in *Oeuvres de Blaise Pascal*, ed. L. BRUNSCHWIG & P. BOUTROUX, Paris, 1904—1914; vol. 1, pp. 329—345.

²⁰ BLAISE PASCAL, *Expériences Nouvelles touchant le Vuide*, Paris, 1647; *Oeuvres* op. ct., vol. II, pp. 74—76.

²¹ MARIN MERSENNE, *Cogitata Physico-Mathematica*, Paris, 1644. "De Hydraulicis et Pneumaticis phenomenonis", Propositio XXIX.

or through the mercury. To them the idea of vacuum was philosophically inadmissible since extension was a defining property of matter.

There was however the second, more popular explanation of the space, which was more strictly in accord with Aristotelian physics. It proposed that the space was a manifestation of the great powers of expansion or rarefaction of air, when released from the compression of the upper layers of the atmosphere. This theory had the twin advantages of conformity with Aristotelian principles and abundant experimental evidence. The idea lost ground only slowly although it was opposed by GASSENDI and DESCARTES alike, its influence being noticeable in HOBBS' criticisms of BOYLE.

Already by 1644, MERSENNE had collected together examples of the great powers of rarefaction and condensation of air, in the course of which he estimated the density of air relative to water. He showed that air could be reversibly condensed to occupy 1/1000th part of its former volume.²²

During the course of his development of the aether hypothesis, ÉTIENNE NOËL, who had been DESCARTES' teacher at La Flèche, found that the introduction of a small volume of air caused a greater reduction in the mercury level than the same volume of water, when introduced above the mercury in the Torricellian experiment.²³ This was a paradoxical result, since water was at least 400 times as dense as air. The depression of the mercury level was therefore not a manifestation of weight alone. NOËL explained the paradox by proposing that the depression in level was caused by the aether, which was present to a much greater extent in air than in water. However, the significance of this experiment was not in its conclusion, which, like other aether explanations was a problem of speculative rather than experimental physics, but in providing the basis for quantitative methods of measuring the degree of expansion of a volume of air. This modification of the Torricellian experiment could be adapted to measure the behaviour of air under either increased or decreased pressure.

This same experiment had been evolved also by GILES PERSONE DE ROBERVAL (1602—1675), who was professor of mathematics at the Collège Royale in Paris, and who was one of DESCARTES' most persistent critics. He was one of the prominent virtuosi of the period, who had explored numerous mathematical and physical problems. He was a friend of both MERSENNE and PASCAL and took part in the regular scientific meetings in Paris. His writings on air pressure betray similarities with PASCAL's. He had a positivist bias, and he was unwilling to declare in favour of either the plenist or vacuist schools. Furthermore, he had great ingenuity in devising experiments, which were scrupulously described and explained. As with PASCAL, his work was known more through the informal demonstrations and discussions of the experiments than by publication, for his writings on air pressure have not been published until the present century. His influence was manifest primarily in the writings of MERSENNE and PECQUET.

ROBERVAL'S first experiments on air pressure were prompted by the disagreements over PETIT'S vindication of the vacuist interpretation of the Torricell-

²² Ibid., Propositio XXIX.

²³ ÉTIENNE NOËL, *Gravitatis comparata, seu comparatio gravitatis aeris cum hydragyri gravitate*, Paris, 1648. pp. 55—56.

ian experiment.²⁴ JACQUES PIERIUS gave a scholastic reply to the Torricellian theory, proposing that nature's abhorrence of vacuum was limited and that the apparent vacua were filled by the vapour of the liquid in the tube. During his examination of the Torricellian experiment, ROBERVAL infiltrated bubbles of air and water into the apparent vacuum, and concluded that bubbles of air did not pass into the vacuum, but remained above the mercury as distinct bubbles.²⁵

But, during the next year, he reversed these opinions and evolved an elaborate theory to explain that the space contained rarefied air, an explanation that he found accorded well with his mechanical principles, as well as disarming the peripatetic opponents of vacuum.²⁶

This change of opinion was induced by the experiments which illustrated air's great powers of expansion, the most pertinent experiment being the carp-bladder experiment, which he devised himself.²⁷ The swim-bladder was removed from a carp, and the air pressed out; the neck of the bladder was then tightly tied, and it was placed at the apex of a Torricellian tube. The tube was filled with mercury, immersed into a dish of mercury, and when the mercury fell, the bladder was suspended in the vacuum. The bladder now inflated. This experiment was visually impressive and its popularity probably exceeded that of the Torricellian experiment itself, until it became an indispensable part of future works on air pressure. Interest in this experiment was greatly enhanced by its use to criticise DESCARTES' aether hypothesis. ROBERVAL proposed that, under normal circumstances air is compressed or condensed (*comprementu seu*

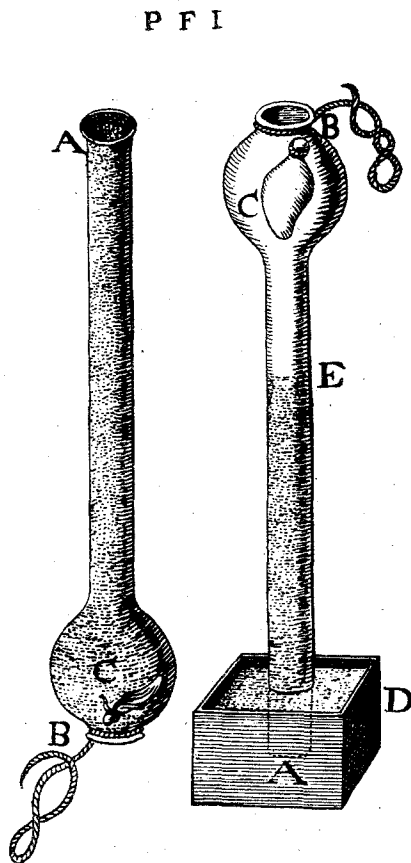


Fig. 1. JEAN PECQUET, *Experimenta Nova Anatomica*, Paris, 1651, Plate I. The carp bladder experiment

²⁴ G. P. DE ROBERVAL, "de Vacuo narratio Ae. P. de Roberval ad nobilissimum virum D. de Noyers, 20 Sept., 1647." See PASCAL'S *Oeuvres* op. cit., vol. II, pp. 21—35. JACQUES PIERIUS published his detailed criticism of the vacuist theory in 1648; *Jacobi Pieri, doctoris medici et philosophiae professoris, Ad experimentiam circa vacuum R. P. Valeriani Magni demonstrationem ocularem et mathematicorum quorundam nova cogitata, Responsio ex Peripateticae Principiis desumpta*, Paris, 1648.

²⁵ G. P. DE ROBERVAL, 1647, op. cit., pp. 26—27.

²⁶ ROBERVAL expressed his change of mind in a further letter to DES NOYERS, which was written in May/June, 1648. Like the former letter it was not published until the present century. "A. P. de Roberval de Vacuo Narratio ad Nobilem virum dominum des Noyers," Pascal's *Oeuvres*, op. cit., vol. II, pp. 310—340.

²⁷ *Ibid.*, pp. 325—328. A translation of the whole of the carp-bladder experiment is given as Appendix I. See footnote 177 and Illustration 1.

condensantis) by the upper layers and that it has a natural power of resilience (*resiliendum facultas*) which enables it to expand when released from compression. Such a circumstance occurred in the bladder, which, although largely deflated, still had a little residual air; when this air was placed in the reduced pressure of the vacuum, it expanded until its force was equalled by the tension of the elastic bladder.

PASCAL had also noted the expansion of air when subjected to reduced pressure, when he noted the increase in volume of a partially inflated bladder when carried up a hill.

Hence if a balloon, only half inflated — not fully so, as they generally are — were carried up a mountain, it would necessarily be more inflated at the mountain top, and would expand in the degree to which it was less burdened.²⁸

Thus already PASCAL had speculated that there was a simple relationship between the volume of air and the external pressure on it, although this was not an expression of the proportionality between atmospheric pressure, and the expansion of the air. ROBERVAL also came to this conclusion in a further experiment, which is quoted as Appendix II. Like NOËL, ROBERVAL introduced equal volumes of air and water above the mercury in the Torricellian experiment, and noted that air caused the greater depression of the mercury level. This could be explained in a similar manner to the carp-bladder experiment — water pressed on the mercury by its weight, while air exerted a force of dilatation (*vi aut appetitum ad rarefactionem*). Further, he noted that a volume of air caused the greatest depression of mercury in smaller tubes and the least expansion in longer tubes. To explain this he assumed that air's spring-like property was similar to the resilience of a bow, which, when released from constraint, had the greatest rebound at the beginning of its spring and this diminished until it returned to quiescence. Likewise, air had the greatest power of spring at the beginning of its expansion, when it was just released from compression of the whole weight of the atmosphere. This force then relaxed as it expanded. Thus in a long tube the mercury was depressed slightly by air in its expanded and weak state.

Ac initio quidem suae rarefactionis magnis viribus rarefit, quia magnis, puta totius elementaris naturae prementis viribus condensabatur. Inde vero sensim languescunt ipsius vires, quia minus ac minus premitur atque condensatur ...²⁹

This passage is reminiscent of BOYLE's conclusion that his hypothesis explained "how much air dilated itself loses of its elastical force".³⁰

ROBERVAL's account showed a greater capacity of reducing this experiment to the form of a problem in mechanics than did other French authors. He had clearly in his mind the idea that the experiment represented an equilibrium between the pressure of the atmosphere on the one hand, and the pressure of

²⁸ BLAISE PASCAL, *Traité de l'Équilibre des liqueurs. et de la Pesanteur de la Masse de l'air*, Paris, 1663. English translation, op. cit. (note 17), pp. 1—75; this quotation is taken from p. 30.

²⁹ G. P. DE ROBERVAL, 1648, op. cit., p. 316.

³⁰ ROBERT BOYLE, *New Experiments Physico-Mechanical*, Second edition Oxford, 1662; *The Works of the Honourable Robert Boyle*, ed. T. BIRCH, London, 1744; vol. 1, p. 102.

the column of mercury and enclosed air on the other. Any disturbance of this equilibrium by altering the volume of air in the tube, caused a readjustment of the mercury level to restore the equilibrium. This principle of conservation, he subsumed under his general principle that gravity was due to the mutual attraction of the ultimate parts of nature:

si respexerit ad vim illam qua partes totius naturae elementaris invicem comprimuntur ad unicum systema elementare constitutendum:³¹

ROBERVAL'S experiments and theories attracted great attention in Paris, and on 5th May, 1648, PECQUET wrote to MERSENNE:

Monsieur de Roberval performed marvels here, and yesterday he performed successfully the experiment with the carp's bladder; he had a great number of observers.³²

MERSENNE, like many other French natural philosophers, was impressed by ROBERVAL'S work, and he disseminated information about the rarefaction of air to his European correspondents. He now hesitated over the vacuist interpretation of the Torricellian experiment, expressing these doubts in his *Reflexiones Physico-mathematica* of 1647.

In his last work, the new preface to the *Harmonicorum Libri XII*, he included an account of the depression of mercury by a volume of air, and in a letter to HEVELIUS in June, 1648³³ he noted that the question of vacuum had been reopened, but expressed his reservations about the Aristotelian theory of rarefaction. However, by July, 1648, he was convinced that the space contained rarefied air.

We see that numerous tracts are written in Poland on the vacuum in the glass tube, but nothing comes of it, and similarly in our experiments, of which even now we are multiplying new ones, but nevertheless we conclude that it is rarefied air, not vacuum.³⁴

This may be taken as MERSENNE'S final statement about the Torricellian experiment, since he died on 1st. September, 1648.

IV. The "Elater" of Air: Jean Pecquet

Another author who was impressed by ROBERVAL'S theory of the spontaneous rarefaction of air was JEAN PECQUET (1622—1674). He has an assured place in the history of science through his discovery of the chyle receptacle and the thoracic duct of the lymphatic system, which was made during his demonstration of ASELLI'S chyle vessels in dogs at Montpellier, when he was a student of medicine at that University. His study of the lymphatic system was one of the most significant contributions to experimental physiology since HARVEY'S discovery of circulation; PECQUET defended HARVEY'S work against JEAN RIOLAN, of Paris, who was the most influential opponent of the idea of circulation. PECQUET inte-

³¹ G. P. DE ROBERVAL, 1648, op. cit., p. 317. This same principle had been stressed in his previous work, *Aristarchi Samii de Mundi Systemate Libellus*, Paris, 1644, p. 4.

³² PECQUET'S letter is translated from the quotation in PASCAL'S *Oeuvres*, vol. II, p. 295.

³³ M. MERSENNE, letter to HEVELIUS, 1 June 1648; PASCAL'S *Oeuvres* vol. II, pp. 302—303.

³⁴ M. MERSENNE, letter to HEVELIUS, 27 July 1648; PASCAL'S *Oeuvres* vol. II, p. 309.

grated the theory of the motion of chyle into the general theory of circulation. His discoveries were made in 1647, and he gathered various short essays on physiology into a small volume, *Experimenta Nova Anatomica*, which was published in 1651.³⁵ This, his first work, became very popular and it passed through many editions, being translated into English in 1653. This is significant, since only the most influential anatomical works were translated at this time.

This book is important for the purposes of this article since it contained a section on experiments on air pressure, which would thus be distributed to a wide audience, by virtue of their association with the physiological work. Thus paradoxically, a physiological treatise became one of the most accessible and widely read accounts of the French experiments on air pressure.

It has been noted that PECQUET reported ROBERVAL'S carp-bladder experiment to MERSENNE in 1647. In the next year he was writing a further account of an experiment on air pressure. At the same time he was attracted to attend the weekly scientific meetings in MONTMOR'S house in Paris, and it may be assumed that the theories of air pressure were of great interest to him at this time. It is therefore not unnatural that he should include an account of air pressure in his first book. Unfortunately, the great acuteness of PECQUET'S intellect, shown in these initial physiological and physical studies, was soon clouded by an addiction to alcohol.

The purpose of the section on air pressure was apparent from the first chapter heading:

Esse non PONDUS tantum, sed rarefactarium Aeri ELATEREM Experimentis demonstratur.³⁶

PECQUET collected together various experiments which illustrated that air had not only weight but also a spring or elater. The originality of the work was therefore not in the introduction of new experiments, for all the experiments were from other, albeit unpublished, sources. He described the Puy de Dôme experiment of PASCAL and PERIER,³⁷ and it was through PECQUET'S account, rather than PASCAL'S that this experiment was introduced to POWER, BOYLE and SINCLAIR in England and Scotland. He gave also AUZOULT'S modification of PASCAL'S vacuum in the vacuum experiment.³⁸ From ROBERVAL he took the carp-bladder experiment and the experiment of enclosing air or water above the mercury of the Torricellian apparatus. This experiment is quoted in full as Appendix III (see also Illustration 3); and it indicates the terse style which was

³⁵ JEAN PECQUET, *Experimenta Nova Anatomica*, Paris, 1651, 4^o. There was another issue published in Paris in the same year; *Experimenta Nova Anatomica ... Hardevici apud J. Tollium, juxta exemplar Parisiis impressum*, 12^o. — A second edition appeared in Paris in 1654. The English edition was produced in conjunction with THOMAS BARTHOLIN'S tract on the lymphatic system; *New Anatomical Experiments of J. Pecquet, ... Also an anatomical dissertation of the movement of blood and chyle ... by T. Bartholinus*, 2 vols., London, 1653, 12^o. — HENRY POWER'S library had both the English and the 1651 edition of this work. — My references are to the most widely available edition, *Joannis Pecqueti Diepaei Experimenta Nova Anatomica, Quibus Incognitum hactenus Chyli Receptaculum, & ab eo per Thoracem in ramos usque Subclavios Vasa Lactea deteguntur ...*, Amsterdam, 1661, 16^o.

³⁶ *Ibid.*, p. 87.

³⁷ *Ibid.*, pp. 99—101.

³⁸ *Ibid.*, pp. 102—106; see Illustration 2.

adopted for these experiments, which contrasts strongly with complex ramifications of ROBERVAL's arguments.

The importance of PECQUET's work lies therefore in its effective publicity of the French experiments and in its stress on the property of the elasticity of air. Further, he adopted the terminology to describe this phenomenon which has become integrated into the English language.

F II

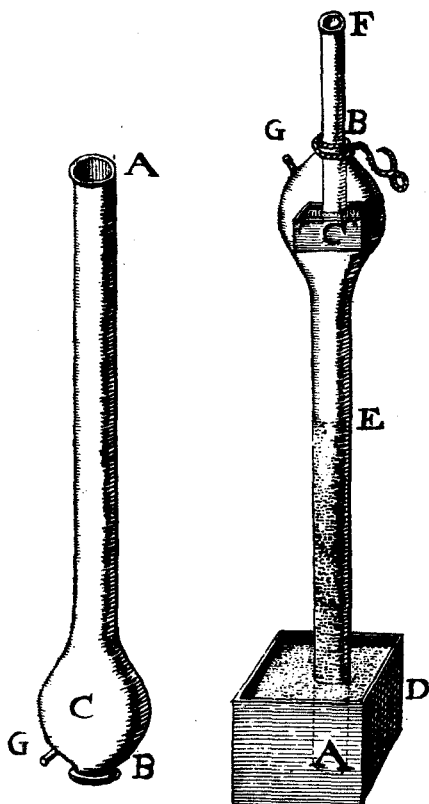


Fig. 2. JEAN PECQUET, *Experimenta Nova Anatomica*, Paris, 1651, Plate II. AUZOULT's vacuum-in-vacuum experiment. The upper tube is shown incompletely and is sawn off at F. The containers C and D hold mercury

F III

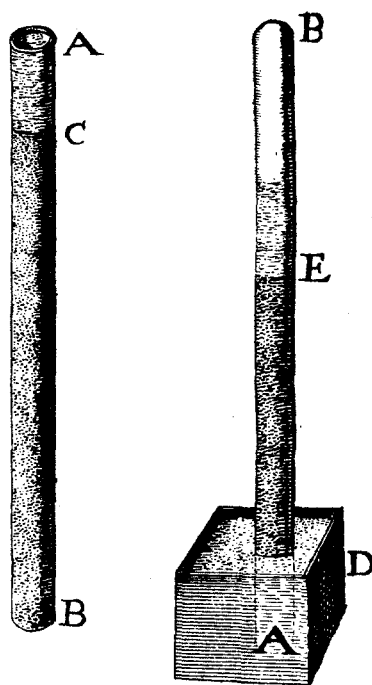


Fig. 3. JEAN PECQUET, *Experimenta Nova Anatomica*, Paris, 1651, Plate III. PECQUET's demonstration that air exerts pressure by virtue of its elasticity, while water exerts pressure by its weight. Equal volumes of water (E) and air (B) are placed above mercury (AE) in a Torricellian tube

ROBERVAL had maintained the Aristotelian terminology in explaining the expansion of air, although he inferred that the condensation and rarefaction was a "spontaneous" or active property. It was perhaps his deference to the plenist theory which prevented him from expressing a physical model to explain this property. PECQUET now adopted the terms "elater" and "elastic" to describe the same phenomenon. Neither term had been used in classical Latin, and PECQUET took them from the Greek noun *ἐλατήρ* (*elater*; that which or one who drives). This term was widely adopted by English authors, and through the works of CHARLETON, POWER and BOYLE it became familiar to seventeenth century readers.

It was often expressed as "elater" and used as an adjective. This term probably suggested the analogous English term "spring" which BOYLE preferred. Both "spring" and "elater" gradually fell into disuse, being succeeded by "elasticity".

PECQUET adopted a transliteration of *ἐλαστικός* (*elasticus*; propulsive or impulsive), and this term was adopted by POWER, BOYLE, HOOKE and HENRY MORE, and has become the standard English term expressing the reversible extension and contraction of physical bodies. PECQUET expressed his concept of air at the beginning of his account. This shows clearly the fusion of the ideas which originated from BEECKMAN and ROBERVAL.

It is my suggestion to you that this (air) is like spongy or more woolly heaps lying on the matter of the Terraquaceous Globe; as a consequence of which each successively higher layer compresses the lower; they are sustained so that, the nearer the layers are to the earth, then also they are more closely compressed by the weight and pressure of those lying on them. On account of the spontaneous dilatation, which I call Elater, however strongly they are compressed by the cumulative burden, if set free the air rarifies.

Hence, I infer that such lower parts that are subjected to the whole burden, so that of all parts they have the maximum degree of condensation. Because of this same cause, whereby it exerted its powerful tendency to rarefy, not only by means of its weight but also by its elater pressing against the surface of the Terraquaceous globe.³⁹

The aims of PECQUET'S experiments were limited; he proposed the hypothesis of the elasticity of air and produced a series of experiments to verify this. Only slight attention was paid to the plenist-vacuist controversy, and his only excursion into the polemic was to point out that DESCARTES' aether theory could not explain the carp-bladder experiment. It is probable that PECQUET, like ROBERVAL, retained the idea of the homogeneity of air, in keeping with the Aristotelian idea of rarefaction, but his terminology was sufficiently suggestive to relay to the English authors, the idea of spring-like particles suspended in a more rarefied medium.

