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**WHO DISCOVERED BOYLE'S LAW? ***

## Preface

The present essay illustrates a historiographic point and makes a historical point. The historiographic point is this: in the history of science, unlike other histories, error and redundancy tend to proliferate, perhaps due to the absence of a traditional requirement from writers to *declare their interests, state their problems, express their viewpoints, and list the difficulties they leave not yet solved to their own satisfaction.* (See [Agassi, (a)], especially final section.) My example here is an error which tallies very well with the 19th century climate of opinion, according to which Robert Boyle observed facts which his assistant Richard Townley generalized into the celebrated gas law. *This error has been criticized by Gerland in 1909 and in* 1913, *but his criticism was ignored.* Later writers have made extensive studies, culminating with those of Webster and Cohen in recent years. Yet the later extensive documentation only confuses matters because even the simplest criteria used are not openly expressed, and different writers use different criteria. The chief testimony concerning the attribution of the law to Townley is allegedly Boyle's original text. And as I shall argue in detail, it appears as if commentators, able to dig up obscure documents which are hard to decipher, have lost the ability to read a straightforward printed exposition; especially when the exposition is generally considered scientific and if an implicit assumption underlying it does not tally with the up-to-date science text-book.

Boyle claims to have formulated and tested the gas law for pressures over one atmosphere, and attributes to Townley the extrapolation of the law to lower pressures. This does not make modern sense, but in the 1650's there were even special names for these two domains. Historians of science misread Boyle's

* The present essay grew out of a brief appendix to my doctoral dissertation, University of London, 1956, unpublished. This is not a priority claim, since the main thesis of this essay goes to Ernst Gerland, 1909 and 1913 (see bibliography at the end of this essay): Boyle's law was discovered by Boyle. The essay grew out of attempts to cope with comments by editors and their referees, as well as by friends and colleagues. Some of these comments save me much embarrassment by correcting some of my worst errors; some are very clever suggestions; others were not so intelligent: those below a certain level of intelligence I simply could not cope with within the framework of this essay. Two major factors of expansion were the new contributions to the topic ─ which will be discussed below ─ and a brief section in physics proper. Now the new contributions perpetrate errors which are discussed in my original appendix. Most editors who have rejected early versions of this essay have claimed that these errors are so obvious as to be in no need of correction in the learned press. The physics included in Section 2 below, on the gas law, is included because most of the comments 1 received were based on errors in physics which I felt I had to take care of. My presentation of that chapter in physics was viewed by a learned historian of science and editor of a first rate journal, as most unsatisfactory, as it presents that chapter in physics somewhere between the historical manner of how it looked in the seventeenth century and the really up-to- date. I think he is right, but will leave my presentation as it is, since it is a simplified and serviceable version of what most physicists offer on the subject. That editor was willing to publish the historical core of this essay were I ready to omit my comments on other writers on the topic, which seemed to him merely polemic and so pointless. Some people will never see any use in the survey of past errors, as if error is so quaint a part of the human understanding that it can be left to students of oddities. I think this inability to alter one's attitude towards human error is the major target of most of my work in this area, especially [Agassi (a)].

The translations from Gerland, Rosenberger, and Heller are mine; the beginning of the Rosenberger translation, however, is from Ornstein (p. 52).

Square brackets refer to the bibliography at the end of this essay. Unspecified page numbers refer to Robert Boyle's *Works,* Volume I, unless specified otherwise A = 1st ed.; B = 2nd ed.

I am grateful to Gerd Buchdahl, Daniel A. Greenberg, Yehuda Elkana, Russel McCormmack, and C. Truesdell, for their comments on earlier versions.

acknowledgment to Townley, and they repeatedly misread the passage since they refuse to consider pressures above one atmosphere and pressures below one atmosphere significantly different: Boyle had a model of atoms of air as springs, and a spring can be compressed and dilated; Boyle's tacit (and perhaps unnoticed) assumption was that the zero point between dilation and compression obtains under the pressure of exactly one atmosphere.

But should we or should we not insist on a difference made once and abolished long since? This question can be generalized, and historians might be fascinated by the generalization. The fact is that their criteria, criteria that they employ in their studies of secondary sources, and criteria that they employ in their studies of primary sources, may and often do vary widely. The present case study may serve as an illustration.

The final discussions of this essay revolve around two recent and prominent essays on Boyle's law: one by C. Webster, who claims priority for Townley, and one by I. B. Cohen, who claims priority for Hooke. Both throw different light on different data; intelligent comparison between these and diverse primary and secondary sources is impossible except by setting the various criteria and the obvious rules of translation from one set of criteria to another.

## 1. Standards of Credit and Acknowledgment

Boyle's law is very important in the history of science for various reasons, not the least of which is that it was published in 1660, when the Royal Society came into being, and that events round it came to mold certain traditions of research practices; the most interesting factors, perhaps, were those of quick publication, of a detailed publication in the high inductive style, and of acknowledgment. All three of these factors will be discussed and illustrated below.

The classical discussion of the implementation of the inductive style may be found in Bacon, especially in his *Parasceve*.[1] A detailed discussion of both the inductive style and the requirement to publish fast, preferably in essay form, is to be found in Boyle's Proëmial Essay to his *Certain Physiological Essays* of 1661. Excerpts from this essay were used as a preface to Peter Shaw's immensely popular 18th century edition of *Boyle's Philosophical Works.* It is known that Boyle was very successful; the inductive style of writing became soon the official style of the records of the meetings of the Royal Society, and the style of the scientific writings which it sponsored in any way. It soon became the standard and is still today for many periodicals, from anthropology to zoology, obligatory for all contributors. So was Boyle's requirement to publish quickly, which, of course,

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[1] The English version of Bacon's *Parasceve,* his *Prescriptive Toward a Natural and Experimental History* is appended to Fulton H. Anderson's edition of Bacon's *Novum Organum* (New York, 1960).

R. F. Jones [b], is the only historian who has published extensive studies of the rise of the inductive style. Unfortunately he omits mention of Bacon's prescriptions, and their role in the development of the inductive style.

For the history of the inductive style see [Jones, (b)] pp. 19, 21, 33, 335 and [Agassi, (a)] pp. 93, 97, where Maxwell is quoted praising Faraday's frank speculative style but recommending Ampere's inductive style. See also [Agassi (c)], and consult Index, Art. *Style*.

was his rationale for the inductive style. The works of Van Helmont were all published posthumously, but Boyle's were almost all published as soon as the ink dried on them. Similarly, the works on the vacuum of Torricelli, Pascal, and Guericke — the most important ones prior to Boyle's — were published after Boyle's work on the elasticity of air. (See [Conant] pp. 6n and 9.) Boyle did much to encourage his associates to publish, particularly Townley, Power and Lord Brouncker whose works he mentions in his work on the elasticity of air; of these, only Power did (and so did Hooke, later on, whose work Boyle does not mention there). For, one way of encouraging them (Hooke needed no encouragement, incidentally) was to do so in print, by reference to their intentions, and by the aid of generous appreciation and, when possible, acknowledgment.

It is hard to speak of the accepted standards of crediting or acknowledgment of a discovery or an idea in the period preceding the year 1660, and for a variety of reasons.

Mediaeval and high Renaissance works have often fancy acknowledgments to ancient authorities — and Copernicus still did. Some modern authors, conspicuously Descartes, preferred to make no acknowledgments, partly from excessive anti-authoritarianism, partly from reluctance to admit dependence on others. (See [Sabra] pp. 100&n, 101&n, 102 and 115.) The very idea of crediting a person with a discovery as a token of gratitude, combined with a reward by posterity, was invented by Bacon, and presented especially forcefully at the end of his *New Atlantis,* where he made the novel suggestion that mechanical inventions should be acknowledged (just as much as, if not more than, philosophical ideas). Bacon even went so far as to suggest an institutionalized mode of crediting, a public acknowledgment by the national college or academy. And this led to the evolution of standards of both credit and acknowledgment, beginning with Robert Boyle and the Royal Society. This makes the study of the case of Boyle of particular interest.

But we will miss the point if we do not notice that before Boyle standards were loose or non-existent. Not only are most of the mediaeval inventions anonymous. And it is no accident that we do not quite know who invented the telescope, nor that it has been repeatedly alleged, ever since Bacon, that it was Galileo, or that it was not Galileo since he plagiarized it, and that he was no plagiarist since he never claimed priority and even reports his having heard about it first.

As Galileo explains, he himself does make some claim to the discovery, even though he thought it was no small achievement to make a telescope on the basis of what he had heard. Nowadays, we do not need such arguments, since in order to establish priority for an experimental discovery or invention, this must be presented to the learned world or to the patent office — depending on the purpose at hand — and in a manner clear enough to render it repeatable. It was

Boyle who instituted priority rules, and as means of prompting the advancement of learning (in accord with Bacon's proposals).[2]

There is more to this than meets the eye. The idea of quick publication involved the invention of the form of scientific essays and of scientific periodicals.[3] This form was advocated first by Boyle in his already mentioned Proëmial Essay, published in 1661 but written over a decade earlier, whose topic is the writing of scientific essays, and whose suggestions were soon incorporated into the rules of the Royal Society, proposed by its president Lord Brouncker, and seconded by Boyle.

This, however, is only the background to hosts of problems. We have legal standards of copyright and priority, there are standards of the community of scholars, and there is commonsense. The legal standards protect ownership and so forbid publication of others' ideas even with full acknowledgment; the tradition aims at incentives for quick publication and so it recognizes, not to say encourages, publications in others' names as establishing others' priority; commonsense recognizes such cases even with no regard to acknowledgment, since the law does not claim to make full restitution; tradition is vague and varied on such points; commonsense may then take over.

Before applying these fine points to Boyle's case, let us consider instances. As we say, all this does not apply to the backward-looking Copernicus. Kepler was declared over-generous: he acknowledged to Maestlin the discovery of *lumière cendrée,* of the ashen-like light of the new moon which he announced; the discovery was attributed to Kepler himself nonetheless, and despite his acknowledgment ([Laplace] p. 343). It 1s alleged ([Singer] p. 189) that Kepler privately complained that in Galileo's *Starry Messenger* an acknowledgment to Bruno and to Kepler himself is missing. Now we know enough of Kepler's generosity to doubt that he could so complain; nevertheless, it is quite likely that

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[2] 'For Galileo on the telescope see his *Opera,* (Favaro edition), 3:60 *(Sidereus);* 6:258 *(Saggiatore); Bacon's view is expressed in his New Atlantis* and *Parasceve;* Boyle's in his "Proemial Essay" and elsewhere; his proposal was translated into a rule proposed by the president of the Royal Society, Lord Brouncker, and seconded by Boyle and adopted early in the day. I have discussed all this in detail in my unpublished doctoral dissertation (University of London, 1956).

