that there might be two or more media by which sound was transmitted, but if so the second medium did not play the principal part in the usual case. His search for a second more "subtle medium" is recorded in Sec. 4.] And by the way it is very well worth noting, that in a vessel so well closed as our receiver, so weak a pulse as that the balance of a watch, should propagate a motion to the air in a physically streight line, notwithstanding the interposition of so close a body as glass, especially glass of such thickness as that of our receiver; since by this it seems the air imprisoned in the glass must, by the motion of the balance, be made to beat against the concave part of the receiver, strongly enough to make its convex part beat upon the contiguous air, and so propagate the motion to the listner's ears. [Boyle here reverts to a discussion of the fact that before the air was pumped out, one could hear the watch imprisoned in the receiver.] I know this cannot but seem strange to those, who, with an eminent modern philosopher, will not allow, that a sound, made in the cavity of a room, or other place so closed, that there is no intercourse betwixt the external and internal air, can be heard by those without, unless the sounding body do immediately strike against some part of the inclosing body. But not having now time to handle controversies, we shall only annex, that after the foregoing experiment, we took a bell of about two inches in diameter at the bottom, which was supported in the midst of the cavity of the receiver by a bent stick, which by reason of its spring pressed with its two ends against the opposite parts of the inside of the vessel: in which, when it was closed up, we observed, that the bell seemed to sound more dead than it did when just before it sounded in the open air. And yet, when afterwards we had (as formerly) emptied the receiver, we could not discern any considerable change (for some said they observed a small one) in the loudness of the sound. Whereby it seemed, that though the air be the principal medium of sound, yet either a more subtle matter may be also a medium of it, or else an ambient body, that contains but very few particles of air, in comparison of those it is easily capable of, is sufficient for that purpose. And this, among other things, invited us to consider, whether in the above-mentioned experiment made with the bell and the load-stone, there might not in the deserted part of the tube remain air enough to produce a sound; since the tubes for the experiment *de vacuo* (not to mention the usual thinness of the glass) being seldom made greater than is requisite, a little air might bear a not inconsiderable proportion to the deserted space: and that also, in the experiment *de vacuo*, as it is wont to be made, there is generally some little air, that gets in from without, or at least store of bubbles, that arise from the body of the quicksilver, or other liquor itself, observations heedfully made have frequently informed us; and it may also appear, by what hath been formerly delivered concerning the Torricellian experiment.

Experimentation with a Torricellian vacuum was certainly difficult. We now know that the two major sources of error in the study of the

propagation of sound in a vacuum are (i) the presence of air in the evacuated space, (ii) the transmission of sound by the solid support of the source of the sound.

We now turn to the record of some experiments performed some six or seven years later with the aid of the second and improved model of Boyle's pneumatic engine. Boyle's original drawing of this arrangement is reproduced in Fig. 7. Here he shows a still more convenient method

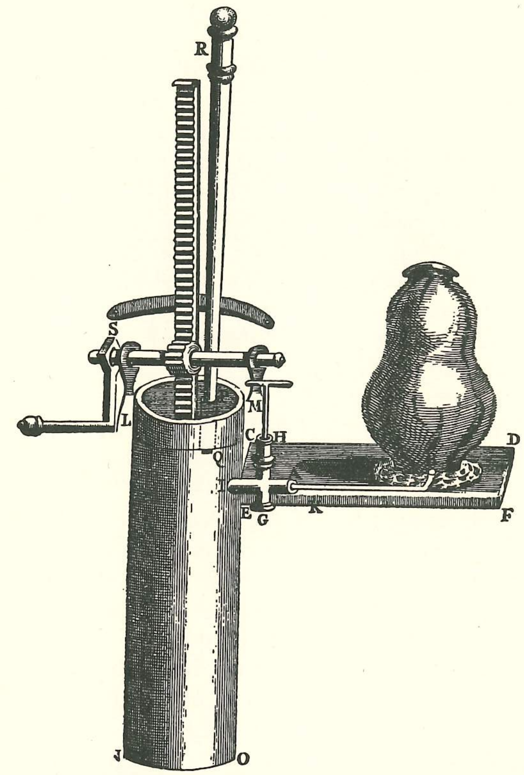

Fig. 7. Boyle's second air pump. This illustration is partly diagrammatic: the iron plate *CDEF* onto which the glass receiver is sealed is imagined to be cut away so as to show the tube *AB* connecting the receiver with the pump through the valve *HG*; the structure of the pump is not indicated, but was essentially the same as in the first model.

of performing experiments *in vacuo*. For in this case the apparatus to be studied rests on an iron plate under a bell jar, which is then sealed by wax to the plate and the air evacuated through a hole in the bottom of the plate connected by a tube to the pump. This is still the usual arrangement in lecture-table demonstrations of experiments in a vacuum.

EXPERIMENT 41 [of the book entitled *A Continuation of New Experiments Physico-Mechanical Touching the Spring and Weight of the Air, and their Effects*, published in 1669].

About the propagation of sounds in the exhausted receiver.

To make some further observation than is mentioned in the published experiments, about the production and conveying of sounds in a glass whence the air is drawn out, we employed a contrivance, of which, because we make use of it in divers other experiments, it will be requisite to give your lordship here some short description.

We caused to be made at the turner's a cylinder of box, or the like close and firm wood, and of a length suitable to that of the receiver it was to be employed in. Out of the lower basis of this cylinder (which might be about an inch and a half in diameter) there came a smaller cylinder or axle-tree, not a quarter so thick as the other, and less than an inch long; this was turned very true, that it might move to and fro; or, as the tradesmen call it, ride very smoothly in a little ferrule or ring of brass, that was by the same turner made for it in the midst of the fixed trencher (as we call a piece of solid wood, shaped like a mill-stone) being four or five inches, more or less (according to the wideness of the receiver) in breadth, and between one and two in thickness; and in a large and round groove or gutter, purposely made in the lower part of this trencher, I caused as much lead as would fill it up to be placed and fastened, that it might keep the trencher from being easily moved out of its place or posture, and in the upper part of this trencher it was intended that holes should be made at such places as should be thought fit, to place bodies at several distances as occasion should require. The upper basis of the cylinder had also coming out of the midst of it another axle-tree, but wider than the former, that, into a cavity made in it, it might receive the lower end of the turning-key divers times already mentioned, to which it was to be fastened by a slender peg of brass thrust through two correspondent holes, the one made in the key, and the other in the newly-mentioned socket (if I may so call it) of the axle-tree. Besides all which, there were divers horizontal perforations bored here and there in the pillar itself, to which this axis belonged, which pillar we shall, to avoid ambiguity, call the vertical cylinder. The general use of this contrivance (whose other parts need not to be mentioned before the experiments where they are employed) is, that the end of the turning-key being put into the socket, and the lower axis of the vertical cylinder into the trencher, by the motion of the key a body fastened at one of the holes to the cylinder may be approached to, or removed from, or made to rub or strike against another body fastened in a convenient posture to the upper part of the trencher. [The apparatus here described was depicted by Boyle as shown in Fig. 8.]

To come now to our trial about sounds, we caused a hand-bell (whose handle and clapper were taken away) to be fastened to a strong wire, that, one end of the wire being made fast in the trencher, the other end,

which was purposely bent downwards, took hold of the bell. In another hole made in the circumference of the same trencher was wedged in (with a wooden peg) a steel-spring, to whose upper part was tied a gad of iron or steel, less than an inch long, but of a pretty thickness. The length of this spring was such, as to make the upper part of the hammer (if I may so call the piece of iron) of the same height with the bell,

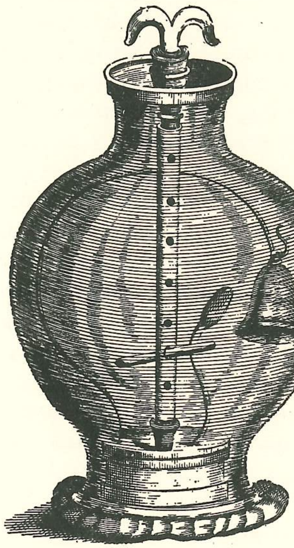

FIG. 8. Wood engraving from Boyle's book, showing the "cylinder or axle tree" connected to a "turning key" which enabled Boyle to strike a bell in a vacuum.

and the distance of the spring from the bell was such, that when it was forced back the other way, it might at its return make the hammer strike briskly upon the outside of the bell.

Boyle used a brass cover on some of his bell jars, thus enabling him to have a "key," fitted through a carefully constructed opening, which could be turned without admitting air. The difficulties of having this key turn in an airtight bearing are very great. There must have been a considerable amount of leakage in the apparatus. In the third paragraph below, Boyle describes an experimental precaution against leakage of air around the key. If he had at this time developed instruments for measuring the air pressure *inside* an evacuated vessel (as he later did), he could have carried out all these experiments with more assurance. He would then have made his observations at the same low pressure, say that corresponding to 1 inch-of-mercury.