V. The Introduction of Experiments on Air Pressure into England

It is probable that the performance of the Torricellian experiment in France preceded its introduction into England, but the delay could not have been long, since JOHN WALLIS recorded that "the weight of Air, the Possibility or Impossibility of Vacuities, and Nature's abhorrence thereof, the Torricellian Experiment in Quicksilver,"⁴⁰ were among the subjects discussed at the weekly scientific meetings in London. The group performing these experiments included JOHN WILKINS, JONATHAN GODDARD, GEORGE ENT, FRANCIS GLISSON, CHRISTOPHER MERRET, SAMUEL FOSTER and THEODORE HAAK, who were among the nation's most distinguished natural philosophers. In about 1648 this group extended their activities to Oxford, and this introduced the Torricellian experiment to a wider group, which soon included ROBERT BOYLE.

³⁹ Ibid., p. 89.

⁴⁰ JOHN WALLIS, letter to Dr. SMITH, 29th Jan., 1696/97; this is found in *Peter Langtoft's Chronicle*, ed. T. HEARNE, Oxford, 1725, 2 vols.; vol. 1, Appendix xi, pp. clxi—clxiv.

THEODORE HAAK was proposed by WALLIS as the initiator of these regular scientific meetings, and it was probably he who introduced the Torricellian experiment to the Society, since he had travelled extensively in Europe and was one of MERSENNE'S correspondents. In 1647 he resumed contact with MERSENNE after a break of nearly seven years, and in a letter of 24 March/3 April, 1648, he thanked MERSENNE for the communication of the experiment, and reported that it had already been performed by his friends.

We have made two or three trials of it, in the company of men of letters and rank ... I shall attempt to encourage some of the best wits to make some investigation of the basis of these observations.⁴¹

In another letter of 3/13 July, he reported placing water with the mercury in the experiment, and their unsuccessful trials of the vacuum-in-vacuum experiment, of which he wished MERSENNE to provide more details.⁴²

SAMUEL HARTLIB (1595/1600—1662),⁴³ who was another of MERSENNE'S correspondents, friend of HAAK and publicist of scientific information, also knew of this interest in air pressure, and in March 1646/7 he informed BOYLE of his support of MERSENNE'S opinions.

... and now it comes into my mind, I read, not long since, in a late mechanical treatise of the excellent Mersennes, both the construction and use of this engine (wind-gun), and amongst the uses, one, whose stratagem obliged me to take of it particular notice; and it was, how by the help of this instrument to discover the weight of the air, which, for all the prattling of our book philosophers, we must believe to be both heavy and ponderable, if we will not refuse belief to our senses.⁴⁴

In a letter of May 1648, we find HARTLIB again providing BOYLE with information about experiments on air pressure; this time as an extract from one of Sir CHARLES CAVENDISH'S letters, which will be soon quoted in full.⁴⁵ BOYLE had consequently many years acquaintance with the problem of air-pressure, before he had begun the construction of his first air pump, and it is probable that the Torricellian experiment was frequently demonstrated at the London and Oxford meetings of this English virtuosi. Thus, when writing his *New Experiments Physico-*

⁴¹ Letter from THEODORE HAAK to MERSENNE, 24th March/3 April, 1648; quoted from HARCOURT BROWN, *Scientific Organisations in seventeenth century France, 1620—1680*, Baltimore, 1934, p. 58.

⁴² Letter from THEODORE HAAK to MERSENNE, 3/13 July, 1648. This is given in the French in Pascal's *Oeuvres*, op. cit., vol. II, p. 307. A translation of this passage is given below. — "We have also tried to mix water with the mercury in the tube, and we find notable variations in it, which will cause us to be more precise in our observations in future. I would like to learn how you arrange the experiments so as not to spoil and lose considerable amounts of mercury; and if you make use of exact glasses rather than any that come to hand. Also, I do not understand the manner of performing your last experiment of the one tube in the other, in which [the vacuum] should empty everything, seeing as we have not yet succeeded in this attempt."

⁴³ The relationship of HAAK and HARTLIB with the early Royal Society has been considered by Miss R. H. SYFRET, *The Origins of the Royal Society, Notes and Records of the Royal Society*, 1948, 5, pp. 75—137; G. H. TURNBULL, *Samuel Hartlib's influence on the early history of the Royal Society, Notes and Records of the Royal Society*, 1953, 10, pp. 101—130; PAMELA R. BARNETT, *Theodore Haak and the early years of the Royal Society, Annals of Science*, 1957, 13, pp. 205—218.

⁴⁴ SAMUEL HARTLIB, letter to BOYLE, 19th March, 1646/7; BOYLE'S *Works*, op. cit., vol. 1, p. 22.

⁴⁵ SAMUEL HARTLIB, letter to BOYLE, May, 1648; *Works*, op. cit., vol. 5, p. 257.

Mechanicall in 1659, he referred to his conjectures about the experiment when he had "several years before often made the experiment *de vacuo* with my own hands."⁴⁶

Had HARTLIB not received information about the Torricellian experiment from HAAK, he could have obtained it from WILLIAM PETTY, who received information about recent research in France from Sir CHARLES CAVENDISH. This letter was passed on to HARTLIB and is preserved in his papers.

April 7/19, 1648, Paris.

To Mr. Petty.

Worthy Sir,

My thankes was due to you long since for many favours received from you and particularlie for your letter wherein you are pleased to acquaint me with some of your new discoveries in Anatomie, and Inquires of other usefull and ingenious knowledges and your invention of writing in many Copies at once, for all which I give you many thankes and had done it sooner but that I hoped to have had some new book or invention of our rare men here to informe you off, but knowing nor hearing of any I thought fit though to trouble you to acknowledge your favours by this Letter. I showed Mr. Hobbes your Letter who liked it soe well that he desired me to send it him which I did — knowing him to be your friend. He is not now here otherwise I know he would either have write to you or desired me to remember him to you. Your worthy Friend and myne Mr. Gassend is reasonable well and hath Printed a Book of Ye Life and Manners of Epicurus since your going from hence as I thinke. He hath now in ye Presse at Lyons, ye philosophie of Epicurus in which I beleeve wee shall have much of his owne philosophy which doutlesse will be an excellent worke. There is an Experiment, how to show as they suppose that there is, or may be, vacuum. It may bee it was here before you went from hence. It were too long to recite all the particulars, but in brief thus, they prepare a long tube like a weather-glasse, which is filled with quicksilver, and being stopt as close as may be with ones finger the tube is inverted and plunged in a vessell halfe or more full of quicksilver. The quicksilver in ye tube will force ye quicksilver in ye vessell to rise by adding more quicksilver to it, and so leaves a space in ye top of the tube vacuum as is supposed but a bladder being hung in that vacuum, was as perfectly seene as could be, so that there must bee some body there to convey ye Action of light to ye and you as I suppose and divers others heere, that the bladder was made as flat as they could, then they put it in, and when then quicksilver lefft it, it swelled in that supposed vacuum like a little football. Sir I have troubled you much therefore committing you and us all to God's holy protection. I remaine.

Your assured friend

So serve you

Charles Cavendish⁴⁷

Sir CHARLES CAVENDISH (1591—1654),⁴⁸ the author of this letter, was an exile in Europe during the Civil War and he spent three years in Paris, beginning

⁴⁶ ROBERT BOYLE, *New Experiments Physico-Mechanicall*, Oxford, 1660; *Works*, op. cit., vol. 1, p. 5. BOYLE may have been referring to the communications between England and France in 1647 and 1648 when he noted: — "perceiving by letters from other ingenious persons at *Paris*, that several of the Virtuosi there were very intent upon the examination of the interest of the air, in hindering the descent of the quicksilver, in the famous experiment touching the vacuum; I thought I could not comply with your desires in a more fit and seasonable manner, than by prosecuting and endeavouring to promote that noble experiment of Torricellius." *Ibid.*, p. 5.

⁴⁷ This letter is in the *Hartlib papers*, Sheffield University Library, Bundle VII, no. 29.

⁴⁸ JEAN JACQUOT, Sir Charles Cavendish and his learned friends, *Annals of Science*, 1952, 8, pp. 13—27, 175—194.

in 1645. He became acquainted with MERSENNE and DESCARTES and was conversant with a wide range of scientific developments. The Cavendish family were patrons of THOMAS HOBBS, who was mentioned in the above letter, and who later became a participant in the discussions of air pressure.

Thus, even without direct access to the writings of PASCAL and TORRICELLI on air pressure, English natural philosophers had ample evidence of the continental research on air pressure, from informal exchanges of correspondence and the publications of MERSENNE, GASSENDI, PECQUET, PETIT, NOËL and PASCAL, as well as certain Italian works.

The first published description of the Torricellian experiment by an English author, which I have been able to trace, was by WALTER CHARLETON (1619—1707). He was educated at Oxford University, where he received his M.D. in 1641, and his first published works were concerned with medicine, but in 1654 he turned his attention to wider philosophical problems. His works were mainly derivative, although they betray wide reading.

His influential work, *Physiologia Epicuro-Gassendo-Charltoniana*,⁴⁹ of 1654, contained a detailed discussion of the controversies between the plenists and vacuists, which had largely overshadowed the experimental studies of the physical properties of air by French authors. Interest in the Torricellian experiment was closely related to this metaphysical discussion, since its interpretation determined the alignment with either the Cartesians or Gassendists.

With CHARLETON, as with the French authors, interest in the Torricellian experiment as a problem in experimental physics, was allied to its relevance to the problem of vacuum. The long fourth chapter of Book I of *Physiologia* was concerned with the establishment of the *vacuum disseminatum* and *vacuum coacervatum*, and it was as support for the latter notion that he introduced the Torricellian experiment.

CHARLETON referred to the experiment as a:

Welcome opportunity to challenge all the Wits of Europe to an aemulous combat for the honour of perspicacity. Now albeit we are not yet fully convinced, that the chief Phaenomenon in this illustrious Experiment doth clearly demonstrate the existence of a *Coacervate* Vacuity, such as thereupon by many conceded, and with all possible subtlety defended by that miracle of natural Science, the incomparable *Meysennus* (in reflexionibus Physicomathemat) yet, insomuch as it affords occasion of many rare and sublime speculations, whereof some cannot be solved either so fully, or perspicuously by any Hypothesis, as that of a *Vacuum Disseminatum* among the insensible particles of Aer and Water; and most promise the pleasure of Novelty, if not the profit of satisfaction to the worthy consider; we judge it no unpardonable

⁴⁹ WALTER CHARLETON, *Physiologica Epicuro-Gassendo-Charltoniana: or, a Fabrick of Science Natural, Upon the Hypothesis of Atoms*, London, 1654. Although CHARLETON quoted a wide range of authorities, he had probably not read more than a few of them and most of his information was abstracted from GASSENDI's work. In France, there had been discussion about the origins of the Torricellian experiment and KIRCHER acknowledged TORRICELLI's priority, although he must have been acquainted with BERTI's work. CHARLETON quoted from KIRCHER's account and in England there was general and unquestioning acceptance of TORRICELLI's priority. — "... nevertheless, it is ascertained to my satisfaction that the experiment was first invented by Torricelli, mathematician to the noble and great Duke of Florence." W. CHARLETON, op. cit., p. 35; A. KIRCHER, *Musurgia universalis sive Ars magna consoni et disoni*, Rome, 1650, p. 11.

Digression, here to present to our judicious Reader, a faithful Transcript of the Experiment, together with the most rational solutions of all admirable Appearances observed therein ...⁵⁰

The account of the experiment was totally derivative, and the mercury height in the Torricellian tube was given as 27 inches.⁵¹ He overlooked the fact that the French inch was a larger unit than the English, and in English inches the height would have been 29 inches. He noted that water should rise to 32 feet in this experiment, but showed no practical knowledge of its trial.⁵²

Most of CHARLETON'S account was devoted to a repetition of GASSENDI'S justification of the Epicurean arguments in favour of Vacuum in Nature, and this led him to discuss the rarefaction and condensation of air. It has been noted the Epicurean explanation of change in density by the alteration of the ratio of void to matter in a body, and the condensation and rarefaction of volumes of air provided extreme examples of change in density. He explained this by reference to the principle of the *dissemmate vacuum*. The air in a closed space was analogous to a vessel filled with wheat seeds or sand particles; by the application of a great weight the particles could be compressed to occupy a smaller volume. This was explained by the closure of the spaces between the particles.

So likewise are the particles of air included in the four-inch space of the Tube, by Compression or Coangustation reduced downe to the impletion of onely the half of that space; because from a more lax or rare Contexture they are contracted into a dense or close, their angles and sides being by that force more disposed for reciprocal Contingence, and leaving less Intervalls, or empty spaces betwixt them then before.⁵³

Other experiments induced him to revise this simple geometrical explanation and adopt a more kinetic model for the air. He recognised that the introduction of a small volume of air into the space above the mercury in the Torricellian experiment caused an immediate and violent descent of the liquid, an effect out of all proportion to the volume of air introduced.⁵⁴ He also noted that the Wind-Gun illustrated the expansion and compression of air, adopting the theoretical model of air as mass of spring-like particles.

For, as the insensible particles of the Aer included in the Tube of a Wind-Gun, being, by the Embolous or Rammer, from a more lax and rare contexture, in order, reduced to a more dense and close (which is effected when they are made more contiguous in the points of their superface, and so compelled to diminish the inane spaces interjacent betwixt them, by subingression) are, in a manner so many Springs or Elaters, such whereof, so soon as the external Force, that compressed them, ceaseth (which is at the remove of the Diaphragme or partition plate in the chamber of the Tube) reflecteth, or is at least reflected by the impulse of another contiguous particle.⁵⁵

This conceptual model of air as a mass of spring-like particles is usually attributed to BOYLE, but it had previously been implied in PECQUET'S book, as well as in CHARLETON'S.⁵⁶

⁵⁰ Ibid., pp. 35—36.

⁵¹ Ibid., p. 36, this account was taken from GASSENDI, and it is found in *Opera Omnia*, Lyons, 1658, vol. 1, p. 204.

⁵² W. CHARLETON, 1654, op. cit., p. 52.

⁵³ Ibid., p. 26.

⁵⁴ Ibid., pp. 55—56.

⁵⁵ Ibid., pp. 55—57.

⁵⁶ See Appendix 5.

VI. Henry Power's Experiments of 1653

Writing at the same time as CHARLETON was HENRY POWER (1623—1668), who adopted a more experimental approach to the problem of air pressure. POWER was born at Halifax in Yorkshire and was educated at Christ's College, Cambridge, where he obtained his M.D. in 1655. During the early part of his fourteen years at Cambridge, he became an enthusiastic experimental scientist, and his first writings were on circulation and the lymphatic system.⁵⁷ Thus, it may have been PECQUET's book which turned his attention towards the study of air pressure and it is probable that the Torricellian experiment was commonly performed at Cambridge, by 1653. Like CHARLETON, POWER was strongly involved in the Gassendist-Cartesian disputes, and, under the influence of HENRY MORE, Christ's College was the centre for Cartesian thought in England.

POWER performed his first series of experiments on air pressure at Cambridge and Halifax in 1653, but his account was not finally edited for publication until 1663, the experiments forming the first part of Book II of his only work, *Experimental Philosophy*, which appeared in the autumn of 1663.⁵⁸ This delay in publication has caused neglect of these pioneer English experiments on air pressure.

Even though POWER was an obscure person at this time, his work was circulated among natural philosophers as late as 1677.⁵⁹ The experiments were not without interest since information about them reached HARTLIB RALPH GREATOREX, who provided him with the information, was one of the most celebrated London instrument makers. He was soon involved in the design of an air pump for ROBERT BOYLE. HARTLIB reported:

One Billingsley a schoolmaster lives in a gentleman's house, writes on Dr. Pascal's rare experiments of quicksilver — tried and augmented by Henry Bower [sic.] 1653, 2 May [sic.] which he is to publish in the gentleman's name.

⁵⁷ HENRY POWER, "Circulatio sanguinis inventio Harveiana," BM *Sloane* MS 1343, ff. 3—40; "Inventio Aselliana de Venis Lacteis et de Motu Chyli," *Ibid.*, ff. 41—56.

⁵⁸ There are three remaining versions of POWER's 1653 experiments. They will be referred to as texts *A*, *B*, and *C*. — *A*, "Experimenta Mercurialia nuper mihi exhibitata a viro undiquaque ornatissimo D. Domino Henrico Power Med^{ae}. Doctorae Hallifaxae. Anno Domini 1703," BM *Sloane* MS 1326, ff. 133—141. The date of this work is added to the original manuscript and is incorrect; it should read 1653. — *B*, "Dr. Pascall's rare experiments of φ tryed and augmented by Henry Power, 1653, May 3rd," Bodleian Library, *Ashmolean* MS 1400, ff. 15—21. — *C*, "Physico-Mechanical Experiments tryed in the yeare 1653 by Henry Power," BM, *Sloane* MS 1393, ff. 134—153. — Only version *C* is in POWER's handwriting, and it is the most complete account of the experiments. It was this version which was incorporated in his publication of 1663. I will refer to *C*, unless otherwise stated, and the page references will be to the published account: *Experimental Philosophy, in Three Books: Containing New Experiments Microscopical, Mercurial, Magnetical. With some Deductions, and Probable Hypotheses, raised from them, in Avouchment and illustration of the now famous Atomical Hypothesis*, London, 1664, [1663]. Version *C* occupies pp. 88—121. — While Version *C* gives a good indication of POWER's experiments of 1653, internal evidence suggests that it is a revised account of an earlier manuscript. Thus versions *A* and *B* may give a better indication of the first draft of this work.

⁵⁹ There is a note appended to *B*; "Written by John Sponge, came after to Rob't Turner, then to Mr. DUGGETT, from him to me [Stansby]ye 1 Sept. 1677". f. 13a. JOHN SPONG (1623—post 1668) was a mathematical practitioner and instrument maker of London.

He hath re-experimented them all and put new experiments to it.⁶⁰

POWER left Cambridge, in 1654 or 1655, without having published these experiments, and he spent his remaining years in the West Riding of Yorkshire, paying only occasional visits to London and Cambridge. He was thus separated from the main centres of scientific activity and had little opportunity to augment and publish his work. As will be explained later, these circumstances altered when the Royal Society was instituted. In 1660 he resuscitated his earlier scientific writings and returned to the experimental study of air pressure.

POWER'S work is a great contrast to CHARLETON'S. He showed knowledge of PECQUET'S book only, as the source of his knowledge of the French investigations, and like PECQUET he presented a series of briefly described experiments, indicating their relationship to his hydrostatical and pneumatical hypothesis. However, his metaphysical position differed from PECQUET, who had adopted ROBERVAL'S theory of rarefaction; while POWER held Cartesian plenism.

It is probable that POWER had already adopted the Cartesian theory of matter before he was introduced to the French experiments on air pressure. He believed that air consisted of infinitely divisible "corpuscles" or "atoms" suspended in an "aether" or "subtle matter". He now was introduced to the hypothetical model of air proposed by PECQUET, and this influenced him to introduce the concept of the innate activity of the particles of matter, differing from DESCARTES who believed that the motion of the corpuscles was due to the activity of the aether. POWER now accorded the aether a more passive rôle, as the innate substratum for the particles. This view differed from that of PECQUET in adopting a more literal interpretation of the spring-like nature of the particles of air, and in introducing the Cartesian aether as the substratum for them. A full quotation of POWER'S theory is given as Appendix IV.⁶¹ This assimilation of the Aristotelian concept of active rarefaction into the structural characteristics of the corpuscular theory was a valuable imaginative aid in the later quantitative studies of the elasticity of air.

The starting point of POWER'S experiments had been the Torricellian experiment,⁶² and like CHARLETON, he was hesitant in giving his own experimental results which differed from those of the French authors. Only the last version gave the height of mercury as 29 inches although he was aware of the difference between English and French measure⁶³ in the earliest composed "version A".

⁶⁰ SAMUEL HARTLIB, *Ephemerides*, 1655. This is a journal kept by HARTLIB between 1634 and 1660; it is in the Sheffield University Library. In 1654, HARTLIB had referred to BILLINGSLEY as he "who necessitated to follow heterogeneous employments as I take it Physick, but his whole genius is bent towards the mathematicks and mechanicks in which he excels." ROBERT BILLINGSLEY was a school-master at the Free School, Thurlow, Suffolk.

⁶¹ Version C pp. 101—103.

⁶² It was not until version C that POWER mentioned TORRICELLIUS (p. 90) or PARICELLIUS (p. 93)!

⁶³ In version C he noted that the difference between the French and English results "may partly arise from the variations of the climates, the Air being more thin and hot then ours, partly from the difference and altitude of the atmosphere here and there . . . , and partly from the diversity of our measures and theirs," pp. 93—94. — BOYLE had reached similar conclusions in his 1659 experiments; *Works* op. cit., vol. 1, p. 25.

It is also probable that air was not completely removed from the apparatus in the early trials of the experiment. He varied the diameter, length and shapes of the tubes and in all cases the mercury remained at 29 inches in vertical height. In a separate experiment, he studied the difficulty of removing air from the apparatus.

