Robert Boyle's Experiments in Pneumatics

INTRODUCTION

If, in the last quarter of the seventeenth century, a well-educated person in England or France had been asked why water rises in a suction pump, the answer would have been in terms familiar to our ears. Phrases such as "pressure of the atmosphere," "creation of a vacuum," "air pressure dependent on the height above sea level" would have been used 250 years ago much as we use them in our own time. But if we jump back in our imagination a little more than three centuries, say to 1620, the picture changes. We have clear evidence from the printed records of those days that no such explanation of the action of suction pumps was available even to the most learned and clear-headed men of that time. People were talking in terms of "nature's abhorring a vacuum" and were unable to account for the fact that at sea level a suction pump will not raise water more than about 34 feet.

The radical change that took place between the first and last quarters of the seventeenth century was not confined to discussions of the action of pumps. During the fifty years in question there was a rapid development of what we now call science and was then known as "experimental philosophy." This changed attitude and the process by which the new knowledge was obtained are very well illustrated by a study of seventeenth-century experiments with air and the effect of air pressure on liquids. This subject was called in those days pneumatics. By tracing the growth of the new ideas (concepts) by which ever since that time people have explained a variety of phenomena, we obtain a "case history" of the way in which the experimental sciences developed.

For convenience, the study of pneumatics between 1630 and 1680 may be thought of in terms of the following subdivisions:

(i) Torricelli's experiment with a column of mercury, which included the invention of the barometer and his formulation of the conceptual scheme of a "sea of air" surrounding the earth;

(ii) Pascal's repetition of the Torricellian experiment and his instigation of the measurement of the barometric height at the foot and on the top of a mountain, in 1648;

(iii) Experiments with pumps to produce a vacuum, by von Guericke and by Boyle, 1650–1660;

(iv) Examination by Boyle of the phenomena accessible for study by means of a vacuum pump, including the search for a "more subtle fluid" than air, 1660–1680;

(v) A study of the compressibility of air as compared with that of water, including the discovery of Boyle's Law, 1660–1680.

Evangelista Torricelli (1608–1647), an Italian mathematician, was strongly influenced by the writings on mechanics of the Italian physicist Galileo. He worked on projectile motion and hydrodynamics, but is probably best known for the experiment that bears his name.

Blaise Pascal (1623–1662) is at least as well known for his philosophic writings and his work in mathematics as for his contributions to pneumatics. A mathematical theorem that bears his name was published when he was sixteen, and by the age of 31 he had assisted in establishing the mathematical theory of probability. He renounced scientific activity shortly thereafter, and during the last eight years of his life he was associated with the religious group known as the Jansenists.

Otto von Guericke (1602–1686), mayor of Magdeburg and a military engineer, performed many experiments similar to those of Boyle, and at about the same time. He built a water barometer some three stories high, and observed the variations of the height of the water from day to day.

Robert Boyle (1627–1691) is the central figure in this case. The seventh and last son of the "great" Earl of Cork, Boyle was a man of wealth who devoted his life to religion and science. Too young to have taken part in the Civil War in England in the middle of the seventeenth century, he resided in Oxford at the time when the Puritan element was in the ascendancy in the University. It was the gathering of amateur scientists in Oxford in the 1650's that led to the formation of the Royal Society in 1660, after the Restoration.

In Section 6 we shall consider briefly the relation of science to the practical arts in the seventeenth century. We shall see that the interest in pneumatics was connected to some degree with a concern of learned men with the performance of the common suction pump for raising water. The fact that water would not rise above a certain height in such a pump was almost certainly known to Torricelli, and it may well be that pondering on this phenomenon led him directly to his experiment with a liquid about 14 times as heavy as water, namely, liquid mercury. From this line of thinking may have developed the idea that a column of mercury only about 1/14 as high as the column of water could be supported by atmospheric pressure. Quite apart from the new interest in technologic matters, interest in pneumatics was also probably in-

creased by the publication in 1575 of a Latin translation of an Alexandrian writer, Hero, on this subject. This new edition of an ancient treatise was well known by the beginning of the seventeenth century and called to peoples' minds many phenomena, including the action of a siphon and the fact that a liquid cannot flow from a closed vessel unless air can get in.

We can start our Case by considering the performance of the following experiment by Torricelli in 1643. Taking a glass tube (see Fig. 1)
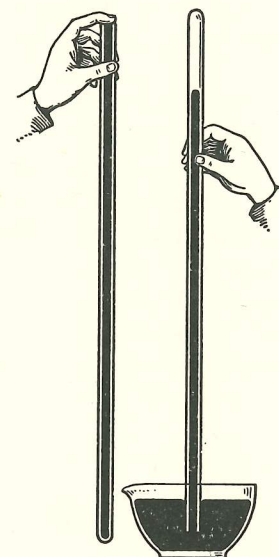

FIG. 1. Torricelli's experiment with a column of mercury in a tube longer than 30 inches.

somewhat less than an inch in diameter and about a yard long, with one end closed, he filled the tube with mercury. Then, placing a finger over the open upper end, he inverted the tube so that the open end was immersed in an open dish of mercury. When he removed his finger from the open end, the mercury in the tube fell until the top of the mercury column was about 30 inches above the level of the mercury in the open dish. Between the top of the mercury column and the upper end of the tube was an empty space, which became known as a Torricellian vacuum. We shall see Boyle referring to this experiment as the experiment of *Torricellius*, or as the experiment *de vacuo*.[1]

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[1] A group in Florence, members of a scientific society called the Accademia del Cimento (Academy of Experiment), continued experiments with vacuum after Torricelli's death. They soon contrived to have the top of the tube consist of a

What led Torricelli to perform this famous experiment we cannot say. It may have been an accidental discovery, the consequence of an interest in the flow of liquids from small orifices; we know that Torricelli had been experimenting in this field. But more probably it was the act of an investigator who wished to test a deduction from a new idea — a working hypothesis on a grand scale. For in the earliest account[2] of the experiment that we have in Torricelli's own words, there is clearly set forth the new conceptual scheme. What most of us today regard as a fact, namely, that the earth is surrounded by a sea of air that exerts pressure, was in the 1640's a new conceptual scheme that had still to weather a series of experimental tests before it would be generally adopted.

Torricelli would never have been able to formulate his ideas as clearly as he did, however, if it had not been for earlier work of those who were concerned with the pressure of *liquids*. The subject is known as hydrostatics and the enunciation of the general principles involved goes back as far as Archimedes (B.C. 287?–212). Thanks to clear-headed writers in the sixteenth century, and in particular to Simon Stevin of Bruges (1548–1620), Torricelli and many of his contemporaries were familiar with such concepts as "pressure," which is force per unit area of surface, and "equilibrium." They knew that the pressure on the bottom of a vessel filled with a liquid depended on the height of the liquid in the vessel but not on its volume or its shape [Fig. 2(a)]. They realized that if the stopcock joining two vessels, one containing water, the other empty of water and open to the air, is quickly opened, the water will flow from one to the other, and soon the heights of the liquid will be the same [Fig. 2(b)]; the system is then in equilibrium. But for a few seconds before equilibrium is reached, the liquid may surge back and forth a little. The principles relating pressure and height of liquid were applicable only in the equilibrium state.

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bulb in which various devices could be placed. The whole could then be filled with mercury and inverted in the usual way, so that the device would be in a Torricellian vacuum. The results of these experiments were not published until after the publication of Boyle's first book, but he must have heard of them by word of mouth or by letter. We shall see that, although many of the experiments performed *in vacuo* by Boyle and by von Guericke could also be performed in a Torricellian vacuum, by using a vacuum produced by an air pump they were able to work on a larger scale and in a less awkward way.

[2] A letter from Torricelli to Cardinal Ricci, dated Florence, June 11, 1644. For an English translation, see *The Physical Treatises of Pascal*, translated by I. H. B. and A. G. H. Spiers (Columbia University Press, New York, 1937), pp. 163–170. Students of this case are strongly urged to read this exchange of letters between Torricelli and Ricci.