The trencher being thus furnished and placed in a capped receiver (as you know, for brevity sake, we use to call one that is fitted with one or other of the brass covers, often mentioned already) the air was diligently pumped out; and then, by the help of the turning-key, the vertical cylinder was made to go round, by which means as often as either of a couple of stiff wires or small pegs that were fastened at right angles into holes, made not far from the bottom of the cylinder, passed (under the bell, and) by the lately mentioned spring, they forcibly did in their passage bend it from the bell, by which means, as soon as the wire was gone by, and the spring ceased to be pressed, it would fly back with violence enough to make the hammer give a smart stroke upon the bell: and by this means we could both continue the experiment at discretion, and make the percussions more equally strong, than it would otherwise have been easy to do.

The event of our trial was, that, when the receiver was well emptied, it sometimes seemed doubtful, especially to some of the by-standers, whether any sound were produced or no; but to me, for the most part, it seemed, that after much attention I heard a sound, that I could but just hear; and yet, which is odd, methought it had somewhat of the nature of shrilness in it, but seemed (which is not strange) to come from a good way off. Whether the often turning of the cylindrical key kept the receiver from being so stanch as else it would have been, upon which score some little air might insinuate itself, I shall not positively determine; but to discover what interest the presence or the absence of the air might have in the loudness or lowness of the sound, I caused the air to be let into the receiver, not all at once, but at several times, with competent intervals between them; by which expedient it was easy to observe, that the vertical cylinder being still made to go round, when a little air was let in, the stroke of the hammer upon the bell (that before could now and then not be heard, and for the most part be but very scarcely heard) began to be easily heard; and when a little more air was let in, the sound grew more and more audible, and so increased, until the receiver was again replenished with air; though even then (that we omit not that phænomenon) the sound was observed to be much less loud than when the receiver was not interposed between the bell and the ear.

And whereas in the already published physico-mechanical experiments [Experiment 27, p. 30], I acquainted your lordship with what I observed about the sound of an ordinary watch in the exhausted receiver, I shall now add, that that experiment was repeated not long since, with the addition of suspending in the receiver a watch with a good alarum, which was purposely so set, that it might, before it should begin to ring, give us time to cement on the receiver very carefully, exhaust it very diligently, and settle ourselves in a silent and attentive posture. And to make this experiment in some respect more accurate than the others we made of sounds, we secured ourselves against any leaking at the top, by imploying a receiver that was made all of one piece of glass (and consequently had no cover cemented on to it) being furnished only within (when it was first blown) with a glass-knob or button, to which a string might be tied. And because it might be suspected, that if the watch were suspended by its own silver chain, the tremulous motion of its sounding bell might be propagated by that metalline chain [the same question that arose in Experiment 27] to the upper part of the glass, to obviate this as well as we could, we hung the watch, not by its chain, but a very slender thread, whose upper end was fastened to the newly mentioned glass-button.

These things being done, and the air being carefully pumped out, we silently expected the time, when the alarum should begin to ring, which it was easy to know by the help of our other watches; but not hearing any noise so soon as we expected, it would perhaps have been doubted whether the watch continued going, if for prevention we had not ordered the matter so, that we could discern it did not stand still: wherefore I desired an ingenious gentleman to hold his ear just over the button at which the watch was suspended, and to hold it also very near to the receiver; upon which he told us, that he could perceive, and but just perceive something of sound that seemed to come from far; though neither we that listened very attentively near other parts of the receiver, nor he, if his ears were no more advantaged in point of position than ours, were satisfied that we heard the watch at all. Wherefore ordering some air to be let in, we did, by the help of attention, begin to hear the alarum, whose sound was odd enough, and, by returning the stop-cock to keep any more air from getting in, we kept the sound thus low for a pretty while, after which a little more air, that was permitted to enter, made it become more audible; and when the air was yet more freely admitted, the by-standers could plainly hear the noise of the yet continuing alarum at a considerable distance from the receiver. [By using a thread for a support, and eliminating the turning key (and thus the leakage), Boyle has finally succeeded in reducing the sound to a point where it cannot be heard. When air is allowed to enter the receiver, the sound is readily audible. The evidence that air is the medium for transmitting sound is now quite convincing.]

From what has hitherto been related, we may learn what is to be thought of what is delivered by the learned *Mersennus*,[10] in that book of his Harmonicks, where he makes this to be the first proposition. *Sonus à campanis, vel altis corporibus non solum producitur in illo vacuo* (*quicquid tandem illud sit*) *quod sit in tubis hydrargyro plenis, posteaque depletis, sed etiam idem acumen, quod in aere libero vel clauso penitus observatur & auditur*.[11] For the proof of which assertion, not long after,

[10] Father Mersenne, the indefatigable reporter of experimental philosophy through whom Pascal first learned of Torricelli's experiment. Boyle seems to be referring to his report of the Florentine work on the propagation of sound in a vacuum carried out with a Torricellian vacuum.

[11] A free translation of the Latin passage is as follows: Not only is the sound of even the shrillest bells produced in the vacuum (whatever that may finally turn

he speaks thus: *porro variis tubis, quorum extremis lagenæ vitreæ adglutinantur, observari campanas in illo vacuo appensas propriisque malleis percussas idem penitus acumen retinere, quod in aere libero habent: atque soni magnitudinem ei sono, qui sit in aere quem tubus clausus includit, nihil cedere.*[12] But though our experiments sufficiently manifest, that the presence or absence of the common air is of no small importance as to the conveying of sounds, and that the interposition of glass may sensibly weaken them; yet so diligent and faithful a writer as *Mersennus* deserves to be favourably treated; and therefore I shall represent on his behalf, that what he says may well enough have been true, as far as could be gathered from the trials he made. For, first, it is no easy matter, especially for those that have not peculiar and very close cements, to keep the air quite out for any considerable time in vessels consisting of divers pieces, such as he appears to have made use of; and next, the bigness of the bell in reference to the capacity of the exhausted glass, and the thickness of the glass, and the manner whereby the bell was fastened to the inside of the glass, and the hammer or clapper was made to strike, may much vary the effect of the trial, for reasons easy to be gathered out of the past discourse, and therefore not needful to be here insisted on. And upon this account we chose to make our experiment with sounds that should not be strong or loud, and to produce them after such a manner, as that as little shaking as could be might be given by the sounding body to the glass it was included in.

4. BOYLE'S ATTEMPT TO DISCOVER A MEDIUM MORE SUBTLE THAN AIR

We have already noted Boyle's concern with the possibility that in addition to the air which he could pump out of his receivers, there might be present in the atmosphere more subtle material that would pass through holes too small to allow the passage of air. Such a medium, which had been postulated by Descartes and to some degree confused with air by subsequent proponents of the Plenist doctrine, might conceivably be still present in an evacuated receiver and still subject to movement by mechanical means. To test this possibility, Boyle contrived a series of ingenious experiments some of which are described in the following account of Experiments 38, 39, and 40 of his book of 1669. All these experiments yielded negative results.

out to be) which he makes by filling tubes with mercury and then pouring them off [i.e., by performing the Torricellian experiment], but also the pitch is observed to be the same as that heard in free but entirely enclosed air.

12 "Further, bells hung in the vacuum, produced in inverted glass flagons to whose mouths tubes have been glued, are observed when struck with their own hammers to maintain the same pitch that they would have in the open air. Also it is noted that the loudness of the tone is no less than that produced by the bells when the tubes contain air."

EXPERIMENT 38 [from Boyle's *Continuation* of 1669]

About an attempt to examine the motions and sensibility of the Cartesian Materia subtilis, *or the Æther, with a pair of bellows made of a bladder, in the exhausted receiver.*

I will not now discuss the controversy betwixt some of the modern atomists and the Cartesians; the former of whom think, that betwixt the earth and the stars, and betwixt these themselves, there are vast tracts of space that are empty, save where the beams of light do pass through them; and the latter of whom tell us, that the intervals betwixt the stars and planets, among which the earth may perhaps be reckoned, are perfectly filled, but by a matter far subtler than our air, which some call celestial, and others aether. I shall not, I say, engage in this controversy; but thus much seems evident, that if there be such a celestial matter, it must make up far the greatest part of the universe known to us. For the interstellar part of the world, if I may so stile it, bears so very great a proportion to the globes, and their atmospheres too, if other stars have any, as well as the earth, that it is almost incomparably greater in respect of them, than all our atmosphere is in respect of the clouds, not to make the comparison between the sea and the fishes that swim in it.