A curious case of a priority claim published as a patent application rather than as a paper in a learned periodical is Edison's invention of the diode-tube. See Matthew Josephson's life of Edison.

[3] It is amazing that historians take for granted the existence of both scientific essays and scientific publications; see e.g. [Kuhn] p. 20:

> Given a textbook, however, the creative scientist can begin his research where it leaves off and thus concentrate upon the subtlest and most esoteric aspects of the natural phenomena that concerns his group. And as he does this, his research communiques will begin to change in ways whose evolution has been too little studied but whose modern end products are obvious to all and oppressive to many. No longer will his researches be embodied in books addressed … to anyone who might be interested in the subject matter of the field. Instead, they will usually appear as brief articles addressed only to professional colleagues, the men whose knowledge of a [shared paradigm] i.e. textbook can be assumed and who prove to be the only ones able to read the papers addressed to them.

Kuhn, too, takes all this for granted; only he wishes to have the evolution of periodicals studied.

he did think Galileo had missed an occasion to express indebtedness. This may be unpleasant, but it is not a case of violation of accepted standards: accepted standards, inasmuch as one could at all discuss them, were hopeless. Galileo even made his own standards as he went along: he was engaged in a controversy about priority concerning the discovery of the sunspots and in the course of the debate he stated that priority must go to the one who adequately explained the discovered phenomenon, namely to Galileo himself. This idea is quite unacceptable, since many discoveries remain unexplained for generations; nor do we think Galileo himself understood the nature of sunspots. (Perhaps Galileo himself saw the weakness of his claim: later in life he offered a somewhat different idea ─ but only in a private letter.) Yet if we reject Galileo's rule we must own that others had seen it before Galileo and his competitor; Kepler, for instance, who so misinterpreted one of them (as transit) that he ignored it. [4]

One last example: Lavoisier considered himself a co-discoverer of oxygen, especially since Priestley never thought of it as causing combustion but merely as sustaining it. Undoubtedly Lavoisier's acknowledgment was grudging, and under pressure; was he a plagiarist? I think not, yet I agree that this is an open and difficult question. It is perhaps an unpleasant one, but also an interesting one ([Kuhn] pp. 7, 53-61, and 117).

An even more complex case could be that of Newton's possible indebtedness to Hooke for the conception of the inverse square law and its possible generation of Keplerian ellipses. Hooke asked Newton a question which Hooke could not answer, but Newton could and did. Supposing the question was new to Newton; did he have to acknowledge it? Only if he deserves some credit, of course. Should we credit him with some merit? On this question standards vary. We are still troubled by even the question, To whom do we credit a hypothesis, to its originator or to the one who has empirically confirmed it? This question receives different standard answers. The eighteenth century attributes, for instance, the inverse square law of electricity not to Franklin or Priestley but to Coulomb; by the same token, for example, von Laue's X-ray crystallography and de Broglie's material waves should be credited to others. If we agree with Newton, as I. B. Cohen suggests, that Hooke is the person who first verified Boyle's law, does Hooke thereby gain priority? That depends on the standard of accreditation: the eighteenth century would credit Hooke with the discovery because he verified it; by early twentieth century standards Boyle should be credited since he invented it.

But at times the real difficulty is to know how to apply existing standards, be they good or bad, while these are taking shape or gaining popular assent. The

[4] See Galileo, *Opera,* 5:95, which is his open publication, and 17:296-7, which is a much later private letter. In the former he declares that he should not be credited because instead of rushing to the press he wanted to prepare correct results for publication. (This problem, we shall see later, occurred to Hooke. Cavendish has priority for the discovery of the decomposition of water because James Watt took his time in a similar manner. The problem is still unsolved.) In his letter and private communication he declared that the person who published first heard it from Galileo himself *via* an intermediary ─ which raises a still more difficult problem.

second problem is easier: we may know what the standards are, yet not know whether to apply them to this person or the other. Take the case of the Abbe Mariotte. Nobody accepts the claim that he discovered the law which is usually called Boyle's law in England, and Mariotte's law in France. Did Mariotte fail to acknowledge Boyle, or did he write as he did because he was not yet integrated in the tradition of the New Philosophy?

In the 19th century, at least, the prevalent answer was that of P. G. Tait, who quoted Newton's sarcastic remark on Mariotte, and concluded ([b], 75) that Mariotte was a "paper scientist"; which, of course, is a euphemism for plagiarist. In this century at least one historian of science, Gerland, agrees with Tait that in all probability Mariotte knew of Boyle's work, though he did not mention him. He says clearly ([b] p. 611),

> ... it is hard to understand, how was it possible that Mariotte could remain unfamiliar with the writing of his predecessor ...

yet he does not change his previous verdict [a], p. 351),

> It must indeed be probable, that the second discoverer knew of the work of the first ... But it does not follow ... that the French abbe must be considered a plagiarist. Likewise one has no right to brand Descartes a plagiarist because he took up in his writings Snell's law without naming its discoverer.

The same topic was later raised by W. S. James who quotes Tait approvingly and ignores Gerland altogether. Later writers simply drop the question, perhaps from not wanting to soil their hands in the dirty question of plagiarism.

Even when standards are established and well-known, problems and difficulties and muddles abound. In recent years this has become common-place. At least, this is the view of Hellen Berman, who opens her review of Anne Sayre's *Rosalind Franklin and DNA (Science,* 170, no 4125 (14 Nov. 1975), p. 665), with the following observation. "It is not really surprising or unusual that credits for some aspects of a discovery as significant as the structure of DNA are often muddled; that often happens in science." Think how more complicated matters looked in the earliest days of the appearance of the standards of crediting and of acknowledgment, and how hard it is to attempt a historical reconstruction of a complicated case!

But if standards are difficult to handle, perhaps we should forget them and try to get the narrative straight: whether or not Newton should have acknowledged Hooke is a matter for Newton; for historians the fact remains that Newton did hear the question from Hooke. As it happens, Newton took care of this point too: he says the question too belongs to him, since he had thought about it before Hooke. No matter: Hooke still asked — even merely repeated — a question, and consequently Newton answered; and so perhaps, hurray for Hooke: perhaps we should credit him with that much! And perhaps not; perhaps Hooke rather pestered Newton and slowed down his progress. Now Newton did postpone publication of his *Opticks* till after Hooke's death, and in it he simply failed to make acknowledgment to Hooke, or perhaps of both Boyle and Hooke, quite in contrast to an acknowledgment to be found in a private letter from Newton to Hooke, concerning the discovery of colors of thin plates, now generally known as Newton rings (Sabra, pp. 331 and 321-323]! How shall we judge Newton’s action? We cannot call it plagiarism since he explicitly disclaims priority. But how shall we view his action? How did Newton’s contemporaries and immediate successors view it? We do not even know the answer to this latter factual question, and (strangely or not) because we do not have an answer to the former and normative question. This contains a general cause for frustration. Unfortunately, there are many questions of fact that, because of our present-day standards, we consider important and want to answer before we can set the record straight to our own satisfaction. Yet we are frustrated in our inability to answer them, an inability rooted in the fact that they are not answered by the chroniclers of the period who gave them no significance: employing different standards, they skipped them altogether.

Sometimes, the situation is worse. Not only are the interests of modern writers absent in earlier ones, but at times conflicting aims interplay: the interests of historical personae and the interests of their intimates may, and often do, conflict with the curiosity of posterity. Sometimes, authors reveal more than their intimates permit them to ─ as was the case with the autobiography of Mill or Darwin, and with Malcolm’s life of Wittgenstein. But to take an example nearer home, the inductive style which Bacon advocated and Boyle made the official style of science, is that which requires the clear presentation of experiments in chronological order in a manner permitting repetition by readers, without reporting any hypotheses, except ─ Boyle added ─ briefly at the end of the report, if one insists. Inductivism, in its original stringent Baconian version, forbids the employment of hypotheses; and so the inductive rules of writing, particularly of the reporting of hypotheses employed during research, seem to agree with the Baconian inductivist philosophy. Hooke, for example, is rather truthful, and confesses this fault. In his address to the Royal Society in the opening pages of his *Micrographia* of 1665, which was published at the Society’s expense, Hooke says,

> ... I have ... added ... some conjectures of my own. And therefore ... I must ... beg your pardon. The rules you have prescribed ... do seem the best ... particularly that of avoiding dogmatizing, and the espousal of any hypothesis not sufficiently grounded and confirmed by experiments ... In saying which, I may seem to condemn my own course in this treatise; in which there may perhaps be some expressions, which may seem more positive than your prescriptions will permit: And though I desire to have them understood only as conjectures and queries (which your method does not altogether disallow) yet if even in those I have exceeded, ‘tis fit that I should declare that it was not done by your directions.

I hope the reader notices how tortuous and tortured this passage is. If from the start recording was as problematic as the above passage indicates, we may well quote cautiously every record of any hypothesis. Otherwise we may even misunderstand the "Queries" at the end of Newton's *Opticks,* and elsewhere, to be queries rather than hypotheses as in Hooke's above quoted remarks. But Newton's work belongs to a later period. The above quoted remarks were published in 1665. The story of Boyle's law begins not earlier than 1660, and not later than 1665 — the book just quoted is the latest published primary source on the topic. The part of it concerning Boyle's law is, we shall later see, quite complicated and unusually tortured even for Hooke. All I wish to say here is, that fact is not very surprising for such a period of transition.

Before going to the complex history, however, let us notice first the relatively simpler facts of the matter, namely the physics itself.

**2. The Gas Law**

The gas law is known by various names, as Boyle's, as Mariotte's as Gay Lussac's, or as Charles', or sometimes by two or even three or all of these names. Anyway, it correlates the volume, pressure and absolute temperature of a gram-molecule of any given gas, where a gram-molecule is a certain number of molecules, the number being called after Avogadro or Loschmidt (but hardly ever after both). Perhaps one should say at once that the law and the number so named are named not after their discoverers, but in honor of some students of the field. The formula,

$$PV = RT$$

says that the product of the pressure $P$ and the volume $V$ of a given gram-molecule of any gas is proportional to the absolute temperature $T$ of that gas, with the factor of proportionality being a universal constant. The universal constant is a product

$$R = N\,k$$

of the Avogadro or Loschmidt number and another universal constant called Boltzmann's constant; Planck, in his *Scientific Autobiography,* complains about this name, claiming to be the person to have introduced the constant (in 1899). With such incongruity about naming, one need not wonder that some historians fuss about the fact that Boyle's contribution on the Continent of Europe is called after Mariotte, whereas other historians are remarkably indifferent to this fact.