Armed with these concepts of hydrostatics, Torricelli and, after him, Pascal could formulate ideas about a sea of air. They could easily answer
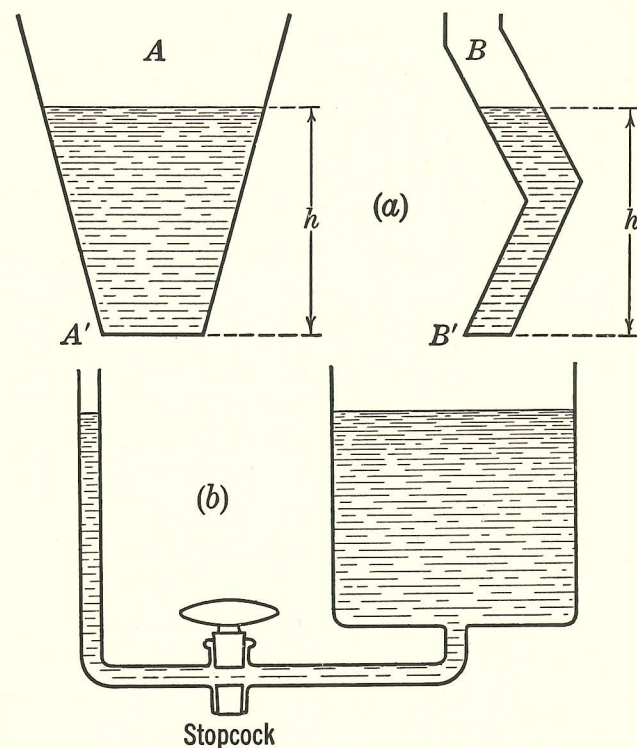

Fig. 2. Diagrams to illustrate the principles of hydrostatics known to Torricelli and his contemporaries: (a) in a homogeneous body of liquid, the pressure (force per unit area) at a given point depends on the depth below the surface; if the liquids in A and B are the same and homogeneous, the pressures at A' and B' are the same if $h = h'$; (b) in equilibrium, the levels of the liquids in connecting vessels are the same whatever the shapes of the vessels.

doubting Thomases who asked why the barometer did not fall if it were placed inside a large glass vessel that was sealed off from the surrounding air (Fig. 3). (This is one of the first objections on record to Torricelli's new idea of a sea of air. The answer was of course that the pressure inside the enclosing vessel was the same as the atmospheric pressure when the vessel was first closed off. There would be no change of pressure on the outer surface of the mercury of the barometer unless some of the air was removed. And it was precisely this that Boyle set out to accomplish!)

Pascal saw that from Torricelli's new conceptual scheme one could draw a logical conclusion susceptible of experimental test. For if the mercury column in Torricelli's new instrument — the barometer — were held up by the pressure of a sea of air, this pressure should be less above the earth's surface than at sea level. Just as the hydrostatic pressure in
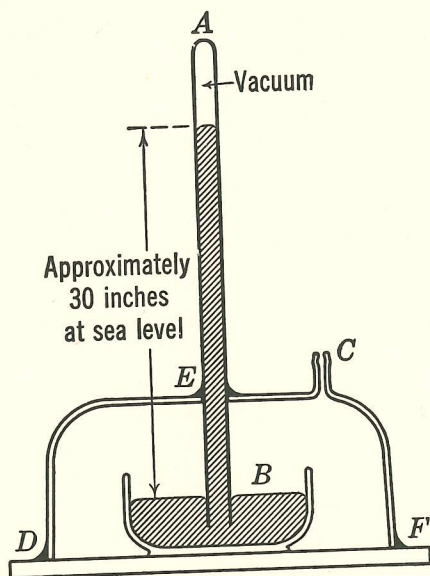

Fig. 3. Diagram of a barometer, with the reservoir B enclosed in a vessel DEF. Will the mercury in tube A fall into the reservoir B if the orifice C is sealed?

the ocean diminishes as a diver ascends from the bottom of a harbor toward the surface, so the pressure of the air should diminish as one ascends a mountain. From this line of reasoning came an experiment performed on the Puy-de-Dôme, a mountain in central France. Translating Pascal's deduction into specific experimental terms, one could say that the height of the mercury in a Torricellian experiment performed high on the mountain should be considerably less than if the experiment were conducted at the foot. The experiment was performed in 1648 by Pascal's brother-in-law, Perier,[3] and the deduction was confirmed. One cannot help, however, but be somewhat skeptical of the high degree of accuracy reported by Perier. To be able to repeat the Torricellian experiment so that there was less than a twelfth of an inch (one "line") difference in successive readings, as Perier claimed, is remarkable. The accidental intrusion of a slight amount of air is very difficult to avoid.

[3] For an English translation of Perier's report, see *The Physical Treatises of Pascal*, pp. 97–120, and also preface, pp. ix–xxiv. This material can be read with profit by the student of this case, and is strongly recommended.

This report of Perier's was written, it must be remembered, before standards of accurate reporting in science had been established. The contrast with Boyle's procedures is striking. It may be that Perier, persuaded of the reality of the large differences in height of the mercury column at the top and bottom of the mountain, succumbed to the temptation of making his argument appear convincing by recording exact reproducibility of his results on repeated trials.

Robert Boyle heard of these experiments of Perier's in the 1650's, although the formal publication of Pascal's treatise dealing with hydrostatics and pneumatics was delayed until 1663. Boyle also knew of the air pump that had been constructed by Otto von Guericke and had heard of the Florentine method of performing experiments in a vacuum. (See footnote 1. The first full account of these experiments of the members of the Accademia del Cimento, however, was not published until 1667; von Guericke's pump was described briefly in a book by K. Schott in 1657.) Boyle saw the importance of having a more convenient method of removing the air from a glass globe in which various pieces of apparatus could be placed. In particular, he was interested in testing one of the deductions from Torricelli's conceptual scheme, namely, that if the air is removed from above the mercury reservoir of a barometer, the mercury column will fall. In other words, he desired to have an instrument with which he could evacuate the vessel DEF in Fig. 3 (with C closed).

The following sections of the Case History deal with (i) the construction of Boyle's pump; (ii) the experiment for which it was particularly designed; (iii) certain experiments on the transmission of sound which illustrate some of the many experiments Boyle was able to perform with his new pump; (iv) Boyle's search for a more subtle fluid; (v) his discovery, as a consequence of a controversy about the validity of his ideas, of what is now known as Boyle's law.

Boyle's work in pneumatics is an excellent illustration of the significance of improvement in experimental equipment for the advance of science. His improved pump made possible the exploration of a wide field of study; it was for his day the equivalent of the x-ray tubes of the late nineteenth century, the cyclotron of the twentieth century and perhaps even the experimental "piles" that since 1945 have produced radioactive isotopes as a consequence of the release of "atomic" energy. Boyle's experiments offered many instances of the care with which an experimenter in a new field must operate in order to obtain significant results. Mechanical difficulties must be overcome and this is by no means easy; moreover, new instruments must be invented, such as a gauge for showing the pressure in an evacuated vessel.

A careful analysis of Boyle's reports of his experiments will bring out the distinction between the limited working hypothesis and its verification or negation on the one hand, and the broad working hypotheses which, if successful, as will be seen, will soon become new conceptual schemes. Of the latter we note in this case four in particular: first, the "sea of air" hypothesis originating with Torricelli; second, the concept of air as an elastic fluid, or in Boyle's words, that air has a spring; third, that sound is transmitted by air; fourth, one that was not successful, namely, that there is a subtle fluid which pervades all space.

The *limited* working hypotheses are as numerous as the experiments. When Boyle built his engine he set out to test one deduction from Torricelli's broad hypothesis. When he had his apparatus all arranged he reasoned somewhat as follows: "If I now operate the pump, then the mercury column in the Torricellian tube should fall." This, be it noted, is a hypothesis strictly limited to that particular experiment. He proceeded with the experiment and the results were as predicted. He employed a similar "if . . . then" type of statement when he considered introducing air into the receiver; the mercury rose, as predicted. The student will find it profitable to identify a number of similar instances of this use of the limited working hypothesis in the experiments on sound and in Boyle's search for a subtle fluid. The connection between these limited working hypotheses and the broader idea that is being tested often involves a number of assumptions, sometimes not made explicit by Boyle. This is particularly clear in the experiments recorded in Sec. 4. Boyle's experiments did not disprove the existence of a subtle fluid in general. They could only test the presence in the air he examined of a specific fluid, more subtle than air, but having certain properties. These properties were such as to cause an effect on the instruments he manipulated in a vacuum (see pp. 39–48) if the subtle fluid were present.