Wherefore I thought it might very well deserve a heedful inquiry, whether we can by sensible experiments (for I hear what has been attempted by speculative arguments) discover any thing about the existence, or the qualifications of this so vast aether; and I hoped our curiosity might be somewhat assisted by our engine, if I could manage in it such a pair of bellows as I designed: for I proposed to myself to fasten a convenient weight to the upper basis, and clog the lower with another great enough to keep it horizontal and immoveable; that when by the help of the turning-key frequently above mentioned, the upper basis should be raised to its full height, the cavity of the bellows might be brought to its full dimensions: this done, I intended to exhaust the receiver, and consequently the thus opened bellows, with more than ordinary diligence, that so both the receiver and they might be carefully freed from air: after which I purposed to let go the upper base of the bellows, that, being hastily depressed by the incumbent weight, it might speedily enough fall down to the lower basis, and by so much, and so quickly lessening the cavity, might expel thence the matter (if any were) before contained in it, and that (if it could by this way be done) at the hole of a slender pipe fastened either near the bottom of the bellows, or in the upper basis; against, or over the orifice, of which pipe there was to be placed at a convenient distance, either a feather, or (if that should prove too light) the sail of a little windmill made of cards, or some other light body, and fit to be put into motion by the impulse of any matter that should be forced out of the pipe.

By this means it seemed not improbable that some such discovery might be made, as would not be altogether useless in our inquiry. For if, notwithstanding the absence of the air, it should appear by the effects, that a stream of other matter capable to set visible bodies a moving, should issue out at the pipe of the compressed bellows, it would also appear that there may be a much subtler body than common air,[13] and as yet unobserved by the vacuists, or (their adversaries) the schools, that may even copiously be found in places deserted by the air; and that it is not safe to conclude from the absence of the air in our receivers, and in the upper part of those tubes where the Torricellian experiment is made, that there is no other body left but an absolute vacuity, or (as the atomists call it) a *vacuum coacervatum*. But if, on the other side, there should appear no motion at all to be produced, so much as in the feather, it seemed that the vacuists might plausibly argue, that either the cavity of the bellows was absolutely empty, or else that it would be very difficult to prove by any sensible experiment that it was full; and if, by any other way of probation, it be demonstrable that it was replenished with aether, we, that have not yet declared for any party, may by our experiment be taught to have no confident expectations of easily making it sensible by mechanical experiments; and may also be informed, that it is really so subtle and yielding a matter that does not either easily impel such light bodies as even feathers, or sensibly resist, as does the air itself, the motions of other bodies through it, and is able, without resistance, to make its passage through the pores of wood and leather, and also of closer bodies, which we find not that the air doth in its natural or wonted state penetrate.

To illustrate this last clause, I shall add, that to make the trial more accurate, I waved the use of other bellows (especially not having such as I desired) and caused a pair of small bellows to be made with a bladder, as a body, which some of our former experiments have evinced to be of so close a texture, that air will rather break it than pass through it; and that the bladder might no where lose its entireness by seams, we glued on the two bases, the one to the bottom and the other to the opposite part of it, so that the neck came out at a hole purposely made for it in the upper basis; and into the neck it was easy to insert what pipe we thought fit, binding the neck very close to it on the outside. We had likewise thoughts to have another pair of tight bellows made with a very light clack [the valve that allows air to be drawn into the bellows] in the lower basis, that by hastily drawing up the other basis, when the receiver and bellows were very carefully exhausted, we might see by the rest, as the lifting up of the clack, whether the subtle matter that was expelled by the upper basis in its ascent would, according to the modern doctrine of the circle made by moving bodies, be impelled up or not. [The phrase "modern doctrine" refers to certain ideas of Descartes which in the middle of the seventeenth century were modern!]

[13] More subtle because it would not have been removed by his pump, yet not so subtle as to fail to be moved in a stream by a quick compression of a bellows; this is a very limited definition of a subtle fluid (see p. 10).

We also thought of placing the little pipe of the bladder-bellows (if I may so call them) beneath the surface of water exquisitely freed from air, that we might see, whether upon the depression of the bellows by the incumbent weight, when the receiver was carefully exhausted, there would be any thing expelled at the pipe that would produce bubbles in the liquor wherein its orifice was immersed.

To bring now our conjectures to some trial, we put into a capped receiver the bladder accommodated as before is mentioned; and though we could have wished it had been somewhat larger, because it contained but between half a pint and a pint, yet in regard it was fine and limber, and otherwise fit for our turn, we resolved to try how it would do; and to depress the upper basis of these little bellows the more easily and uniformly, we covered the round piece of pasteboard that made the upper basis with a pewter-plate (with a hole in it for the neck of the bladder) which nevertheless, upon trial, proved not ponderous enough, whereby we were obliged to assist it by laying on it a weight of lead. And to secure the above-mentioned feather (which had a slender and flexible stem, and was left broad at one end, and fastened by cement at the other, so as to stand with its broad end at a convenient distance just over the orifice of the pipe) from being blown aside to either hand, we made it to move in a perpendicular slit in a piece of pasteboard that was fastened to one part of the upper basis, as that which the feather was glued to was to another part. [Figure 9 is a reproduction of Boyle's pictures of this apparatus, the details of the arrangement of the feather being shown separately. Turning the key raised the top of the bellows; the lead weight caused it to fall when desired.] These things being thus provided, the pump was set a-work; and as the ambient air was from time to time withdrawn, so the air in the bladder expanded itself so strongly, as to lift up the metalline weight, and yet in part to sally out at the little glass-pipe of our bellows, as appeared by its blowing up the feather and keeping it suspended till the spring of the air in the bladder was too far weakened to continue to do as it had done. In the meantime we did now and then, by the help of a string fastened to the turning-key and the upper basis of the bellows, let down that basis a little, to observe how upon its sinking the blast against the feather would decrease as the receiver was further and further exhausted: and when we judged it to be sufficiently freed from air, we then let down the weight, but could not perceive that by shutting of the bellows, the feather was at all blown up, as it had been wont to be, though the upper basis were more than usually depressed: and yet it seems somewhat odd, that when, for curiosity, in order to a further trial, the weight was drawn up again, as the upper basis was raised from the lower, the sides of the bladder were sensibly (though not very much) pressed, or drawn inwards. The bellows being thus opened, we let down the upper basis again, but could not perceive that any blast was produced; for though the feather that lay just over and near the orifice of the little glass pipe had some motion, yet this seemed plainly to be but a shaking and almost vibrating motion (to the right and left hand) which it was put into by the upper basis, which the string kept from a smooth and uniform descent, but not to proceed from any blast issuing out of the cavity of the bladder: and for further satisfaction we caused some air to be let into the receiver, because there was a possibility, that unawares to us the slender pipe might by some accident be choaked; but though upon the

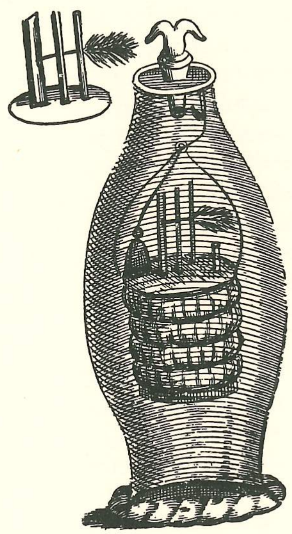

FIG. 9. Boyle's picture of the "bladder-bellows" and feather that he used in trying to find a medium more subtle than air.

return of the air into the receiver, the bases of the bellows were pressed closer together, yet it seemed, that, according to our expectation, some little air got through the pipe into the cavity of the bladder: for when we began to withdraw again the air we had let into the receiver, the bladder began to swell again, and upon our letting down the weight, to blow up and keep up the feather, as had been done before the receiver had been so well exhausted. What conjecture the opening and shutting of our little bellows, more than once or twice, without producing any blast sensible by the raising of the feather, gave some of the by-standers, may be easily guessed by the preamble of this experiment; but whilst I was endeavouring to prosecute it for my own farther information, a mischance that befel the instrument kept me from giving myself the desired satisfaction.

EXPERIMENT 39

About a further attempt to prosecute the inquiry proposed in the foregoing Experiment.

Considering with myself, that by the help of some contrivances not difficult, a syringe might be made to serve, as far as our present occasion required, instead of a pair of bellows; I thought it would not be improper to try a differing, and, in some regards, a better way to prosecute an attempt which seemed to me to deserve our curiosity.