The interpretation of the space above the mercury was of the greatest importance to him. Since the mercury space could be completely replaced by water, the space could not contain air.⁶⁴ He was also satisfied that it contained neither light nor mercury vapour alone. Although this "seeming vacuity" had no positive properties, it would transmit light and magnetism and it had extension, the Cartesian determinant of matter. These three points, and the associated belief that all action was by contact, convinced him that the space could not be empty and must contain an "aether".⁶⁵

Like PASCAL, he noted that meteorological conditions influenced the mercury level, and the possibility of using the tube for weather-forecasting interested him at various times from this date⁶⁶. PECQUET had given an account of PASCAL'S Puy de Dôme experiment, and POWER was in a good position to repeat this, since the hills rise steeply to 1000 feet around Halifax. This is to be contrasted with the position of BOYLE and HOOKE, who were never able to give a satisfactory verification of this experiment, for want of high hills in the south-east of England. On May 6th 1653, he carried the Torricellian experiment to the summit of Halifax hill, to the east of Halifax.⁶⁷ The earliest account of this experiment, although the briefest, indicates the manner in which he reached the hypothesis that the height of all mountains could be derived by proportion, from the results of a barometric experiment.

⁶⁴ HENRY POWER, version C, p. 100.

⁶⁵ *Ibid.*, pp. 94—103.

⁶⁶ HENRY POWER; in version B, f. 16, he noted "that if any thing considerably hot or cold be apply'd to the top of the tube it will proportionally ascend or descend (as the water in a weather glass though not by farr so much) the like is to be observed in the changes of weather, and therefore it is not unalterable at all times of the year". — In 1661 POWER and TOWNELEY reaffirmed the value of barometric readings in reflecting the meteorological conditions. The mercury level did "vary and alter its Standard, which we found it to do considerably; for sometimes it was half an inch higher or lower then the Mark and Standard we left it first at. I think, according to the variation of the *Atmosphaere* in its temperature: and if you observe strictly, you shall see that the Quicksilver in the Tube does never precisely observe the same Standard not a day together, nay sometimes not an hour." H. POWER, 1963, *op. cit.*, p. 123. — BOYLE also noted that the Torricellian tube "did sometimes faintly imitate the liquor of a weather glass." He attempted to associate this fluctuation with meteorological changes and tide levels; 1660, *op. cit.*, *Works*, vol. 1, pp. 26—28. In his later works he frequently returned to this problem. On 1st January, 1662 "Mr. Croune was desired to write to Dr. Power to observe the weather at Hallifax," T. BIRCH, *History of the Royal Society*, 4 vols., London, 1756/7; vol. 1, p. 68. Although there are no records of POWER'S studies, the systematic meteorological records of his colleague, RICHARD TOWNELEY, are preserved. His barometric records were sent to BOYLE, and his rainfall measurements were transmitted to the Royal Society and CHARLES LEIGH; Boyle's *Works*, vol. 5, p. 135; CHARLES LEIGH, *The Natural History of Lancashire, Cheshire and the Peak, in Derbyshire*, Oxford, 1700, pp. 21—25.

⁶⁷ Halifax Hill is 775 ft. high. BOYLE and HOOKE overcame the deficiency of hills by carrying the Torricellian experiment to the top of London churches. BOYLE, 1662, *op. cit.*, *Works*, vol. 1, p. 102, vol. 5, p. 535.

HENRY POWER attempted this experiment on 6th May 1653, with tubes of various lengths and diameter, on Halifax Hill, which is situated above the town of Halifax.

The mercury cylinder was carried to the base of the hill and the mercury rose to equal 27 inches, whence at the summit of the hill it was shorter in height by $4/5$ ths. inch, this being nearly a whole inch lower.

This experiment was performed with tubes of 45 and $35\frac{1}{2}$ inches in length, so that by analogy and proportion in each of these experiments, the perpendicular height of Halifax hill exceeds $290.2/3$ rds. yards, that is 872 feet; (I estimate that 6 French feet are equal to one English pole). Likewise this "golden rule" applies in this and subsequent experiments:

$\frac{3}{4}$ inches [fall in mercury]: $\frac{3000}{1}$ [feet in altitude]

[therefore] $\frac{4}{5}$ inches [fall in mercury]: $\frac{12000}{5}$ [feet in altitude].

Which, when reduced into French feet, constitutes 800 feet. Likewise 100 French feet (according to Herigone) equal 109 English feet, so that 800 French feet are equal to 872 English feet or $290.2/3$ rds. yards, which discovers the height of this hill very satisfactorily.⁶⁸

In the final version of this account he was satisfied with a generalisation that the height of hills could be found by the principle, but he gave no numerical example.

... we might not only Mechanically find out the Perpendicular height of our great Hill here at *Hallifax*, or any other Mountain whatsoever, but venture notably at the height of the Atmosphere it self.⁶⁹

POWER'S confusion over mathematical examples may have been the reason for not presenting his original calculations, but also the experimental inaccuracies on such a small hill would prevent consistency of results.⁷⁰

The consideration of the elasticity of air contained POWER'S most original experiments, and concluded the investigations of 1653. He followed PECQUET in stressing that the elasticity of air required separate consideration from its density.

... the whole mass of Ayr hath a Spontaneous Eleter [sic.] or natural aptitude in it self to dilate and expand it self upon the removal of all circumambient obstacles

⁶⁸ Version B, f. 139. The calculation in the middle of the quotation has little connection with the text. — In version C, the mercury height at the base of the hill was given as 29 inches, and the fall during the ascent was "more then half an inch." p. 104.

⁶⁹ Version C, p. 104.

⁷⁰ POWER suggested that his hypothesis for determining the heights of mountains should be tested on Mount Teneriff (3,711 m.), in the Canary Islands. It was generally considered that this was the highest mountain in the world (version C pp. 105—106), and sixteenth century authors had estimated its height as 90,000 m. (SCALIGER) and 105,000 m. (PATRIZZO)- BOYLE and the members of the Royal Society also suggested that the Torricellian tube should be carried up this mountain; T. BIRCH, 1756/57 op. cit., vol. 1, p. 8; R. BOYLE, *Continuation of New Experiments Physico-Mechanical*, Oxford, 1669?, *Works*, op. cit., vol. 3, pp. 225—228. — In October, 1661, GEORGE SINCLAIR (d. 1696) estimated the heights of Scottish hills by the barometric method, and he, like Power, had been stimulated by PECQUET'S work. While POWER used PASCAL'S Puy de Dôme altitude as his standard, SINCLAIR obtained his own standard by carrying the tube up buildings of known height. He reached a similar conclusion to POWER, that the barometer fell one inch for every 250 paces ascended (1,250 ft.). However, it must be noted that POWER'S height for the Puy de Dôme (3,000 ft. or 973 m.) was too low; it is actually 1,465 m. GEORGE SINCLAIR, *Ars nova et magna gravitatis et levitatis*, Rotterdam, 1669, pp. 125—149. F. CAJORI, *History of determinations of the heights of mountains*, *Isis*, 1929, 12, pp. 482—514.

(which he Pecquet calls the Elastical motion of that Element) so that the particles of Ayr may be understood to be as so many little Springs, which if at liberty, and not bound and squeezed up, will powerfully, strongly, and spontaneously dilate and stretch out themselves, not onely to fill up a large room but to remove great bodies.⁷¹

This "elater" or "spring" of air was the property which later interested BOYLE also, and both he and POWER made initial unsuccessful attempts to reduce this concept to some simple law. POWER began by repeating the simple experiments which PECQUET had performed to illustrate the "spring of air, including the enclosure of a fish's bladder in a vacuum, which has already been quoted from CAVENDISH's correspondence.⁷² But the experiment that impressed POWER most was PECQUET's fourth experiment. "That Water onely by its weight compresseth the Earth's watery Globe; But the Air compresseth it, not onely by its weight but by its Elatery."⁷³

POWER repeated this experiment, but gave a more precise result than PECQUET.

Fill the Tube (as in the former Experiment) and let the Segment A of 14. inches, which was formerly fill'd with Water, be onely fill'd with Ayr; then, after you have revers'd it into the vessel'd Quicksilver D, and with drawing your finger, you shall see the Quicksilver in the Vessel so to fall, that it came down 16. inches lower then its wonted and determinate Altitude:⁷⁴

He concluded that this phenomenon could not be explained other than by the spring of air.

That, before you withdraw your finger, you shall perceive the water and Quicksilver in the Tube, to press so sensibly upon your finger: as if it would force an entrance out, both before and after it was immerg'd in the Vessel'd Quicksilver: which protrusion cannot possibly be supposed to proceed from any other cause, but the Elatery of the included Ayr.⁷⁵

POWER now undertook a series of experiments to examine the spring of different volumes of air.⁷⁶ In each case the experimental details were similar to the first trial, and I have extracted the results from POWER's account, to construct the following Table 1. All units are in inches.

Table 1

Liquid in the apparatus	Tube length	Original air volume	Air volume after inversion of the tube
Mercury . . .	45	2	4
Mercury . . .	45	14	16
Mercury . . .	45	40	42
Mercury . . .	29	14 $\frac{1}{2}$	15 $\frac{1}{2}$
Mercury . . .	27	13 $\frac{1}{2}$	18 $\frac{1}{2}$
Mercury . . .	21	10 $\frac{1}{2}$	14 $\frac{1}{2}$
Mercury . . .	18	9	10
Water . . .	45	22 $\frac{1}{2}$	24 $\frac{1}{2}$

⁷¹ HENRY POWER, Version C, p. 105.

⁷² *Ibid.*, pp. 116—117.

⁷³ J. PECQUET, 1651, *op. cit.* The title is quoted from the English translation, 1653, p. 109.

⁷⁴ HENRY POWER, Version C, p. 113.

⁷⁵ *Ibid.*, p. 114.

⁷⁶ *Ibid.*, pp. 114—116; Version A, ff. 20—21; Version B, ff. 139—141.

In a further experiment a twelve-inch tube was half filled with water, and this was inverted and totally immersed in a cylinder of water. In this case the volume of air was reduced. This modification of the experiment was probably suggested by RICHARD TOWNELEY who had originated the experiment in which the Torricellian tube was completely immersed in Mercury.⁷⁷

It is apparent, from scrutiny of the Table, that POWER's results were too incomplete to enable any satisfactory law to be induced, but he concluded:

The descent or fall of the Quicksilver or Water, was most notable about the midst of the Tube, viz. when it was equally fill'd with Ayr and Quicksilver, or Ayr and Water.⁷⁸

This generalisation was correct within the confines of these particular results, but would not have applied for tubes of different lengths. It also shows that, at this time, POWER was concerned with comparing the change in volume of the air, as an expression of its elasticity, with the height of the mercury column in the same tube. He was not then comparing the air's elasticity with the external pressure exerted on it, which must be calculated from the barometric height and the height of the mercury in the experimental tube.

A final significant feature of POWER's experiments was, that in his enthusiasm to derive a quantitative expression for the elasticity of air, he lost sight of its weight, which led him in another experiment to give an incorrect explanation for the action of the siphon. His experiments as a whole show that he had only a limited ability to reduce his experiments into the terms of quantitative hydrostatics.

These tentative and inconclusive experiments on the spring of air bring to an end POWER's first series of experiments, and his interest in the subject was not revived until 1660, when he read the work of ROBERT BOYLE.

VII. Boyle's First Researches on Air Pressure: The Pneumatic Engine

As I have previously pointed out, BOYLE was probably familiar with the Torricellian experiment from the time of its introduction into England in 1647, but it was not until 1658 that he began his more systematic study of the physical and chemical properties of air. The greatest stimulus to this work was the invention of the air pump by OTTO VON GUERICKE in about 1654, and BOYLE realised that this apparatus could be used for performing experiments *in vacuo*. It is not certain how BOYLE was introduced to VON GUERICKE's apparatus,⁷⁹ since the latter's book was not published until 1672, but it is probable that he was told about it by one of his correspondents, who might have seen the experiments or read the account of the pump in SCHOTT's *Mechanica hydraulico-pneumatica*, which was published in 1657.⁸⁰ According to BOYLE's account he had contemplated the evacuation of a vessel with a suction pump prior to 1658, but the success of the German experiments convinced him to prosecute his own investigations.⁸¹

⁷⁷ Ibid., p. 107.

⁷⁸ Ibid., p. 116. While it would have been possible to derive a functional relationship between the mercury height and the air volume, this would have been completely beyond POWER's mathematical abilities.

⁷⁹ OTTO VON GUERICKE, *Experimenta Nova (ut vocantur) Magdeburgica de Vacuo Spatium a R. P. Gaspare Schotto*, Amsterdam, 1672.

⁸⁰ CASPAR SCHOTT, *Mechanica Hydraulico-Pneumatica*, Würzburg, 1657.

⁸¹ ROBERT BOYLE, 1660, op. cit., *Works*, vol. 1, pp. 4—5.

By January 1658 BOYLE had written to HARTLIB about "the German vacuum as of no ordinary beauty",⁸² and in the same year he employed RALPH GREATOR, the London instrument maker, and ROBERT HOOKE, his assistant at Oxford, to construct an air pump. The final design and construction of the apparatus were mainly the work of HOOKE, but GREATOR may have been responsible for drilling the piston cylinder.⁸³

BOYLE could have gained little practical information about the construction of the pump from SCHOTT's book. Indeed, it is probable that HOOKE's pump greatly exceeded VON GUERICKE's in efficiency and design, since the German later modified his own design to conform to HOOKE's, thus accounting for the similarity of even superficial points of construction between the pumps illustrated in BOYLE's work of 1660 and VON GUERICKE's of 1672.⁸⁴

There were two major differences between the German and English pumps; the German pump used a lever to operate the piston and the evacuated globe had only one opening, which was attached to the pump; while BOYLE's pump used a more efficient rack and pinion for moving the piston and there was a second opening into the globe, which enabled the insertion of experimental objects.

Since there are many adequate descriptions of BOYLE's air pump⁸⁵ or "pneumatic engine", I will consider only its application to the examination of air's elasticity. BOYLE's description of the pump and numerous chemical and physical

⁸² Letter from SAMUEL HARTLIB to BOYLE, January, 1658; *Works*, vol. 5, p. 271.

⁸³ HOOKE's contribution to the construction and design of BOYLE's air pump has been discussed by, R. WALLER, *Posthumous Works of Hooke*, London, 1705, pp. iii—iv; R. T. GUNTHER, *Early Science at Oxford*, vol. 6, Oxford, 1936, pp. 8—9, 70—72; R. F. FULTON, *Bibliography of the Honourable Robert Boyle*, Second edition, Oxford, 1960, p. 11; H. D. TURNER, Robert Hooke and Boyle's air pump, *Nature*, 1959, 184, pp. 395—397.

⁸⁴ See Figure 4. This apparatus is very similar to that on the title page of VON GUERICKE's work. See J. R. PARTINGTON, *History of Chemistry*, vol. 2, 1961, p. 516.

⁸⁵ Good descriptions of BOYLE's air pump are to be found in, R. BOYLE, *Works*, op. cit., vol. 1, pp. 4—5; E. N. DA C. ANDRADE, The early history of the vacuum pump, *Endeavour*, 1957, 16, pp. 29—41; J. B. CONANT, *Harvard Case Histories in Experimental Science*, 2 vols. Camb., Mass. 1950, vol. 1, pp. 1—64; G. WILSON, *Religio Chemicæ*, London, 1862, pp. 193—218.

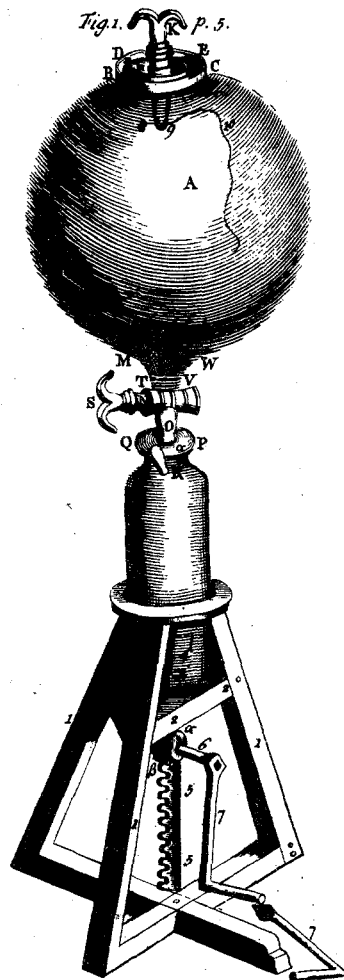


Fig. 4. R. BOYLE, *New Experiments Physico-Mechanicall*, London, 1660, *Works*, vol. 1, Endplate Fig. 1. BOYLE's air pump or "pneumatic engine"

experiments⁸⁶ using it were published in Oxford in 1660, as *New Experiments Physico-Mechanicall, Touching the Spring of the Air*.⁸⁷

As the title and introductory experiments indicate, BOYLE was particularly concerned with the examination of the hypothesis of the spring of air, having once repeated the proofs of its weight.

That he was stimulated to examine the elasticity of air by the French experiments is suggested at the very beginning of the work, when he admitted that he had been induced to study the Torricellian experiment by "perceiving by letters from some other ingenious persons at Paris, that several of the Virtuosi there were very intent upon the examination of the interest of the air, in hindering the descent of the quicksilver". Since he knew of PECQUET'S book, and quoted from it in the second edition of his own work, it is possible that this was an important "French" influence on his concept of air pressure.

The first part of the work was focussed on proving that the air had the properties of weight and spring. Weight was the primary quality of air, and this characteristic was deduced from PASCAL'S Puy de Dôme experiment, the Torricellian experiment and by the direct weighing of air. He was not satisfied that previous trials had been sufficiently accurate; he used methods similar to those of GALILEO and MERSENNE, but achieved considerably greater accuracy, finding that water was 938 times as dense as air.⁸⁸

He was equally convinced of the elasticity of air, and he sought a theoretical model which would explain this phenomenon. He favoured most a model very like that proposed by POWER, except that he was undecided whether the corpuscles were suspended in an aether or void. His opinion is quoted in full as Appendix V. As the investigations of the physical properties of air progressed he incessantly referred to this theory of elater or spring. However, he was undecided whether this represented the true structure of air, since air was able to dilate without prior compression.⁸⁹ He presented DESCARTES' theory of the structure of air and noted that it was of little significance what shape was suggested for the corpuscles, since their pressure resulted from "the vehement agitation ... which they receive from the fluid aether."⁹⁰

While BOYLE adopted explanations of physical processes by his "Mechanical or Corpuscular Philosophy", he carefully avoided commitment in the disputes about the ultimate mechanical principles. Thus he used the plenist and vacuist models of air to illustrate the phenomena of elasticity, but he was careful to conclude that it was probable that neither represented the air's true physical constitution. This attitude helps to explain the indignation which he felt when HOBBS accused him of introducing the vacuist interpretation, and it was perhaps

⁸⁶ The various experiments performed with BOYLE'S pump have been discussed by G. WILSON, *op. cit.*, pp. 219—228; J. R. PARTINGTON, 1961, *op. cit.*, pp. 520—528.

⁸⁷ ROBERT BOYLE, *New Experiments Physico-Mechanicall, Touching the Spring of the Air*, Oxford, 1660. — *New Experiments Physico-Mechanical, Touching the Air. The Second Edition. Whereunto is added A Defence of the Authors Explication of the Experiments, Against the Objections of Franciscus Linus, And, Thomas Hobbes*, Oxford, 1662.

⁸⁸ R. BOYLE, 1660, *op. cit.*; *Works*, vol. 1, pp. 12—13, 55—56.

⁸⁹ *Ibid.*, p. 9.

⁹⁰ *Ibid.*, p. 8.

this misinterpretation which provoked the pathologically impartial BOYLE to write a stinging criticism of HOBBS in the second edition of the *New Experiments Physico-Mechanical* in 1662.

He demonstrated the elasticity of air by ROBERVAL's carp-bladder experiment, which had originally been mentioned to him by HARTLIB in 1648.⁹¹ He then showed that a similar result ensued if the bladder was placed in an exhausted glass globe. The elasticity of air was influenced by temperature as well as external pressure. This was shown by moving a partially inflated bladder nearer and further from a fire. When the bladder was brought nearer to the fire "the elastical power of the same quantity of air may be as well increased by the agitation of the aerial particles". When the bladder was moved away from the fire, the bladder deflated; but if it was taken too near, it burst.⁹²

It has often been overlooked that BOYLE was not only concerned to give a qualitative demonstration of air's elasticity, but he also wished to reduce this phenomenon to a quantitative law. As early as Experiment 7⁹³ he turned to this problem, when he examined the extent to which a volume of air would expand when subjected to reduced pressure.

A small glass tube, 6 or 7 inches long, was sealed at one end and a parchment calibrated scale was attached to the side. The scale ended just below the apex of the tube, which was expanded into a small glass globe. The apparatus was filled with water, except for this globe, which contained an air bubble. The open end of the tube was immersed in a small dish of water, and the whole apparatus was placed in the globe of the evacuation apparatus.