The most famous up-to-date variant of the gas law is due to Van der Waals, and seems to fare best, but is not sufficiently unproblematic to be declared the last word by any physicist, least of all by Van der Waals himself. Now all contenders to the status of the gas law are improvements on the most famous version, itself distinctly unsatisfactory but the point of departure nonetheless, *the* gas law, or the *ideal* gas law. The law is applicable only to ideal gases, and there are no ideal gases, strictly speaking; under normal conditions, though, a few gases are fairly nearly ideal: including, say, common air and hydrogen. Let us then assume that the gas law

$$p\,V = R\,T = k\,NT \qquad \text{(I)}$$

applies to a gram-molecule, or to 24.4 liters under normal pressure and temperature, of any gas. Now, for our historical study we need not go into so much refinement; we may replace it by its corollary, if $T$ = Constant, then $P\ V$ = Constant, (2)

since prior to Amontons' study of 1699 there were no studies of the variations of temperature beyond what is fairly common knowledge anyway. Moreover, we may restrict (2) to air alone, since, prior to the mid-eighteenth century, there were no other known gases. One may feel that this is a sufficient simplification, but we still have to abolish explicitly the idea of a gram-molecule. Let us say, instead of gram-molecule, any given quantity of air. But this is an over-simplification; that is to say, this change leads to the loss of some important information, well-known to Boyle himself.

The information thus omitted is rather subtle, and not at all easy to capture, since it is on the one hand intuitive, and on the other hand, hard to formulate. The more air, we known, the more $P\ V$; for example, two equal quantities of air will have the same $P\ V$, and put together should yield $2P\ V$. nut how do you measure two equal quantities of air? Formula (I) says, in effect, they have the same number of molecules. How do we count these? We take a quantity such that for it P·V / T equals $R$ and we know it has $N$ molecules. This is, two volumes with equal P·V / T have equally many molecules. At constant $T$, two volumes with the same P·V have equally many molecules. At constant $T$ and $P$, $V$ is proportional to the number of molecules.

All this follows from (I), *not* from (2). Hence the latter is too weak. To remedy this, we must use another corollary of (1) in addition to (2), which is

> Under constant temperatures and pressures equal volumes of air have equal weights. (3)

It is rather easy to combine laws (2) and (3) into one. All one has to remember is the ancient formula which defines density.

density = weight. · volume

and we can see that (2) plus (3) is equivalent to the following law: For any volume of air, under fixed temperature,

pressure is proportional to density (4)

Formula (4) is the one used by Boyle. I found, and it is an interesting empirical fact, that when historians of science are faced with the story of Boyle's discovery which refers to (4) and to (4) alone, they feel that something is at fault here ─ I do not know why. The historian who reports the fact that Boyle presented his law as (4) is Webster. He says ([b] p. 486),

> this particular expression of the law is rarely mentioned by historians of science.

(Boyle speaks of "spring" rather than of pressure; we shall come to that later on.) Now Webster does not say why historians usually avoid formula (4), nor what its merit is. I think I have explained its merit as compared with (1) and (2). As to the reason that (4) is seldom stated, it is quite obvious: when our discussion is meant to be accurate we use (1), and when it is inaccurate we use (2) and unawares assume (3) and even employ it in calculations ─ check any high-school text with exercises employing Boyle's law. Hence, there is no need to state (4) in preference to (2), and, though (4) is more informative, (2) looks more akin to (1) ─ quite misleadingly of course. .

The two simplest ways of changing pressure and volume are these: first, using the same quantity of air, one may compress and expand it, and second, using the same fixed volume, one may pump air in and out of it. In the first case it is easier to use (2), in the second case (4). If we wish to illustrate (2) we can change the size of a container ─ say in a tube with a movable piston, or with a tube sliding atop another tube, as in any trombone ─ and measure both the volume in various cases, and the pressure in each of these cases. If, however, we wish to illustrate (4) in the "same" manner, we must measure both pressure and density. There are formulae for measuring density, but the earliest is nineteenth century, and they are all operationally quite involved. And so, even nowadays when we speak of high vacuum, we do not speak of very low densities but rather of very low pressures: we employ (4) both for practical purposes and as means of testing other formulas, but (4) was never illustrated in the same "straightforward" manner in which (2) was.

Historically the case was this: it was easier to use (2) for examining Boyle's law for pressures of more than one atmosphere and (4) for pressures lower than one atmosphere. For high pressures one can use J type tubes, with air captured in the closed shorter leg: when more mercury is poured into the long leg the air in the shorter leg gets more compressed. To use (2) and perform the same experiment for low pressures, one may use an expandable chamber. like a trombone valve (Hooke). But, instead, one may use (4), and simply repeat Torricelli's experiment a few times, each time with the tube initially filled with different quantities of mercury before being turned upside-down and dipped into a mercury dish (Mariotte). The correlation of the mercury column heights before and after the turnings of the column upside-down is *via* (4).

This, too, is of historical interest. Ernst Mach ends his report on the historical development ([Mach, (a)] end of Chapter 1), by saying, first, that the second method ─ of using a Torricelli tube ─ is Mariotte's; and second, that today (1893) both high and low pressure experiments are performed with the aid of two glass tubes, each closed and with a stopper, connected to each other by a rubber tube, so that by lowering and raising each relative to the other, all desirable data are easily obtainable. This is very impressive, as In this way Mach expresses appreciation to both past difficulties and the much derided Mariotte. Somehow this has escaped notice. I suggest that had Mach presented the problems he was solving and had he criticized other historians, his achievement could have had a better chance of meeting some appreciation. In brief, the history of science may benefit from explicit criticism.

**3. Who Designed Boyle's Vacuum Pump?**

One result from the previous discussion is that to test Boyle's law we have no use for vacuum-pump or an air pump. And yet, though the logical genesis of Boyle's law can dispense with the vacuum pump, both the psychological and historical genesis of the law are closely linked to the vacuum pump, as I shall explain in the next section. The fact is that Boyle's own studies on the elasticity of the air began with the construction of a vacuum pump, that indeed much of his work centers round vacuum pumps. Thus, the vacuum pump has won an honorable mention in the title of his best known book, his *New Experiment Physico-Mechanical, Touching the Spring of the Air, and its Effects, (Made, for the most part, in a New Pneumatic Engine) Written by way of a Letter to etc.* As the parenthetic clause indicates, the vacuum pump plays a distinct role in the book. Indeed, it was a splendid toy and was used for all it was worth by the Royal Society, to whom Boyle made a present of it. The first edition itself, of the year 1660, variably known as *New Experiments,* or as *Spring of the Air,* or even as *Spring and Weight of the Air* (from the title of the third edition), is of much less historical importance than the second edition, in which Boyle's law is found. This is the 1662 edition, subtitled The *Second Edition Whereunto is added a Defense of the Author's Explication of the Experiments, Against the Objections of Franciscus Linus, and, Thomas Hobbes,.* the added part or appendix is usually referred to as the appendix, or the *Defense;* at times it is called *Defense of a Doctrine* (after the title of a separate edit ion of the added part). In 1669 there is *A Continuation of New Experiments, Physico-Mechanical, Touching the Spring and Weight of the Air, etc.,* known as the first *Continuation;* the second *Continuation* is of 1680; here, too, a pump ─ a new one each time ─ plays a significant role, but these works themselves are not significant.

Though it is well-known that Boyle's pumps have nothing "direct" to do with his law, historians of science usually prefer to speak of the importance of air pumps in general than to point out the facts of the matter. But this in itself does not mean that Boyle gets complimented thereby: the pump is usually attributed to Hooke.

Boyle had bad luck among historians of science.[5] The nineteenth century almost entirely ignored him. In our century, though his *Skeptical Chymist* was republished by Everyman in 1911, he was largely ignored by historians of science who hardly noted him except in connexion with his law. Nowadays he holds a much more important position, and his name came back mainly due to the influence of J. F. Fulton, who published an extensive and impressive bibliography of his works, first in the *Proceedings of the Oxford Bibliographical Society* of 1932-33, and then, a second edition, in 1961. Another influence was

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[5] For details see Section 5 below and references there. The wealth or poverty of the literature on Boyle can be read off from Fulton's bibliography. The reader may not easily learn from that, however, what incredibly bad luck Boyle had with biographers. Why William Wotton never wrote his intended life of Boyle I cannot say; that Thomas Birch was too busy to do him justice is obvious, and at least he published his letters; of the rest of the Boyle biographies the less said here. the better. The most respected one is L. T. More's and I have failed thus far to publish my views on More because my paper in which they are discussed contains historical conjectures, and I found editors of history of science journals still reluctant to publish conjectures. Indeed, L. Pearce Williams, "Should Philosophers Be Allowed to Write History?", *Brit. J. Phil. Sci.*. 26 (1975),241-253, which is a review of my *Faraday As a Natural Philosopher* (University of Chicago Press, 1971), blasts at me for daring to publish historical conjectures. I concede, however, that since historians of science more often than not study the classical period, that is also the inductivist period, they may easily fall prey to the demands of the inductive style.

that of E. A. Burtt, who gave him an unusually prominent position in his *Metaphysical Foundations of Modern Physical Science* of 1925. But until the mid-century Burtt himself had almost no noticeable influence, and as far as the present study is concerned, this is still regrettably true. All this may be said of R. F. Jones's study as well. So, for the present purpose, it was J. F. Fulton who has put Boyle on the map. And following his lead, the latest authorities, such as Conant, Webster, and Cohen, accept from him that not Boyle but Hooke designed the first two vacuum pumps. Though to begin with his only source of information is Boyle, Fulton says both that Boyle made no acknowledgment to Hooke, and yet that Hooke had designed the pump. Later, Fulton says Boyle's omission is quite understandable as it was in those early days when Hooke was more of a mechanic than a research assistant; but enough paraphrase; let me quote Fulton (p. 11):

> ... Boyle himself probably did not design or improve any of the three air pumps which he describes in the first edition of *The Spring and Weight of the Air* and in the successive 'Continuations' ... .

To continue from Fulton's first edition (1932, p. 20),

> The first pump was designed by Hooke, although there is no acknowledgment to him in the first edition.

This is corrected in Fulton's second edition (p. 11):

> The first pump was designed by Hooke, who had been taken on by Boyle as a paid assistant about 1655. Regarding him at first as a skilled mechanic, Boyle made only passing reference to him (pp. 6-7) in the first edition.

Fulton goes on quoting Boyle's claim of 1669 that Hooke and he himself made essential improvements to the design of the second engine. Fulton goes on in the first edition (p. 20),

> There is no doubt that he and Hooke were influenced by ... Guericke ... Schott having published a description ...

and in the second edition (p. 12) Fulton clarifies,

> There is no doubt that Boyle and Hooke in designing their instrument were influenced by ...

etc.