I caused then to be made for the formerly mentioned syringe [mentioned in an earlier experiment], instead of its straight pipe, a crooked one, whose shorter leg was parallel to the longer; and this pipe was for greater closeness, after it was screwed on carefully, fastened with cement to the barrel; and because the brass-pipe could scarce be made small enough, we caused a short and very slender pipe of glass to be put into the orifice of the shorter leg, and diligently fastened to it with close cement: then we caused the sucker (by the help of oil, water, and moving it up and down) to be made to go as smoothly as might be, without lessening the stanchness of the syringe. After this there was fastened to the handle of the rammer a weight, made in the form of a ring or hoop, which, by reason of its figure, might be suspended from the newly mentioned handle of the rammer, and hang loose on the outside of the cylinder, and which, both by its figure and its weight, might evenly and swiftly enough depress the sucker, when that being drawn up the weight should be let go. This syringe, thus furnished, was fastened to a broad and heavy pedestal, to keep it in its vertical posture, and to hinder it from tottering, notwithstanding the weight that clogged it. And besides all these things, there was taken a feather which was about two inches long, and of which there was left at the end a piece about the breadth of a man's thumb-nail (the rest on either side of the slender stalk, if I may so call it, being stript off) to cover the hole of the slender glass-pipe of the syringe; for which purpose the other extreme of it was so fastened with cement to the lower part of the syringe (or to its pedestal) that the broad end of the feather was placed (as the other feather was in the foregoing experiment) just over the little orifice of the glass, at such a convenient distance, that when the sucker was a little (though but very little) drawn up and let go again, the weight would depress it fast enough to blow up the broad part of the feather, as high as was permitted by the resistance of the stalk (and that was a good way) the spring of which would presently restore the whole feather to its former position. [Figure 10 shows the syringe with the feather, and Fig. 11, the arrangement by which a syringe could be operated in an evacuated receiver, though in this figure the syringe is used to raise liquid from a small vessel.]

All these things being done, and the handle of the rammer being tied to the turning-key of a capped receiver, the syringe and its pedes-

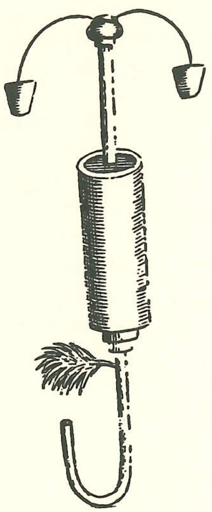

Fig. 10. Boyle's syringe with a feather, used in his further search for a more subtle medium.

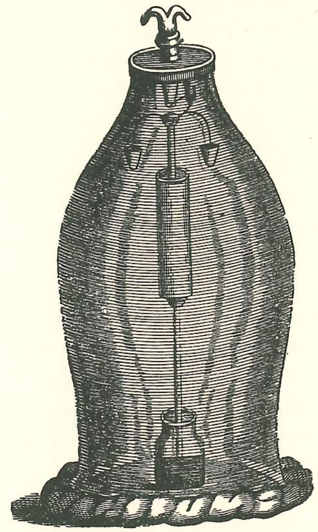

Fig. 11. The syringe of Fig. 10 mounted in a receiver and arranged to raise a liquid from a small vessel.

tal were inclosed in a capacious receiver (for none but such an one could contain them, and give scope for the rammer's motions) and the pump being set on work, we did, after some quantity of air was drawn out, raise the sucker a little by the help of the turning-key, and then turning the same key the contrary way, we suffered the weight to depress the sucker, that we might see at what rate the feather would be blown up; and finding that it was impelled forcibly enough, we caused the pumping to be so continued that a pretty many pauses were made, during each of which we raised and depressed the sucker as before, and had the opportunity to observe, that as the receiver was more and more exhausted of the air, so the feather was less and less briskly driven up, till at length, when the receiver was well emptied, the usual elevations and depressions of the sucker would not blow it up at all that I could perceive, though they were far more frequently repeated than ever before; nor was I content to look heedfully myself, but I made one, whom I had often employed about pneumatical experiments, to watch attentively, whilst I drew up and let down the sucker; but he affirmed that he could not discern the least beginning of ascension in the feather. And indeed to both of us it seemed that the little and inconsiderable motion that was sometimes (not always) to be discerned in the feather, proceeded not from anything that issued out of the pipe, but from some little shake, which it was difficult not to give the syringe and pedestal, by the raising and depressing of the sucker.

And that which made our phænomenon the more considerable was, that the weight that carried down the sucker being still the same, and the motions of the turning-key being easy to be made equal at several times, there seemed no reason to suspect that contingencies did much (if at all) favor the success; but there happened a thing which did manifestly enough disfavor it. For I remember, that before the syringe was put into the receiver, when we were trying how the weight would depress it, and it was thought, that though the weight were conveniently shaped, yet it was a little of the least, I would not alter it, but foretold, that when the air in the cavity of the syringe (that now resisted the quickness of its descent, because so much air could not easily and nimbly get out at so small a pipe) should be exhausted with the other air of the receiver, the elevated sucker would fall down more easily, which he that was employed to manage the syringe whilst I watched the feather, affirmed himself afterwards to observe very evidently: so that when the receiver was exhausted, if there had been in the cavity of the syringe a matter as fit as air to make a wind of, the blast ought to have been greater, because the celerity that the sucker was depressed with was so.

After we had long enough tried in vain to raise the feather, I ordered some air to be let into the receiver; and though when the admitted air was but very little, the motions of the sucker had scarce, if at all, any sensible operation upon the feather, yet when the quantity of air began to be somewhat considerable, the feather began to be a little moved upwards, and so by letting in air not all at once, but more and more from time to time, and by moving the sucker up and down in the intervals of those times of admission, we had the opportunity to observe, that as the receiver had more air in it, the feather would be more briskly blown up. [This experiment was devised to test Boyle's supposition that air at low pressure, a somewhat more subtle fluid than air at atmospheric pressure, would manifest its presence by raising the feather at least a little; the result bore out Boyle's expectation.]

But not content with a single trial of an experiment of this consequence, we caused the receiver to be again exhausted, and prosecuted the trial with the like success as before, only this one circumstance that we added, for confirmation, may be fit to be here taken notice of. Having, after the receiver was exhausted, drawn up and let fall the sucker divers times ineffectually, though hitherto we had not usually raised it any higher at a time, than we could by one turn of the hand, both because we could not so conveniently raise it higher by the hand alone, and because we thought it unnecessary, since that height sufficed to make the air briskly toss up the feather; yet *ex abundanti* we now took an instrument that was pretty long, and fit so to take hold on the turning-key, that we could easily raise the sucker between two and three inches, by our estimate, at a time, and nimbly depress it again; and for all this, which would much have increased the blast, if there had been a matter fit for it in the cavity of the syringe, we could not sensibly blow up the feather till we had let a little air into the receiver.

To be able to make an estimate of the quantity of air pumped out, or let in, when the feather was strongly or faintly, or not at all raised by the fall of the sucker, we took off the receiver, and conveyed a gage into it, but though for a while we made some use of our gage, yet a mischance befalling it before the operation was quite ended, I shall forbear to add anything concerning that trial, and proceed to say something of another attempt, wherein, though I foresaw and met with such difficulties, as kept me from doing altogether what I desired, yet the success being almost as good as could be expected, I shall venture to acquaint your lordship with the trial, which was this.

At this point Boyle describes an experiment in which the exit tube from his syringe was so arranged that any effluent would bubble through water. He found that in his exhausted receiver a syringe worked up and down gave no evidence that any bubbles could be forced through the liquid. He then continues as follows.

I had indeed thoughts of prosecuting the inquiry by dropping from the top of the exhausted receiver light bodies conveniently shaped, to be turned around or otherwise put out of their simplest motion of descent, if they met with any resistance in their fall; and by making such bodies move horizontally and otherwise in the receiver, as would probably discover whether they were assisted by the medium. And other contrivances and ways I had in my thoughts, whereby to prosecute our enquiry; but wanting time for other experiments, I could not spare so much as was necessary to exhaust large receivers so diligently as such nice trials would exact; and therefore I resolved to desist till I had more leisure than I then had, or have since been master of.

In the interim, thus much we seem to have already discovered by our past trials, that if when our vessels are very diligently freed from air, they are full of æther, that æther is such a body as will not be made sensibly to move a light feather by such an impulse as would make the air manifestly move it, not only whilst it is no thinner than common air, but when it is very highly rarefied (which, if I mistake not, it was in our experiment so much, as to be brought to take up above an hundred times more room than before). . . .

EXPERIMENT 40

About the falling, in the exhausted receiver, of a light body, fitted to have its motion visibly varied by a small resistance of the air.

Partly to try, whether in the space deserted by the air, drawn out of our receivers, there would be any thing more fit to resist the motion of other light bodies through it, than in the former experiment we found it to impel them into motion; and partly for another purpose to be mentioned by and by, we made the following trials.

We took a receiver, which, though less tall than we would have had, was the longest we could procure; and that we might be able, not so properly to let down as to let fall a body in it, we so fastened a small pair of tobacco-tongs to the inside of the receiver's brass-cover, that by moving the turning-key we might, by a string tied to one part of them, open the tongs, which else their own spring would keep shut. This being done, the next thing was to provide a body which would not fall down like a stone, or another dead weight through the air, but would, in the manner of its descent, shew, that its motion was somewhat resisted by the air; wherefore, that we might have a body that would be turned about horizontally, as it were, in its fall, we thought fit to join crosswise four broad and light feathers (each about an inch long) at their quills with a little cement, into which we also stuck perpendicularly a small label of paper, about an 8th of an inch in breadth, and somewhat more in height, by which the tongs might take hold of our light instrument without touching the cement, which else might stick to them. [Figure 12 is a reproduction of Boyle's drawing of this apparatus.]