As the globe was exhausted the air bubble expanded until it filled the whole tube. In the next experiment he found that air had an even greater capacity for expansion and it could increase its volume its volume by 252 times. This confirmed MERSENNE'S experiments on the rarefaction of air.

A more satisfactory method for studying the quantitative aspect of the elasticity of air was described in Experiment 17. The Torricellian experiment was set up, and the dish, containing the residual mercury and the lower end of the Torricellian tube, was enclosed with the glass globe of the exhaustion apparatus. BOYLE pointed out that the mercury was now cut off from the weight of the atmosphere and must be supported by the elasticity rather than the weight of the air.

... upon which closure there appeared not any change in the height of the mercurial cylinder, no more than if the interposed glass receiver did not hinder the immediate pressure of the ambient atmosphere upon the inclosed air; which hereby appears to bear upon the mercury, rather by virtue of its spring than of its weight; since its weight cannot be supposed to amount to above two or three ounces, which is inconsiderable in comparison of such a cylinder of mercury as it would keep from subsiding.⁹⁴

The hypothesis that the mercury in the Torricellian tube was now supported by the elasticity rather than the spring of air, was an essential assumption for his following attempt to detect the relationship between the elasticity and volume

⁹¹ See footnote 45.

⁹² *Ibid.*, p. 13.

⁹³ *Ibid.*, p. 14.

⁹⁴ *Ibid.*, p. 22.

of air. BOYLE saw that the 29 inches of mercury in the Torricellian tube could not now be supported by the *weight of a column of air* since it was effectively cut off from the atmosphere by the sealed glass globe. The mercury was now supported by the resistance of the particles of air.

A similar conclusion had previously been reached by TORRICELLI, who had been asked by RICCI⁹⁵ to explain the suspension of the mercury cylinder under similar circumstances; he had replied that the lowest layers of air were compressed by the upper, and this degree of compression was uninfluenced by placing the air in an enclosed place:

... but if the air that you include is more rarefied than the external air, then the suspended metal will descend by the proper amount; if now it were infinitely rarefied, i.e., a vacuum, then the metal would descend all the way, provided that the enclosed space were able to take it.⁹⁶

However, there is no evidence that TORRICELLI made any experimental confirmation of this important hypothesis. Indeed, there is evidence that he avoided further development of this work, perhaps in deference to religious disapproval of unorthodox tendencies. Also, while he appreciated the general nature of the air's elasticity, he was not sure that density alone was not responsible for changes in pressure. Thus, reduction of air pressure at high altitudes was due to its greater "purity", and at low altitudes heat caused air to be "more subtle and light"

It was left for PASCAL to examine the influence of altitude on the height of the mercury column and for BOYLE to rarefy air with his vacuum pump. More than this, he hoped that he might derive a law from the results, which would relate the density of the enclosed air to its elasticity. The former could be estimated from the stroke of the pump, since the dimensions of the piston cylinder were accurately known; the latter was measured directly as the height of the mercury column.

This experiment was conducted in the presence of WREN, WALLIS and WARD, three of the nation's most prominent mathematicians, and they found that the descent of mercury decreased with the increasing exhaustion.

We formerly mentioned, that the quicksilver did not, in its descent, fall as much at a time, after the two or three first exsuctions of the air, as at the beginning. For, having marked its several stages upon the tube, we found, that at the first suck it descended an inch and $\frac{3}{8}$, and at the second an inch and $\frac{3}{8}$, and when the vessel was almost emptied, it would scarce at one exsuction be drawn down above the breadth of a barley-corn.⁹⁷

He concluded that:

We could not hitherto make observations accurate enough concerning the measures of the quicksilver's descent, to reduce them to any hypothesis, yet would we not discourage any from attempting it, since, if it could be reduced to a certainty, it is probable, that the discovery would not be unuseful.⁹⁸

⁹⁵ Letter from RICCI to TORRICELLI, 18 June, 1644; TORRICELLI *Opere*, op. cit., vol. 3, pp. 21—22.

⁹⁶ Letter from TORRICELLI to RICCI, 28 June 1644, *Opere*, vol. 3, pp. 198—201. The quotation is taken from KNOWLES MIDDLETON 1963 *op. cit.* (see footnote 1), p. 23.

⁹⁷ R. BOYLE, 1660, op. cit., *Works*, vol. 1, p. 23.

⁹⁸ *Ibid.*, p. 23.

This failure to obtain a satisfactory hypothesis was not surprising, even given the help of prominent mathematicians. Comparisons between the readings would be rendered useless, since the leakage of the container increased as it was progressively exhausted. In addition, the Torricellian tube was not calibrated, and when the mercury level had fallen below the level of the lip of the globe, the level was guessed by eye.⁹⁹

It was realised that leakage was insignificant during the first strokes of the pump, therefore he repeated his experiment, hoping to compare the results for the initial exsuctions of globes of different capacities. This, he hoped, might lead to a satisfactory hypothesis.

But on this occasion, I hold it not unfit to give your Lordship notice, that I hoped from the descent of the quicksilver in the tube, upon the first suck, to derive this advantage; that I should thence be enabled to give a nearer guess at the proportion of force betwixt the pressure of the air (according to its various states, as to density and rarefaction) and the gravity of quicksilver, than hitherto hath been done.¹⁰⁰

This method would have proved satisfactory had he chosen globes of suitable sizes, but his first choice was a globe which held less than a quart of liquid, or less than 69 cubic inches of air. Thus, the piston was capable of withdrawing 99 cubic inches in one stroke, and almost completely exhausting the globe. However, such a great reduction in pressure would cause considerable leakage, and BOYLE found that the mercury fell to $8\frac{1}{2}$ or $9\frac{1}{2}$ inches. Therefore, this result could not be compared with the first strokes in his former experiment. He also suggested that the use of a small globe introduced other difficulties. The residual mercury volume decreased as that in the tube fell; he suggested that this would influence the accuracy of the pressure determination. Further, the subsequent volumes of air extracted from the globe would be more rarefied, and consequently it would not be comparable to an equal volume of air at atmospheric pressure.

With these difficulties in mind, he concluded:

Because of these (I say) and some other difficulties, that require more skill in mathematics than I pretend to, and much more leisure than my present occasions would allow me, I was willing to refer the nicer considerations of this matter to some of our learned and accurate mathematicians, thinking it enough for me to have given the hint already suggested.¹⁰¹

This confession should not be read as indicating BOYLE's weakness at mathematics, as many commentators¹⁰² have understood it, but an indication of the

⁹⁹ Ibid., p. 23.

¹⁰⁰ Ibid., p. 24. BOYLE gave sufficient information for it to be established that there was little leakage during the first few strokes of the pump. The glass globe of the apparatus held 30 quarts of liquid, or about 1.2 cubic feet. Since it was approximately spherical, its diameter was about 8 inches. The piston cylinder was 14 inches long and 3 inches in diameter and its maximum capacity was therefore 99 cubic inches. A single stroke of the piston was able to exhaust about 4.8% of the sphere's content of air. At the first stroke the mercury fell from 27 to $25\frac{5}{8}$ inches, which was a fall of 5.1%. Had there been leakage, the mercury would have fallen less.

¹⁰¹ Ibid., p. 24.

¹⁰² R. T. GUNTHER, *Early Science in Oxford*, vol. 6, Oxford, 1930, p. 73. GUNTHER attributes the failure of BOYLE's work to his lack of mathematical ability combined with weak eyesight.

inherent difficulties of this particular experiment for determining the relationship between the pressure and volume of air. The importance of these experiments are that they represent BOYLE'S first systematic attempt to determine the law relating the two dependent variables, the "spring" of air (which he now terms its "pressure") and its density.¹⁰³

Although BOYLE'S initial attempt to determine the law was a failure, the experiment is of the greatest interest as an illustration of BOYLE'S approach to an experimental problem. He had clearly in his mind the goal of the experiment which was to obtain a table which would compare the different densities and related pressures of air. This ambition was thwarted by the technical imperfections of the apparatus, and it is interesting that neither he nor his mathematical colleague were willing to risk the formulation of a hypothesis on the initial results of their first experiment, although we now realise that these indicated a proportionality between density and pressure.

BOYLE then sought to avoid the former experimental errors, but unfortunately he chose an expedient which increased the risk of leakage and introduced further complications which reduced the validity of comparisons with the former experiment.

The publication of BOYLE'S experiments in 1660 led to great interest in the hypothesis of the weight and spring of air. LINUS and HOBBS¹⁰⁴ produced protracted refutations of his explanations, which had the valuable function of stimulating BOYLE to add a lengthy Appendix to the former work, and to embark on further experiments. It also stimulated various authors to engage in this fruitful experimental study. These included a large committee¹⁰⁵ of the newly instituted Royal Society, GEORGE SINCLAIR in Scotland, and POWER and TOWNELEY in the north of England.

VIII. Power and Towneley: The Direct Measurement of the Elasticity of Air

It has previously been mentioned that RICHARD TOWNELEY was to some extent involved in POWER'S experiments of 1653, and when these experiments were resumed in 1660, it was upon the initiative of both men.

I have already given the major biographical details by POWER¹⁰⁶, but have only made incidental reference to RICHARD TOWNELEY.

TOWNELEY'S name is not insignificant in the history of seventeenth century science, for he was associated with the invention of the rain-gauge, the improvement and publication of the details of GASCOIGNE'S micrometer, meteorological

¹⁰³ These experiments correct the misconception that BOYLE made no attempt at a quantitative estimation of the spring of air during his initial work in 1658 and 1659.

¹⁰⁴ FRANCISCUS LINUS, *Tractatus de corporum inseparabilitate*, London, 1661. — THOMAS HOBBS, *Dialogus physicus de natura aeris*, London, 1661.

¹⁰⁵ On January 16th 1660/61, the Society appointed a committee to study the Torricellian experiment. This consisted of BALL, BRÖUNCKER, BOYLE, CLARKE, HILL, NEILE, MORAY, ROOKE and WREN. T. BIRCH, 1756/57, *op. cit.*, vol. 1, p. 12.

¹⁰⁶ For further details of HENRY POWER'S life and work, see my M. Sc. Dissertation "The Scientific Work of Henry Power, M. D." London University Library: also T. COWLES, Dr. Henry Power, Disciple of Sir Thomas Browne, *Isis*, 1933/34, 20, pp. 344—366; J. W. CLAY, Dr. Henry Power of New Hall, F. R. S., *Halifax Antiquarian Society, papers, reports*, 1917, pp. 1—31.

records and the suggestion of the hypothesis, which has become known as BOYLE'S Law. There has, however, been a noticeable lack of biographical information about him, and he has been both placed in the wrong century and confused with his uncle, CHRISTOPHER TOWNELEY of Carr and Moorhiles (1603—1674).¹⁰⁷

I will therefore include a few biographical details in order to clarify the position of this mysterious figure of seventeenth century science.¹⁰⁸ The TOWNELEY family were important landowners in South Lancashire, and they traced their descent to an ecclesiastical dean of Whalley, two centuries before the Norman Conquest. Their chief seat was at Towneley, near Burnley. RICHARD TOWNELEY (TOWNLEY) was born in 1629 and was the eldest son of CHARLES TOWNELEY, who was killed at the battle of Marston Moor in 1644. WILLIAM GASCOIGNE, a friend of the TOWNELEY family and physicist was killed in the next year. The Royalist and Catholic sympathies of his family, as well as a retiring disposition, may explain why RICHARD entered little into public affairs and avoided becoming a member of the Royal Society.

He married MARGARET PASTON,¹⁰⁹ daughter of CLEMENT PASTON of Barningham, Norfolk, who was from the famous Norfolk family whose letters provide indispensable evidence about the social history of England in the fifteenth and sixteenth centuries. RICHARD lived at Nocton, near Lincoln, and at Towneley, where he devoted himself to the study of science and mathematics, leaving the management of his estates to his younger brother, CHARLES (1631—1711). CHARLES, and another brother, JOHN (1630?—1678), assisted RICHARD in his scientific work. Although he was not educated at a University, he had a sound understanding of Latin and he was familiar with the major activities in many branches of natural philosophy.¹¹⁰ He added to the library at Towneley, which was already one of the finest private libraries in the north of England by the beginning of the seventeenth century.

His scientific interests were very similar to those of POWER, and the latter was both physician to the TOWNELEY family and collaborator in scientific activities, between 1653 and 1664. Both men were deeply influenced by the writings of DESCARTES as well as the English authors, BOYLE, WILLIS and HENRY MORE.

¹⁰⁷ CHRISTOPHER TOWNELEY was a celebrated antiquarian.

¹⁰⁸ Information about RICHARD TOWNELEY and his family is scattered through many works. These include; — CHARLES LEIGH, *The Natural History of Lancashire, Cheshire, and the Peak, in Derbyshire*, Oxford, 1700, pp. 17—26. — RALPH THORESBY, *Ducatus Leodiensis*, Leeds, 1715. — J. HUNTER ed. *The Diary of Ralph Thoresby F. R. S., 1677—1724*, 2 vols., London, 1830; *Letters of Eminent Men addressed to Ralph Thoresby*, 2 vols., London, 1832. — THOMAS D. WHITTAKER, *An History of the Original Parish of Whalley*, Blackburn, 1801. — F. R. RAINES, ed., *The visitation of the County Palatine of Lancaster, made in the year 1664/65 by Sir William Dugdale*, Chetham Society Publications, vol. 88, Manchester, 1873, pp. 304—307. — W. T. LANCASTER, *Letters addressed to Ralph Thoresby F. R. S.*, Thoresby Society, Leeds, 1912.

¹⁰⁹ *The Paston Letters A. D. 1422—1509*, 6 vols., ed. JAMES GAIRDNER, London, 1904.

¹¹⁰ RICHARD TOWNELEY'S manuscript "Some short considerations upon Mr. Hooke's attempt for ye Explication of ye Expt. of waters ascent into small Glasse Canes," is preserved in Yale University Library. An appended note by R. T. GUNTHER wrongly attributes the work to CHRISTOPHER TOWNELEY.

Their collaboration was probably hindered when POWER moved to Wakefield in 1664; this was followed by intermittent illhealth, and he died in 1668.

Towneley Hall was to some extent the focus of scientific activity in the north of England; writings of JEREMIAH HORROCKS and WILLIAM GASCOIGNE passed into RICHARD TOWNELEY's hands, and he was visited by many intellectuals. During the last years of his life he lived at York and he died there on 22nd January, 1706/7.

POWER and TOWNELEY read BOYLE's *New Experiments Physico-Mechanicall* in the year of its publication, and this resulted in their returning to the study of air pressure and particularly to the investigation of the elasticity of air which they had so effectively demonstrated in 1653.

They now undertook a second series of experiments which were performed at Towneley Hall in 1660 and 1661. POWER wrote an account of these experiments in the early summer of 1661, entitled:

Additional Experiments made at Townley Hall, in the years 1660 and 1661, by the advice and assistance of that Heroick and Worthy Gentleman, RICHARD TOWNLEY, ESQR. and those Ingenious Gentlemen Mr. JOHN, and CHARLES TOWNLEY, and Mr. GEORGE KEMP.

This was eventually printed in the second part of his *Experimental Philosophy*, in 1663.¹¹¹ The account began by eulogising BOYLE's work.

The last year, 1660, came out that excellent Tractate of *Experiments* of Esqr. Boyle's, with his Pneumatical Engin, or Ayr-pump, invented, and published by him, wherein he has by virtue of that rare Contrivance, outdone all that ever possibly could be performed by our late *Mercurial* and *Experimental* Philosophers: And, indeed, to give a true and deserved Character of that worthy Production of his, I must needs say, I never read any such Tractate in all my life, wherein all things are so curiously and critically handled, the Experiments so judiciously and accurately tried, and so candidly and intelligibly delivered. I no sooner read it, but rubbed up all my old dormant Notions, and gave me a fresh view of all my former, and almost forgotten, *Mercurial* Experiments. Nay, it had not that effect onely on me, but likewise it excited and stirr'd up the noble soul of my ever honoured Friend, Mr. Townley, together with me, to attempt these following Experiments.¹¹²

POWER's account of these experiments had features in common with his previous tract of 1653. The work was in the form of a series of experiments, which were described briefly, and in certain cases incompletely. Again, the elasticity of air was the subject which received greatest emphasis, but there was less discussion of the vacuist-plenist controversy than in the former work. As before,

¹¹¹ HENRY POWER, *Experimental Philosophy*, London, 1663, pp. 121—137. A manuscript version of the same experiments, having the same title, is in the British Museum, *Sloane* MS 1393, ff. 154—167. — The italics and capital letters in the quotation are as written by POWER. I have previously pointed out that the presentation of the title in this form was probably the cause of BOYLE attributing the experiments to TOWNELEY; Richard Towneley and Boyle's Law, *Nature*, 1963, 197, pp. 226—228. — POWER wrote two other short tracts on air pressure between 1660 and 1663. The first, "A Confutation of this Funicular Hypothesis of Linus; by Henry Power, M^{ac}.D^r;" was published in *Experimental Philosophy*, pp. 138—142. The second, "Physical experiments using Mr. Boyle's Improved Engine, July 1663," BM *Sloane* MS 1326, ff. 46—48. He was one of the first scientists to give independent confirmation of BOYLE's experiments on the air pump.

¹¹² HENRY POWER, 1663, op. cit., pp. 121—122.

a few experiments on siphons, were incompletely explained and included as curiosities.

In 1653 he was aware of PASCAL'S experiment, which supported TORRICELLI'S prediction that water should rise to 33 feet when supported by the weight of the atmosphere. He had, however, been unable to construct an apparatus to confirm this experiment. They now attempted it again at Townley Hall,¹¹³ and their account is interesting in illustrating the technical imperfections of their methods. It was not possible to construct the apparatus in glass; therefore, tin tubes were constructed out of sheets of tin; these were fastened together by pewter solder to form a tube 33 feet long. The only part of the apparatus made of glass was the end section. This tube was erected against the corner turret of Towneley Hall, and it was filled from its upper end. When the tube was full of water the top was sealed, and the bottom was immersed in a cistern of water. The water fell, it was guessed, to 32 feet, but it had unfortunately fallen below the glass tube and it continued to subside, since there was a leakage at the junction of the glass and tin tubes.

PASCAL'S experiment was supported by another experience. A neighbour, at Heath Hall, near Wakefield, wished to raise water to a height of 48 feet, and the scientists derived satisfaction in the failure of this exercise.

For I remember in my *Lady Bowles* her new Water-work at Heath-Hall, near *Wakefield*, where the Water is raised at least 16 yards high, the simple workman undertook first to do it by a single Pump; but seeing his endeavours were frustrated, he was forced to cut his Cylinder into two Pumps, and to raise it, first, eight yards into a head-cistern, and then by another Pump to raise it out of that other, eight yards, into a cistern above.¹¹⁴

On 27th April, 1661 they returned to their examination of the elasticity of air, adopting the apparatus which had been used in their first quantitative studies in 1653. Equal volumes of air and mercury were placed in a Torricellian tube and upon inversion into a dish of mercury, the expansion of the included air was measured. The new feature of this experiment was, that the expansion of the air was measured at different altitudes on Pendle Hill, in Lancashire, which was conveniently near Towneley.¹¹⁵

At the top of the said Hill, we put into the same Tube 42 inches long (which was divided into 102. equal divisions of spaces) as much Quicksilver, as being stop'd and inverted, the Ayr remaining at the top of the Tube, fill'd 50/15, or there about, of the forementioned divisions, and the quicksilver, the remaining part of the Tube. The tube being thus immers'd, and the finger withdrawn, the internal Ayr dilated so as to fill of the above mentioned parts 84/75 and there remained in the Tube a Cylinder of Quicksilver containing in length 11/26 inches. We tried the same Ex-

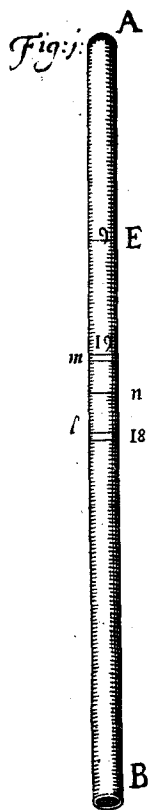
¹¹³ *Ibid.*, pp. 131—132. BOYLE had described this experiment in 1660, *Works*, vol. 1, pp. 28—29. Like POWER, the Royal Society had difficulty in obtaining the apparatus for this experiment, and the first successful performance, recorded by BIRCH, was on 16th July, 1663; T. BIRCH, 1756/57 op. cit., pp. 76—80, 86, 103, 255, 266, 271, 273, 279, 286, 259.

¹¹⁴ HENRY POWER, 1663, op. cit., pp. 131—132. — GALILEO, 1638, op. cit., pp. 16—17.