All this is not too complimentary to Boyle: the first engine is allegedly thanks to Hooke, yet Boyle makes no or little acknowledgment to him; the second engine allegedly has acknowledgment ─ rather grudgingly ─ to Hooke, but not to Guericke and Schott. No doubt, Fulton was familiar with Boyle's works ─ more than any other modern writer. Yet one wonders if he had not overlooked the relevant passages in Boyle! How ill founded are his allegations is quite clear from the following quotations from Boyle. In his *Spring of the Air* Boyle clearly says (A, 5a; B, 7).[6]

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[6] Boyle's *Works* Vol. I, A = 1st ed; B = 2nd ed.

> ... I put both Mr. G. and R. Hook ... to contrive some air pump ... And after an unsuccessful trial or two ... the last named person fitted me with a pump.

If this is not a clear acknowledgment, I do not know what is. Moreover, the paid mechanic, "Mr. G." (Ralf Gratorix; see [Jacob], is not given the same acknowledgment as "R. Hook". Also, for the record, ,the paragraph preceding the acknowledgment to "Mr. G. and R. Hook" contains an explicit acknowledgment to Guericke and Schott. Nevertheless, C. Webster's most complete and latest study of the story as a whole ([Webster, (b)]), overlooks this, perhaps following Fulton's error, and says (p. 464),

> It is not certain how Boyle was introduced to Geuricke's apparatus ... it is probable that he was told about it by one of his correspondents, who might have seen the experiment or read the account of the pump in … Schott's *Mechanica hydraulico-pneumatica.* which was published in 1657.

I do not know why Webster cannot assume that Boyle read Schott, but freely assumes that Boyle received an important letter that somehow escaped Birch's publication of Boyle's correspondence. As I say, Boyle simply makes an acknowledgment to Schott and even explains his debt very disarmingly in a period (see quote from Gerland on p. 195 above) when no acknowledgment was required:

> I think myself obliged [says Boyle] to acknowledge the assistance and encouragement the report [by Schott] of his [Guericke's] performances hath afforded me.

This explanation is somewhat of an exaggeration. Boyle had worked with a vacuum and intended to build a pump even before he had heard of Schott. No matter. Also, there are two "imperfections" in the Guericke pump, one "was in good measure, though not perfectly remedied" by Hooke; "and to supply the second defect it was considered ... " ─ one might suspect that Boyle lapses into an indirect mode of speech because he wishes to avoid acknowledging too much to others. The continuation of that sentence makes it clear that the embarrassment lies in the opposite direction; the innovation stems from another of Boyle's experiments: "because I remembered, that having several years before often made the experiment *de vacuo* with my own hands". The innovation is not important: it is of a hole in the vacuum chamber, with a sleeve and a glove in it, which enables the experimenter to use his hand to move things in the vacuum chamber. The point merely is that Boyle uses an impersonal tone rather to avoid self-credit than to avoid credit to others, and that he credited Hooke with as much as he could.

It is hard to know what made Fulton so inaccurate and so ungenerous to Boyle. Perhaps Boyle's cumbersome prolix slow style may be at fault here. For example, Boyle's sentence partly quoted above, is made up of over twelve long lines, and exactly 186 words. But this cannot be the whole story. Fulton himself quotes Boyle's opening to his own description of the second engine (first

*Continuation).* In it Boyle says clearly of the improvement in the design of the second engine that they were partly "suggested by others (especially the ingenious Mr. Hook)" and partly "I added myself, as finding that without them I could not do my work". Clearly, Boyle speaks as the man in charge of the construction of the second engine. He explicitly claims that Hooke designed the first engine to which he, Boyle, had contributed one improvement. He makes implicitly the claim that he himself designed the second engine while Hooke and others suggested improvements to his own design. After having claimed that the first engine is Hooke's, Fulton lists (p. 12) Boyle's engines thus:

> *The first English air pump* constructed in 1658-9 by Hooke and Boyle ... *The Second English air pump* also constructed by Boyle and Hooke ... *The third air pump* used in England ... Denis Papin had designed ... and ... brought it with him from France ...

I am not at all clear about all this; it is practically out of the question that Boyle did with his own hands any of the construction (meaning by 'construction' the manual labor itself as distinct from 'design' which is not manual labor and from 'improvement' which is ambiguous). Anyway, who did perform the actual manual labor of construction, whether the two whom Boyle mentions, "Mr. G. and R. Hook", or people under the supervision of Hooke or of Gratorix or both, matters too little to make the variation (between 'constructed' in Fulton's mention of the first two engines, and 'designed' in his mention of the third engine) anything more than an elegant variation.

There is another work of Fulton — a brief life of Boyle — in which all this insignificant affair is discussed. It was published in 1960, long after the first edition (1932) of his bibliography, and just before the second edition (1961), in which it (the brief life of 1960) is mentioned. There Fulton makes use of a private note[7] by Hooke on the pump; says Hooke,

> ... in 1658 or 9, I contrived and perfected the air pump for Mr. Boyle, having first seen [one] ... which was too gross ...

The quote seems to me characteristic of Hooke in an uneasy match between the love to stake a rich claim and the love of truth (which won: the note is private, not published). Fulton says, Hooke completed the pump in 1658, and starts the above quote with "I contrived", thus skipping Hooke's somewhat different dating. I think this is very sad. But to end this rather pointless story on a happy note, in the same life of Boyle ([Fulton, (c)] p. 124) he says,

> Boyle gives [Hooke] full and generous credit for devising the air pump in *New Experiment* (pp. 6-7).

Will it be too much to expect from a historian of science, at least from one of Fulton's stature and standing, to correct an error explicitly? Why he should say in one place that on pp. 6-7 there is only "passing reference to" Hooke, and on another that on the very same pages "Boyle gives him full and generous credit", will remain a secret. Could he not say that he had erred? Did he want the error to

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[7] Hooke's *Posthumous Works,* p. iii, quoted in McKie, [b] p. 28, and Fulton, [c] p. 123.

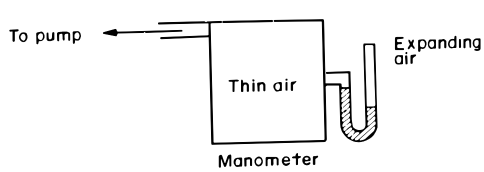
be corrected tacitly? It would be a pleasure to oblige so great and gentle a scholar as Fulton; but the fact is that tacit corrections do not work so well as explicit ones. Webster, for example, now speaks of “Hooke’s pump” as a matter of course ([Webster, (b)] p. 454), even though he mentions in his references *(loc. cit.,* note 83) not only [Fulton, (b)] but also the correct [Turner]. And it is time to restore to Boyle some of the honor he deserves. Let me say why the air pump was so important and then discuss the place of Boyle in history.

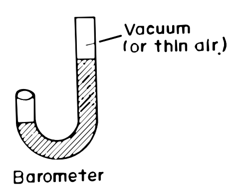
**4. The Importance of the Vacuum Pump**

The air pump is a means to create vacuum chambers. Its only significance as compared with Guericke’s pump is technical: Guericke pumped out water, whereas Boyle pumped out air. Torricelli, indeed, created vacuum chambers with no pumps at all. Take a glass bottle as large as you please, with a neck over thirty inches long, fill it with mercury (or even, with some dexterity, mercury and water) and turn it upside-down: you have created in the bottle a Torricelli-vacuum. Still, this was done by a group of followers of Torricelli in the Florentine Academia del Cimento (academy of experiments). They did not publish, but, as Conant says ([Conant] p. 6n), Boyle “must have heard of them by word of mouth or by letter”. Indeed, we remember from his report of his first air pump, that he had made with his own hands a few years earlier a sleeve to work with in the Torricellian vacuum. So, certain vacuum experiments could be performed without the use of any pump.

Ernst Mach says that most of Boyle’s experiments with the vacuum pump were variants on Guericke’s experiments. He notes that Guericke was a believer in *horror vacui,* but adds that he believed in the weight of air, as well as in the variability of its density. He mentions Guericke’s experiments about fish blowing up in a vacuum, about sealed barrels which hiss when opened up on a mountain, *etc.*

In brief, though air pumps are not essential to the illustrations of the elasticity and weight of air, they did help observation of these facts, both directly and indirectly — by raising curiosity, *etc.* Also they were useful in other fields, especially in Boyle’s demonstration of the increased approximation to Galileo’s law in vacuum (where a feather and a marble fall almost together) and of Harvey’s theory of the heart as a pump, but we need not discuss all this. Nowadays, it is hard to notice much difference between a manometer (Greek “manos”, thin) and a barometer (Greek “baros”, weight), and for two reasons. First, soon after the publication of Boyle’s studies the




Manometer Barometer

difference between compressing air and thinning or elating or expanding it, was omitted as irrelevant; but prior to Boyle's researches and until 1665, this was not so clear: perhaps Hooke's contribution was just on this matter. (All this will be explained in detail below.) Second, barometric measurements depend on two variables: height of the location of the measurement as compared to sea-level, and atmospheric fluctuations. It will be easily understood that in order to sort these two out we must have a standard of air-pressure ─ one atmosphere or 76 millimeters of mercury is as good as any ─ and for the operation of that standard manometry is necessary.

Indeed, we know that barometric experiments were performed prior to Boyle's studies to determine mountain-heights. Pascal sent his brother-in-law to climb a mountain with a barometer. Later he discovered barometric fluctuations due to changes in weather conditions. Power and Townley did similar work in England, which will engage our attention a few times in the present study. And so we can raise the question as to their value right away. Were they mere repetitions of Pascal's experiment? If not, what was their value? What problem did they come to solve?

That air pressure was higher at sea level than on a mountain is a result of Torricelli's work and simple hydrostatics. Since hydrostatics was not as obvious in the 1650's as in the 1670's (see Boyle's *Hydrostatic Paradoxes,* for example), one might want a simple qualitative proof. Both Pascal and Guericke had them (though Guericke published much later): Pascal showed that a partly inflated ball fully inflates on a mountaintop; Guericke showed that a barrel hermetically sealed on the plain and opened on a mountain will hiss when opened. But Pascal's brother-in-law took quantitative measurements. They were inaccurate of course, since they fused the height of the barometer and the variation of weather conditions into one reading. Power and Townley could have been trying for a higher accuracy; but if so, they failed. And so, their value is doubtful: when Webster reproduced Power's table (see p. 231 below) he claims he had to correct some misprints. He says Boyle's law could be read from these tables ─ which means that he could ignore all atmospheric variations. But how could Power know this? We can add that Boyle conceived of a third factor, in addition to variations of height and of the weather, which may affect the barometer: he conceived of atmospheric tides akin to ocean tides (pp. 26-28). Could Power ignore this factor too? The barometric experiments and Boyle's manometric experiments were the chief instruments which Boyle used on his way to the discovery of Boyle's law. After all, for Boyle's law the simplest instrument is not at all a pump; and yet such instruments were developed only after the vacuum chamber was used quite extensively. In retrospect we may miss the point: the simple experiments with the barometer were open to the many sorts of fluctuations and led research to different directions. When relatively high vacuum was created by the pump, and also high pressures, the scope of the experiments was so drastically altered that many fluctuations could be ignored. We shall later see Boyle taking regular advantage of all this and clearly aiming at rather crude measurements.