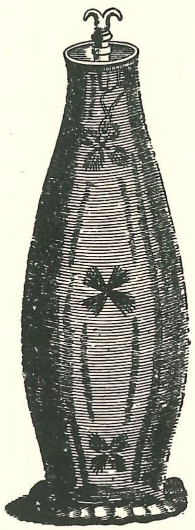

FIG. 12. Boyle's arrangement for allowing a feather cross to fall in an evacuated receiver.

By the help of this small piece of paper the little instrument, of which it made a part, was so taken hold of by the tongs, that it hung as horizontal as such a thing could well be placed; and then the receiver being cemented on to the engine, the pump was diligently plied, till it appeared by a gage [here Boyle begins to use a gage, and to good purpose; he had previously met misfortune with this device (p. 46)] which had been conveyed in. that the receiver had been carefully exhausted; lastly, our eyes being attentively fixed upon the connected feathers, the tongs were by the help of the turning-key opened, and the little instrument let fall, which, though in the air it had made some turns in its descent from the same height which it now fell from, yet now it descended like a dead weight, without being perceived by any of us to make so much as one turn, or a part of it: notwithstanding which I did, for greater security, cause the receiver to be taken off and put on again, after the feathers were taken hold of by the tongs; whence being let fall in the receiver unexhausted, they made some turns in their descent, as they also did being a second time let fall after the same manner.

But when after this, the feathers being placed as before, we repeated the experiment by carefully pumping out the air, neither I nor any of the by-standers could perceive any thing of turning in the descent of the feathers; and yet for further security we let them fall twice more in the unexhausted receiver, and found them to turn in falling as before; whereas when we did a third time let them fall in the well exhausted receiver, they fell after the same manner as they had done formerly, when the air, that would by its resistance have turned them around, was removed out of their way.

N.B. 1. Though, as I intimated above, the glass wherein this experiment was made, were nothing near so tall as I would have had it, yet it was taller than any of our ordinary receivers, it being in height about 22 inches.

2. One that had more leisure and conveniency might have made a more commodious instrument than that we made use of; for being accidentally visited by that sagacious mathematician Dr. *Wren*, and speaking to him of this matter, he was pleased with great dexterity as well as readiness to make me a little instrument of paper, on which, when it was let fall, the resistance of the air had so manifest an operation, that I should have made use of it in our experiment, had it not been casually lost when the ingenious maker was gone out of these parts.

3. Though I have but briefly related our having so ordered the matter that we could conveniently let fall a body in the receiver when very well exhausted; yet, to contrive and put in practice what was necessary to perform this, was not so very easy, and it would be difficult to describe it circumstantially without very many words; for which reason I forbear an account that would prove too tedious to us both. . . .

ANNOTATIONS

1. But here I must be so sincere as to inform your lordship, that this fortieth experiment seemed not to prove so much as did the foregoing made with the syringe; for being suspicious, that, to make the feathered body above mentioned turn in its fall, there would need a resistance not altogether inconsiderable, I caused the experiment to be repeated, when the receiver was, by our estimate, little or nothing more than half exhausted, and yet the remaining air was too far rarefied to make the falling body manifestly turn.

The Annotation of the experiment just described shows Boyle in his tedious vein. By his own admission the experiment with the falling feathers is of little value, certainly of less value than the preceding experiments designed for essentially the same purpose. "Then why not omit the long description and merely summarize the result?" an impatient modern reader may be inclined to exclaim. The essence of reporting experimental results is to record in detail only those experiments that because of their outcome seem to have real significance (the results may be positive or negative). While admiring Boyle's candor and his determination to report all the details, which set the standard for subsequent investigations, one must admit that unless a greater degree of selection had been made by later experimenters the literature of science would have become impossibly burdened with irrelevant details of inconclusive inquiries. Many generations of experimentalists have gradually evolved an unwritten code that governs the way in which experiments are now reported. The essence of this code is accurate and complete recording of those experiments that the experimenter himself believes to be significant; inconclusive and incomplete experiments need not be reported or indeed even mentioned. But in the seventeenth century the danger was that too little would be reported rather than too much.

5. THE DISCOVERY OF BOYLE'S LAW

As has been noted, the first edition of Robert Boyle's book on *New Experiments Physico-Mechanical Touching the Spring of the Air* was published in 1660. Not long after, two books appeared in which the authors vigorously attacked both Boyle's experiments and his interpretations. One was by the famous writer on political philosophy, Thomas Hobbes (1588–1679), the other by an obscure supporter of the Aristotelian position (as interpreted by scholars of the Middle Ages) by the name of Franciscus Linus (1595–1675). Hobbes's position was that of a Plenist (see p. 26) and his arguments were based in part on a misunderstanding of Boyle's views and in part on the premise that a subtle matter existed which filled all the space. The experiments recorded in the preceding section deal with Boyle's attempts to obtain evidence for the existence of the "subtle matter" postulated by the Plenists. Linus's objections were directed against the whole conceptual scheme developed by Torricelli and Pascal, to which Boyle had made few additions. Probably similar objections had been expressed more than once before in the decade or more in which the news of Torricelli's experiment had spread and the new ideas were being discussed.

Linus put forward the hypothesis that the space above the mercury column in a Torricellian tube contained an invisible membrane or cord which he called a *funiculus*. The nature of this membrane was such that the maximum height to which it could draw up a column of mercury was about 29½ inches. In support of this fantastic notion Linus cited the well-known fact that if the upper end of the Torricellian tube is closed with a finger one seems to feel the flesh being pulled in. This way of performing the Torricellian experiment with a tube open at both ends Linus described as follows (as quoted by Boyle):

> If you take a tube open at both ends of a good length, suppose forty inches long, and fill it with mercury, and place your finger on the top as before, taking away your lower finger, you will find the mercury to descend even to its wonted station [i.e., to a height of approximately 29½ inches], and your finger on the top to be strongly drawn within the tube, and to stick close unto it. Whence again it is evidently concluded that the mercury placed in its own station is not there upheld by the external air, but suspended by a certain internal cord [Linus's alleged *funiculus*], whose upper end being fastened to the finger, draws and fastens it after this manner into the tube.

Boyle replied to this and similar arguments that the pressure of the outside air forced the flesh of one's finger into the top of a barometric tube; there was no need to assume that an invisible funiculus was pulling the finger down. But Boyle was always anxious to answer arguments by experiments. So he devised a new experiment, the results of which could not be explained by the aid of his adversary's hypothesis of a funiculus. In the course of this experiment Boyle noted the numerical relation between pressure and volume that we now call Boyle's Law. The discovery of an important physical law was in this instance rather in the nature of a by-product of Boyle's desire to bring overwhelming evidence to bear against a rival conceptual scheme. Controversy has often been of great importance in stimulating new advances in experimental science.

Boyle's Description of his Discovery of the Relation Between Pressure and Volume. Boyle published the results of the new experiment, as well as lengthy arguments against both Hobbes and Linus, as an Appendix to the second edition of his book. This appeared in 1662. In Part II of this Appendix, entitled "Wherein the Adversaries Funicular Hypothesis is Examined," Boyle refers to Linus and his funiculus hypothesis in these words:

> The other thing, that I would have considered touching our adversary's hypothesis is, that it is needless. For whereas he denies not, that the air has some weight and spring, but affirms, that it is very insufficient to perform such great matters as the counterpoising of a mercurial cylinder of 29 inches, as we teach that it may; we shall now endeavour to manifest by experiments purposely made [note this phrase], that the spring of the air is capable of doing far more than it is necessary for us to ascribe to it, to salve [14] the phænomena of the Torricellian experiment.

Boyle at this point proceeds to describe how he prepared a J tube of glass with the short leg sealed off and the long leg open (Fig. 13). He kept pouring mercury into the open end until the difference in levels of the mercury in the two legs was 29 inches. This meant to him, as

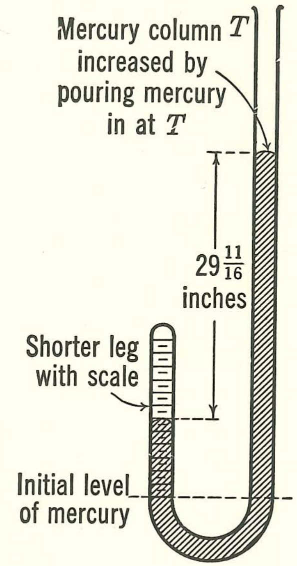

Fig. 13. Boyle's J tube, with a scale on the shorter leg reading 24 as compared to the initial reading of 48 (see Table 1, Column *A*).

it does to us, that the total pressure on the enclosed air in the short leg (see Fig. 13) was about twice the usual atmospheric pressure, and Boyle noted "not without delight and satisfaction" that the volume of the compressed air was reduced about half. But before proceeding to discuss the numerical relation between pressure and volume that he obtained in these experiments, we may be permitted to skip to the conclusions that he drew from these experiments in regard to the crucial point, namely, the existence or nonexistence of Linus's funiculus.