The experiments that Boyle performed in his search for a subtle fluid may seem naïve and foolish to us, but a little thought will make it clear that they were well conceived to test a real possibility. For all the seventeenth-century investigators knew, air might have been composed of two or more materials differing in their ability to pass through very fine holes; such a difference is taken advantage of whenever we strain out a finely divided solid from a liquid, for example. Indeed, we now know that there is a very slight difference in the rate at which the constituent gases of the atmosphere (chiefly oxygen and nitrogen) flow through a tube of very small diameter. But this difference is so slight that it is not reflected in any behavior of air in experiments that could be performed with the equipment available in the seventeenth or even the eighteenth century. The great difference in "subtlety" for which Boyle was looking does not exist in any mixture of gases. It is in the

nature of a gas that there can be no gross nonhomogeneity in the mixture, such as occurs in a suspension of fine particles of clay in water or even in water solutions of the materials that are present in blood or milk. More than a century elapsed, however, before it became obvious that such was indeed the case. And it was almost two centuries before the conceptual scheme was developed which we now use in all our explanations of the behavior of air and other gases (the kinetic theory of gases).

In reading the original records of the seventeenth-century investigators, the student will wish to have firmly in mind the simple ideas about atmospheric pressure that are almost common knowledge today. There are one or two less obvious points that now seem clear to us but long were puzzles to those who studied pneumatics in the seventeenth and eighteenth centuries. The first concerns the presence of water vapor in the atmosphere; the second, the evaporation of liquids both below the temperature at which they boil and during the process of boiling itself. Since the first notions that developed about the evaporation of water into the atmosphere were either wrong or confused, we are omitting the seventeenth-century experiments dealing with this subject.

At first Boyle was confused by the fact that water contains dissolved air, but he eventually came to understand the relation between boiling point and the pressure of the surrounding atmosphere; indeed, he invented an apparatus for distilling *in vacuo*. The question of the chemical homogeneity of the atmosphere had to be explored before a satisfactory picture could be developed and the relation of liquid water to water vapor properly understood. This hiatus must be mentioned, for today every reader of the weather reports is familiar not only with variations in atmospheric pressure but also with the degree of humidity on a given day. For a long time little or no sense could be made of the fluctuations in the barometer because it was believed that these fluctuations were directly related to what we now call humidity (that is, the relative amount of water vapor in the air). The student will naturally wonder what Boyle and his contemporaries made of their observations of the changes in the atmospheric pressure and of the behavior of water in the vacuum that they produced. Boyle and his contemporaries studied these phenomena but came to no satisfactory and enduring conclusions. This serves to illustrate the slow stages by which science often advances.

1. THE AIR PUMP OR VACUUM PUMP AS A NEW SCIENTIFIC INSTRUMENT

There were three models of Boyle's "pneumatical engine," as he called his pump for producing a vacuum. The first, described in a book published in 1660 (dated December 20, 1659), is shown in Figs.

4 and 5; a second is described in another book dated March 24, 1667 and published in 1669; and a third was described in a volume published in 1680. In the construction of the first two of these engines Robert Hooke played an important and perhaps determining role; the third was designed by Denis Papin in 1676 and was brought with him from France in the same year that he joined Boyle. As compared with the first air pump of von Guericke, Boyle's first model was far more convenient for those who wished to perform experiments *in vacuo*. The opening at the top of the glass bulb was the important new addition. The second model of Boyle's engine (Fig. 7) allowed various types of receivers to be evacuated, such as those shown in Fig. 8. Much larger equipment could be placed *in vacuo* than with the first model. The experiments were correspondingly more ambitious. The third model was more rapid in its action because it had two plungers and two pistons and was operated by foot power. The valves were automatic. Boyle likewise devised methods of measuring the diminished pressure by means of what are now called vacuum gauges; he also built compression pumps that enabled him to study air under pressure.

Boyle's public announcement of the construction of his first pump and the description of the experiments he performed were given in his book of 1660, carrying the title page:

NEW

EXPERIMENTS

Physico-Mechanicall,

Touching

The SPRING of the AIR,

and its EFFECTS,

(Made, for the most part, in a New

PNEUMATICAL ENGINE)

Written by way of LETTER

To the Right Honorable *Charles*

Lord Vicount of *Dungarvan,*

Eldest Son to the EARL of *CORKE*.

The first sections of the preface are of some general interest even today and therefore are given below:[4]

To the Reader

Although the following treatise being far more prolix than becomes a letter, and than I at first intended it, I am very unwilling to encrease

[4] All quotations of Robert Boyle's writings are taken from a late edition of his collected writings, *The Works of the Honourable Robert Boyle* (London, 1772).

the already excessive bulk of the book by a preface; yet there are some particulars, that I think myself obliged to take notice of to the reader, as things that will either concern him to know, or me to have known.

In the first place then: If it be demanded why I publish to the world a letter, which, by its style and divers passages, appears to have been written as well for, as to a particular person; I have chiefly these two things to answer; the one, that the experiments therein related, having been many of them tried in the presence of ingenious men, and by that means having made some noise among the Virtuosi (insomuch that some of them have been sent into foreign countries, where they have had the luck not to be despised) I could not, without quite tiring more than one amanuensis, give out half as many copies of them as were so earnestly desired, that I could not civilly refuse them. The other, that intelligent persons in matters of this kind persuade me, that the publication of what I had observed touching the nature of the air, would not be useless to the world; and that in an age so taken with novelties as is ours, these new experiments would be grateful to the lovers of free and real learning: so that I might at once comply with my grand design of promoting experimental and useful philosophy, and obtain the great satisfaction of giving some to ingenious men; the hope of which is, I confess, a temptation, that I cannot easily resist.

Of my being somewhat prolix in many of my experiments, I have these reasons to render: that some of them being altogether new, seemed to need the being circumstantially related, to keep the reader from distrusting them: that divers circumstances I did here and there set down for fear of forgetting them, when I may hereafter have occasion to make use of them in my other writings: that in divers cases I thought it necessary to deliver things circumstantially, that the person I addressed them to might, without mistake, and with as little trouble as is possible, be able to repeat such unusual experiments: and that after I consented to let my observations be made public, the most ordinary reason of my prolixity was, that foreseeing, that such a trouble as I met with in making those trials carefully, and the great expence of time that they necessarily require (not to mention the charges of making the engine, and employing a man to manage it) will probably keep most men from trying again these experiments, I thought I might do the generality of my readers no unacceptable piece of service, by so punctually relating what I carefully observed, that they may look upon these narratives as standing records in our new pneumatics, and need not reiterate themselves an experiment to have as distinct an idea of it, as may suffice them to ground their reflexions and speculations upon. . . .

Boyle's description of the construction of his engine is very long and rather tedious. A few paragraphs will illustrate the great detail with which he reported his work. In so doing, Boyle was setting the model for subsequent scientists. Unless experiments are reported in detail and

with accuracy, other scientists are often unable to repeat the experiment in question. The more complicated the phenomena investigated, the more necessary it becomes to establish the practice of recording and reporting details of construction of the apparatus and the results obtained. Failure to adhere to these rules in the seventeenth and eighteenth centuries often made worthless the reports of many investigators. The published works of Stevin, Galileo, and Pascal (to name the more important of Boyle's predecessors), are written for the most part in the form of geometric propositions and it is often not clear whether the experiments described were actually carried out or are to be regarded as *possible* demonstrations.

The introduction of Boyle's book is in the form of a letter to his nephew, of which the first sections are printed below.