To conclude, the role of the pump is no more than that of an intermediary or a means of creating a lot of variations on some rather simple experiments, and thus as means for determining the upper and lower bound of level of accuracy required in the experiments to insure both stability and repeatability of results. We shall later see that developing the simplest experimental tool was by no means an easy task, when stability and repeatability became the prerequisite. This, indeed, is what marks Boyle’s experiments as compared with those of all his immediate predecessors.

**5. The Place of Boyle in History**

Robert Boyle was a very important man: there was a plaque on his gate saying that on doctor’s orders he could not see visitors on Mondays and Wednesday mornings. He was already very important when, as a teen-ager, he returned from his European tour and joined the group of scholars which he later christened “The Invisible College” and which, consensus o( opinions goes, was the nucleus of the Royal Society.[8] The Society was founded in 1660, soon- after Boyle published two famous works, his *Seraphick Love* and *New Experiment Physico-Mechanical, Touching the Spring of the Air.* The title- page of *The History of the Royal Society* of 1666 by Bishop Spratt contains pictures of Francis Bacon and John Evelyn. Evelyn read *Seraphick Love* with tears in his eyes and wrote a letter to Boyle to say we must do something, and then Boyle suggested that a meeting be called and so things started rolling. The rules of the Society were introduced by the first president, Lord Brouncker, and seconded by Boyle. They were expounded in the Proëmial Essay to *Certain Physiological Essays,* written a few years earlier. Once he published his *Seraphick Love,* his *Spring of the Air,* and his *Certain Physiological Essays,* he became, as we know from contemporary documents, the most important intellectual in Europe.

Boyle was held in the greatest esteem during his life-time and at least until the revolution in chemistry. Dr. Johnson’s essay on him *(The Rambler,* No. 106, 23.3.51) still reflects a very high respect for him. The first somewhat unkind remark about him seems to be Hume’s *(History of England,* Appendix to Reign of King Charles II), and Hume, on the whole, quite admired him. The real collapse of his reputation I attribute to the deep and widespread influence of Sir John Herschel’s shallow *Preliminary Discourse* of 1831.[9] Herschel had two different reasons, I think, for his low view of Boyle, first that Boyle’s philosophy was mistaken, and second that he failed to discover Newton’s theory of gravitation.

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[8] For details see my review of Margery Purver’s book, “The Origins of the Royal Society”, *Organon,* 7, t970, 117-135, reissued in my *Science and Society,* 1981, as Chapter 25.

[9] See my "Sir John Herschel's Philosophy of Success", in *Historical Studies in the Physical Sciences.* Russell McCormmack, ed., Vol. I, Philadelphia, 1969, 1-36, reissued in my *Science and Society,* 1981, as Chapter 27.

In his well-known and once very influential brief *The Excellence of the Mechanical Hypothesis,* Boyle had advocated the Cartesian or the mechanical philosophy, according to which the ultimate cause, or the essence, of all changes is collision, or impact, or push, of one kind or another. Newton, who reintroduced forces into physics, hesitated, at least until he wrote his third letter to Bentley, to view forces as (ultimate) causes. Newton's pupil Cotes argued in his famous preface to the second edition of Newton's *Principia* that forces are indeed essential causes. The philosophical world was on the whole very much in two minds about all this during the whole of the eighteenth century, with great exceptions like Boscovich and Kant who decidedly followed Cotes, and with some more exceptions like Euler, who decidedly followed Descartes. The majority followed Newton in their vacillation between the Cartesian and the Cotesian views. A famous example is Franklin who spoke about his own electric forces using the same words with which Newton had spoken of gravitational forces. Another example is the arch- Newtonian Laplace who partly followed Cotes, partly tried to explain Newtonian gravitational forces mechanically, *i.e.* as results of collisions (by his famous theory of *gravifique).* With the failure of such efforts and with the success of Newtonianism in the fields of electricity, magnetism, elasticity, and chemistry, and perhaps also with the spread of Kant's and Boscovich's theories of matter, Cartesianism or mechanism became unpopular in nineteenth century England, and with it most of Boyle's theoretical writings. Boyle made another serious error of speaking about the particles of fire, about 'igneous particles' *etc.,* the question of whose affinity to, or identity with, phlogiston, is still being disputed. Consequently, in the anti-phlogiston period Boyle's chemical works became less and less popular (as Fulton's bibliography shows).

All this explains, I think, why Herschel was disposed to speak ill of Boyle whereas his immediate predecessors had almost nothing to say of Boyle but words of praise. Herschel's actual attack stems from a quite different position. In the last part of his *System of the World* Laplace states that Newton was not only the greatest but also the most fortunate man in having been born in a time ripe for the greatest generalization. That time, we know, saw also Boyle and Hooke, and Herschel seems to have wondered why Newton and not these other two men of genius, took up this unique opportunity. Anyway, Herschel does explain this ─ by claiming (p. 115) that Boyle was concerned with so great a multitude of experiments that he had no time for theorizing ─ being under the bad influence of 'remnants of alchemy and natural magic' ─ while Hooke was too busy with the microscope (under the same bad influence?). This, to my knowledge, is the first appearance of the incredibly ignorant assertion that Boyle was concerned with facts, not with theory.

I have discussed elsewhere ([a] pp. 14-19) the phenomenon of historians of science transcribing each others' errors and in the process smoothing the events of the history of science. Here is another example. Boyle allegedly had a prejudice against theories; hence, any attempt to minimize his share in the rise of

Boyle's law will readily be endorsed. And so a minor, understandable, and insignificant error has developed into a whole literature, of which, hopefully, the present essay is the last contribution.

In 1809 von Lindenau published his *Tables Barometriques* where he attributes Boyle's law to Townley (on p. xx). This was quoted by Gehler in 1829, (p. 283), and perhaps by others. In itself this signifies very little. As I have tried to show elsewhere, errors of historical fact of .this sort are so common in the vast and over-detailed scientific literature that it is beyond hope to list and rectify them. The error only gained significance when it was picked up and interpreted by the famous F. A. Lange. In his classical *History of Materialism,* which was first published in 1865, he discussed Boyle's metaphysics, perhaps for the first time. He noticed the similarity between Boyle's and Newton's metaphysics, but he left the question of a possible influence entirely open (i, 300, 303). In spite of his high view of Boyle, Lange's exposition makes him appear as an eclectic Cartesian-cum-Gassendian, who was in addition, a strict Baconian. To illustrate Boyle's Baconianism Lange claims in a footnote (p. 302) that Boyle missed the chance of generalizing the data he obtained into what is now known as Boyle's law, and left this task to his assistant Richard Townley; this is an example of Boyle's interest in facts but not in theories. That was an ominous footnote.

After Lange's remark, Boyle's reputation was gone; it would be unimportant had the mistake in it not been so typically an inductivist one (namely containing the mistake that science begins with observation). It was restated by the famous German historian of physics, F. Rosenberger who described the story in some detail. Lange's verdict is more or less endorsed by Rosenberger (p. 135):

> The great exerpimenter Boyle thought to little of drawing conclusions from his obervations that he left the discovery of the generalization known as 'Boyle's law' to one of his helpers.

This is Rosenberger's version of the story (p. 138), with my italics:

> In order to convince (his opponent) Linus of the resistance capacity of the air, Boyle used a J shaped glass phial, the shorter leg of which he sealed. When, after this, he poured mercury through the long leg into the phial it pressed the air which occupied the shorter leg in proportion to the amount of mercury which has been poured into it. But the air always managed to hold balance with the larger mercury columns, while contracting respectively. *Subsequently,* Boyle *worked out tables* for the different amounts of increase of pressure in the long leg and the respective volumes of air in the short leg. But *he did not draw from this any further conclusions* about the relations between the magnitudes. Only after one of his pupils, Richard Townley, *noticed that according to those tables the volumes of the air were inversely proportional to the pressure,* did Boyle take up this law and prove further, that the law holds also for pressures which are *smaller than the pressure of the atmosphere ...*

There is little doubt that Rosenberger read Boyle: there are many details in the above quotation which he could get almost only from the original. Yet, as we shall see, the whole story is a fabrication which runs contrary to explicit statements in the original. In particular, we shall see, Boyle ascribes to Townley not the first set of experiments on high pressure but the second set of experiments on low pressure.

What of it? Is this perhaps a small error, which can easily be patched up? Perhaps. August Heller also noticed that somehow there is the high pressure experiment and the low pressure experiment. He, too, got it wrong, though quite differently. Says Heller (p. 171):

> Boyle stated the theorem such that air gets denser with the compressing force. His pupil Richard Townely noticed that the height of the mercury in the manometer is the inverse of the volume of the air. Boyle now made experiments both with air made denser and with air made thinner; he found that, indeed, the elasticity stands in the opposite relation to the corresponding volumes of the air.

Heller is perhaps careless: I cannot find in Boyle's work the experiment with the manometer which Heller mentions as described by Boyle. But perhaps I am using the word' 'manometer" too strictly. No matter. According to Heller, Boyle made the experiments, but only "now", to wit, after Townley pronounced his theory on the basis of facts, and in order to test it. Whereas to Rosenberger induction is a generalization from tables of data, to Heller it is a generalization from less precise data. Each could reconstruct history from his theory of induction, the text being only perused to provide some details.

One may, of course, use the text in order to refute these two theories of scientific method in action. I shall do so presently. First let me stress that to Heller, clearly, Boyle's law was not formulated by Boyle: in the same volume, in a paragraph devoted to Townley (p. 320), Heller explicitly identifies Townley as the one who "formulated Boyle's law of the inverse proportionality between pressure and volume of a gas"!