[14] The word "solve" has been changed to "salve" (a form of "save") in agreement with the first edition (1660) of Boyle's book.

When the volume of the compressed air in the short arm was reduced to one fourth of the original volume, Boyle noted that the difference in levels of the mercury was a little over 88 inches. When to this is added the pressure of the air in terms of inches of mercury, we can readily see, as Boyle did, that the total pressure was $88 + 29 = 117$ inches-of-mercury as compared with 29 and a fraction, the original pressure. This is within an inch of being equal to four times the original pressure (116 inches-of-mercury). Boyle then concludes:

> It is evident, that as common air, when reduced to half its wonted extent, obtained near about twice as forcible a spring as it had before; so this thus comprest air being further thrust into half this narrow room, obtained thereby a spring about as strong again as that it last had, and consequently four times as strong as that of the common air. And there is no cause to doubt, that if we had been here furnished with a greater quantity of quicksilver and a very strong tube, we might, by a further compression of the included air, have made it counterbalance the pressure of a far taller and heavier cylinder of mercury. For no man perhaps yet knows, how near to an infinite compression the air may be capable of, if the compressing force be competently increased. [We now know that at room temperature some gases are converted to liquids if sufficiently compressed, others like those composing the atmosphere are not; for all gases, however, there is a temperature below which sufficient compression will cause liquefaction.] So that here our adversary [Linus] may plainly see, that the spring of the air, which he makes so light of, may not only be able to resist the weight of 29 inches, but in some cases of above a hundred inches [that is, including the atmospheric pressure] of quicksilver, and that without the assistance of his Funiculus, which in our present case has nothing to do. And to let you see, that we did not (as a little above) inconsiderately mention the weight of the incumbent atmospherical cylinder as a part of the weight resisted by the imprisoned air, we will here annex, that we took care, when the mercurial cylinder in the longer leg of the pipe was about an hundred inches high, to cause one to suck at the open orifice [*T*, Fig. 13]; whereupon (as we expected) the mercury in the tube did notably ascend. Which considerable phænomenon cannot be ascribed to our examiner's Funiculus, since by his own confession that cannot pull up the mercury, if the mercurial cylinder be above 29 or 30 inches of mercury.

Here then is the point of the experiment. Linus, to explain the height of the mercury column in the Torricellian experiment, had to postulate a maximum pull of the funiculus corresponding to only 29 inches of mercury. Yet by combining the force of expansion of the compressed air in the short arm of his apparatus with the partial evacuation of the air above the long arm (by means of sucking with his mouth) Boyle is able to pull up a column of mercury whose length is several times

29 inches. If a funiculus is involved in this (as Linus postulated), how can it pull up such a long column, Boyle asks. His answer follows.

> And therefore we shall render this reason of it, that the pressure of the incumbent air being in part taken off by its expanding itself into the sucker's dilated chest; the imprisoned air was thereby enabled to dilate itself manifestly, and repel the mercury, that compress it, till there was an equality of force betwixt the strong spring of that comprest air on the one part, and the tall mercurial cylinder, together with the contiguous dilated air, on the other part.

Numerical Relation of Pressure and Volume. Boyle's method of measuring volume was crude; it consisted of measuring the distance between the top of the sealed off shorter leg and the mercury level in the same leg. Clearly this measurement of a distance is a true measure of the volume only if the tube is of uniform diameter, which would be true only approximately. A paper scale divided into inches and fractions was pasted on the outside of the shorter leg and a similar but longer scale on the outside of the longer leg. The position of the two mercury levels could then be noted and the difference in pressure recorded. The results were given by Boyle in a table which is reproduced in Table 1.

TABLE 1. Compression of air (Boyle's original data).

A	*B*	*C*	*D*	*E*
48	00	Added to 29 1/8 makes	29 2/16	29 2/16
46	01 7/16		30 9/16	30 6/16
44	02 13/16		31 15/16	31 12/16
42	04 6/16		33 8/16	33 1/7
40	06 3/16		35 5/16	35
38	07 14/16		37	36 15/19
36	10 2/16		39 5/16	38 7/8
34	12 8/16		41 10/16	41 2/17
32	15 1/16		44 3/16	43 11/16
30	17 15/16		47 1/16	46 3/5
28	21 3/16		50 5/16	50
26	25 3/16		54 5/16	53 10/13
24	29 11/16		58 13/16	58 2/8
23	32 3/16		61 5/16	60 18/23
22	34 15/16		64 1/16	63 6/11
21	37 15/16		67 1/16	66 4/7
20	41 9/16		70 11/16	70
19	45		74 2/16	73 11/19
18	48 12/16		77 14/16	77 2/3
17	53 11/16		82 12/16	82 4/17
16	58 2/16		87 14/16	87 3/8
15	63 15/16		93 1/16	93 1/5
14	71 5/16		100 7/16	99 6/7
13	78 11/16		107 13/16	107 7/13
12	88 7/16		117 9/16	116 4/8

A. The number of equal spaces in the shorter leg, that contained the same parcel of air diversely extended.

B. The height of the mercurial cylinder in the longer leg, that compressed the air into those dimensions.

C. The height of the mercurial cylinder that counterbalanced the pressure of the atmosphere.

D. The aggregate of the two last columns *B* and *C*, exhibiting the pressure sustained by the included air.

E. What that pressure should be according to the hypothesis, that supposes the pressures and expansions to be in reciprocal proportion.

As to the origin of the hypothesis that pressure and volume are reciprocally related, Boyle has this to say:

> I shall readily acknowledge, that I had not reduced the trials I had made about measuring the expansion of the air to any certain hypothesis, when that ingenious gentleman Mr. *Richard Townley* was pleased to inform me, that having by the perusal of my physico-mechanical 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 of itself loses of its elastical force, according to the measures of its dilatation. [Boyle in his book had tried without success to reduce to numerical terms the effectiveness of his engine in terms of the ratio of the volumes of the cylinder and the receiver. One of his readers seems to have grasped the point that probably pressure and volume were inversely proportional to each other.] 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 shew 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 an 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 took notice of in the former chapter, as having written something about rarefaction [this appears to refer to Hooke]: 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 (and not long after the publication of my pneumatical treatise) made observations to the same purpose, which he acknowledged to agree well enough with Mr. *Townley's* theory: and so did (as their author was pleased to tell me) some trials made about the same time by that noble virtuoso and eminent mathematician the Lord *Brouncker*, from whose further enquiries into this matter, if his occasions will allow him to make them, the curious may well hope for something very accurate.

It is interesting that at least three of Boyle's contemporaries suggested the relation that we now know as Boyle's law as a result of reading about Boyle's difficulties in calculating the effectiveness of his engine. Boyle obviously became so interested in the numerical relation between pressure and volume that his initial objective, namely, to raise a column of mercury more than 29 inches by suction, is rather lost sight of. Certainly in the presentation of all this material, his adversary's point of view is brought in rather casually, though he does finally conclude with the statement,

> I suppose we have already said enough to shew what was intended: namely, that to salve the phænomena there is not of our adversary's hypothesis [i.e., the funiculus] any need: the evincing of which will appear to be of no small moment in our present controversy to him that considers, that the two main things, that induced the learned examiner to reject our hypothesis, are, that nature abhors a vacuum; and that though the air have some weight and spring, yet, these are insufficient to make out the known phænomena; for which we must therefore have recourse to his Funiculus. Now as we have formerly seen, that he has not so satisfactorily disproved as resolutely rejected a vacuum, so we have now manifested, that the spring of the air may suffice to perform greater things than what our explication of the Torricellian experiments and those of our engine obliges us to ascribe to it. Wherefore since besides the several difficulties, that incumber the hypothesis we oppose, and especially its being scarce, if at all, intelligible, we can add that it is unnecessary; we dare expect, that such readers as are not biassed by their reverence for *Aristotle*, or the Peripatetick schools, will hardly reject an hypothesis, which, besides that it is very intelligible, is now proved to be sufficient, only to imbrace a doctrine, that supposes such a rarefaction and condensation, as many famous Naturalists rejected for its not being comprehensible, even when they knew of no other way (that was probable) of salving the phænomena wont to be explicated by it.

In this same chapter Boyle describes some experiments on what he calls the "debilitated force of expanded air." This amounts to another way of measuring the relation between volume and pressure. In this case the original sample of air is not compressed but expanded by diminishing the pressure. This Boyle accomplished very simply by enclosing a sample of air in a long thin tube closed at the upper end and immersed for several feet in a long tube of mercury. When the inner tube is *raised,* the contained air is allowed to expand, a suitable scale serving to measure the change in volume (assuming uniform bore) and the diminished pressure. (This experiment can be conveniently performed today by using an inverted glass burette for the inner tube and a tall glass cylinder to contain the mercury.)