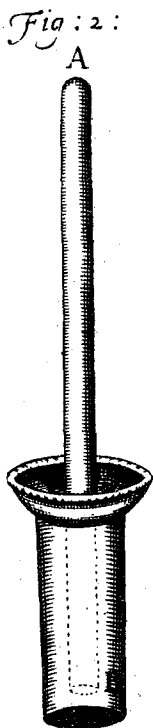
¹¹⁵ HENRY POWER, 1663, op. cit., p. 127. The altitudes of the stations mentioned by Power were: Pendle New Church — 850 ft.; Barlow (Barley) — 750 ft.; Pendle Hill summit or Beacon, 1,834 ft.

periment at the bottom of the said Hill, the Tubes being fill'd, as above, and the Ayr 50/15. dilated to 83/8. and the Cylinder was in height 11/78 inches.¹¹⁶

The elasticity of the air had thus *decreased* as Pendle Hill was descended. This observation was correlated to another experiment in which the "extended"



Tab: 5:



air pressure was found to *increase* during the descent in height. At Pendle New Church, the level of mercury in the Torricellian tube was 28.4, while at the summit of the hill it was 27.4 inches. This result was consonant with POWER's former experiment on Halifax Hill, where the fall was less than an inch, this hill being considerably lower than Pendle Hill.

The experiment was now repeated using a tube of 26 inches, which was divided into 31 divisions. At the apex of the hill 9 divisions of air were included, above the mercury. Upon inversion into the residual mercury, the air expanded to occupy 17.8 divisions and the mercury stood at 13.86 inches. The tube was again carried down the hill.

We brought this Tube, with the same Mountain Ayr in it, by the help of a long Tube of wood, having a dish fastned to the open end of it, and both full of Quicksilver, into which we put our Tube, AB, (which instrument you have here represented) and at the bottom of the Hill the Quicksilver rose up unto the mark *m*, under the 17. division. So that the Ayr dilated, fill'd of the equal parts 17/35, and the Quicksilver in B was in height 14/41. inches.¹¹⁷

They had ensured that the same sample of "Mountain Ayr" was measured at the two altitudes. They now exchanged that air for "Valley Ayr" and repeated the experiment at the lower

Fig. 5. H. POWER, *Experimental Philosophy*, London, 1663; Endplate, Tab. 5. The apparatus for carrying the Torricellian tube up Pendle Hill. Air occupied the space *AE* before expansion. This coincided with the 9 unit mark. The air expanded to either 17.35 (*m*) or 17.8 (*l*). It is obvious that POWER's diagram is ambiguous; the 19 mark is in the wrong position and the meaning of the various height levels is not explained

altitude. A slight difference in the result was found; nine units of air expanded to 17.58 units, and the mercury stood at 14.20.¹¹⁸

The results were now presented in tabular form, in which the mercury heights were given in inches and the air volumes in the previous^{ly} mentioned arbitrary units. Scrutiny of the diagram and the table which accompanied this experiment

¹¹⁶ *Ibid.*, p. 127; the apparatus is shown in Figure 5.

¹¹⁷ *Ibid.*, p. 128.

¹¹⁸ *Ibid.*, p. 128. In POWER's Table of results on p. 129, the final mercury level was given as 14.02 units; see Figure 6.

shows them to contain numerous minor errors, which are typical of POWER'S work. The lack of attention to details of presentation, diagrams and tabulated numerical results, contrast POWER'S *Experimental Philosophy* with HOOKE'S *Micrographia*, which was also published by MARTIN and ALLESTREE, only a short time after POWER'S book. Indeed the plates for the two works were being executed at the same time in 1663.¹¹⁹

In the long Tube.

<i>At the top of the Hill.</i>	<i>At the bottom of it at Barlow.</i>	
A E — 50 15	— 50 15	} Equal parts of Spaces, Inches.
A D — 84 75	— 83 8	
B D — 11 26	— 11 78	

In the lesser Tube.

<i>At the top of the Hill.</i>	<i>At Barlow with Ayr.</i>	<i>At Barlow with Valley-Ayr.</i>
A E — 9	— 9	— 9
A D — 17 8	— 17 35	— 17 58
B D — 13 86	— 14 31	— 14 02

Fig. 6. H. POWER, *Experimental Philosophy*, London, 1663; Table from page 129. This is POWER'S only attempt at a tabular presentation of his results. It has certain ambiguities. In the upper part of the Table the bracket on the right hand side should apply to the first two lines only. The units are not indicated at all in the lower half of the Table, and 14.02 should be 14.20

The account was terminated with a short consideration of the theoretical implications of these results.¹²⁰ This also was deficient in clarity of expression. The height of the mercury in the Torricellian tube was called the *Mercurial Standard*, and the difference between this and the final level of mercury in the experimental tubes (the *Mercury*), was called the *Mercurial Complement*. Each of these values were presented in inches, and the *Mercurial Standard* represent the pressure of the atmosphere, whereas the *Mercurial Complement* represented the strength of the Spring or Elatery of the air enclosed in the experimental tubes.

The original volume of air enclosed in the tube at atmospheric pressure was the *Ayr*, this expanded, and the new volume was *The Ayr Dilated*. The difference between these two values was the *Ayr's Dilation*. These were measured in either of two different arbitrary units.¹²¹

¹¹⁹ Letters from MARTYN and ALLESTREE to POWER, August and September, 1663, BM Sloane MS 1326, ff. 39—40.

¹²⁰ HENRY POWER, 1663, op. cit., pp. 129—130. I have given a preliminary analysis of POWER'S results in an earlier article; — see footnote 111.

¹²¹ POWER'S original Table of results is given in Figure 6; my amplified and corrected version of his results is given as Table 2.

Table 2. *The expansions of air at different pressures*

	Altitude (ft.)	Long tube		Short tube			(1)	(2)
		800	1,800	800	800	1,800		
Volume of the enclosed air Air Pressure	(0.41 inch units)	50.15	50.15				AE	} The Ayr Mercurial Standard
	(0.84 inch units)			9	9	9	AE	
	(inches)	28.4	27.4	28.4	28.4	27.4	B ₁ C	
Volume of ex- panded air Reduced mer- cury pressure	(0.41 inch units)	83.8	84.75				AD	} The Ayr Dilated The Mercury
	(0.84 inch units)			17.58	17.35	17.8	AD	
	(inches)	11.78	11.26	14.2	14.31	13.86	B ₁ D	

(1) Lengths indicated in POWER's text diagrams, and Diagram 1.

(2) The term used by POWER to designate each volume.

They now concluded:

So that here is now four Proportionals, and by any three given, you may strike out the fourth, by Conversion, Transposition, and Division of them. So that by these Analogies you may prognosticate the effects, which follow in all *Mercurial* Experiments, and predemonstrate them, by calculation, before the senses give an Experimental eviction thereof.¹²²

Unfortunately, the reader was left to guess the identity of the "four Proportionals". However, it is possible to assess them from the previous paragraphs. It is clear that they were comparing mercury values with air volumes. In the account gave three mercury variables — the *Mercury*, *Mercurial Standard* and *Mercurial Complement*, and three air volume variables — *The Ayr*, *Ayr Dilated* and *Ayr's Dilation*. Two pairs out of these two sets of three must be chosen. Since it is improbable that the above generalisation would have been made without having performed elementary calculations with the figures from these experiments, four of the above variables would be chosen which would enable the mentioned "prognostication". Their hypothesis would work if the Mercurial Standard and Mercurial Complement were compared with the Ayr and the Ayr Dilated. The *Mercury* can be eliminated, since it alone of the three figures is not a measure of external pressure, neither does it allow prediction of the results of other experiments. The *Ayr Dilated* is not used since it is obtained simply by subtraction from the two basic measurements of air volume.

Thus if Mercurial standard = p_1 .

Mercurial complement = p_2 .

Ayr = v_1 .

Ayr Dilated = v_2 .

The equation $p_1 v_1 = p_2 v_2$ enables the calculation of the results of similar experiments in which one of the values is unknown. It assumes the inverse proportio-

¹²² HENRY POWER, 1663, op. cit., p. 130; "eviction" is added from the *errata*.

nality between the volume and pressure of air, and any other assumption would prevent the prediction of the results of future experiments.¹²³

Although POWER presented the experimental results and hypothesis in the simplest form, it is obvious that this joint investigation had achieved considerably greater understanding of the elasticity of air, than in the experiments of 1653. The most important advances were that the hypothesis was no longer the result of a comparison between *the volume of air* and the *height of mercury* in the same tube, but between the *volume of air* and a measure of *its elasticity*, determined by the comparison of the atmospheric pressure and the height of mercury in the experimental tube.

This method had the initial advantage over BOYLE'S, that the volume of air was measured directly, whereas in BOYLE'S experiment it was calculated. BOYLE had measured the pressure of the air directly, whereas in the northern experiments it was calculated. This was however not as liable to error as the calculation of the volume in BOYLE'S experiment.

POWER and TOWNELEY had been familiar with the concept of the reduction of atmospheric pressure with altitude for many years; when they observed that the spring of air was increased by an increase in altitude, it is probable that they immediately associated it with the reduction in atmospheric pressure. Thus a few trials were sufficient to verify this and give them the intuitive realisation of the reciprocal relationship between the pressure and volume of air, although they did not verify this over a wide range of pressures, or explore its theoretical significance.

However, in 1662 they performed a series of experiments in coal mines, and they carried a weather glass down the mine and noted that the water in the apparatus rose 3 inches during the descent. In the second trial of this experiment, POWER noted:

Now we observ'd that in carrying idown of the said Glass in a Scoop from the top to the middle of the Pit, there the water did not rise so much as it did from the middle to the bottom, by half an inch; so that it seems the rise of the water was not proportional to the Glasses descent in the Pit.¹²⁴

It would be interesting to know if POWER realised the significance of this observation in relation to his former rule of proportionality for calculating the heights of mountains, or to his hypothesis for calculating the volume of air at increased pressures.

¹²³ The application of this hypothesis may be illustrated by an example taken from one of the experiments described in *Experimental Philosophy*. 50.15 units of air (v_1) were enclosed in a tube at atmospheric pressure, which was 27.4 inches (p_1); the hypothesis can be used to predict the volume (v_2) at the reduced pressure of 16.14 inches (p_2).

The hypothesis assumes that $p_1 v_1 = p_2 v_2$.

By substitution $27.4 \times 50.15 = (27.4 - 11.26) \times v_2$
 $90 = v_2$.

Therefore the hypothesis predicts that the air would expand from 50.15 units to 90 units, whereas in the experiment this volume expanded to 84.75 units. — In the experiment where 9 units were taken at 27.4 inches atmospheric pressure, the predicted increase in volume at 13.54 inches pressure was 18.3 units, whereas the experimental result was 17.8 units.

¹²⁴ *Ibid.*, p. 176.

Certain improvements in the technique of experimentation may be noted in the account of 1661, although it is inadequate by modern standards. The tubes were calibrated, and the most accurate figures were given to the nearest 1/20th of an inch. The levels of mercury reached in the experiment were not liable to fluctuation, which was one of the deficiencies of BOYLE'S experiment. The initial volume of air included in the tube was constant for any tube used, whereas in the 1653 experiments a different initial volume was used for each trial. Finally the experimental results were arranged in a Table which allowed the ready comparison of the variables.

Before considering the dissemination of the works of POWER and TOWNELEY, it is appropriate to consider briefly their general attitude to the plenist-vacuist controversy. Since their basic presuppositions showed no change, between 1653 and 1661, it is possible to consider the evidence from both treatises together. In addition, it should be noted that TOWNELEY'S criticism of HOOKE, written in two parts, in 1665 and 1667, contained an amplification of the opinions stated in *Experimental Philosophy*.

Both POWER and TOWNELEY were concerned to relate the experiments on air pressure to their general philosophy of nature, as had been the case with the French observers. However, there was a difference of emphasis; the English writers gave most emphasis to the spring of air and, since this had little relevance to the vacuist-plenist controversy, there was greater separation of the discussion of the experimental and philosophical problems. BOYLE, even more than TOWNELEY and POWER, by-passed the philosophical problem and was content with a mechanical repetition of the vacuist and plenist arguments, without indicating his own alignment.¹²⁵

Neither did BOYLE'S attitude change in the second edition of the *New Experiments Physico-Mechanical*. HOBBS accused him of adhering to the vacuist philosophy, but BOYLE indignantly accused his opponent of misrepresentation.

For neither has the society declared either for or against a vacuum, nor have I: nay, I have not only forbore to profess my self a Vacuist, or a Plenist, but I have in a fit place in my epistle expressly said, that I reserve the declaring of my own opinion touching the point to another discourse.¹²⁶

On the other hand, POWER and TOWNELEY were plenists and both based their physics on Cartesian principles. This is generally overlooked in discussions of the Cartesian influence in England. While the Cartesian influence on the philosophy of MORE and the physics of BOYLE is well known, POWER and TOWNELEY have been overlooked, although they provide outstanding examples of devotion to Cartesian physical principles among English scientists of the mid-seventeenth century.¹²⁷

¹²⁵ R. BOYLE, 1660, op. cit., *Works*, op. cit., vol. 1, pp. 24—25, 37, 48—49.

¹²⁶ *Ibid.*, pp. 121—122.

¹²⁷ The importance of POWER and TOWNELEY in the history of Cartesianism in England is overlooked in the two most important articles on this subject. MARJORIE NICOLSON, *The Early Stages of Cartesianism in England*, *Studies in Philology*, 1929, 26, pp. 356—374. S. P. LAMPRECHT, *The Role of Descartes in Seventeenth Century England*, *Studies in the History of Ideas* (Columbia University, New York), 1935, 3, pp. 181—242.

TOWNELEY and POWER were aware that there was little support for Aristotelian physics among their contemporaries, and both realised that the vacuist philosophy was in the ascendency in England. TOWNELEY began his "Preliminarie Discourse" with the statement that he would base his work on the principles of "Motion, Rest, figure and Size, ye principles I thinke only to bee made use of in ye Solution of Physicall problemes", and that he had adopted the "Cartesian Hypothesis exclusively to any othere. I know that though I hope for favour even in this particular, for by what I have seeie it appears to mee, that des Cartes and Epicure, (or rathere Gassendus) are at ye head of ye two factions which tend for superioritee in this mention'd Philosophicall reformation."¹²⁸

He went on to justify Cartesian principles, and devoted the majority of his discussion to the denial of vacuum and justification of aether.¹²⁹

POWER too, in the Preface to *Experimental Philosophy*, announced the principles of the "ever-to-be-admired Des-Cartes" which he intended to follow in his work.

He realised that the Torricellian experiment created difficulties for his Cartesian principles, and that French observers favoured the idea of vacuum. He therefore proposed that:

whereas they will have Rarefaction and Condensation to be performed without any increase or loss of quantity (which can never be conceived) we admit of an aethereal substance of Matter intromitted and excluded, the Bodies so changed as we formerly explicated.¹³⁰

At the same time POWER realised that the spring of air could be explained by the vacuist's principles as well as the plenist, but he preferred to retain the concept of aether.¹³¹

His reasons for denying vacuum were largely Cartesian; the concept was "non-philosophical", "very ridiculous;" all volumes having extension must contain matter, since extension was the designation of matter.

In addition he adopted the Aristotelian opinion that there could be no motion in a vacuum; since the space above the mercury in the Torricellian tube transmitted light and magnetism, and since these were substances, this space must also contain a medium. His statement of this principle illustrates the persistence of the Aristotelian notions of substance and quality, even among the mechanical philosophers ...

Again we have the sensible eviction of our own eyes to confute this Suppositional Vacuity, for we see the whole space to be Luminous (as by *Obser.*) Now Light must either be a Substance, or else how should it subsist (if a bare Quality) in a Vacuity where there is nothing to support it?¹³²

IX. Power and Towneley: Their Relationship with Boyle and the Royal Society

The experiments of POWER and TOWNELEY included valuable advances on the study of the elasticity of air, but they were not published until late in 1663

¹²⁸ RICHARD TOWNELEY, "A Preliminarie discourse by way of Preface, 10th April, 1667," ff. [3—4]. This is the introductory chapter to the work already mentioned: — see footnote 110.

¹²⁹ *Ibid.*, ff. [13—27].

¹³⁰ HENRY POWER, 1663, *op. cit.*, p. 132.

¹³¹ *Ibid.*, p. 133.

¹³² *Ibid.*, p. 95.

It would considerably reduce their significance in the history and science if they were not circulated to a wider audience before this date, since the second edition of the *New Experiments Physico-Mechanical* appeared in mid-1662. It is important to assess the extent that POWER's manuscripts were circulated and integrated into the fabric of contemporary knowledge, for the significance of an experimental advance lies not only in its recording, but also on the extent it is publicised.

Fortunately, there is good evidence about the activities of POWER and TOWNELEY between 1661 and 1663 from POWER's correspondence, which is preserved in the Sloane Collection of the British Museum. His letters show that their experiments on air pressure were circulated amongst the members of the Royal Society, and were of particular interest to ROBERT BOYLE.

POWER became interested in the activities of the Royal Society at an early stage, his contact with this group allowing them to benefit from the records of his researches, while he in turn was stimulated to pursue new investigations.

He first mentioned the Society in a letter to his friend, REUBEN ROBINSON, of Maldon, Essex, on 30 April, 1661, and in the same letter he referred to his recent experiments on air pressure.

I have here only sent you 2 or 3 of those observations I made in it, Besides this wee tried the Pascalian Experiment of the descent of Water in a Tin-Tube above 32 foot High: which wee found to be exactly true and proportionall to the mercurial cylinder in weight — Nay before wee dissolved our socyety wee all marched to the topp of Pendle Hill, One of the 3 famous Hills in England for Altitude, and there wee tryed the Torricellian experiment of the descent of mercury which was near there abundantly lower then at the bottom of the same Hill beside [sic.] Beside, wee try'd the difference of mountain-Aire and Valley-Aire counterchaning [sic.] their places, carrying the one upp and the other down from the topp of the Hill to the bottom in Tubes closely luted, to try the differençe of the elasticity; of these experiments I will send you a Transcript of the chiefe ere long: I heare much of the College for experimental learning you write of: is there never a member of it that you or I know that I might have Correspondence with; it might, I perchance do us both a courtesy.¹³³

It was about the same time that the Society heard of POWER's interest in science and WILLIAM CROUNE wrote his first letter to him on July 20th 1661.¹³⁴ It contained a request for POWER to send records of his investigations to the Society; he complied by sending his experiments on air pressure and magnetism, on 15th August.¹³⁵

¹³³ HENRY POWER, letter to REUBEN ROBINSON, 30 April, 1661, BM Sloane MS 1326, ff. 19—20. The following references to POWER's correspondence will refer to MS 1326 unless otherwise stated.

¹³⁴ WILLIAM CROUNE, letter to HENRY POWER, 20th July, 1661, f. 26. — POWER's name was introduced to the Royal Society by JOHN TILLOTSON the future Archbishop of Canterbury, who was one of POWER's friends and correspondents; he was a native of Sowerby near Halifax. TILLOTSON reported to CROUNE that POWER "had made a great many experiments with Mr Boyle's engin not try'd by him, very many magneticall, and some with the mercuriall cylinder," f. 26. On 8th May 1661, CROUNE was instructed by the Society to correspond with POWER. F. BIRCH, 1756/57, op. cit., vol. 1, p. 22. POWER reported to TILLOTSON that the correspondence with CROUNE had already begun in the early summer of 1661, f. 27.

¹³⁵ HENRY POWER, letter to CROUNE, 15 August, 1661; f. 27.

CROUNE replied on 14th September, in a letter, interesting because it referred to BOYLE'S particular interest in their later experiments on air pressure and his intention of referring to them in his forthcoming reply against LINUS.

I hold mysefve very particularly oblig'd to you for the favour you did mee in sending hither your Booke of Mercuriall Experiments, and I hope you will not thinke me unkind that I have not sooner return'd you both my owne thankes, and the hearty thankes of all our Company (which I am commanded by them to doe) when I shall informe you that I could not conveniently doe it before, because your Booke was in the hand of Mr. Boyle who importunately desired it, for a present concern of his own, being engag'd with one who calls him selfe Fransise: Linus (Indeed a Father of the Society of Jes:) here in Town, who pretends a differing Hypothesis of explicateing all the mercuriall phenomena from that of the weight of the Atmosphear. Some of your last experiments have donne Mr. Boyle a kindnesse, which I am certaine hee will mention in his answer to Linus, whose Booke I doubt not but you have received before this.¹³⁶

The letter continued with a brief account of LINUS' hypothesis and an apology for not having returned the account of the experiments on air pressure previously, since they were being transcribed by one of the Society.

When CROUNE acknowledged POWER'S reply against LINUS, in January 1662, he mentioned that BOYLE had still not completed his reply to LINUS, and intended to use information from this tract also:

especially that of the weather glasse.

He assured POWER that BOYLE intended to acknowledge this also.¹³⁷

BOYLE had thus informed CROUNE that he intended to use information from two manuscripts, which the latter had passed on to him. The first was the experiments of 1660 and 1661 which were concerned with the elasticity of air, and were the joint investigation of POWER and TOWNELEY. The second was HENRY POWER'S reply to LINUS, which contained an experiment in which a weather glass was carried up the steep hill above his home at Elland, near Halifax.

We are now in a position to elucidate the scanty references by ROBERT BOYLE to TOWNELEY'S work, in order to obtain an estimate of his debt to the northern investigators.