Be this as it may, and leaving now what other writers *(e.g.* Poggendorff p. 479) have said on the matter, we can close this story with the important contribution of Gerland who, in (a) 1909 and (b) 1913, simply declared the whole attribution to Townley to be based on the misreading of Boyle. Let me quote Gerland in full, especially, we shall later notice, since he was strangely mis-quoted in recent years, even though the title of his 1909 paper contains the expression "Boyle (not Townley)". The chief consideration of Gerland in 1909 was to refute the attribution of the law to Mariotte; on this Gerland says:

> Since one is finally convinced that the law was expressed by Boyle already in 1662 it is not called Mariotte's law but Boyle's law unless one considers it preferable, for safety's sake, to call it Boyle-Mariotte law. However, this labelling confers equal rights on both discoverers. Yet these do not exist since Boyle beyond doubt discovered it fourteen

> years earlier than Mariotte and it is not the custom in the history of science to let a discovery be considered new when it has already been communicated by someone else and in such a conspicuous place as one must consider Boyle's works. One must assume as very probable that the second discoverer knew the work of the first.
>
> ... It does not follow from this that he did not know Boyle's writings as also it does not follow that we must consider the French Abbe a Plagiarist. …

Is the law, then, due to neither Mariotte, nor Boyle, but Townley? If so, should we label it Townley's law? Gerland raises this question and rejects this suggestion, claiming that all that we know of the case

> justifies in no way the presupposition that Townley was the one who gave the law its formulation and thus it carries the name of Boyle with full justice.

And yet Gerland himself notices something puzzling here.

> It is interesting [says Gerland] that [Newton] copied out Hooke's table of experimental results relating to "Mr. Townley's hypothesis" though he could have obtained similar data on Boyle's law from Boyle's own second edition of the *Physico-Mechanical Experiments* (1662). This may explain the otherwise odd fact that years later in the semi-popular *De Systemate Mundi* he referred to the relations between the pressure and volume of air as having been "proved by the experiments of Hooke and others".

I do not know that anybody has ever taken up this matter or even noticed it.[10] Yet it is possible that at this time Gerland's puzzlement led his contemporaries to disregard his view. But let me first quote from Gerland's history ([b] p. 501), to clinch matters against the misquotation, in order to make it quite clear that in spite of his puzzlement, Gerland was convinced it was Boyle and not Townley who had made the discovery.

> But, that the hypothesis which surmises the experimental results of Boyle could originate with Townley, is unthinkable, since Boyle expresses it [the hyphothesis] at least with regards to the experiments with compressed air before he mentions Townley at all; after having done this, and speaking now only of experiment in rarefaction of air and producing its results, he emphasizes that these [experiments] were not performed by Townley but by himself. Thus, the only merit which one might attribute to Townley is that he pointed out to Boyle that the law which he [Boyle] had found for

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[10] Indeed, Cohen says (p. 621b, note 18), "Indeed, Hall, A.R., and Hall, M.B. (p, 399) were the first to call attention to Newton's citation of Hooke in relation to Boyle's law." Cohen's reference to Gerland (p. 618b) is "The Towneley-Boyle relation was studied carefully in 1909 by Prof. E. Gerland, who concluded that, despite the suggestion made by Towneley, the credit for the law should be assigned to Boyle (and surely not to Mariotte)," Cohen says nothing more about Gerland's careful study, many points of which he has overlooked despite its great brevity.

> compression of air applies also to rarefaction of air; the law itself, also concerning the rarefaction experiments, belongs to Boyle.

Here Gerland has solved the mystery. In Boyle's works Boyle's hypothesis is the name of the law of compression, *i.e.* for pressures above one atmosphere. And Newton said that Townley's hypothesis was verified by Hooke, not accepting Boyle's verification as sufficient. Using our last quotation from Gerland, we can thus solve the problem posed in the previous quotation. This is a small bit of the job to be completed. Gerland somehow did not solve it, and the confusion was not cleared up but, as we shall see, further amplified. Let me, then, quote Boyle extensively: in spite of the discovery of additional historical records, published and unpublished, the chief record still is Boyle's original work of 1662, which is still misread by all his students except Gerland.

## 6. Boyle's report on his discovery

In the beginning of *Spring of the Air* of 1660 Boyle asserts (Experiment I) that (under constant temperatures)[11] the density of air varies **monotonously** with the pressure exerted on it. Boyle's law is a more precise, quantitative version of this hypothesis, namely that the density of air varies **proportionally** with the pressure exerted on it. The qualitative law is at the basis of the whole of this work (see especially Experiment XVII).

Boyle's law first appears two years later, in his *Defense of the Doctrine Touching the Spring and Weight of the Air … Against the Objections of Franciscus Linus.* This work is written in a historical manner, something very unusual before Faraday's time. I shall now present a summary of it, preserving the order, adding comments and quoting some relevant passages.

Linus had agreed with Boyle that air was elastic, but he vehemently denied that it was as highly elastic as Boyle supposed. Boyle showed that all Linus's objections could be answered, that Boyle's hypothesis did explain all the facts which Linus had referred to. Nonetheless, the fact that Linus had made (though only by the way and inconsistently with other of his assertions) some *comparative assertions* as to the degree of elasticity of air was of great significance; it started a train of thought leading to Boyle's law. But it is still untraditional to admit that an Aristotelian like Linus may make a contribution to learning while making an invalid criticism! Boyle, however, explicitly admits in the following story that it was Linus's objection which led him in stages to search for a quantitative hypothesis.[12]

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[11] The proviso, qualifying Boyle's law to constant temperatures alone: is often omitted in contemporary works, but it is never ignored. Boyle even worries often about unnoticed temperature variations, which Linus often blames on Boyle's results.

[12] Notice that what Rosenberger says is, in effect, as follows: Boyle's experiment with compressed air was not initially connected with a quantitative hypothesis, as evidenced from the fact that it was meant as a reply to Linus. In other words, Rosenberger tacitly agrees with all inductive philosophers that criticism is a preliminary to constructive scientific work, and hence not itself constructive. A similar opinion is expressed in Webster lb] p. 467. Contrary to this I have tried to explain how Boyle came to the view that his reply had to be quantitative.

In spite of his rebuttal of Linus's objections, and in spite of the severity of his own objections to Linus's view of the matter, Boyle still feels uneasy: Linus's hypothesis may still be true, unless experiments 'render it improbable'. To this end Boyle suggests a series of experiments. The first, says Boyle, is even a crucial experiment: it is Pascal's observation that the higher the Torricelli tube is stationed, the lower the mercury column in it. But Linus has questioned the truth of Pascal's report, although, Boyle says, it was confirmed.

> I can confirm these observations of Pascal, by two more, made on distant hills in England: the one of which I procured from that known Virtuoso Mr. J. Ball, whom I desired to make experiments ... in Devonshire ... ; and the other made in Lancashire by that ingenious gentleman Mr. Rich Townley.

These people were his assistants.[13] It may be noted that Boyle, the teacher, encouraged his assistants from the beginning to become independent scientists. First (we remember) he makes a magnanimous acknowledgment to Hooke; now he praises Ball and Townley. To return, Pascal's experiment is only the first crucial experiment (98a).

> To all this I shall add two things, that will very much confirm our hypothesis [the *qualitative* law]. The one is, that the freshly named Mr. Townley, and diverse ingenious persons that assisted at the trial, bethought themselves of so making the Torricellian experiment at the top the hill, as to leave a determinate quantity of air in the tube, before the mouth of it was opened under the vesseled mercury; and taking notice how low such a quantity of the air depressed the mercurial cylinder, they likewise observed, that at the mountain's foot the included air was not able to depress the quicksilver so much ...

Townley's experiment was clever ─ Boyle thinks it was too clever to yield results without simplification. Whereas Roberval added air to Torricelli's vacuum, and Pascal took it up to a high mountain, Townley did both. But neither he nor Boyle could work out the results. But Boyle saw in the experiment a possibility for

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See also Huygens' comment on this very point in S.J. Rigaud, *17th Century Correspondence.* Vol. I, Oxford, (1841) p. 93:

I was at first astonished to see that he has taken the pain to write so big a book against objections so frivolous as those of his two adversaries. But having begun to peruse it and seeing that among his refutations he has inserted many new discoveries and observations not yet seen I wished it had been bigger.

[13] Both Webster (Ia) p. 227a) and Cohen (6l9a), wonder why Boyle refrains from making an acknowledgment to Power here. Webster explains (and Cohen enthusiastically agrees) the omission as an oversight as the result of a cumbersome title to Power's section on the Power-Townley experiment on rarefaction. I really think that this is too much of an insult to Boyle, who had read many a cumbersome title and a cumbersome report very carefully, who may have had a few conversations with both Townley and Power now and then while they functioned as his assistants, and who did refer to Power's tables of rarefaction in a passage misread by Cohen and ignored ─ either not read or misread ─ by Webster. For Boyle's reference to Power see note 22 below.

designing the sought-after crucial experiment between himself and Linus. The pace begins to quicken.

The detailed report of the experiment by "Townley, and diverse ingenious persons", which Boyle views as of crucial importance in the development of his own work is reported by Henry Power in 1663 and cited in [Webster, (b)] pp. 473-4. The report is in first person plural ─ an intriguing fact that deserves notice (and is used by Webster and Cohen as evidence that Boyle was a bit confused on these matters). Power's report says, the experiment took place in spring 1661; Webster reports (p. 481) that Boyle had access to Power's manuscript in summer 1661 and repeatedly promised to make acknowledgment to Power. Why did Power refrain from publication? Certainly, if Power had discovered Boyle's law, or Townley, the delay in publication looked odd, particularly since Power was fully informed of the pace of Boyle's progress. Boyle used Power's manuscript ─ and Townley's experiment ─ in summer 1661; his final discovery was made in the following fall and published in spring 1662 (pp. 481 and 486, notes). Contrary to Power's hesitance and Townley's even greater hesitance (his book is still in manuscript; p. 471) Boyle's research evidently entered that phase of frantic acceleration towards a grand finale and rush to the press which is so often associated with great discoveries. The image of Boyle toiling with tables of facts and Townley musing about it, as well as the image of Townley fancying an idea and coaxing Boyle and Hooke to test it, both these images bespeak false views of scientific method.

To return to Boyle's report: he saw something new, he had a flash of an idea about a new crucial test: Townley had designed an ingenious variant of the Torricellian experiment, which could be put to use just here.[14]

It seem that Boyle himself took the next step: his report is in the first person plural. He saw in Townley's old idea an opportunity of making experiments of Pascal's design without climbing high hills. But for these experiments more sensitive instruments were required.[15] To make the instrument sensitive they returned from mercury to water. The choice of mercury rather than water is of course the result of inconvenicence of working with a high water column (about 12 yards) as compared with a low mercury column (about 30 inches). But for the sake of sensitivity the convenience was sacrificed and experiments were carried out with high water columns (in Westminster Abbey). The experiment showed slight deviations from regularity, which Boyle ascribes to variations in temperature. Then, the apparatus broke. Thus, the experiment was a failure and so, I conjecture, the party tried to improve upon it. Though the experiment has led to nothing Boyle reports it in detail: he obviously considered it important.

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[14] Hooke has described all experiments with pressures below one atmosphere as a variant on Torricelli's experiment, but of high pressure as "another experiment". See pp. 238 and 240 below.