Receiving in your last from *Paris* a desire, that I would add some more experiments to those I formerly sent you over; I could not be so much your servant as I am, without looking upon that desire as a command; and consequently, without thinking myself obliged to consider by what sort of experiments it might the most acceptably be obeyed. And at the same time, 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, in the famous experiment touching a 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* [see p. 5]; and by presenting your Lordship an account of my attempts to illustrate a subject, about which its being so much discoursed of where you are, together with your inbred curiosity, and love of experimental learning, made me suppose you sufficiently inquisitive.

And though I pretend not to acquaint you, on this occasion, with any store of new discoveries, yet possibly I shall be so happy, as to assist you to know some things, which you did formerly but suppose; and shall present you, if not with new theories, at least with new proofs of such as are not yet become unquestionable. And if what I shall deliver hath the good fortune to encourage and assist you to prosecute the hints it will afford, I shall account myself, in paying of a duty to you, to have done a piece of service to the commonwealth of learning. Since it may conduce to the advancement of that experimental philosophy, the highly pursuit of which requires as well a purse as a brain, to endear it to hopeful persons of your quality, who may accomplish many things, which others cannot but wish, or at most but design, by being able to employ the presents of fortune in the search of the mysteries of nature.

And I am not faintly induced to make choice of this subject, rather than any of the expected chymical ones, to entertain your Lordship upon, by these two considerations: the one, that the air being so necessary to human life, that not only the generality of men, but most other creatures that breathe, cannot live many minutes without it, any considerable discovery of its nature seems likely to prove of moment to mankind. And the other is, that the ambient air being that, whereto both our own bodies, and most of the others we deal with here below, are almost perpetually contiguous, not only its alterations have a notable and manifest share in those obvious effects, that men have already been invited to ascribe thereunto, (such as are the various distempers incident to human bodies, especially if crazy in the spring, the autumn, and also on most of the great and sudden changes of weather;) but likewise, the further discovery of the nature of the air will probably discover to us, that it concurs more or less to the exhibiting of many phænomena, in which it hath hitherto scarce been suspected to have any interest. So that a true account of any experiment that is new concerning a thing, wherewith we have such constant and necessary intercourse, may not only prove of some advantage to human life, but gratify philosophers, by promoting their speculations on a subject, which hath so much opportunity to solicit their curiosity. . . .

You may be pleased to remember, that a while before our separation in *England*, I told you of a book, that I had heard of, but not perused, published by the industrious Jesuit *Schottus*; wherein, it was said, he related how that ingenious gentleman, *Otto Gericke*, consul of *Magdeburg*, had lately practised in *Germany* a way of emptying glass vessels, by sucking out the air at the mouth of the vessel, plunged under water. And you may also perhaps remember, that I expressed myself much delighted with this experiment, since thereby the great force of the external air (either rushing in at the opened orifice of the emptied vessel, or violently forcing up the water into it) was rendered more obvious and conspicuous than in any experiment that I had formerly seen. And though it may appear by some of those writings I sometimes shewed your Lordship, that I had been solicitous to try things upon the same ground; yet in regard this gentleman was before-hand with me in producing such considerable effects by means of the exsuction of air, I think myself obliged to acknowledge the assistance and encouragement the report of his performances hath afforded me.

But as few inventions happen to be at first so complete, as not to be either blemished with some deficiencies needful to be remedied, or otherwise capable of improvement; so when the engine, we have been speaking of, comes to be more attentively considered, there will appear two very considerable things to be desired in it. For first, the wind-pump (as somebody not improperly calls it) is so contrived, that to evacuate the vessel, there is required the continual labour of two strong men for divers hours. And next (which is an imperfection of much greater moment) the receiver, or glass to be emptied, consisting of one entire and uninterrupted globe and neck of glass; the whole engine is so made, that things cannot be conveyed into it, whereon to try experi-

ments: so that there seems but little (if any thing) more to be expected from it, than those very few phaenomena, that have been already observed by the author, and recorded by *Schottus*. Wherefore to remedy these inconveniences, I put both Mr. *G.* and *R. Hook* (who hath also the honour to be known to your Lordship, and was with me when I had these things under consideration) to contrive some air-pump, that might not, like the other, need to be kept under water (which on divers occasions is inconvenient) and might be more easily managed: and after an unsuccessful trial or two of ways proposed by others, the last-named person fitted me with a pump, anon to be described. And thus the first imperfection of the German engine was in good measure, though not perfectly remedied: and to supply the second defect, it was considered, that it would not perhaps prove impossible to leave in the glass to be emptied a hole large enough to put in a man's arm cloathed; and consequently other bodies, not bigger than it, or longer than the inside of the vessel. And this design seemed the more hopeful, because I remembered, that having several years before often made the experiment *de vacuo* [see p. 5] with my own hands; I had, to examine some conjectures that occurred to me about it, caused glasses to be made with a hole at that end, which uses to be sealed up, and had nevertheless been able, as occasion required, to make use of such tubes, as if no such holes had been left in them, by devising stopples for them, made of the common plaister called diachylon [a sealing wax]; which, I rightly enough guessed, would, by reason of the exquisite commixtion of its small parts, and closeness of its texture, deny all access to the external air. Wherefore, supposing that by the help of such plaisters carefully laid upon the commissures of the stopple and hole to be made in the receiver, the external air might be hindered from insinuating itself between them into the vessel, we caused several such glasses, as you will find described a little lower, to be blown at the glass-house. And though we could not get the workmen to blow any of them so large, or of so convenient a shape as we would fain have had; yet finding one to be tolerably fit, and less unfit than any of the rest, we were content to make use of it in that engine; of which, I suppose, you by this time expect a description, in order to the recital of the phænomena exhibited by it.

To give your Lordship then, in the first place, some account of the engine itself; it consists of two principal parts; a glass vessel, and a pump to draw the air out of it [Figs. 4 and 5].

The former of these (which we, with the glass-men, shall often call a receiver, for its affinity to the large vessels of that name, used by chymists) consists of a glass with a wide hole at the top, of a cover to that hole, and of a stop-cock fastened to the end of the neck, at the bottom.

The shape of the glass, you will find expressed in the first figure of the annexed scheme. And for the size of it, it contained about 30 wine quarts, each of them containing near two pound (of 16 ounces to the pound) of water. We should have been better pleased with a more

capacious vessel; but the glass-men professed themselves unable to blow a larger, of such a thickness and shape as was requisite to our purpose.
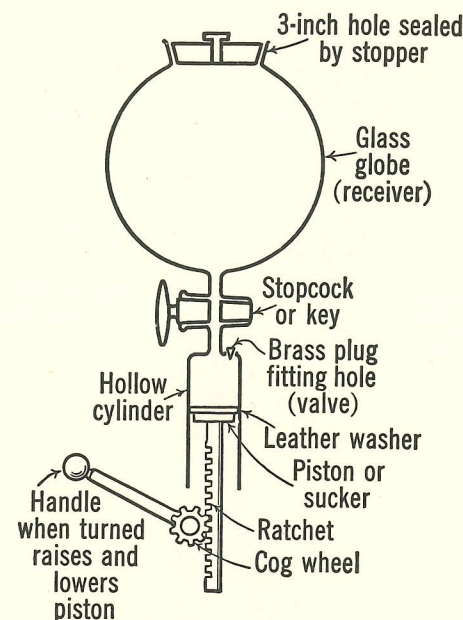

FIG. 4. Diagram of the first model of Boyle's air pump.

2. THE BEHAVIOR OF A TORRICELLIAN BAROMETER IN A VACUUM

The seventeenth experiment reported by Boyle in his volume of 1660 was the critical one for which he says he built the engine. No one had ever put this particular consequence of the new conceptual scheme to the experimental test. This experiment is, therefore, typical of a procedure repeatedly used with great effectiveness in the advance of the experimental sciences. From a new concept or conceptual scheme one can deduce that if the concept or set of concepts is a satisfactory scheme, then certain deductions follow that may be susceptible of experimental test.