Boyle makes no explicit statement about the effect of temperature on the accuracy of his results. He was quite aware, however, of the fact that air expands on heating and contracts on cooling. He was curious to see whether the air compressed to a quarter of its volume behaved in this respect like air under atmospheric pressure. He therefore warmed the short leg of his bent tube with a candle and cooled it with water, noting in qualitative terms the changes of volume that occurred.

They were not large. This fact must have assured Boyle that the minor fluctuations in the room temperature during the experiment in which he varied the height of the column of mercury would not affect the significance of his results. We now know that the pressure of a given volume of gas increases by about $\frac{1}{530}$ of its value at room temperature for every (Fahrenheit) degree increase in temperature. Therefore, an increase of as much as five degrees during Boyle's experiment would have introduced an error of only about $\frac{5}{530}$ or a little less than $\frac{1}{100}$, which is about the difference between the observed and calculated pressures (Columns *D* and *E*, Table 1) in the extreme case.

Today, no one would think of measuring the relation between pressure and volume of a gas with any pretense to accuracy unless the temperature were controlled. Careful experiments have shown that even at constant temperature Boyle's law,

$$\text{Pressure} \times \text{Volume} = \text{constant},$$

or

$$\frac{P_1}{P_2} = \frac{V_2}{V_1},$$

is only approximately true for gases at atmospheric pressure. The deviations from Boyle's law (the change in the product of volume and pressure as pressure increases) are greater the greater is the pressure for a given gas; the gases that are not far from the point of condensation deviate widely from Boyle's law.

An interesting comparison can be made between Boyle's law and the relation between pressure in a liquid and depth below the surface. The first is based on experimental findings and is only approximate; the second *appears* to be a consequence of definitions and was presented in the sixteenth century as a deduction from self-evident propositions in a manner reminiscent of geometry. The hydrostatic principle here involved may be expressed by a "law" in the form P (pressure in the liquid) $= D$ (depth) $+ A$ (atmospheric pressure), if appropriate units are taken. On analysis, it becomes evident that this is true only if the change of density of the liquid with pressure can be neglected (it can be for all practical purposes for considerable depths of water). Constant temperature throughout the liquid must be assumed (just as Boyle's law is true only for constant temperature). The deviations from Boyle's law decrease with decreasing pressure, and for very attenuated gases the observed relations between pressure and volume follow Boyle's law closely. To the extent that the hydrostatic law defines an ideal liquid, it may be comparable with Boyle's law as a definition of an ideal gas.

Boyle's formulation of the relation between two variables, volume

and pressure of a gas, is typical of a vast amount of scientific information that began to be accumulated about the middle of the seventeenth century. In the case at hand, this information was obtained incidentally to a controversy about the Torricellian conceptual scheme as extended by Boyle. As scientific experimentation continued, however, the aim of the investigator was often more directly to obtain quantitative data about the relation between two variables, one of which was said to be a function of the other. The realization of what were the significant variables was usually closely connected with the development of new concepts or conceptual schemes (theories). But the experimental difficulties of controlling or measuring the other variables were often considerable. The recognition of the variable factors in a changing situation and the development of methods of measuring these factors often constitute a major advance in science.

The concept that air was compressible did not originate with Boyle. Torricelli, in his letter of June 28, 1644 describing his first experiments, in explaining what he means by the pressure of the atmosphere uses the analogy of a cylinder of wool (easily compressible by hand), and there had been earlier discussions of the compressibility of air. On the other hand, Pascal, in his treatise written in 1648 (published in 1663), makes little reference to the vast difference in compressibility between water and air. He makes use of the analogy between hydrostatic pressure and air pressure but treats air for the most part as though it were water of very low density. One can say that Boyle extended Torricelli's conceptual scheme by emphasizing the "spring of the air." He was led to do so because the operation of von Guericke's pump depends on the fact that air expands and contracts with changing pressure almost instantaneously and to a very large extent; one can "feel" the spring of the air when one operates an air pump!

Boyle noted in his first book that there were at least two ways of imagining the composition of air to account for its great compressibility. One was to think of the particles as each being compressible like a spring or a bit of wool. One can conceive of the air, wrote Boyle, as "a heap of little bodies, lying one upon another, as may be resembled to a fleece of wool." The other way was to think of the particles as whirled around in the subtle fluid postulated by Descartes as filling all space. According to this latter view, Boyle said, "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."

Boyle declares he is not willing "to declare peremptorily for either of them [i.e., the ideas] against the other." He goes on to say, "I shall decline meddling with a subject, which is much more hard to be explicated than necessary to be so by him, whose business it is not, in this letter, to assign the adequate cause of the spring of the air, but only to manifest, that the air hath a spring, and to relate some of its effects."

Boyle was an adherent of what is sometimes called the corpuscular philosophy — a point of view that derives from one branch of ancient Greek thought. One could speculate and argue whether matter could be divided and subdivided indefinitely or whether there were ultimate particles, often called atoms. Either of the "explanations" put forward by Boyle for the "spring of the air" was in harmony with the atomistic idea of the structure of matter. Though reserving the right in his first account of the subject to experiment later to test the alternative concepts, he seems to have done so only indirectly by searching for Descartes' subtle fluid. When speculation about the nature of gases became important for the advance of chemistry at the end of the eighteenth century, the picture of a gas then in favor was that of contiguous but easily compressible particles filling the space. This atomic picture was still a speculative idea, however, hardly a working hypothesis, until Dalton used it to relate the constant ratio by weights of elements in compounds. The distinction between a general speculative idea, a working hypothesis on a grand scale, and a new conceptual scheme is well illustrated by comparing the history, in the seventeenth century, of the notion of matter being composed of atoms with that of the idea of a sea of air surrounding the earth. The first remained a speculative idea throughout the period; the second soon emerged as a new conceptual scheme which by 1700 was almost universally accepted. It was not until Dalton in 1805 put forward his "atomic theory" (certainly at first only a working hypothesis on a grand scale) that from the general speculative idea one could draw deductions that could be tested by experiment (Case 4 of this series).

At what point in history the conceptual scheme about air and air pressure became a "fact" and whether or not the atomic nature of matter is now a "fact" can be left for the reader to debate. If one adopts a cautious attitude about science, one will reserve the use of the word "fact" to designate reproducible observations (at least when one is attempting to speak carefully about science). The word "theory" is commonly used to mean either a working hypothesis or a well-accepted conceptual scheme. Because of the resulting ambiguities, we prefer to use the phrases "working hypothesis on a grand scale" or "broad working hypothesis" for a new idea in its initial phases. As soon as the deductions from such a hypothesis have been confirmed by experimental test and the hypothesis is accepted by several scientists, it is convenient to speak of it as a conceptual scheme. In a cautious mood

one retains the phrase no matter how certain one may feel about the postulates. The reader need hardly be reminded that in 1950 the ideas about the structure of the nucleus of the atom are in a state where working hypotheses on a grand scale are in the process of becoming conceptual schemes (or if one must use the word, new theories!).

6. NOTES ON THE DEVELOPMENT OF SCIENCE IN THE MIDDLE OF THE SEVENTEENTH CENTURY

The Two Traditions. As was pointed out in the Foreword, the development of experimental science in the seventeenth century was the consequence of the combination of deductive reasoning with the cut-and-try type of experimentation. Two great figures of this period who contributed to the study of pneumatics symbolize the two traditions whose combination produced modern science. Blaise Pascal was primarily a mathematician, Robert Boyle primarily an experimentalist. A study of their writings illustrates the two streams of thought and action that were found in the seventeenth century. In Pascal's treatise on hydrostatics and his work on pneumatics it is hard to tell whether or not most of the so-called experiments were ever performed. They may well have been intended rather as pedagogic devices — as demonstrations that the reader performs in his imagination in order better to understand the principles expounded. The argument is by deductive reasoning from a few postulates; here and there a check by actual experiment, as the case of the Puy-de-Dôme observations, may be of great significance. Yet it will be recalled that one may question whether Pascal's collaborator, Perier, was an accurate reporter; it seems probable that, convinced of the general effect he was looking for, namely, the change of barometric reading with height, he could not resist the temptation to give results with such an accuracy as to carry conviction to those steeped in the tradition of mathematical reasoning (see pp. 8 and 28).