[15] These two sentences constitute ample reply to Webster's and Cohen's attribution of the law to Townley or to Power and Townley. See note 10 above; and see note 27 below for Webster's reading of Boyle's law into the experiment which Boyle here simplifies on his way to obtaining the quantitative law.

Another crucial experiment against Linus, says Boyle, and a qualitative experiment it is again, is Pascal's experiment showing that

> a weakly-blown foot ball ... appears as if it were fu\l blown at the top of the mountain.

Still, Boyle is not satisfied. At this junction he comes to realize that Linus' objection demands a careful quantitative measurement, since Linus has ascribed the phenomena found by Pascal to variation in temperature, rather than of pressure, and there is no denying that the hilly air is cooler. Townley's idea seems to provide the key to the refutation of this view, since in his experiment (with a barometer in which rare air replaces the vacuum) not only the weight of the atmosphere but also the elasticity of the rare air plays an important role.[16]

The reasoning is simple, and it is explicitly presented as Boyle's own: In Pascal's experiment, carried out simultaneously with columns of mercury and of water, both columns should fall when ascending to a high hill, and the water column should fall nearly fourteen times as much as the mercury column, being so much lighter. In Townley's experiment this is not so obvious, since the expansion of similar volumes of air play some role in it, but the volume of air over

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[16] Webster, and Cohen, read Boyle's law into Power's measurements of high altitude experiments performed prior to Boyle's vacuum-pump experiments. Reading Boyle's text carefully we can easily note Webster's hindsight, and the element of truth in it fully stressed by Boyle. Note also Boyle's uneasy vacillations between the Quantitative and the Qualitative, which psychologically indicates a reluctance to use exact measurements as an argument, and which intellectually indicates certain reservations.

These may be rooted in Boyle's skepticism about any exact measure at such an early stage of crude experimentation. Or they may be rooted in his fear that too much calculation may drive the amateur away. After all, his attraction to chemistry and pneumatics, as opposed to the more traditional astronomy and rational mechanics, is related to the openness of these new fields to amateurs!

I do not think this is true; in my own view, there was an excess of Quantitative data available, and so one had to discard some, and to decide the limits of accuracy recommendable. This is standard practice, but still not commented on by most philosophers of science who think of precision as something limited only by the grossness of our experiments (see my *Science in Flux,* in R. S. Cohen and M. W. Wartofsky, eds., *Boston Studies in the Philosophy of Science,* 28, (Dordrect and Boston Reidel, 1975). Even if the mistakes in Power's tables are, as Webster insists «(b) p. 475), mere misprints, the difficulty Boyle faced was real enough even if he saw a better manuscript. Indeed, it is the kind of difficulty ever present, and an especially hard one for pioneers.

I wish to stress all this because to modern writers on the subject it is hard to see what the whole fuss is about. The Qualitative hypothesis was known to diverse writers even before Boyle began his researches, and even to young man Boyle himself [Webster, (b)] p. 467); the difference between the Qualitative and the Quantitative forms of the law, *"a* increases together with *b"* and *"a* increases proportionally to *b"* is so small, that a careless formulation of the Qualitative form of the law may look like the Quantitative law even now, not to mention the period between Torricelli and Boyle.

One may well remember how few Quantitative laws physics had at the time ─ Archimedes', Snell's, Galileo's. It is time to notice the problems anyone faces when attempting a Quantitative law, especially before the development of approximation methods by Newton and his followers.

An example illustrating the difficulty is (Webster, (b) p. 450, where a passage from Roberval is Quoted in the Latin which, Webster rightly says, is reminiscent of Boyle's law. On the same page Pascal's half-blown football which expands on a mountain is Quoted, but with no reference to Boyle ─ even though Webster knows (p. 467) that Boyle knew of it early in his career. The reason is that Webster erroneously expects to see in Roberval more than there is in Pascal. What Roberval was commenting on, incidentally, is his own experiment of adding equal quantities of air to the top of a Torricelli tube and seeing the mercury drop. This experiment of Roberval was a predecessor to Townley's which was predecessor to Boyle's experiments that culminated with Boyle's law.

the water expands much more than the volume of air over the mercury, and we do not know exactly (but only qualitatively) the law connecting the pressure and volume of a given quantity of air. By comparing the water and mercury columns at different air pressures, it seems,[17] the quantitative law may perhaps be found!

Here Boyle mentions again that Linus admits air to have weight and elasticity, though not so much as Boyle ascribes to them. Next comes the quantitative experiment with the **J** tube and the high pressure on the air in the short closed leg which was already described (in the quote from Rosenberger, p. 210 above): the refutation of Linus must be quantitative upon and the experiment with which this was done is a further improvement upon Boyle's improvements on Townley's experiment. Whose it is I do not know: again Boyle uses the first person plural. I should mention at once the significant aspect of all this, which will be discussed below. Whereas Townley's experiment, and Boyle's first improvement on it, relate to the elasticity of air under pressure of less than one atmosphere, the later improvement concerns the elasticity of air under pressure of more than one atmosphere.

Rosenberger first tells about the aperture, and next about the tables of measurements omitting any reference to Boyle's intentions, except to refute Linus; Boyle, however, tells us of the expectations he had when he had built the aperture and made the experiment. The passage in which he describes this is by now well known, but somehow it has failed to convince historians that Boyle had an anticipation prior to his experiment. As we shall see later, Boyle's report is still not accepted. Let us first take note of what he says, when he describes his aperture, describes the results coming up as expected, and presents the table of observations.

Boyle now describes the aperture and the first experiment with it. They have attached, he tells us, rulers to the two legs of the **J**-shape tube, one of which has been sealed; they then poured mercury into the open leg and had the mercury leveled equally in both legs; hence, the pressure on the air in the sealed legs was exactly one atmosphere; they then continued to pour mercury without letting any air escape from the closed leg, until its volume decreased to half of what it was when the mercury was leveled in both legs. Then, the result:

> we[18] cast our eyes upon the longer leg and we observed, not without delight and satisfaction, that the quicksilver in that part of the tube was 29 inches [*i.e.* one atmosphere] higher than the other

[17] It seems that. Boyle's own rather contrary view (Proëmial Essay) notwithstanding. he really did hope to read a quantitative hypothesis off the data. No doubt, when he did perform the experiment, he clearly indicates (see below), he did have a definite quantitative estimate in mind; but a few steps (and a few days) earlier he was still groping, hoping that the data would provide the exact quantitative hypothesis! In all of Boyle's works that I have read, this is the only strictly Baconian passage that I have found. But perhaps I am reading too much philosophy into a casual narrative.

[18] There is a conjecture [Gunther, p 731 that 'we' designates Hooke: a mere speculation, based on nothing else but Boyle's well known near-blindness. Not being inductivist, I consider the whole question of whose eyes Boyle used somewhat unimportant. But surely we have no evidence that Hooke assisted Boyle in Boyle's quantitative experiments, while we know that Townley did.

> ... So here the same air being brought to a degree of density about [*sic*!] twice as great as that it had before, obtains a spring twice as strong as formerly.

In the next paragraph but one I shall quote evidence according to which Boyle's 'delight and satisfaction' is due to his having anticipated the observed result; but he hardly alludes in the above description to any anticipation. This is the beginning of the inductive style of suppressing all use of hypothesis prior to and leading to the experiments described. But to continue with the story.

The tube was broken. Boyle decided to continue with the precise measurements. He invents a simple method of facilitating the experiment by blocking the way of the compressed air by inserting a piece of paper between the air surface and mercury surface. They (again the ambiguity) made careful measurements which they recorded in the table which follows. This *is the* table.

Rosenberger tells us of tables which relate observed volumes of compressed air and observed pressures, from which the law could be adduced. Heller tells us of manometric experiments leading to Townley's estimate leading to Boyle's tables. These stories are not in the present text from which they are allegedly borrowed. Boyle introduces his tables after telling us that the estimate had been made and provisionally confirmed. The table presented by Boyle at this point of his report relates, indeed, observed volumes, observed pressures, and pressures as they should be according to the hypothesis, that supposes the pressures and expansions to be in reciprocal proportions. This is the first explicit mention of the quantitative hypothesis (belated in the inductive style) and (as Gerland so rightly insists) with no acknowledgment to Townley or to anyone else. As usual Rosenberger's and Heller's claims that Boyle's observations preceded the formation of his theory are but attempts to write history as (according to their different views) it should have been, and without even telling us that it is their theoretical interpretations, namely their use of the theory that facts precede scientific theory in time, rather than presentations of mere facts.

So far, Townley's most important function as a collaborator and assistant is definitely not the function imputed to him by Rosenberger or Heller. Townley did not provide the hypothesis discussed so far. But he had an ingenious idea which Boyle simplified and which simplification helped Boyle to develop his hypothesis; namely, Townley's idea of having a Torricelli column of mercury with a little air added to the vacuum above it.

Townley's hypothesis, however, exists; it is not Boyle's hypothesis; and it does not enter our story until the next step but one. Boyle's quantitative hypothesis (Boyle's law for pressures higher than one atmosphere) had already been found. Now, Boyle thinks, Linus's objection is already satisfactorily refuted (A 102a; B 152). The task is now really finished, even by the most charitable standards towards one's opponents. Next comes the crucial and often quoted-out-of-context passage (italics mine), where the acknowledgment of Boyle's law to Townley is allegedly made by Boyle himself.

> Now, if to what we have thus delivered concerning the *compression* of the air, we add some observations concerning its *spontaneous expansion,* it will better appear, how much the phenomena ... depends upon the differing measures of strength to be met with in the air's spring, according to its various degrees of compression and laxity. But before I enter upon this subject, 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 the 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 had 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 measure of its dilation. He added, that he had begun to set down what occurred to him to this purpose in a short discourse, whereof he afterwards did me the favour to show me the beginning, which gives me a just curiosity to see it perfected. But because I neither know, not (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 ... met with fit glasses to make any 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 ...

Here come tables which compare observed *low* pressures *(i.e.* less than one . atmosphere) with what they should be according to Townley's hypothesis: *it is Boyle's test of Townley's extension of Boyle's hypothesis.* (The extension is from pressures above one atmosphere to pressures below one atmosphere.) As Boyle explicitly states, Townley saw *no* tables concerning low pressures, as he had none to show him as yet. So his guess, like Boyle's, preceded his knowledge of any relevant experiment. The passage shows again how inductivists can create myths, and how hard it is to eradicate them. Boyle tells us that he had started experimenting on dilation without having a numerical hypothesis, a 'precise estimate', that he then heard and read Townley about. Townley's hypothesis, his 'endeavor to supply what I [Boyle] had omitted [*sic*!] concerning the reducing to a precise estimate', *i.e.,* concerning the move from the qualitative law to some quantitative law for low pressures (below one atmosphere). There are no tables here, but a clear and explicit statement: Boyle — who had started with a qualitative hypothesis concerning compression *and* rarefaction; who had passed to a quantitative hypothesis concerning compression *only;* but who made no such a hypothesis concerning rarefaction — first heard about such a hypothesis from Townley. Mr. Townley's theory clearly is about the proportion, wherein air loses its spring by dilation, *not* wherein it gains spring. This is the hard core of the present essay.[19]

**7. The significance of Townley's hypothesis**

But are not Boyle's and Townley's hypotheses identical? And if so, is there a problem of priority?