Boyle saw that a new apparatus (von Guericke's pump), if *improved* and somewhat changed, would enable him to put to the experimental test another consequence of the new concepts about the atmosphere and its pressure. This he did in the manner described in the extract presented below. This combination of the possibilities inherent in a new type of machine — or a new chemical process — and the necessary consequences of a new concept has been one of the most fruitful sources of progress in the experimental sciences. For this reason, as a
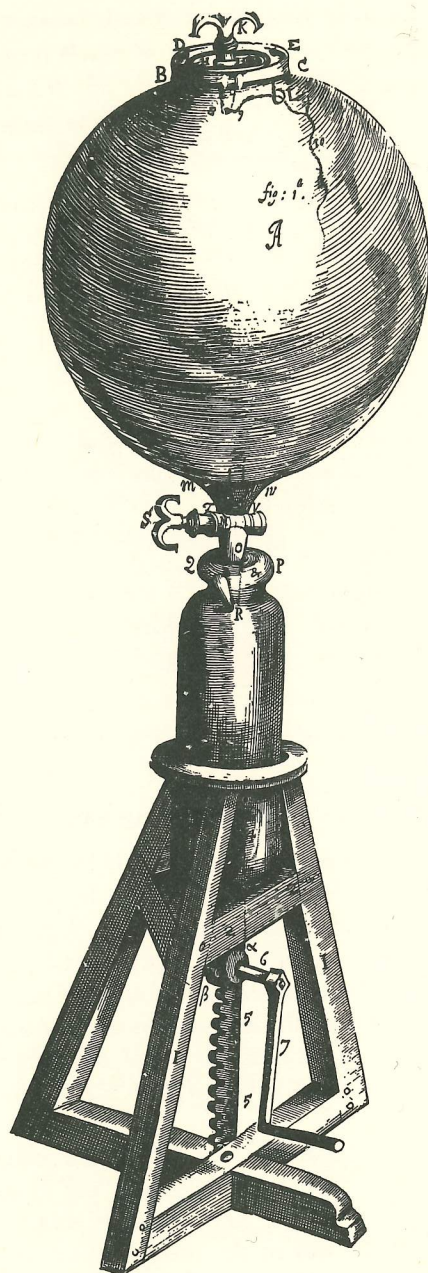
FIG. 5. Reproduction of a wood engraving of Boyle's first air pump, from his own book.

case in point, the details of Boyle's reasoning merit careful consideration by anyone who attempts to understand the methods of modern science.

Boyle's description of his seventeenth experiment now follows (the footnotes and the material enclosed in brackets have been added to assist the reader).

Proceed we now to the mention of that experiment, whereof the satisfactory trial was the principal fruit I promised myself from our engine, it being then sufficiently known, that in the experiment *de vacuo*, the quicksilver in the tube is wont to remain elevated, above the surface of that whereon it leans, about 27 digits [about 29.5 inches, as Boyle explains later]. I considered, that, if the true and only reason why the quicksilver falls no lower, be, that at that altitude the mercurial cylinder in the tube is in an æquilibrium with the cylinder of air supposed to reach from the adjacent mercury to the top of the atmosphere [this is the conceptual scheme, suggested by Torricelli and elaborated by Pascal, that has been accepted ever since; note the use of the concept of equilibrium]; then if this experiment could be tried out of the atmosphere, the quicksilver in the tube would fall down to a level with that in the vessel, since then there would be no pressure upon the subjacent, to resist the weight of the incumbent mercury. Whence I inferred (as easily I might) that if the experiment could be tried in our engine, the quicksilver would subside below 27 digits, in proportion to the exsuction of air, that should be made out of the receiver. For, as when the air is shut into the receiver, it doth (according to what hath above been taught) continue there as strongly compressed, as it did whilst all the incumbent cylinder of the atmosphere leaned immediately upon it; because the glass, wherein it is penned up, hinders it to deliver itself, by an expansion of its parts, from the pressure wherewith it was shut up. So if we could perfectly draw the air out of the receiver, it would conduce as well to our purpose, as if we were allowed to try the experiment beyond the atmosphere.

It should be noted that throughout the descriptions of his experiments Boyle spells everything out in great detail. That the pressure within the glass receiver is just as great after the receiver is closed off as it was before is obvious today, but it was far from clear at first. One of the first objections (see p. 7 and Fig. 3) to Torricelli's new ideas was that if the weight of the air on the outside mercury was responsible for the mercury's standing about 30 inches in the Torricellian tube, then sealing the whole apparatus inside a box should cause the mercury to fall, since the weight of the air would then only be that of the small amount in the surrounding box (Fig. 3). The error here, as Torricelli showed, is a confusion of weight and pressure. Boyle had probably heard of these arguments but had probably not read the account of them that is now available to us.

Wherefore (after having surmounted some little difficulties, which occurred at the beginning) the experiment was made after this manner: we took a slender and very curiously blown cylinder of glass, of near three foot in length, and whose bore had in diameter a quarter of an inch, wanting a hair's breadth: this pipe being hermetically sealed at one end [i.e., the glass being melted together so that no air could subsequently leak in], was, at the other, filled with quicksilver, care being taken in the filling, that as few bubbles as was possible should be left in the mercury. Then the tube being stopt with the finger and inverted, was opened, according to the manner of the experiment, into a somewhat long and slender cylindrical box (instead of which we now are wont to use a glass of the same form) half filled with quicksilver: and so, the liquid metal being suffered to subside, and a piece of paper being pasted on level with its upper surface, the box and tube and all were by strings carefully let down into the receiver [through the opening at the top; see Fig. 6]: and then, by means of the hole formerly mentioned to be left in the cover, the said cover was slipt along as much of the tube as reached above the top of the receiver; and the interval, left betwixt the sides of the hole and those of the tube, was very exquisitely filled up with melted (but not over-hot) diachylon, and the round chink, betwixt the cover and the receiver, was likewise very carefully closed up: 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 to such a cylinder of mercury as it would keep from subsiding.

All things being thus in a readiness, the sucker [Fig. 4] was drawn down; and, immediately upon the egress of a cylinder of air out of the receiver, the quicksilver in the tube did, according to expectation, subside: and notice being carefully taken (by a mark fastened to the outside) of the place where it stopt, we caused him that managed the pump to pump again, and marked how low the quicksilver fell at the second exsuction; but continuing this work, we were quickly hindered from accurately marking the stages made by the mercury, in its descent, because it soon sunk below the top of the receiver, so that we could henceforward mark it no other ways than by the eye. And thus, continuing the labour of pumping for about a quarter of an hour, we found ourselves unable to bring the quicksilver in the tube totally to subside; because, when the receiver was considerably emptied of its air, and consequently that little that remained grown unable to resist the irruption of the external, that air would (in spight of whatever we could do) press in at some little avenue or other; and though much could not thereat get in, yet a little was sufficient to counterbalance the pressure of so small a cylinder of quicksilver, as then remained in the tube.

Boyle subsequently used the length of such a column of mercury or its equivalent as a measure of the completeness of the vacuum he succeeded in producing in any experiment. We do the same today, but express our results in millimeters of mercury or in fractions of a millimeter of mercury. A well-constructed pump of Boyle's type today will hardly lower the pressure below a quarter of an inch of mercury. Pumps of a
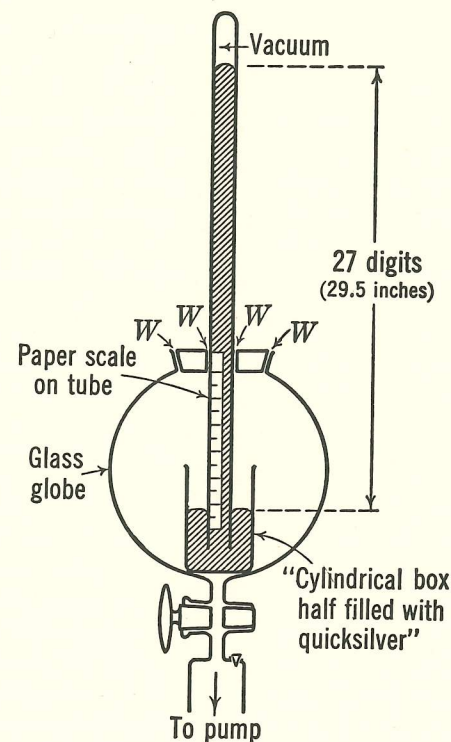

FIG. 6. Diagram of Boyle's apparatus for the experiment of removing the air above the reservoir of a barometer; W indicates "intervals" filled with "diachylon." The pump was that shown in Fig. 4.

different type are required to produce the high vacua used in the modern laboratory and in the manufacture of electric light bulbs and radio tubes.