Direct reference was made to both POWER and TOWNELEY in the second edition of the *New Experiments Physico-Mechanical* which appeared in 1662. It was on the basis of this work and oral communications from BOYLE that the proportionality between the density of air and its pressure became known

¹³⁶ WILLIAM CROUNE, letter to HENRY POWER, 14 Sept. 1661; f. 25. — In a letter to ROBINSON (ff. 20—21) POWER hoped that his manuscripts would be communicated to BOYLE, and added that he was composing a reply to LINUS (see footnote 111). When this was completed, it was sent to the Royal Society and was acknowledged by CROUNE on January 9th, 1661/62; ff. 28—29.

¹³⁷ WILLIAM CROUNE, letter to HENRY POWER, 9 January, 1661/62, f. 28. — POWER sent experiments on siphons and capillaries to the Society in February, 1661/62, BM *Sloane* MS, 1393, f. 166). These were acknowledged by CROUNE on March 1st 1661/62, and in the same letter he reported that BOYLE'S reply to LINUS was not yet published; f. 30. It had not appeared by May 17th (f. 31), and it was finally sent to POWER on October 16th 1662 (f. 32). POWER wrote a letter of acknowledgement to BOYLE on 10 November, 1662, which was enclosed in a further letter to CROUNE on 19 November, 1662; ff. 33—34. There was consequently no direct correspondence between POWER and BOYLE.

as "Towneley's hypothesis" or "Townley's Theory" by seventeenth century authors, such as HOOKE and NEWTON.¹³⁸

As has been previously noted,¹³⁹ the second edition of BOYLE'S work was in three parts, which were probably available separately. The experiments of the 1660 edition formed the bulk of the volume, and the remainder consisted of a reply to HOBBS and one to LINUS. It was in this last part of the work, which was written during the closing months of 1661, that BOYLE returned to an experimental investigation of the elasticity of air. I have indicated that he had the experiments of POWER and TOWNELEY to aid him; as well as certain contributions by members of the Royal Society, which will be referred to in the following account.

LINUS had proposed that the mercury in the Torricellian experiment was supported by a rarefied cord of mercury or funiculum. BOYLE saw that PASCAL'S Puy de Dôme experiment was an "experimentum-crucis" supporting the concept of the weight of air, since this hypothesis rather than the funiculum would explain the mercury's descent with increased altitude.¹⁴⁰ BOYLE, like most other English observers, had taken the details of this experiment from PECQUET.

LINUS had no recourse but to deny PASCAL'S evidence, and BOYLE retorted that the truth of PASCAL'S experiment had been confirmed by two English observers.

Especially since I can confirm these observations by two more made on distant hills in *England*. The one which I procured from that known Virtuoso Mr. J. Ball, whom I desired to make the experiment at a mountain in *Devonshire*, on the side thereof he dwelt; and the other made in *Lancashire* by that ingenious gentelman Mr. *Rich. Townley*. Both which observations, since I have mentioned them at large in the Appendix to the Physico-Mechanical treatise, I shall not now repeat; contenting myself to observe to our present purpose, that however the proportion of the descent of quicksilver may vary, according to the differing consistence and other accidents of the neighbouring air, in the particular places and times of the experiments being made, yet all observations agree in this, that nearer the top of the atmosphere the quicksilver falls lower than it does further from it. To this I shall add two things, that will very much confirm our hypothesis. The one is, that the freshy named Mr. *Townley*, and divers ingenious persons that assisted at the trial, bethought themselves of so making the Torricellian experiment at the top of the hill, as to leave a determinate quantity of air in the tube, before the mouth of it was opened under the vesseled mercury; and taking notice how low such a quantity of that air depressed the mercurial cylinder, they likewise observed, that at the mountain's foot the included air was not able to depress the quicksilver so much. Whence we infer, that the cylinder of air at the top of the hill being shorter and lighter, did not so strongly press against the included air, as did the ambient air at the bottom of the hill, where the aerial cylinder was longer and heavier.¹⁴¹

This quotation requires certain clarification. Since BOYLE included no fuller account of the observations of BALL and TOWNELEY as a separate appendix, did he refer to them at another point in his work, and have their accounts been preserved?

¹³⁸ R. BOYLE, 1662, op. cit., *Works*, op. cit., vol. 1, p. 102. — R. HOOKE, *Micrographia*, London, 1665, p. 225. — For NEWTON'S references to TOWNELEY'S hypothesis, see I. BERNARD COHEN, Newton, Hooke and Boyle's Law; Discovered by Power and Towneley, *Nature*, 1964, 204, pp. 618—621.

¹³⁹ See note 87.

¹⁴⁰ R. BOYLE, 1662 op. cit., *Works*, vol. 1, p. 97. This account formed the first part of Chapter 4 of Part II of the reply to LINUS.

¹⁴¹ *Ibid.*, p. 98. I have inserted the italics at the end of the quotation.

In the earlier edition of *New Experiments Physico-Mechanical*, he referred to BALL's experiment, which had been performed in the West of England. This account was later given to the Royal Society, and preserved in the *Register Book*.¹⁴²

TOWNELEY's description of this experiment was not alluded to elsewhere in BOYLE's work, but examination of the passages which I have placed in italics in the above quotation, make it obvious that BOYLE was referring to the first of the manuscripts sent to him by POWER.

The first part of the passage refers to the title of the manuscript,¹⁴³ which referred to the experiments performed at Towneley Hill "by the advice and assistance of that Heroick and Worthy Gentleman, Mr. RICHARD TOWNELEY, ESQR. and those Ingenious Gentlemen Mr. JOHN and Mr. CHARLES TOWNELEY and Mr. GEORGE KEMP."¹⁴⁴

The sequence of events and results of the experiment are exactly as POWER described them in his account, which has already been quoted.¹⁴⁵ BOYLE's concluding remarks about the experiment make it probable that he did not understand the significance of the hypothesis which was suggested at the end of the experiment. This was probably too cryptic for BOYLE to derive the idea of the reciprocal proportions between the volume of air and pressure.

Thus, BOYLE received the above manuscript from CROUNE at the end of August, 1661, when it probably inspired the structure of the fourth chapter of the reply to LINUS; he quoted from the most significant experiments in the manuscript, but misread the title and attributed its composition to TOWNELEY, while there was no internal evidence to suggest that it was actually written by POWER.¹⁴⁶

We have no further records of experiments on air pressure by TOWNELEY and POWER, although their active research continued after 1662. POWER's contact with the Royal Society gave him the opportunity to prepare his pamphlets for

¹⁴² Register Book of the Royal Society, BM Sloane MS 243, ff. 94—95; "Account of the quicksilver experiment of Mr. Ball." This was an account of experiments performed between October 1659 and February 1660, presumably by WILLIAM BALL, at his estate at Mamhead, S. Devon. The account was read at the meeting of the Royal Society in December 1661, "Mr. Balle brought in his account of the quicksilver experiment at Mainhead [sic.]; which was ordered to be registered." T. BIRCH, 1756/57, op. cit., vol. 1, p. 67. BOYLE's reference was to Mr. J. BALL, but BIRCH mentioned only WILLIAM BALL (1627—1690), who was in the original committee, established by the Society, to examine the Torricellian experiment. He was also asked to communicate with POWER, presumably about the Puy de Dôme experiment. See T. BIRCH, 1756/57, op. cit., vol. 1, pp. 12, 22, 25, 66—67.

¹⁴³ The full title of the manuscript is given on p. 472.

¹⁴⁴ H. POWER, 1663, op. cit., p. 121.

¹⁴⁵ See p. 473—475.

¹⁴⁶ At the end of Chapter 4 of Part II of the reply to LINUS, BOYLE made direct reference to POWER's own reply to LINUS. BOYLE stated that he had casually met "with an experiment lately sent in a letter to a very ingenious acquaintance of his and mine [WILLIAM CROUNE], by a very industrious physician [HENRY POWER]." Their names are given in marginal notes. This was POWER's account of the Puy de Dôme experiment which he had verified again on October 15th 1661. This was also included in *Experimental Philosophy*; H. POWER, 1663, op. cit., p. 141; R. BOYLE, 1662, op. cit., *Works*, vol. 1, pp. 99—100.

publication, but after the appearance of *Experimental Philosophy*, there was a noticeable diminution in his contributions to the Society. TOWNELEY passed on to a criticism of HOOKE'S work on capillarity and the justification of Cartesian mechanism.

While their final publication of their work on air pressure was disappointingly fragmentary and ambiguous, the communication of the manuscripts to BOYLE was of the greatest service to him. They had provided a direct way of measuring the spring of air at different pressures, and further, given a method which could give BOYLE the comprehensive table of results which he had sought in his initial experiments before 1660.

X. Boyle's Experiments on the Compression and Dilation of Air

In Chapter V, the last in his book against LINUS, BOYLE returned to the quantitative study of the spring of air. This was probably prompted by the experiments performed at the weekly Royal Society meetings. As has already been noted, in January 1661, an influential committee had been established to report on the Torricellian experiment,¹⁴⁷ and at the meeting on September 4th the members of the Society were introduced to the idea of compressing air in a *J* shaped tube, the experiment being described by POWER'S correspondent, WILLIAM CROUNE.

Mr. Croune was requested to procure a syphon of glass to be made, with the end nipt up, in order to try the compression of air with quicksilver, and also to try an experiment with the weight of liquors in a siphon.¹⁴⁸

Interest in siphons was universal among seventeenth century scientists, and this quotation shows that the *J*-tube was evolved by simply inverting and closing the short arm of the siphon; this could now contain a volume of air, which might be compressed by a volume of liquid in the longer arm. BOYLE himself had performed various experiments with siphons¹⁴⁹ and had used an inverted siphon for comparing the densities of water and mercury, by measuring the heights of the volumes of water and mercury which balanced one another. Thus, it provided a method for measuring the *volume of air* and the *pressure* in a single apparatus, although to obtain the total pressure on the air, the atmospheric pressure was required also. The weakness of the apparatus was at the sealed end, which was liable to break under the pressure, as CROUNE found at the next meeting.

Mr. Croune produced two experiments, one of the compression of air with quicksilver in a crooked tube of glass, the nipt end of which broke; ...

Mr. Boyle gave an account of his having made the former of these experiments by compressing twelve inches of air to three inches, with about a hundred inches of quicksilver.¹⁵⁰

Already BOYLE had an intuitive understanding of the nature of the relationship between the elasticity of air and its pressure, he realised that an extremely

¹⁴⁷ T. BIRCH, 1756/57, *op. cit.*, vol. 1, pp. 8, 10, 12, 16, 19. BOYLE'S air pump was first demonstrated at the meeting on 15th May, 1661; *Ibid.*, p. 23.

¹⁴⁸ *Ibid.*, p. 43.

¹⁴⁹ R. BOYLE, 1662, *p. cit.*, *Works*, vol. 1, pp. 54—57.

¹⁵⁰ T. BIRCH, 1756/57 *op. cit.*, p. 45. This demonstration was at the meeting on 11 September, 1661.

long arm was required for the mercury, since the spring of air increased considerably as its volume diminished.

On September 18th his investigation progressed further, and he presented the Society with a table of results, showing the relationship between the volume of air and the height of mercury. This indicated the nature of the spring of air even more clearly,¹⁵¹ and this Table, which was entered in the Register Book on October 2nd, 1661, had a list of results which was identical to that in the Table produced in the second edition of the *New Experiments* in the next year.¹⁵² The detailed account of this experiment, which was probably the one performed before the Royal Society in September 1662, formed the first part of Chapter V, the last chapter of his reply to LINUS.¹⁵³

The greatest technical difficulty in the experiment was the production of tubes of even bore and a long enough arm. Many tubes were broken in the trials.¹⁵⁴ In the published experiment the short arm was 42 inches long and the long one about 120 inches.

As in the experiments of 1660, he gave full experimental details, paying particular attention to causes of error, and technical difficulties. In particular the great length of the mercurial tube necessitated two persons to perform the experiment, one slowly pouring mercury in, and the other measuring the volume of the compressed air.¹⁵⁵ An improvement on the experiments of POWER and TOWNELEY was that the volumes of air *and* mercury were measured in inches, and the smallest units calibrated were $\frac{1}{8}$ inches.¹⁵⁶ The results were given in both inches and $\frac{1}{4}$ inches. It is interesting that none of the observers used a decimal system of calibration. The mercury was gradually poured in and its vertical height was recorded at each reduction of the air's volume by $\frac{1}{2}$ an inch, between 12 inches and 6 inches; when the volume of air had been reduced by a half, the measurements were made at each subsequent $\frac{1}{4}$ inch of compression.

The observation which BOYLE notes with the greatest pleasure was that the volume of air was halved when the mercury stood at 29 inches in the long arm, that is, when the pressure on the enclosed air was doubled. He now arrived at the following hypothesis.

Now that this observation does both very well agree and confirm our hypothesis, will be easily discerned by him that takes notice what we teach; and Monsieur *Pascal* and our English friend's experiments prove, that the greater the weight is that leans upon the air; the

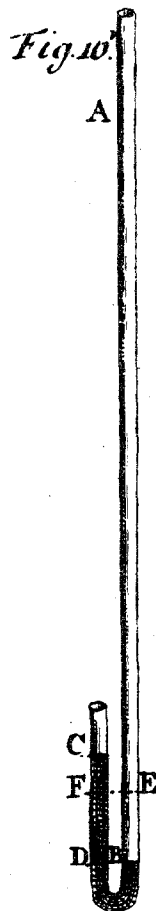


Fig. 7. R. BOYLE, *New Experiments Physico-Mechanical*, London, 1662; *Works*, vol. 1, Endplate Fig. 16. BOYLE'S J-tube apparatus

¹⁵¹ *Ibid.*, p. 45. D. MCKIE has reproduced BOYLE'S Table of results from the manuscript Register Book of the Royal Society; *op. cit.*, see footnote 2.

¹⁵² R. BOYLE, 1662, *op. cit.*, *Works*, vol. 1, p. 101.

¹⁵³ *Ibid.*, pp. 100—102. The title of Chapter 5 was "Two new Experiments touching the measure of the force of the spring of air compressed and dilated."

¹⁵⁴ *Ibid.*, p. 101. BOYLE'S J-tube apparatus is shown in Figure 7.

¹⁵⁵ *Ibid.*, p. 101.

¹⁵⁶ *Ibid.*, p. 100.

more forcible is its endeavour of dilatation, and consequently its power of resistance (as other springs are stronger when bent by greater weights). For this being considered, it will appear to agree rarely-well with the hypothesis, that as according to it the air in that degree of density and correspondent measure of resistance, to which the weight of the incumbent atmosphere had brought it, was able to counterbalance and resist the pressure of a mercurial cylinder of about 29 inches, as we are taught by the Torricellian experiment; so here the same air being brought to a degree of density about twice as great as that it had before obtains a spring twice as strong as formerly.¹⁵⁷

BOYLE acknowledged no assistance in evolving this hypothesis, although he noted that it was in accord with his "English friend's experiments". Once the experiment was devised and the importance of noting the barometric height as well as the mercury height was realised, it would have been obvious, even to one of the slight mathematical ability, that "the resistance to compression had doubled with the doubling of the pressure".¹⁵⁸

It should be emphasised that BOYLE'S Law, as evolved during September 1661, stated that the spring of air (its resistance to compression) is proportional to its density. This particular expression of the law is rarely mentioned by historians of science.

It is by no means certain that he realised at this time that this expression implied *reciprocal proportion* between the external pressure and the "Expansion" of a volume of air. This expression occurred only in a later place in the chapter and in the notes appended to the tables of results.¹⁵⁹

He now wished to examine the influence that the expansion of air had on its pressure, or spring. For this he used a modified form of the Torricellian apparatus. The tube was six feet long, and this could be almost completely submerged in a cylinder of mercury. One inch of air was included in the tube, which was submerged in the mercury, with only the inch of air protruding. The tube was now raised and the height of mercury recorded for each increase of one inch in the volume of included air. Later in the experiment the mercury level was measured against larger increases in the volume of air.¹⁶⁰ As in the former experiment he tabulated the results, giving the mercury heights against air volumes.¹⁶¹ It may seem strange to the modern observer that BOYLE could not reduce these results to any hypothesis, considering that he had already produced an hypothesis for the compression of air. The barrier to understanding the application of this hypothesis to the expansion experiment may have been his failure to understand that the true pressure of the volume of contained air was obtained from the barometric height minus the height of the mercury in the tube. Thus it was necessary for BOYLE to realise that, in the J-tube experiment, $spring = Barometric\ height + mercury\ height$ in the long arm, while in the dilatation experiment, $spring + mercury\ height = Barometric\ height$.

¹⁵⁷ Ibid., p. 100.

¹⁵⁸ Ibid., p. 101.

¹⁵⁹ In this article it is assumed that the order of experiments described by BOYLE in Chapters 4 and 5 of Part II of the reply to LINUS represents a continuous chronological sequence of composition.

¹⁶⁰ Ibid., pp. 102—103.

¹⁶¹ Ibid., p. 102.

Perhaps in BOYLE'S mind there still lingered the scholastic notion that condensation and rarefaction were qualitatively different.

In the previous experiment the table of results showed the decrease in the air's volume with increase in the mercurial column, even without addition of the barometric height to the total pressure sustained by the mercury: the air volume reduced from 12 to 3 inches as the mercurial column increased from 0 to $88\frac{1}{2}$ inches.

But the table of results for the dilation experiment was less clear. The immediate set of results gave two increasing magnitudes, for as the air expanded the mercury column increased in height. The proportionate changes might also have been confusing, since, as the volume of air increased from 1 to 18 inches, the mercury increased from 0 to 26 inches; while as the air increased from 8 to 32 inches, the mercury only increased from 26 to $28\frac{1}{2}$ inches. Direct comparisons between these results would thus have been far more confusing than between the direct results of the previous experiment. They would certainly not have indicated the "debilitation of the force of the expanding air".

Before presenting the table in its final form, showing the agreement of the results with his own hypothesis, BOYLE expressed his indebtedness to RICHARD TOWNELEY for clarifying the meaning of his results.

... I shall readily acknowledge, that I had not reduced the trials I had made about the measuring the expansion of air to any certain hypothesis, when that ingenious gentleman Mr. *Richard Townley* was pleased to inform me, that having by the perusal of my physicommechanical experiments, been satisfied that the spring of the air was the cause of it, he endeavoured (and I wish in such attempts other ingenious men would follow his example) to supply what I had omitted concerning the reducing to a precise estimate, how much air dilated itself loses of its elastical force, according to the measure of its dilatation. He added, that he had begun to set down what occurred to him to this purpose in a short discourse, whereof he afterwards did me the favour to show me the beginning, which gives me a just curiosity to see it perfected. But, because I neither know, nor (by reason of the great distance betwixt our places of residence) have at present the opportunity to inquire, whether he will think fit to annex his discourse to our appendix, or to publish it by itself, or at all; and because he hath not yet, for aught I know, met with fit glasses to make any-thing-accurate table of the decrement of the force of the dilated air; our present design invites us to present the reader with that which follows, wherein I had the assistance of the same person that I tooke notice of in the former chapter, as having written something about rarefaction: whom I the rather make mention of on this occasion, because when he first heard me speak of Mr. Townley's suppositions about the proportion wherein air loses of its spring by dilatation, he told me he had the year before ... made observations to the same purpose, which he acknowledged to agree well enough with Mr. *Townley's* theory.¹⁶²

I think that this quotation has been slightly misread in the past, and yet it provides an accurate indication of TOWNELEY'S rôle in the evolution of the gas law.

Firstly, BOYLE acknowledges that TOWNELEY pointed out the significance of the experiment on "how much air dilated itself loses of its elastical force". It was not suggested that TOWNELEY had derived the law to account for the experiments on the compression of air. Thus, it is probable that BOYLE derived the law from

¹⁶² *Ibid.*, p. 102.

his experiments on the compression of air, whereas TOWNELEY pointed out that it also applied to the experiments on expansion.

BOYLE's comments about TOWNELEY are also consonant with our former evidence. TOWNELEY had been impressed by BOYLE's *New Experiments Physico-Mechanicall*, and he had experimented with POWER on the expansion of air, and finally they had reduced these experiments to the "precise estimate". This had been stated in their manuscript of 1661 and it presumed knowledge of the gas law by the authors. In their experiments they realised that the pressure of the volume of enclosed air was estimated by subtraction of the mercury level in the experimental tube from the barometric pressure. The result of this subtraction they termed "the mercurial complement". This was probably the aspect of the experiment pointed out by TOWNELEY to BOYLE, who, in turn, used the term "the complement of *B* [mercury] to *C* [barometric pressure], exhibiting the pressure sustained by the included air".¹⁶³

Such an explanation assumes that BOYLE carried out a correspondence with TOWNELEY, or that they had a conversation. This latter possibility has been ignored, yet it would explain the lack of surviving letters between BOYLE and TOWNELEY. TOWNELEY was also a most reluctant correspondent. It has been the lack of surviving correspondence and absence of the dissertation, mentioned in the previous quotation, which have caused modern writers to minimise TOWNELEY's rôle in the enunciation of the Law.¹⁶⁴

Fortunately the POWER correspondence provides evidence for TOWNELEY's activities during the winter of 1661/1662. In October 1661, POWER wrote to CROUNE that TOWNELEY was intending to visit London.