Boyle in one of his discussions of hydrostatics (1666) gently pokes fun at Pascal for having written of experiments that appeared impossible of execution. Pascal, said Boyle, does not state that he actually tried the experiments; "he might possibly have set them down as things that *must* happen, upon a just confidence that he was not mistaken in his ratiocinations." He further chides Pascal for not giving sufficient details that anyone can repeat his experiments (if he ever really did them). Boyle was on solid ground here, for, as we have seen, he was if anything overconscientious in recording all possible information that might be of assistance to another investigator. As an example of some of the things Pascal described that strained one's credulity, Boyle refers to an experiment in which a man sits 20 feet under water and places against his thigh a tube that extends above the surface of the water. But, writes Boyle, Pascal "neither teaches us how a man shall be enabled to continue under water, nor how, in a great cistern full of water 20 feet deep, the experimenter shall be able to discern the alterations. . ."

One can trace through the history of physics and chemistry the two traditions represented by Pascal and Boyle, though sometimes both appear to be almost equally represented in the work of a single man, as in the cases of Galileo, Newton, and perhaps Lavoisier. In the twentieth century one thinks of the names of Einstein and Lord Rutherford, one a mathematician, the other an experimentalist, each representing by his revolutionary work the best in the two approaches that together made modern science.

Science and the Practical Arts. The study of pneumatics may have started as the result of an interest by a professor in the practical art of pumping water. Galileo,[15] in his *Dialogues Concerning Two New Sciences*, published in 1638, places in the scientific record for the first time what must have been a well-known fact to those who built and operated pumps, namely, that water will not rise in a lift pump above about 34 feet. One of the characters in the dialogues says, "I once saw a cistern which had been provided with a pump under the mistaken impression that the water might thus be drawn with less effort or in greater quantity than by means of the ordinary bucket. . . This pump worked perfectly so long as the water in the cistern stood above a certain level; but below this level the pump failed to work. When I first noticed this phenomenon I thought the machine was out of order; but the workman whom I called in to repair it told me the defect was not in the pump but in the water, which had fallen too low to be raised through such a height; *and he added that it was not possible, either by a pump or by any other machine working on the principle of attraction, to lift water a hair's breadth above eighteen cubits.*"

The words that we have italicized convey important historical information. It seems quite clear that Galileo's interest in a scientific problem had arisen from the observation of a practical art, namely, pumping water; furthermore, it seems evident that the knowledge about the limitations of a lift pump was common among the workmen. Indeed, illustrations from books of the sixteenth century show tandem pumps (one above the other) lifting water from mines. It is interesting that while Galileo himself made little or no contribution to the solution of the scientific problem of relating the limitation of a water pump to other phenomena, his pupil Torricelli did.

[15] Galileo Galilei (1564–1642), regarded by many as the real founder of modern science; certainly the greatest single figure in physical science after Archimedes and before Newton; a professor at the universities of Pisa and Padua and later resident at the court of the Grand Duke of Florence.

Although pneumatics in origin was thus closely related to a practical art (and pumps with their various parts — valves, cylinders, plungers — recur throughout the story), the advance in science did not change the art of pumping water — at least not in the seventeenth century. There is no evidence, indeed, that any of Boyle's work had immediate consequences of practical value. Even in scientific work his devices for collecting gases *in vacuo* and transferring them under pressure to an evacuated vessel were scarcely employed by chemists until the twentieth century. An alternative procedure — the use of the pneumatic trough — which was invented somewhat later than Boyle's time, was found to be preferable. A study of the work of the "pneumatic chemists" of the late eighteenth century (Case 2 of this series) shows how little was the influence of the techniques for handling gases worked out by Boyle and Papin.

The failure of the scientific world to use vacuum pumps in the eighteenth century is readily explained by anyone who has had experience with evacuated systems. Boyle's pumps were expensive and difficult to operate, and before glass blowing and metal working had reached a high state of development, it was almost impossible to insure against leaks. It was only in the second half of the nineteenth century that distillations at pressures of an inch of mercury came into common practice. The development of the incandescent light which originally required a high vacuum (pressure of 10^{-3} mm-of-mercury or less) stimulated the improvement of vacuum pumps for industrial purposes. New types of pumps together with chemical procedures now make it possible to evacuate large vessels to a pressure as low as 10^{-5} mm-of-mercury; indeed, pressures as low as 10^{-10} mm-of-mercury have been reported.

Denis Papin (1647–1712) was Boyle's collaborator in his later work in pneumatics. He was the inventor of the pressure cooker (originally called Papin's digester) which has only in the middle of the twentieth century come into its own as a device useful to the housewife. This invention was closely related to Boyle's studies of the behavior of materials not only *in vacuo* but in compressed air. John Evelyn, in his famous diary under date of April 15, 1682, records with appreciation that the members of the Royal Society partook of a supper cooked in a digester. He remarks, "This philosophical supper caused much mirth amongst us and exceedingly pleased all the company." The fact that Papin later made several designs for steam engines serves to connect Boyle's work with the practical developments of the eighteenth century.

Newcomen's atmospheric engine (1712) can be thought of as a practical outcome of the work of the investigators of pneumatics in the seventeenth century. But the connection is far from direct. By the end of the seventeenth century, the Torricellian scheme was accepted as a matter of course. So, too, was the concept of air as an elastic fluid, and the connection between water and steam was beginning to be understood. Therefore, while no *direct* applications of the new discoveries in pneumatics to the practical arts can be traced in the seventeenth century, it is clear that all scientists and inventors who were concerned with gases or vapors were from Boyle's time on thinking in terms of the new concepts and Torricelli's conceptual scheme. In the seventeenth and eighteenth centuries, advances in science and progress in the practical arts by continued empirical experimentation were two parallel activities. The scientists and inventors were in communication and shared the same ideas, but it was not until the nineteenth century that the concepts of science and the accumulated scientific information became of major importance to the practical men of industry and commerce. And only in the twentieth century have the two activities — science and the practical arts — become intimately associated in almost every industrial activity.

Science and Society. The casual way in which Robert Boyle refers to the work of other investigators is worthy of special notice. So incomplete are his references that we cannot tell today how much of his work in pneumatics was original. This disorganized state of scientific communications is typical of the first half of the seventeenth century. When the new experimental philosophy was beginning to attract attention, news of scientific discoveries usually circulated by means of letters between learned men. The publication of scientific books was sporadic and often greatly delayed. The need for some regular method of recording short notices about scientific experiments led to the establishment of scientific journals in the second half of the century. The formation of scientific societies was of great significance in this connection since they sponsored scientific journals which, in the case of the *Transactions* of the Royal Society (London) and the *Mémoires* of the French Academy (Paris), have continued almost without interruption to the present day. For those students who are interested in either the political ferment in England of the seventeenth century or the connection between the development of literature and philosophic thought, a study of the founding of the Royal Society will be rewarding. The books listed below in the first section are recommended as a basis for a consideration of the interaction of science and society in this period.

SUGGESTED READING

1. *A Bibliography of the Honourable Robert Boyle,* by J. F. Fulton (Oxford University Press, Oxford, 1932).

2. Science and Society

The Life and Works of the Honourable Robert Boyle, by Louis Trenchard More (Oxford University Press, New York, 1944). The first 113 pages are recommended; with the author's evaluation of Robert Boyle's work in chemistry the editor of this Case cannot agree.

Scientists and Amateurs: A History of the Royal Society, by Dorothy Stimson (H. Schuman, New York, 1948).

The Role of Scientific Societies in the Seventeenth Century, by Martha Ornstein (University of Chicago Press, Chicago, ed. 3, 1938).

The Seventeenth Century Background, by Basil Willey (Columbia University Press, New York, 1942), is to be especially recommended for students interested in literature as collateral reading. The first three chapters present an interesting view of the impact of Bacon's ideas and Galileo's work.

3. Supplementary Reading to the Case History

The Physical Treatises of Pascal. The Equilibrium of Liquids and the Weight of the Mass of the Air [by Blaise Pascal], translated by I. H. B. and A. G. H. Spiers, with introduction and notes by Frederick Barry (Columbia University Press, New York, 1937).

This little volume, containing all of Pascal's "brief but brilliantly ingenious labors in natural science, together with the remarkably well-executed investigations of Perier which completed them, was put together by Perier and published at Paris in 1663, a year after Pascal's death." Since the text translated includes a summary of Boyle's early work on pneumatics, and since this Columbia edition furthermore contains translations of relevant passages from the writings of Stevin, Galileo, and Torricelli, the volume is a useful one indeed. Information about the influence of Hero of Alexandria's *Pneumatica* is given in an article by Marie Boas in *Isis,* vol. 40 (1949), p. 38.

The Science of Mechanics, by Ernst Mach (first German edition, 1883; last major revisions, 1911; English translation by T. J. McCormack, Open Court Publishing Co., Chicago, 1893; current (1942) edition considerably rearranged). For hydrostatics and pneumatics, see especially chap. i, secs. vi and vii and chap. iii, sec. x.

This historical and critical survey of mechanics has become a landmark in the development of the logical analysis of science. Although it contains a wealth of useful facts and penetrating critiques, some caution is needed in using it today. The historical research available to Mach was both limited and biased in such a way that he inevitably tended to overemphasize the contributions of certain individuals.