Now (to satisfy ourselves farther, that the falling of the quicksilver in the tube to a determinate height, proceedeth from the æquilibrium, wherein it is at that height with the external air, the one gravitating, the other pressing with equal force upon the subjacent mercury) we returned the key [Fig. 4] and let in some new air; upon which the mercury immediately began to ascend (or rather to be impelled upwards) in the tube, and continued ascending, till, having returned the key, it immediately rested at the height which it had then attained: and so, by

turning and returning the key, we did several times at pleasure impel it upwards, and check its ascent. And lastly, having given a free egress at the stop-cock to as much of the external air as would come in, the quick-silver was impelled up almost to its first height: I say almost, because it stopt near a quarter of an inch beneath the paper-mark formerly men-tioned; which we ascribed to this, that there was (as is usual in this ex-periment) some little particles of air engaged among those of the quick-silver; which particles, upon the descent of the quicksilver, did manifestly rise up in bubbles towards the top of the tube, and by their pressure, as well as by lessening the cylinder by as much room as they formerly took up in it, hindered the quicksilver from regaining its first height.

This experiment was a few days after repeated, in the presence of those excellent and deservedly famous Mathematic Professors, Dr. *Wallis*, Dr. *Ward*, and Mr. *Wren*,[5] who were pleased to honour it with their pres-ence; and whom I name, both as justly counting it an honour to be known to them, and as being glad of such judicious and illustrious wit-nesses of our experiment; and it was by their guess, that the top of the quicksilver in the tube was defined to be brought within an inch of the surface of that in the vessel.

And here, for the illustration of the foregoing experiment, it will not be amiss to mention some other particulars relating to it.

First then, when we endeavoured to make the experiment with the tube closed at one end with diachylon instead of an hermetical seal, we perceived, that upon the drawing of some of the air out of the receiver, the mercury did indeed begin to fall, but continued afterwards to sub-side, though we did not continue pumping. When it appeared, that though the diachylon, that stopt the end of the tube, were so thick and strong, that the external air could not press it in, (as experience taught us that it would have done, if there had been but little of it;) yet the subtler parts of it were able (though slowly) to insinuate themselves through the very body of the plaister, which it seems was not of so close a texture, as that which we mentioned ourselves to have successfully made use of, in the experiment *de vacuo* some years ago. So that now we begin to suspect, that perhaps one reason, why we cannot perfectly pump out the air, may be, that when the vessel is almost empty, some of the subtler parts of the external air may, by the pressure of the atmos-phere, be strained through the very body of the diachylon into the re-ceiver. But this is only conjecture.

[5] These men were all at Oxford in the period 1655–1660 when the embryonic Royal Society was forming. Wren is the famous architect who rebuilt London after the great fire; Wallis and Ward were distinguished mathematicians. Wallis served the Parliamentary Armies in the Civil War by deciphering Royal-ist dispatches (a fact of which little was probably said after the Restoration in 1660); Wren was "intruded" into All Souls College by a parliamentary com-mittee during the Cromwellian period.

Here we see Boyle recording his experimental troubles. A tube sealed at the upper end with wax (diachylon) was often not leakproof. The conjecture that air might be a mixture of materials of differing degrees of "subtlety" is the basis of the experiments described in Sec. 4 of this Case History, and we see here how this thought could well have arisen from the experimental problem of obtaining airtight seals.

Another circumstance of our experiment was this, that if (when the quicksilver in the tube was fallen low) too much ingress were, at the hole of the stop-cock, suddenly permitted to the external air; it would rush in with that violence, and bear so forcibly upon the surface of the subjacent quicksilver, that it would impel it up into the tube rudely enough to endanger the breaking of the glass.

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 ⅜, and at the second an inch and ⅜; and when the vessel was almost emptied, it could scarce at one exsuction be drawn down above the breadth of a barley-corn. And indeed we found it very difficult to measure, in what proportion these decrements of the mercurial cylinder did proceed; partly, because (as we have already intimated) the quicksilver was soon drawn below the top of the receiver; and partly because, upon its descent at each exsuction, it would immediately reascend a little upwards; either by reason of the leaking of the vessel at some imperceptible hole or other, or by reason of the motion of restitution in the air, which, being some-what compressed by the fall as well as weight of the quicksilver, would repel it a little upwards, and make it vibrate a little up and down, before they could reduce each other to such an æquilibrium as both might rest in.

But though we could not hitherto make observations accurate enough, concerning the measures of the quicksilver's descent, to reduce them into 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.

And, to illustrate this matter a little more, we will add, that we made a shift to try the experiment in one of our above mentioned [in a section of Boyle's book not reproduced herein] small receivers, not containing a quart; but (agreeably to what we formerly observed) we found it as diffi-cult to bring this to be quite empty as to evacuate the greater; the least external air that could get in (and we could not possibly keep it all per-fectly out) sufficing, in so small a vessel, to display a considerable pressure upon the surface of the mercury, and thereby hinder that in the tube from falling to a level with it. But this is remarkable, that having two or three times tried the experiment in a small vessel upon the very first cylin-der of air that was drawn out of the receiver, the mercury fell in the tube 18 inches and a half, and another trial 19 inches and a half. . . .

The ratio of the volume of the receiver — i.e., the vessel being evacuated — to the volume of the cylinder of the pump determines the effects of each stroke of the piston. With Boyle's large receiver, probably this ratio was something like 20 to 1. Each stroke of the piston would thus reduce the pressure by about 1/21 which for the first stroke would mean a fall in mercury level of about 1½ inch; with a small receiver whose volume was less than that of the pump cylinder the pressure would be reduced by more than one half (by 18 or 19 inches, Boyle records).

The next few paragraphs of the book, which are omitted here, discuss Boyle's futile attempts to reason in numerical terms about the phenomena he had observed. He was unable to reduce his qualitative observations to a quantitative basis; he was unable to use the new conceptual scheme for he did not see at that time that if two vessels of equal volume, one full of air at atmospheric pressure, the other essentially empty, are connected, the pressure becomes the same in both vessels, namely, half of what it originally was in the first vessel.

For farther confirmation of what hath been delivered, we likewise tried the experiment in a tube of less than two foot long: and, when there was so much air drawn out of the vessel, that the remaining air was not able to counterbalance the mercurial cylinder, the quicksilver in the tube subsided so visibly, that (the experiment being tried in the little vessel lately mentioned) at the first suck it fell above a span, and was afterwards drawn lower and lower for a little while; and the external air being let in upon it, impelled it up again almost to the top of the tube: so little it matters, how heavy or light the cylinder of quicksilver to subside is, provided its gravity overpower the pressure of as much external air as bears upon the surface of that mercury into which it is to fall.

In other words, it is unnecessary to start with a barometer in this experiment, for a short inverted tube filled with mercury will suffice. This is the equivalent of the lower portion of the Torricellian tube; it is far more convenient than the long tube, and the simplest vacuum gauges used today in chemical and physical laboratories are constructed in this way.

Lastly, we also observed, that if (when the mercury in the tube had been drawn down, and by an ingress permitted to the external air, impelled up again to its former height) there were some more air thrust up by the help of the pump into the receiver, the quicksilver in the tube would ascend much above the wonted height of 27 digits, and immediately upon the letting out of that air would fall again to the height it rested at before. [Here Boyle pumps air *into* the receiver and shows that the increased pressure causes the height of the mercury to increase beyond the barometric height.]

Your Lordship will here perhaps expect, that as those, who have treated of the Torricellian experiment, have for the most part maintained the affirmative, or the negative of that famous question, whether or no that noble experiment infer a vacuum? so I should on this occasion interpose my opinion touching that controversy; or at least declare, whether or no, in our engine, the exsuction of the air do prove the place deserted by the air sucked out to be truly empty, that is, devoid of all corporeal substance. But besides that I have neither the leisure, nor the ability, to enter into a solemn debate of so nice a question; your Lordship may, if you think it worth the trouble, in the Dialogues not long since referred to, find the difficulties on both sides represented, which then made me yield but a very wavering assent to either of the parties contending about the question: nor dare I yet take upon me to determine so difficult a controversy.