As for the Magneticall Experiments, I will return them to you by Mr. Townley who is for London at the re-session of the Parliament.¹⁶⁵

On November 27th he wrote:

I by the bearer here of Mr. Townley my ever honoured friend, returned you those few magnetical observations I had long since made ... Mr. Townley whilst hee is resident in ye city will return mee anything from you. I pray you doe me that honour, as be acquainted with him: were he not a person above my Commission I would say something to you of him but I should therein prevent ye abilities and judgement, which is soe acute, as it needs not the least hurt or intermation.¹⁶⁶

TOWNELEY met CROUNE in London, and the latter wrote to POWER.

Let mee thank you in the first place for the honour you did mee in the knowledge of so worthy a person as Mr. Townley, of whom I shall not bee so [illegible] as to say any other thing then that I wish wee had many more such Gentlemen as hee.¹⁶⁷

Although there is no further evidence about TOWNELEY's activities in London during this visit, I suggest that he attended scientific meetings at the Royal Society, and there became acquainted with ROBERT BOYLE. On the basis of the experiments of 1660 and 1661 he was able to aid BOYLE's interpretation of the

¹⁶³ *Ibid.*, p. 102.

¹⁶⁴ In all accounts which I have seen it has been assumed that TOWNELEY suggested the hypothesis in a letter to BOYLE.

¹⁶⁵ HENRY POWER, letter to Croune, 16 October, 1661, f. 26.

¹⁶⁶ HENRY POWER, letter to Croune, 27 November, 1661, f. 28.

¹⁶⁷ WILLIAM CROUNE, letter to HENRY POWER, 1 March 1661/62, f. 30.

experiment on the expansion of air. The treatise mentioned was probably his own account of the experiments of 1660 and 1661 which he had performed with HENRY POWER. Unfortunately he was as reluctant to publish this work as he was to advertise his activities, and it was either lost or placed in the large collections of manuscripts at Towneley Hall, which were dispersed in 1883, to our national disgrace.¹⁶⁸

ROBERT HOOKE has so far been little mentioned in the experiments that led to BOYLE'S Law, yet he has, in some accounts, been given a major rôle in its evolution.¹⁶⁹ However, the previous account shows that HOOKE'S contributions were in no way essential to the evolution of the Law. His most important work was in the technical construction of the air pump, but the experiments using this pump did not lead to a quantitative estimation of the elasticity of air.

In the experiments described in the second edition of *New Experiments* he was again referred to occasionally. He was probably the person who assisted BOYLE in carrying the Torricellian experiment up Westminster Abbey, and he also helped in the experiment on the expansion of air; but BOYLE does not mention him as the initiator of these experiments.¹⁷⁰

Most important of all he verified the experiment on the expansion of air and included his account and Table of results in the *Micrographia*,¹⁷¹ however, it should be noted that he was not mentioned in the *History of the Royal Society* as participating in the experiments on air pressure, before December 1662, by which time the major advances had already been made.¹⁷²

One more person was mentioned in connection with the experiments on the expansion of air; this was Lord BRONCKER.¹⁷³ He too had made experiments,

¹⁶⁸ *Catalogue of the Towneley MSS., sold 27 June 1883, Sotheby, London, 1883.* — That TOWNELEY interchanged letters with BOYLE is proved by the letters written by TOWNELEY to OLDENBURG. From these it is apparent that BOYLE had sent TOWNELEY a barometer in the summer of 1672, but there was some delay before the apparatus was used. — My interpretation of TOWNELEY'S part in BOYLE'S experiments receives confirmation in one of the letters. — "Sir. It was some satisfaction to me to find in ye Transactions of July y^t ye hypothesis (w^{ch} Mr. Boile was pleased to owne as mine) about ye force of aire condenst and rarefied, doth succede as well in deepe immersions, as in those I made triall of, and that ir doth administer now to ye learned matter of further speculation, as formerlie it did to me of writing some few things, (of w^{ch} I then showed unto Mr. Boile) . . ." R. TOWNELEY to H. OLDENBURG, January 29, 1672/73, Royal Society, Guardbook T, no. 25.

¹⁶⁹ HOOKE'S rôle in the formulation of BOYLE'S Law has been stressed by R. T. GUNTHER and E. N. DA C. ANDRADE and more recently by I. BERNARD COHEN (op. cit., see footnote 138), but careful consideration of the chronology of BOYLE'S experiments and estimation of the date of composition of HOOKE'S account largely invalidate claims for the priority of HOOKE'S work. See my forthcoming article in *Nature*.

¹⁷⁰ R. BOYLE, 1662, op. cit., p. 102.

¹⁷¹ R. HOOKE, *Micrographia*, London, 1665, pp. 222—225.

¹⁷² On December 10th 1662, HOOKE demonstrated the influence of reduced pressure on a volume of air at a meeting of the Royal Society. On January 28th 1662/63, he adapted the experiment, in order to measure the influence of increased pressure on air. A 12 inch tube containing air was gradually immersed to a depth of 142 inches in a deep glass cylinder. The subsequent decrease in volume was measured. T. BIRCH, 1756/57, op. cit., vol. 1, pp. 141—142, 180—182. POWER and TOWNELEY had performed a similar experiment in 1653.

¹⁷³ R. BOYLE, 1662, op. cit., *Works*, vol. 1, p. 102.

which, like those of HOOKE and BOYLE, were explained by TOWNELEY'S hypothesis. It is interesting that neither BRONCKER, HOOKE, nor BOYLE saw the significance of their tables of results until it was pointed out by TOWNELEY. BRONCKER, unlike HOOKE, was in the committee appointed by the Society to examine the Torricellian experiment, and he was probably familiar with BOYLE'S experiments before they were officially recorded.

The successful formulation of the law of the elasticity of air during the autumn of 1661 was the result of patient investigations of many authors; POWER and TOWNELEY'S experiments had begun in 1653, and BOYLE'S in 1658/9. While both the northern workers and BOYLE reach a similar hypothesis, their experimental method differed considerably, and it is entirely equitable that BOYLE'S work should be remembered as a model example of a sustained experimental investigation of a quantitative physical problem. On the other hand, it should be pointed out that it was left to NEWTON to realise the wider physical significance of the law of elasticity, while BOYLE and POWER were more interested in elasticity as a qualitative phenomenon for use in arguments relating to the nature of air. To them, the Law was an example of the fruitful application of experiment, but it was subsidiary to their main interests and it occupied only an inconspicuous position in their works.

Finally, the last stages of this investigation indicate the value of the Royal Society in stimulating experimental research. It increased the number of investigators concerned with the problem, facilitated communication between widely scattered workers and encouraged or even patronised the publication of their results.

Conclusions

This present article has confirmed that BOYLE'S achievements in the study of air pressure were the climax of a co-operative enterprise in which he was the major contributor, and that his concept of air pressure was greatly influenced by the European researches. In this respect BOYLE'S position is similar to that of PASCAL and TORRICELLI, who also benefitted greatly by the interchange of ideas with other contemporary investigators, but each triumphed over their fellows in their ability to separate the experimental aspect of the problem from general questions of the philosophy of nature and even more in developing informal models of the structure of air, the most important feature of which guided them into fruitful experiments.

The works of PASCAL and BOYLE represent the results of completed investigations. In each case the section concerned with devising simple experiments to support their hypotheses stands sharply apart from general considerations of the plenist-vaculist controversy, and the emphasis of their works lies in the former part. This contrasts with HENRY POWER'S work, in which the chapters start with an experiment which is examined in order to shed light on his Cartesian axioms. It is interesting that he presented his work in a form nearer to BOYLE'S model after he had read the *New Experiments*, but his experiments were still presented in a random manner, often inserted, for the sake of curiosity than for relevance to a major physical problem.

The great advantages which BOYLE, PASCAL and TORRICELLI were able to derive from other workers was probably increased by their presence in centres

of great scientific activity. It is probably not coincidental that their major works were undertaken in Florence, Paris and London, which were the very centres of the developing scientific academies and which attracted to their informal meetings many of the most fertile intellects in the three nations. It has been noted that many of the experiments on air pressure were inaugurated at such meetings, and there were many advantages to accrue from group study of this particular problem.

Firstly, many of the experiments were related to the vacuist-plenist controversy which was actively discussed at the meetings, and this vigorous debate provided a substratum of speculation and criticism out of which the experimental problem might emerge. Certainly much of the discussion was academic, but even the scholastic authors originated new experiments and hypotheses to support their axioms, and they were quick to see weaknesses in other hypotheses or explanations.

Secondly, group enterprise was valuable in overcoming the many technical problems connected with the study of air pressure. Glass apparatus was required of a form and precision unknown in chemical experiments. It was necessary to evolve methods of producing long glass tubes of uniform bore, and produce methods of calibrating them. The construction and design of the air pump was itself difficult; once it was evolved special glass evacuation globes were required and methods of producing air-tight seals.

The technical imperfections of the experiments of POWER and TOWNELEY are witness to the problems of independent investigators. Even in 1661 they were unable to construct a glass apparatus 133 feet long, although this had been achieved in France fourteen years before.

Finally, the societies were a centre for the publication of information and correspondence radiated from them; while both of these methods improved the dissemination of information and co-operation between geographically isolated groups. In addition, at a later date, the societies were able to employ assistants, who enabled them to overcome technical problems, or as in the case of HOOKE, became essential members of the groups.

The importance of the distribution of information by correspondence and the distribution of manuscript pamphlets was important in the study of air pressure, as it was in many other spheres of science. Such information has often been overlooked as trivial compared with published writings, yet it must be remembered that of the numerous investigators mentioned above, only BOYLE and MERSENNE were rapid to publish their researches in a form which had general circulation.

Even without publication there was surprisingly rapid and efficient transmission of knowledge. TORRICELLI's letters to RICCI were not published until 1663, but MERSENNE had extracts from them shortly after they were written in 1644. Sir CHARLES CAVENDISH communicated ROBERVAL's carp-bladder experiment to PETTY in England in April, 1648, and in May of that year we find HARTLIB relaying the same extract to BOYLE. HAAK's letters show that there was little delay in transmitting new experiments from France to England and that MERSENNE was a key figure in this international correspondence. In England at this time, SAMUEL HARTLIB was an equally enthusiastic correspondent, although he, like MERSENNE, lacked public support for his activities as an "intelligencer"

However, once the Royal Society was founded, not the least of its activities was the delegation of duties concerned with correspondence, and this is illustrated by the exchange of information between WILLIAM CROUNE and HENRY POWER.

It is appropriate at this point to give a resumé of the essential stages which led from the initial understanding of the elasticity of air to the formulation of BOYLE'S law, before going on to resolve certain questions related to BOYLE'S own work. The stages are given as a numerical sequence, and the dates refer to the original investigations rather than the date of publication.

1. Associated with the "ocean of air" hypothesis was the idea that the lower layers of air existed in a state of compression; BEECKMAN (1614 onwards), BALIANI (1630), TORRICELLI (1644).

2. Many experiments were devised which illustrated this compressibility of air, and also its great powers of expansion. These were summarised by MERSENNE (1644). Others were the vacuum-in-vacuum experiment and balloon experiments, PASCAL (1647), and the carp-bladder experiment, ROBERVAL (1647/8).

3. It was realised that air enclosed in a vessel exerts a force different from the simple "weight resulting from its location at the bottom of the ocean of air". This was visualized separately as the resistance to compression by an external weight and the tendency to expand; NOËL (1648), ROBERVAL (1648), PECQUET (1651), POWER (1653), BOYLE (1658/59).

4. This phenomenon was explained by the principles of mechanics, and a conceptual model was utilised which stressed the spring-like nature of air, ROBERVAL (1648), PECQUET (1651), POWER (1653), BOYLE (1660). PASCAL proposed that air was compressed in proportion to the pressure exerted on it, and ROBERVAL, that the air's capacity of expansion decreased with its increase in volume.

5. The invention of the air pump by VON GUERICKE (1654) and its improvement by HOOKE (1658), allowed the experimental rarefaction of air by BOYLE (1659), who attempted to measure the reduction in pressure and the accompanying degrees of rarefaction. He sought unsuccessfully to reduce these results to a law.

6. Systematic measurements of the expansion of air, enclosed above mercury in ROBERVAL'S apparatus, were made by POWER and TOWNELEY (April 1661). The change in pressure was produced by the ascent of a hill. They deduced the reciprocal proportionality between the volume of air and external pressure from these experiments.

7. The J-tube apparatus was evolved at the Royal Society meetings (September 1661) by BOYLE and CROUNE. This was used by BOYLE to produce tables of the compression of air. He deduced that the *spring of air was proportional to its density*.

8. ROBERVAL'S apparatus was adapted by BOYLE for measuring the expansion of a volume of air under reduced pressure. From the table of results TOWNELEY pointed out that *the pressure of air was reciprocally proportional to its expansion* (December? 1661). HOOKE verified the results of this experiments (1661/62).

While the above summary indicates the major contributors to the discovery of BOYLE'S Law, it leaves unanswered the question of priority of discovery and

the correct title of the Law, problems which are of limited importance compared with that of obtaining an accurate historical account of discoveries relating to air pressure.

There has already been much discussion of the merits of various titles for BOYLE'S Law, in which various combinations of BOYLE-HOOKE-MARIOTTE-TOWNELEY have been proposed.¹⁷⁴ However, it must not be overlooked that there is no law of priority in the naming of Laws, as there is in botanical nomenclature, and that it is in no way to be assumed that the naming of Laws has any historical significance. The titles merely serve to facilitate association with a particular generalisation, and as such have a certain psychological assistance in teaching. If the name BOYLE brings to the student's mind the relationship between the pressure and density of a gas, then its purpose is served, and it is of little importance if the law is known by another name in European countries. Only if there was slight conflict of meaning would there be significance in the use of one or the other name.

In the preceding account it has become obvious that the Law was to some extent realised by PASCAL and ROBERVAL, POWER and TOWNELEY, independently and possibly before BOYLE. This places BOYLE in a similar position to DARWIN in the discovery of the principle of natural selection, for since the year of publication of the *Origin of Species*, it has become clear that he had numerous anticipators. However, DARWIN'S achievement is not minimised by these anticipations, and neither is BOYLE'S. For BOYLE overshadows the other investigators, both in the comparison of his work with theirs, and the ultimate influence of his work on future generations.

From the inception of his interest in air pressure, he realised that there was little immediate progress to be made by experiments applied to the ultimate problems which had dominated the discussions until that date and which had indeed given rise to most of the experimental studies.

Like PASCAL, he was content to apply experiment to the consequences of the model of air adopted, and he explored this thoroughly and reserved consideration of the more profound issues until later works. He devoted equal skill to the examination and refutation of false hypotheses. Thus there arose, out of a substratum of unresolved philosophical problems in his work, an impressive hypothetico-deductive exploration of the physical properties of air which eventually by passed the philosophical problems, to become an independent investigation of natural laws.

BOYLE had a further similarity with PASCAL in his skill in devising experiments, bettering him in the production of elaborate apparatus and in the systematic recording of experimental results. This latter facet of his work, which was probably one of the few direct manifestations of BACON'S influence, as well of his own personality, reached a climax in the tabular records of air volumes and related pressures.

¹⁷⁴ This article will not consider the contribution to the study of air pressure made by MARIOTTE, since the relationship between his work and BOYLE'S has already been discussed thoroughly. See D. MCKIE, 1948, *op. cit.*, pp. 269—272; W. S. JAMES, 1928, *op. cit.*, pp. 269—272; A. WOLF, *A History of Science, Technology and Philosophy in the 16th and 17th Centuries*, London, 1935, p. 235.

During the course of his experiments BOYLE subjected the elasticity of air to a more thorough examination than any other investigator, and selected it as a particularly significant concept from the works of GASSENDI, MERSENNE, CHARLETON, and PECQUET. Of these, only the last author had given prominent place to the hypothesis of elasticity. Also from the earliest experiments he sought to reduce the phenomenon to a quantitative law. To this end he made more elaborate experiments than the other observers, and over two years he did not lose sight of his objective until his work was successful at the end of 1661.

He recorded the final stages of his investigations with the greatest accuracy, and was diligent in admitting assistance from other English scientists; perhaps most significantly, these complete investigations were promptly published in 1662.

The second edition of the *New Experiments* is one evidence for the background to BOYLE'S work, which must be examined in the light of unpublished records and correspondence, but it is the sole means of the future influence of his work, for it had a wide circulation and its influence was not surpassed by other works on air pressure. It preceded the publication of the writings of PASCAL, TORRICELLI, POWER and HOOKE. Thus this work stands in a similar position to the *Origin of Species* in its sphere, as the most definitive and original expression of the theory of the elasticity of air.

The establishment of BOYLE'S Law has considerable philosophical importance in the history of science, because it was the first numerical law which illustrated the functional dependence of two variable magnitudes. Such laws provide important illustrations of the failure of the idea of causation in the Aristotelian sense, since any change in volume is strictly concurrent with the change in pressure, there being no justification in assuming that the relationship is asymmetrical or sequential. But BOYLE must not be accorded the position of proponent of the concept of functional dependence in opposition to the idea of causation in the case of his Law. His whole experience in experimental physics and chemistry had been based on the principle of cause, and effect; and his description of his experiences leading to the formulation of his law illustrate the same bias of language. The introduction of a cause in the experiment led to a temporal effect. Thus the greater the weight "leaning" upon the air, the more "forcible is its endeavour of dilatation". The air "resists" the increase in pressure. Upon the reduction of pressure the air "loses of its elastical force". The inch of air which was subjected to reduced pressure "when expanded to force its former dimensions, was able with the help of a mercurial cylinder of about 15 inches to counterpoise the weight of the atmosphere, which the weight of the external air gravitating upon the restagnant mercury was able to impel up into the pipe, and sustain twenty eight inches of mercury, when the internal air by its great expansion, had its spring too far debilitated to make any considerable . . . resistance."¹⁷⁵

I have stressed that BOYLE'S initial formulation of his law stated that the pressure of air was proportional to its density, and indeed he used two different models and sets of causal terms to describe contraction and dilation. This has a different emphasis to most expressions which give the law as "the volume of a gas is inversely proportional to the pressure exerted on it".

¹⁷⁵ R. BOYLE, 1662, op. cit., *Works*, vol. 1, p. 102.

The modern expression of the Law, using "volume" as one of the two variables has no physical connotation and although it is the volume (or usually length of a column) of air that is measured in experiments; the pressure is not due to the space itself. When BOYLE noted the contraction of air, he stressed that it was "reduced to take up but half the space it possessed (I say possessed, not filled) before". Thus the volume of air was of no significance, the force being caused by the increased density and possibly physical compression of the hypothetical particles. This expression also indicates that BOYLE, in the tradition of PECQUET, regarded the pressure of air as a dynamic property, in the tradition of the future kinetic theory of gases.

The law also applied to air only, and the term "gas" at this period had no precise meaning, and as applied by VAN HELMONT, generally indicated a mixture. BOYLE was hesitant about the physical construction of air, and he was generally content with the hypothetical model used by PECQUET; like ROBERVAL he believed that air's properties of resistance were common to other springs. Thus air increased its resistance with increased pressure, while "other springs are stronger when bent by greater weights".¹⁷⁶ This conclusion was of course wrong, but it illustrates the importance of the analogy in framing BOYLE'S concepts of the nature of air, and his experiments appeared so well to confirm the well known hypothetical model.

Finally, BOYLE'S initial statement applied only to the compression of air, and it was only some months later that he realised that it applied to its expansion when he accepted the reciprocal proportionality between the "expansion" and pressure. But the term expansion was still meant in its physical sense, of "laxity" or "debilitation" of structure.

In the present article I have attempted to illustrate the value of careful examination of the investigations which form a background to BOYLE'S work and to show the great extent to which BOYLE assimilated the experimental and hypothetical notions of his predecessors during the emergence of his own theory of air pressure. In reading his well organised descriptions and carefully phrased explanations it becomes apparent that he was able to derive the maximum benefit from the working model of air which he favoured, while remaining careful to reserve judgment on the ultimate physical validity of that model.

It is also apparent that the foundation for BOYLE'S work was laid during the first half of the seventeenth century, and after 1640 investigations led clearly in the direction of the law. At first there was an understanding of the elasticity of air as a qualitative phenomenon, but by 1647 tentative quantitative studies had begun. Finally, it was ROBERT BOYLE'S genius which evaluated the contributions of diverse natural philosophers and from this synthesis built his own outstanding contribution to experimental science.

During the preparation of this article the author has benefitted considerably from the advice of Professor J. R. RAVETZ of the University of Utrecht. Thanks are also expressed to Professor W. H. G. ARMYTAGE of the University of SHEFFIELD, who originally introduced the work of HENRY POWER to the author and to various members of the staff of the City Grammar School, Sheffield and Miss JOAN HEPPENSTALL of Cambridge University.

¹⁷⁶ *Ibid.*, p. 100.