For on the one side it appears, that notwithstanding the exsuction of the air, our receiver may not be destitute of all bodies, since any thing placed in it, may be seen there; which would not be, if it were not pervious to those beams of light, which rebounding from the seen object to our eyes, affect us with the sense of it: and that either these beams are corporeal emanations from some lucid body, or else at least the light they convey doth result from the brisk motion of some subtle matter, I could, if I mistake not, sufficiently manifest out of the Dialogues abovementioned, if I thought your Lordship could seriously imagine that light could be conveyed without, at least, having (if I may so speak) a body for its vehicle.

In the eighteenth and nineteenth centuries, as in the seventeenth, it would have been taken for granted that light was either a beam of particles that would pass through glass or else a motion in a medium that pervaded glass. The latter view seemed to be established by experiment early in the nineteenth century and the medium was given the name "luminiferous ether" or "ether" (not to be confused with the anaesthetic with the same name). The same medium could be invoked to explain the action of magnetism (see Boyle's next two paragraphs). This medium was imagined to be far too subtle, to use Boyle's phrase, to be subject to mechanical rarefaction or compression as is air. As the study of radiant energy proceeded, the conceptual scheme that postulated ether as a medium became inadequate because it failed to account for certain phenomena. The answer to the question raised by Boyle's contemporaries, if a vacuum is really empty how can you see through it, cannot be given today in terms of any one simple conceptual scheme. Modern views simply challenge the assumption that seemed so obvious to Boyle and many later scientists, namely, that light must for its conveyance require "a body for its vehicle."

By the sixteenth experiment, it also appears that the closeness of our receiver hinders it not from admitting the effluvia of the load-stone;[6] which makes it very probable that it also freely admits the magnetical steams of the earth; concerning which, we have in another treatise endeavoured to manifest that numbers of them do always permeate our air.

But on the other side it may be said, that as for the subtle matter which makes the objects enclosed in our evacuated receiver, visible, and the magnetical effluvia of the earth that may be presumed to pass through it, though we should grant our vessel not to be quite devoid of them, yet we cannot so reasonably affirm it to be replenished with them, as we may suppose, that if they were gathered together into one place without intervals between them, they would fill but a small part of the whole receiver. As in the thirteenth experiment, a piece of match was inconsiderable for its bulk, whilst its parts lay close together, that afterwards (when the fire had scattered them into smoke) seemed to replenish all the vessel. For (as elsewhere our experiments have demonstrated) both light and the effluvia of the load-stone may be readily admitted into a glass, hermetically sealed, though before their admission, as full of air as hollow bodies here below are wont to be; so that upon the exsuction of the air, the large space deserted by it, may remain empty, notwithstanding the pretence of those subtle corpuscles, by which lucid and magnetical bodies produce their effects.

In short, "those subtle corpuscles, by which lucid and magnetical bodies produce their effects" are quite independent of the particles that compose the air. This may be considered a preview of the doctrine of the ether as it was expounded by all scientists 75 years ago. The relevance of the experiment with the match is not obvious. The thirteenth experiment consisted in allowing the smoke from a "slow match" — a slow-burning material used for ignition of cannon — to fill an evacuated receiver, which it did, of course, rapidly. This phenomenon inspired Boyle perhaps unduly; he saw in it a visualization of the way "subtle material" such as air will expand at once and fill a space; he likewise recognized that a very minute amount of match was consumed in producing enough smoke to fill a large receiver. Therefore, he argues that the still more subtle corpuscles that convey light need be of but little bulk if solidified all together.

The controversy between the Vacuists and the Plenists, referred to in the next paragraph, goes back at least to Aristotle. In the form referred

[6] What we would now call the field of a magnet. In short, a magnet — a piece of the naturally occurring magnetized iron ore is called a loadstone — will exert a force on iron placed in a vacuum. This had been demonstrated by the Florentine experiments and also by von Guericke before Boyle's experiments. The phrase "magnetical steams of the earth" in the same sentence refers to the earth's magnetic field, about which Boyle speculated in another book.

to by Boyle, it continued until the close of the century. The plenists confused the "subtle material the vehicle of light" with air. To them the explanation of why water will not run out of an inverted bottle with a narrow neck (unless air is shaken in or another opening made) was as follows: if the water comes out, the surrounding medium must be displaced, and it can be displaced only if there is somewhere for it to go. If a second opening is provided in the bottle, the displaced medium can enter; therefore, the water runs out.

According to the Plenists the world was full, by definition; a vacuum was unthinkable; these were the postulates of their position. A further premise of their position, but one not recognized, was that the medium was essentially incompressible; otherwise the water might run out of an inverted bottle by compressing rather than displacing the surrounding medium. It may be left to the reader to see how the position of the Plenists became untenable in the light of the Torricellian experiment unless some additional and arbitrary assumptions were introduced.

And as for the allegations above-mentioned, they seemed to prove but that the receiver devoid of air, may be replenished with some etherial matter, as some modern Naturalists write of, but not that it really is so. And indeed to me it yet seems, that as to those spaces which the Vacuists would have to be empty, because they are manifestly devoid of air and all grosser bodies; the Plenists (if I may so call them) do not prove that such spaces are replenished with such a subtle matter as they speak of, by any sensible effects, or operations of it (of which divers new trials purposely made, have not yet shewn me any) but only conclude that there must be such a body, because there cannot be a void. And the reason why there cannot be a void, being by them taken, not from any experiments, or phænomena of nature, that clearly and particularly prove their hypothesis, but from their notion of a body, whose nature, according to them, consisting only in extension (which indeed seems the property most essential to, because inseparable from a body) to say a space devoid of body, is, to speak in the schoolmen's phrase, a contradiction *in adjecto*. This reason, I say, being thus desumed, seems to make the controversy about a vacuum rather a metaphysical, than a physiological question;[7] which therefore we shall here no longer debate, finding it very difficult either to satisfy Naturalists with this Cartesian notion of a body, or to manifest wherein it is erroneous, and substitute a better in its stead.

But though we are unwilling to examine any farther the inferences wont to be made from the Torricellian experiment, yet we think it not

[7] This curious use of the word "physiological" is now obsolete; in the seventeenth century the word "physiology" was sometimes used as equivalent to natural science.

impertinent to present your Lordship with a couple of advertisements concerning it.

First then, if in trying the experiment here or elsewhere, you make use of the English measures that mathematicians and tradesmen are here wont to employ, you will, unless you be forewarned of it, be apt to suspect that those that have written of the experiment have been mistaken. For whereas men are wont generally to talk of the quicksilver's remaining suspended at the height of between six or seven and twenty inches; we commonly observed, when divers years since we first were solicitous about this experiment, that the quicksilver in the tube rested at about 29 inches and a half above the surface of the restagnant quicksilver in the vessel, which did at first both amaze and perplex us, because though we held it not improbable that the difference of the grosser English air, and that of Italy and France, might keep the quicksilver from falling quite as low in this colder, as in those warmer climates; yet we could not believe that that difference in the air should alone be able to make so great an one in the heights of the mercurial cylinders; and accordingly upon enquiry we found, that though the various density of the air be not to be overlooked in this experiment, yet the main reason why we found the cylinder of mercury to consist of so many inches, was this, that our English inches are somewhat inferior in length to the digits made use of in foreign parts, by the writers of the experiment.[8]

The next thing I desire your Lordship to take notice of, is, that the height of the mercurial cylinder is not wont to be found altogether so great as really it might prove, by reason of the negligence or incogitancy of most that make the experiment. For oftentimes upon the opening of the inverted tube into the vesselled mercury, you may observe a bubble of air to ascend from the bottom of the tube through the subsiding quicksilver to the top; and almost always you may, if you look narrowly, take notice of a multitude of small bubbles all along the inside of the tube betwixt the quicksilver and the glass; (not now to mention the particles of air that lie concealed in the very body of the mercury:) many of which, upon the quicksilver's forsaking the upper part of the tube, do break into that deserted space where they find little or no resistance to their expanding of themselves. [It is difficulties such as this that are the basis of one's skepticism about the accuracy of Perier's reports (see p. 8).] Whether this be the reason, that upon the application of warm bodies to the emptied part of the tube,[9] the subjacent mercury would be depressed somewhat lower, we shall not determine; though it

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[8] Difficulties of this sort have led to an international agreement on standards of measurement. The accuracy required in modern experiments has meant that providing standards has become a rather elaborate matter.