Appendix I

"Ae. P. de Roberval de Vacuo Narratio ad Nobilem Virum Dominum des Noyers ..." May-June, 1648. Part five.¹⁷⁷

(From B. PASCAL, *Oeuvres*, Vol. 2, op. cit., pp. 325—328.)

When we demonstrated that remarkable rarefaction of air from our previous experiment, to many it seemed so incredible that they would rather suspect some unknown cause, than agree unreservedly to our assertions, and I wished if possible to free them from all doubts: I began to ponder in my mind, if perhaps there was some body available to us, which was both flexible and satisfactorily hold air. The convenient thing which came to my mind was the swim-bladder of the carp, because it is quite flexible and is thought to have been given to this animal by nature for the express purpose of containing air. Now this bladder is a double structure and the two parts are connected together by a narrow neck through which the air communicates. Of the two parts I selected the one which is more pointed and more nearly approaches the form of a cone, because the membrane of this second part is far stronger and splits with greater difficulty.

This was now emptied of nearly all the air, so that the proportion of air remaining in it was not in fact 1000th part of that which it had formerly held. A thread was tied round the neck and I tied it so tight that it could not let out its air, nor admit any. This was then placed in the tube in which we had previously placed small birds and mice, the superior part of which has the capacity of a goose egg.

This being prepared, I made the experiment using mercury, so that the space or seeming vacuum appeared as usual at the upper part of the tube which held the bladder. But to the complete astonishment of the bystanders, the bladder appeared quite turgid and distended, just as if it was still inside the carp's belly, for, in fact, that very small amount of air which remained in it, liberated at last from compression, being in a position in which it was no longer compressed neither by our condensed air, nor by other surrounding bodies, had expanded itself to the size which the bladder would permit. And with the inclination of the tube, the mercury was sucked back, the bladder became flaccid, just as if its air was exhausted. Upon re-erecting the same tube, the mercury fell, the bladder expanded again.

At length, by virtue of perforating the pig's bladder, which closed the upper end of the tube, using a fine needle, with but the minutest hole so that air gradually penetrated the tube, and the air condensed around the bladder. The bladder deflated and gradually subsided until it returned to the state which it had been in when it was placed in the tube. Otherwise, at another time, if the hole was larger, the air rushed in, in a moment, the deflation would occur more rapidly.¹⁷⁸

All this confirmed our assertions about the air's rarefaction and condensation so that no one can any longer doubt it, but all openly assented — except the few, who have long been our adversaries, who now became a laughing stock. Nevertheless, as it became obvious that they themselves were denying a thing so obvious not¹⁷⁹ because they thought it untrue, but because the truth of it had been firstly

¹⁷⁷ It has been pointed out that this experiment became very popular and was widely quoted in support of the concept of the elasticity of air. It was also relevant to the vacuist-plenist controversy. The passage is translated from the Latin. — The experiment was described by various French writers; M. MERSENNE, *Harmonicorum libri XII*, Paris, 1648, "Liber novus praelusorius"; J. PECQUET, *Experimenta Nova Anatomica*, Paris, 1651, 1661 ed. pp. 91—98; P. GASSENDI, *Opera Omnia*, Lyons, 1658, vol. 1, pp. 214—215. In England it was described by POWER and BOYLE, *Experimental Philosophy* 1663, p. 117, *New Experiments Physico-Mechanicall*, London, 1660, *Works*, vol. 1, p. 12. The Italian trials of the experiment have been described by L. BELLONI, *Scheime modelli della machina vivente nel seicento*, *Physis*, 1963, 6, pp. 262—267.

¹⁷⁸ "more rapidly" is inserted at a point where words are missing in the manuscript.

¹⁷⁹ I have inserted "not" in the text to improve the sense of the sentence.

detected by us. But this experiment had been tried more than a hundred times, in public and privately, in various ways and I had never failed.

Sometimes great industry was applied to totally removing air from the bladder, and yet perfect evacuation was not possible, for there were always some small air bubbles lying in the folds of the bladder. Nevertheless, the result of this was that the bladder became less inflated in proportion doubtless to the rarefaction of the surrounding air in the apparent vacuum. Doubtless, by the rules of mechanics, that air remaining inside the bladder cannot be more rarefied than the rarefied air inside the tube which surrounds the bladder.

I also tried what would happen in the same place when the bladder was perforated. However, as soon as it was left in the space or apparent vacuum, it inflated but at once steadily deflated, because the air inside it rarefied and finding an exit, it expanded itself through space in the tube or apparent vacuum, and the walls of the membrane, not being held up by any air, fell together.

I experimented also, in the same way, using a bladder full of our condensed air and properly tied up, and that air in the space or apparent vacuum being held back only by the membrane of the bladder, pressed by force on all sides, seeking to dilate, so that it burst some of them, principally around the thread which tied them, certainly because the membrane was weakened at that point.

Appendix II

“Ae. P. de Roberval de Vacuo Narratio ad Nobilem Virum Dominum des Noyers... May-June, 1648; part three.¹⁸⁰

(From B. PASCAL, *Oeuvres*, vol. 2, op. cit., p. 313—318.)

I so refilled a tube, three feet or a little less in length, with mercury so that one and a half spaces were left, which were filled with our air. The tube was then inverted and its mouth immersed in the mercury of the dish, the one and a half inches of air ascended into the upper part, above the mercury. Nor did there appear any space there other than the apparent vacuum, into which there arose bubbles out of the mercury, as was seen upon closer scrutiny. They were not in large numbers as in the above experiment, but some were somewhat larger; nor indeed was there any doubt that a large number ascended, but we were not able to see them. Of course, they were not seen in the mercury because it is opaque, or in the space or apparent vacuum, because there was nothing of distinct colour between the bubbles and this space. This was afterwards confirmed by suddenly admitting two one and a half inch portions, one of water, the other of air, into the tube besides the mercury, by which means the ascension of the innumerable bubbles of air into the water — were easily seen. And indeed, in both these experiments, either with the air alone, or with both air and water, above the mercury, a great change with regard to the mercurial level appeared. For, in that case a depression of the customary level by wholly four inches is seen, so that it did not ascend to the said height of two feet [sic.]. And, however often the same experiment was repeated in the same kind of tube, the same result appeared, either with air alone, or with the admittance of water besides this air. Though, with the admission of water alone there was not the same result. Moreover, in other tubes, the shorter they were the lower the mercury became, while the longer it went higher, to the extent however that once some measure had been admitted, whatever the height of the tube, the mercury never reached its customary height of 2.7/24ths. feet. However, when I reasoned using the laws of mechanics, about the inducement of such a depression of mercury by air, it has not been possible to take up a very satisfactory position or provide a better explanation according to the laws of nature, than if it were agreed that the air spontaneously and of itself became rarefied in the tube, (although I have tried other explanations in my first Narration) in such a way that the apparent vacuum occupies the whole of that

¹⁸⁰ This passage is translated from the Latin.

space; yet is not true to say that the whole of the air's whole force towards rarefaction is exhausted. The same air, while seeking to fill the whole space exerts a pressure in all directions, on the adjacent bodies, the tube keeps in the particles from all other directions, mercury being the only one of them that can give way towards the lower part of the tube. Moreover this explanation must be adopted: that the air which we respire only possesses such force towards dilatation and rarefaction as is equal to the power of the natural element compressing or condensing it. Besides which, this also agrees with the laws of nature itself and applies in all other bodies which nature has granted powers of spring, as in bows and innumerable other examples. All of which bodies, as long as they are compressed by force, but not extended beyond the limits of their own power, never cease to resist. They are carried by an innate force of resilience, which is the same as that force with which they are drawn or impelled by other bodies. So, at the beginning of its rebound the force is at its greatest, then it gradually grows less and less, and finally disappears altogether when the body has returned to its normal state. In the same manner, air, included in the tube, as long as the tube remains closed at both ends, is only compressed and condensed to the extent of the force of the natural element exerting pressure on our air; and for that reason the included air, by an equal and opposite force, resists such compression, attempting to become dilated to acquire a larger space than it does in nature. For, the air included in the tube is not less condensed and compressed than that outside, because, once it has been compressed and condensed, it does not hereafter obtain any freedom to dilate and rarefy. Nor is the same air compressed beyond its strength, for it is consistent with experience that air is capable of much greater compression and condensation. This happens daily in numberless ways, but especially in brass pipes with the application of a plunger. And so, with its strength unimpaired, it continually tends to rarefy and in fact it does so immediately, the very moment it is set free. And this is what happens when the lower end of the tube is enclosed within the mercury of the dish and a free descent is left for the mercury of the tube into the same dish. For then, the air which was compressed in the upper part of the tube and at the same time exerted a pressure in all directions, is bound to rarefy upon the withdrawal of the mercury. At the beginning of its expansion it had a great power of rarefaction, because, one must suppose that it was condensed by the force of pressure of the entire natural element. Then, its force gradually relaxes because it is less and less compressed and condensed, so that, one and a half inches, after rarefying through 6 or 8 inches of space, is able to move to the extent of only four inches of mercury, although at the start of its rarefaction it easily displaced 27 [inches]. And hence, in a tube three feet long or a little less, in which the air rarefies through 6 or 8 inches, the mercury is driven out by that air, only to the extent of about four inches below its accustomed height of 2.7/24ths. feet. Moreover this effect is greater in shorter, and less in longer tubes: because evidently, in shorter tubes, it [mercury] is forced down by a smaller space, the air rarefying less; and further, the air in dilating holds greater forces and for that reason it has more power of expansion and displacement of mercury, and vice versa.

I know that there remains quite a considerable doubt in some peoples' minds when I say that greater forces are required to drive down to the greater extent below its appointed height of 2.7/24ths. feet; and a smaller force to drive it down less, for they will imagine that no force at all is required. Under the hypothesis which we hold, air rarefies spontaneously and freely expands into the space left by the mercury and seeks to occupy an ever greater space. By which manner, it seems likely that the mercury would discharge itself completely by its innate gravity, from the tube into the vessel, and that air is dilated by its own nature through the whole of the same tube and it would occupy a longer tube if longer were provided for it. But this difficulty will easily be removed by anyone with a moderate knowledge of mechanics, if he reflects on that force by which the elementary parts of the whole of nature are mutually compressed to constitute a single elementary system. I consider that this force, which we commonly call gravity, and which vulgar philosophy considers is just like simple gravity, is indeed more substantial, like a mutual pres-

sure or oscillation of all parts together. Once it has been agreed that gravity in this lower region of the air, or at the surface of the earth, is as great as the weight of 2.7/24ths. feet in altitude of mercury, and that the tendency to unification between the higher and lower parts of the elementary system is in the lower regions of the air, assumed to be equivalent to the weight of the height of 2.7/24ths. feet of mercury, or the weight of about 31 feet in height of water, which amounts to the same thing. Once these two points have been conceded in their entirety, if there is no impediment, the mercury rises to a height of 2.7/24ths. feet, or water to a height of about 31 feet. Thus finally, the parts of elementary nature form an equilibrium, which is the aim of the whole of nature, and when the equilibrium is disturbed, these parts will immediately be recalled, by their natural effort towards their innate endeavour. But as a matter of fact, if besides mercury or water, there be admitted into any part of the tube some of our compressed and condensed air, as we have stated above, this air obtains its freedom and all its parts recoil and become rarefied and drive out the mercury or water, which for that reason will be depressed below the aforesaid height, either more or less, according to the air itself possesses greater or lesser power of rarefaction.

Appendix III

J. PECQUET, *Experimenta Nova Anatomica*, Amsterdam, 1661, pp. 106—109.¹⁸¹

“Water only compresses the Terraquaeous Globe by virtue of its weight, but air not only by virtue of its weight but also by Elater.”

Let this be firmly and invincibly established by these easily performed experiments. A cylindrical glass tube AB, exceeding a little less than three feet in length and if you wish, about four lines in diameter. The end B is hermetically sealed, that is the glass is itself completely closed up, the other end A being left open. Mercury is poured into the whole tube with the exception of seven inches CA, water occupying the residual space. The mouth is completely closed by the finger, and the tube is then inverted, when the lighter water changes places with the mercury, and at length condensed to the other end B. The tube was then immersed in the restagnant mercury prepared in the vessel D, while still being supported by the finger, so that when the finger was swiftly withdrawn the metal flowed down out of the tube. That remaining in the tube is the mercury cylinder AE. It rests, not to the usual height of 27 inches above the surface of restagnant liquid, but is actually lowered by about 6 lines, on account of the constant pressure of water, which was itself permanently 7 inches. It was in fact no wonder that water was in this proportion to mercury since seven and a half inches of the latter are about equal in weight to half an inch of the former.

Hence it is plainly evident that water exerts only the force by weight but not by elater; just as the mercury is affected inside the tube, so it weighs down on the surface of the earth's mass.

But, on the other hand, air possesses that virtue to a great extent, for if this experiment is repeated, not with water, but with air filling the seven residual inches CA. That is the tube is filled up to BC with mercury, and to the mouth A with seven inches CA, which are alone filled with air and then close by the finger. Next, invert and immerse in D. You will be surprised by this contrivance, the mercury cylinder AE falls more than seven inches below the twenty-seventh. So it is evident that the adjacent mercury was not forced down as much by weight as by the strongest elater; and it is that thus the Terraquaeous Globe is compressed by air.

At this point I must point out that where it has brought to rest above 20 inches of mercury, by reaching the 17th inside the tube, just as is frequently described in the experiment on the cause of variation of level in the thermometer tube, which fluctuations are in proportion as the air is at one time rarefied and at another time condensed.

¹⁸¹ This passage is translated from the Latin.

Appendix IV

H. POWER, *Experimental Philosophy*, London, 1664, 1663, pp. 101—103.

Pecquet (who I think follows *Robervalius* therein) ingeniously conceives, that the whole mass of Ayr hath a Spontaneous Eleter, or natural aptitude in in self to dilate and expand it self upon the removal of all circumambient obstacles (which he calls the Elastical motion of that Element) so that the particle[s] of Ayr may be understood to be as many little Springs, which if at liberty, and not bound and squeezed up, will powerfully, strongly, and spontaneously dilate and stretch out themselves, not onely to fill up a large room, but to remove great bodies: So that he compares this vast Element of Air, circumfused about this terraqueous Globe, to a great heap of Woolfleeces or Sponges, piled one upon another, the superior particles of the Ayr pressing the inferior, and hindring their continual tendency to a self-dilatation; so that all the particles of this *Atmosphere* (especially the inferiour sort) strive at all times to expand and dilate themselves: and when the circumresistency of other contiguous Bodies to them is removed, then they flye out into their desired expansion (or at least will dilate so far as neighbouring Obstacles will permit:) Just like the Spring of a Watch (which if the String be broke, presently flyes out into its fullest expansion which Elastick motion in the Ayr then ceases, when it comes to an aequilibration with those circumjacent Bodies that resisted it.

That this is not onely an Ingenious Hypothesis, but that there is much of reality and truth in it, I think our following Experiment will to safety of satisfaction demonstrate.

Onely we differ from *Pecquet* in the strict notion he hath of Rarefaction and Condensation, which he supposeth to be performed without either intromission or exclusion of any other extraneous Body whatsoever. Now how Ayr or any other Body should diminish or augment its Quantity (which is the most close and essential Attribute to Bodies) without change of its own Substance, or at least without a reception or exclusion of some other extrinsecal Body, either into, or out of the Porosities thereof, sounds not onely harsh to our ears, but is besides an unintelligible difficulty.

Now though we cannot by Sensible and Mechanical Demonstration show how any new Substance or Subtler matter (than Ayr is) which enters into the Tube to replenish that seeming vacuity, and to fill up the aerial interstices (which must needs be considerable in so great a self-dilation), yet we must (considering the nature of rarefaction aforesaid) be forced to believe it: and perhaps some happy Experimenter hereafter may come to give us a better then this Speculative and Metaphysical Evidence of it.

That the hollow Cylinder in the Tube is not onely fill'd up with the dilated particles of Ayr, but also with a thin Aethereal Substance intermingled with them:

1. Let us suppose therefore (at random if you please) that there is a thin subtle aethereal substance diffused throughout the Universe; nay, which indeed, by farr the greatest thereof: in which all these Luminous and Opaque Bodies (I mean the Stars and Planets) with their Luminous and Vaporious Sphaeres (continually effluviating from them) do swim at free and full Liberty.

2. Let us consider that this aether is of that Subtil and Penetrative Nature, that like the Magnetical Effluvioms, it shoots it self through all Bodies whatsoever, whose small pores and interstices are supplied and fill'd up with this aethereal Substance, as a Sponge with water.

3. Let us add to the former Considerations, that the Ayr hath not onely a strong Elatery of its own (by which it presses continually upon the Earth, and all Bodies circuminclosed by it) and it also ponderates, and is heavy, in its own Atmosphere.

But because I am resolved you shall take nothing upon the trust and reputation of the best Authour, take this Experiment to prove the Ayr's gravitation (*in proprio Loco*) as the vulgar Philosophy calls it.

Appendix V

R. BOYLE, *New Experiments Physico-Mechanicall*, London, 1660; *Works*, London, 1744, Vol. I. p. 8.

For the more easy understanding of the experiments triable by our engine, I thought it not superfluous nor unreasonable in the recital of this first of them, to insinuate that notion, by which it seems likely, that most, if not all of them, will prove explicable. Your Lordship will easily suppose, that the notion I speak of is, that there is a spring, or elastical power in the air we live in. By which *ελατήρ* or spring of the air, that which I mean is this; that our air either consists of, or at least abounds with, parts of such a nature, that in case they be bent or compressed by the weight of the incumbent part of the atmosphere, or by any other body, they do endeavour, as much as in them lieth, to free themselves from that pressure, by bearing against the contiguous bodies that keep them bent; and, as soon as those bodies are removed, or reduced to give them say, by presently unbending and stretching out themselves, either quite, or so far forth as the contiguous bodies that resist them will permit, and thereby expanding the whole parcel of air, these elastical bodies compose.

This notion may perhaps be somewhat further explained, by conceiving the air near the earth to be such a heap of little bodies, lying one upon another, as may be resembled to a fleece of wool. For this (to omit other likenesses betwixt them) consists of many slender and flexible hairs; each of which may indeed, like a little spring, be easily bent or rolled up; but will also, like a spring, be still endeavouring to stretch it self out again. For though both these hairs, and the aerial corpuscles to which we liken them, do easily yield to external pressures; yet each of them (by virtue of its structure) is endowed with a power or principle of self-dilatation; by virtue whereof, though the hairs may by a man's hand be bent and crouded closer together, and into a narrower room than suits best with the nature of the body; yet, whilst the compression lasts, there is in the fleece they compose an endeavour outwards, whereby it continually thrusts against the hand that opposes its expansion. And upon the removal of the external pressure, by opening the hand more or less, the compressed wool doth, as it were, spontaneously expand or display it self towards the recovery of its former more loose and free condition, till the fleece hath either regained its former dimensions, or at least approached them as near as the compressing hand (perchance not quite opened) will permit. This power of self-dilatation is somewhat more conspicuous in a dry sponge compressed, than in a fleece of wool. But yet we rather chose to employ the latter on this occasion, because it is not, like a sponge, an entire body, but a number of slender and flexible bodies, loosely complicated, as the air it self seems to be.

There is yet another way to explicate the spring of the air; namely, by supposing with that most ingenious gentleman, Monsieur Des Cartes, that the air is nothing but a congeries or heap of small and (for the most part) of flexible particles, of several sizes, and of all kind of figures, which are raised by heat (especially that of the sun) into that fluid and subtle ethereal body that surrounds the earth; and by the restless agitation of that celestial matter, wherein those particles swim, are so whirled round, that each corpuscle endeavours to beat off all others from coming within the little sphere requisite to its motion about its own centre; and in case any, by intruding into that sphere, shall oppose its free rotation, to expel or drive it away: so that, according to this doctrine, it imports very little whether the particles of the air have the structure requisite to springs, or be of any other form (how irregular soever) since their elastical power is not made to depend upon their shape or structure, but upon the vehement agitation, and (as it were) brandishing motion, which they receive from the fluid aether, that swiftly flows between them, and whirling about each of them (independently from the rest) not only keeps those slender aerial bodies separated and stretched out (at least, as far as the neighbouring ones will permit) which otherwise, by reason of their flexibility and weight, would flag or curl; but also makes

them hit against, and knock away each other, and consequently require more room than that, which if they were compressed, they would take up.

By these two differing ways, my Lord, may the springs of the air be explicated. But though the former of them be that, which by reason of its seeming somewhat more easy, I shall for the most part make use of in the following discourse; yet am I not willing to declare peremptorily for either of them against the other. And indeed, though I have in another treatise endeavoured to make it probable, that the returning of elastical bodies (if I may so call them) forcibly bent, to their former position, may be mechanically explicated; yet I must confess, that to determine whether the motion of restitution in bodies proceed from this, that the parts of a body of a peculiar structure are put into motion by the bending of the spring, or from the endeavour of some subtle ambient body, whose passage may be opposed or obstructed, or else its pressure unequally resisted by reason of the new shape or magnitude, which the bending of a spring may give the pores of it: to determine this, I say, seems to me a matter of more difficulty, than at first sight one would easily imagine it.

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(Received February 26, 1965)