[9] We are now quite certain that this is the reason. To the extent that there is air in the space above the mercury in the Torricellian tube, warming and cooling this space will affect the height of the column since air expands and contracts with changes in temperature, a fact well known by 1660.

seem very probable, especially since we found, that, upon the application of linen cloths dipped in water, to the same part of the tube, the quicksilver would somewhat ascend; as if the cold had condensed the imprisoned air (that pressed upon it) into a lesser room. But that the deserted space is not wont to be totally devoid of air, we were induced to think by several circumstances: for when an eminent mathematician, and excellent experimenter, had taken great pains and spent much time in accurately filling up a tube of mercury, we found that yet there remained store of inconspicuous bubbles, by inverting the tube, letting the quicksilver fall to its wonted height; and by applying (by degrees) a red-hot iron to the outside of the tube, over against the upper part of the mercurial cylinder, (for hereby the little unheeded bubbles, being mightily expanded, ascended in such numbers, and so fast to the deserted space, that the upper part of the quicksilver seemed, to our wonder, to boil.) We farther observed, that in the trials of the Torricellian experiment, we have seen made by others, and (one excepted) all our own, we never found that, upon the inclining of the tube, the quicksilver would fully reach to the very top of the sealed end: which argued, that there was some air retreated thither that kept the mercury out of the unreplenished space. [This is the forerunner of many such methods of checking on the performance of an apparatus. If Perier had reported that he had made this test in each instance, one would be more inclined to take seriously the reported accuracy of his results. But despite Perier's statement that he "carefully rid the tube of air," one remains skeptical of his ability to repeat the Torricellian experiment with an accuracy of a twelfth of an inch.]

If your Lordship should now demand what are the best expedients to hinder the intrusion of the air in this experiment; we must answer, that of those which are easily intelligible without ocular demonstration; we can at present suggest, upon our own trials, no better than these. First, at the open end of the tube the glass must not only be made as even at the edges as you can, but it is very convenient (especially if the tube be large) that the bottom be every way bent inwards, that so the orifice not much exceeding a quarter of an inch in diameter, may be the more easily and exactly stopped by the experimenter's finger; between which and the quicksilver, that there may be no air intercepted (as very often it happens that there is) it is requisite that the tube be filled as full as possibly it can be, that the finger which is to stop it, pressing upon the accumulated and protuberant mercury, may rather throw down some, than not find enough exactly to keep out the air. It is also an useful and compendious way not to fill the tube at first quite of mercury, but to leave near the top about a quarter of an inch empty; for if you then stop the open end with your finger, and invert the tube, that quarter of an inch of air will ascend in a great bubble to the top, and in its passage thither, will gather up all the little bubbles, and unite them with itself into one great one; so that if by reinverting the tube, you let that bubble return to the open end of it, you will have a much closer mercurial cylin-

der than before, and need but to add a very little quicksilver more to fill up the tube exactly. And lastly, as for those lesser and inconspicuous parcels of air which cannot this way be gleaned up, you may endeavour, before you invert the tube, to free the quicksilver from them by shaking the tube, and gently knocking on the outside of it, after every little parcel of quicksilver which you pour in; and afterwards, by forcing the small latitant bubbles of air to disclose themselves and break, by imploying a hot iron in such manner as we lately mentioned. I remember that by carefully filling the tube, though yet it were not quite free from air, we have made the mercurial cylinder reach to 30 inches and above an eighth, and this in a very short tube: which we therefore mention, because we have found, by experience, that in short tubes a little air is more prejudicial to the experiment than in long ones, where the air having more room to expand itself, doth less potently press upon the subjacent mercury.

Note the type of extremely helpful suggestions given by Boyle for the benefit of others who wished likewise to experiment; before the publication of this book in 1660 few if any instances are on record of a similar concern with the difficulties of other experimenters except in so far as the recipes of the alchemists can be considered in this category.

3. BOYLE'S EXPERIMENTS ON AIR AS A MEDIUM FOR TRANSMITTING SOUND

Boyle's published record of two experiments on air as a medium for transmitting sound is given in this section. The first is Experiment 27 in his book of 1660; the second is Experiment 41 of his second book on pneumatics, published in 1669.

Boyle's description of his twenty-seventh experiment in his account of 1660 follows.

That the air is the medium, whereby sounds are conveyed to the ear, hath been for many ages, and is yet the common doctrine of the schools. But this received opinion hath been of late opposed by some philosophers upon the account of an experiment made by the industrious *Kircher*, and other learned men; who have (as they assure us) observed, that if a bell, with a steel clapper, be so fastened to the inside of a tube, that upon the making the experiment *de vacuo* [see footnote 1] with that tube, the bell remained suspended in the deserted space at the upper end of the tube: and if also a vigorous load-stone be applied on the outside of the tube to the bell, it will attract the clapper, which, upon the removal of the load-stone falling back, will strike against the opposite side of the bell, and thereby produce a very audible sound; whence divers have concluded, that it is not the air, but some more subtle body, that is the medium of sounds. But because we conceived, that, to invalidate such a consequence from this ingenious experiment, (though the most luciferous that could well be made without some such engine as ours) some things

might be speciously enough alledged; we thought fit to make a trial or two, in order to the discovery of what the air doth in conveying of sounds, reserving divers other experiments triable in our engine concerning sounds, till we can obtain more leisure to prosecute them. Conceiving it then the best way to make our trial with such a noise, as might not be loud enough to make it difficult to discern slighter variations in it, but rather might be, both lasting (that we might take notice by what degrees it decreased) and so small, that it could not grow much weaker without becoming imperceptible; we took a watch, whose case we opened, that the contained air might have free egress into that of the receiver. And this watch was suspended in the cavity of the vessel only by a pack-thread, as the unlikeliest thing to convey a sound to the top of the receiver; and then closing up the vessel with melted plaister, we listened near the sides of it, and plainly enough heard the noise made by the balance. [Boyle clearly recognized the importance of controlling the conditions in an experiment. The method of supporting the source of the noise at first sight appears irrelevant. On further reflection, however, it is clear that the sound might be transmitted through this support. If so, a thread seemed less likely to convey sound than a metal or wooden support. To make sure that a watch so suspended by a thread in air could still be heard, Boyle proceeded to determine whether he could hear the watch *before* he pumped out the air.] Those also of us, that watched for that circumstance, observed, that the noise seemed to come directly in a streight line from the watch unto the ear. And it was observable to this purpose, that we found a manifest disparity of noise, by holding our ears near the sides of the receiver, and near the cover of it: which difference seemed to proceed from that of the texture of the glass, from the structure of the cover (and the cement) through which the sound was propagated from the watch to the ear. But let us prosecute our experiment [that is, let us start pumping the air out of the receiver in which the watch is suspended by a thread]. The pump after this being employed, it seemed, that from time to time the sound grew fainter and fainter; so that when the receiver was emptied as much as it used to be for the foregoing experiments, neither we, nor some strangers, that chanced to be then in the room, could, by applying our ears to the very sides, hear any noise from within; though we could easily perceive, that by the moving of the hand, which marked the second minutes, and by that of the balance, that the watch neither stood still, nor remarkably varied from its wonted motion. And to satisfy ourselves farther, that it was indeed the absence of the air about the watch, that hindered us from hearing it, we let in the external air at the stop-cock; and then though we turned the key and stopt the valve, yet we could plainly hear the noise made by the balance, though we held our ears sometimes at two foot distance from the outside of the receiver; and this experiment being reiterated into another place, succeeded after the like manner. Which seems to prove, that whether or no the air be the only, it is at least the principal medium of sounds. [A very cautious interpretation of the experimental findings. Boyle recognizes