A few points have to be considered concerning this question. First and foremost, there is a point of sheer inaccuracy or misquotation. I think I have made my case that all those who claim, with D. McKie ([a] p. 149, italics mine), that

> the 'hypothesis, that supposed the pressure and expansions to be in reciprocal proportion' had been suggested to Boyle by Richard Townley, *as Boyle himself stated at this point,*

is historically untrue whether Boyle's and Townley's hypotheses are identical or not. Boyle himself made a clear distinction which, when reporting what he "himself stated", should be preserved, whatever our comment on it may be. As Boyle claimed the hypothesis quoted by McKie (about both pressure and expansion) to be partly his own, partly Townley's, one cannot say that he attributed it wholly to Townley. Similarly, McKie is insensitive at least to Boyle's terminology, by which, Boyle's law comprises Boyle's hypothesis (high pressures) plus Townley's hypothesis (low pressure).

McKie is not alone in ascribing to Boyle the acknowledgment he never made. Webster, too, says ([a] p. 227a),

> Both Boyle and Robert Hooke, his [Boyle's] closest associate, referred to the gas law as "Mr. Towneley's hypothesis", and it is clear that it was on Towneley's initiative that they embarked on experiments which confirmed Towneley's suggested law.

Again, I shall not labor the obvious: the claim in Boyle's name is an error. Again, as in the case of Rosenberger and Heller in the previous century, McKie believes that facts precede scientific hypotheses and he makes Townley read the law off a table of fact whereas Webster believes that facts confirm hypotheses and he makes Townley urge Boyle and Hooke to confirm his guess.

But to return to Boyle's presentation of the quantitative law in two parts, one for pressures higher than one atmosphere for which he takes credit and one for pressures lower than one atmosphere for which he credits Townley. Are the two different? From our viewpoint the difference is this. If we go far enough in either compressing or rarifying air we find deviations from Boyle's law. And as we know

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[19] Gerland quotes ([b] pp. 499, 500) the above passage, and its continuation, quoted here on p. 223 below ff., as sufficient evidence against the claim that Boyle's law is Townley's. Evidently, Gerland has failed to make his point. Anyone who still insists that Boyle admitted that Townley was the discoverer of Boyle's law should, in deference to Gerland, read Boyle's passage carefully and offer a detailed and different reading of it. One should, in particular, explain the systematic use, shared by Boyle, of "compression", "condensation", and "elasticity" for high pressures, and "rarefaction", "dilation", and "elator" for low pressures, see in particular, the above quotation, the quotation on p. 224 in text to note 22, also p. 229 and note 28 on p. 236 and also p. 234. See also [Tait (b)], 73 and 75.

now, the deviations from Boyle’s hypothesis are attainable much sooner than those from Townley’s hypothesis. (This follows from Van der Waals’s hypothesis.) Thus, assuming linearity between pressure and density in one domain may be sufficiently accurate and in another not.

Suppose we ignore all question of accuracy, since the whole business is not too precise anyway. Are Boyle’s and Townley’s hypotheses still different? Most historians take Boyle’s model seriously, and so they should say, yes. The model describes each air particle as a spring. A spring can dilate and it can compress; it can be strained or stressed. These two are different phenomena. And, historically, another difference was more important before it was ignored: whereas in Boyle’s case air is pressed, in Townley’s it is ‘strained’; we may not notice the difference, but only because we do not notice that we use the by then still unknown Hooke’s law ─ strain equals stress ─ on which more in the next section. Now, Hooke’s work was first published in 1675, namely, over ten years later. Knowledge of much later ideas about gases, such as Newton’s or Clausius’, may lead us to the denial of the existence of strain in gases; but this is an extravagant hindsight.

But let us ignore all later knowledge, assume that Boyle ascribed to air both strain and stress, and take this seriously for a while. Let me draw attention to a very simple difficulty regarding all this: between the strain and the stress of a spring there is the state of zero displacement, where the spring maintains its natural shape as undistorted by any force. Boyle tacitly assumes that at the point of one atmosphere the displacement is zero. Here already the qualitative hypothesis, or the model Boyle offers of air particles as springs, makes a serious difference between high and low pressure: under low pressure air does not expand under its own force, as we think since Newton, but it is stretched. This is a remnant of Galileo’s theory of the force of the vacuum, which is well- known, especially since Mach ([a] p. 136) has censured Galileo for it. Webster refers to it too ([b] p. 444 & n), but not in connection with Boyle. The fact is, however, that this Boylean idea of the strain of air particles, and with it Boyle’s model of air particles as springs, is destroyed with the unlimited application of Townley’s hypothesis. This point was discovered by Hooke ─ who could not, of course, openly express criticism of Boyle ─ and taken over with some (not sufficient) acknowledgment by Newton (who postulated repulsive force only). Newton’s acknowledgment has puzzled both Gerland (see above p. 212) and Cohen (see note 10 to p. 212 and p. 243). Cohen, we shall see, takes the acknowledgment as testimony that Newton ascribes Boyle’s law to Hooke.

My final observation on Boyle and Townley concerns Boyle’s function as a propagator of science and as a public teacher. This function, I believe, is so evident in the passage in which he makes the acknowledgment to Townley, that, to say the least, it is somewhat quaint that it should have been ignored until now.

It is not the problem of Boyle’s priority that matters. Nor do I think it matters whether the difference between Boyle’s and Townley’s hypotheses is great or small. I only contend that the tradition is mistaken which claims that Boyle was interested in facts only. Against the accepted views, especially Rosenberger’s, I have so far shown (a) that the whole work, experiment and theory, was undertaken in order to refute Linus’s objections to Boyle’s theory of the elasticity of air, and (b) that Boyle made the original hypothesis. But, in addition, there is this to say about Townley’s hypothesis. Townley’s hypothesis was made independently by at least Hooke, Power, and Lord Brouncker as well. Thus, his unique contribution was not here, but rather in the experiment described above, on p.214.

Boyle’s fairness to Townley has never been mentioned, as it was taken for granted. To me it seems extravagant: it was the *first* acknowledgment to an assistant, and Boyle was the first scientist who carefully acknowledged all sources of his information, whether factual or theoretical, published or unpublished (with one occasional exception).[20] Moreover, in his own time there were hardly more than a few scores of people who cared at all about this question of priority. At that time the qualitative hypothesis was much more important than the quantitative one, for reasons discussed below. But Boyle, always with an eye on progress and a large scientific society, set standards which are by now taken for granted. His concern with his own priority in many discoveries is well known, and his well known humility makes it quite clear that this was not motivated only by a personal interest.

It was this point, Boyle’s function as a recruiting officer for the new scientific brotherhood, which misled Lange, Rosenberger, and many others, and which was first noticed by R. F. Jones ([b], Chapter 5). In trying to recruit people, Boyle indeed laid double emphasis on experiment and on facts, for the reason that more people could experiment than conjecture. It was also his attempt to encourage Townley (and others), it seems, which gave the impression that he was indebted to them more than he really was. And it was the quantitative hypothesis which seemed later to be more important, because this hypothesis won Boyle the greatest inductivist reward, namely his being mentioned in up- to-date textbooks of science, his secure place in posterity, his inductive surrogate immortality (as Carl Becker would say).

Before leaving all this, I confess that for my taste Boyle was rather prone to too much exaggeration here. In his zeal to encourage people Boyle did make too much of a minor idea, with misleading results. Remembering that the hypothesis of monotonous increase of density with compression is Boyle’s, and that the extension of the latter hypothesis to the whole domain for which the first hypothesis was assumed independently by Townley and others (see below), we can hardly deny that Boyle behaved somewhat like a schoolmaster here.

In the long passage quoted above (p.219), in which Boyle makes an acknowledgment to Townley, he also mentions two others who are similarly

[20] See my doctoral dissertation, *The Function of Interpretations in Physics,* Pt. II, Ch. IV, sec. 8, (University of London, 1956, unpublished) on Boyle’s (seemingly unconscious) plagiarism from Browne; *cf.* Browne’s *Pseudodoxia Epidemica,* 1646, p. 59 (Bk. 2, Ch. 2) and Boyle’s *Usefulness of Natural Philosophy,* written in 1648 or 49 and published much later.

encouraged. Townley, we remember, intends to continue his experiments a bit further first, perhaps to publish his own results, perhap5 as an additional appendix to Boyle's book; [21] meanwhile,

> 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 a former chapter, as having written something about rarefaction:

that is to say, Dr. Henry Power, whose work was published indeed in 1663, over a year later.[22] And Boyle's intention is to encourage Power (as the immediate continuation of the above quotation makes clear):

> whom I the rather make mention of on this occasion, because when he first heard me speak of Mr. Townley's supposition about the proportion, wherein air losses its spring by dilation, he told me he had the year before (and not long after the publication of my pneumatical treatise) made observations to the same purpose [*sic*], which he acknowledges 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 [*sic*!] by the 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 hope for something very accurate

which, need one say, is flattery, cajoling, and wishful thinking; it all amounted to very little by way of direct influence on Townley or Lord Brouncker; but it might have helped establish science as an amateur occupation; I cannot say.

**8. The history of the study of elasticity**

The question that intrigues seventeenth century thinkers is, can there be a vacuum? It was Boyle who gave this question two distinct versions, scientific and metaphysical. Scientifically, the question was, can there be space without gross

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[21] Boyle's intention to add an appendix with Townley's result was expressed (in the passage between the two quoted on p. 219) as a fact, as noticed in [Webster, (b)] p. 482. Evidently Townley was both attracted to the idea of publishing and inhibited about it; Boyle tried to help but evidently failed.

[22] Power's manuscripts were studied by Webster. He says, "Although the manuscript emanated from Power, it is quite possible that Boyle overlooked Power's part in the work ... Examination of ... Boyle's appendix i.e. *Defense* shows Isic!1 that the author leant heavily on the information from ... Power's manuscript. Power himself is mentioned by Boyle ... " later on ([a] p. 227a). He also suggests that Boyle used Power's tables ([b] p. 483) — an allegation contradicted by Boyle's narrative as quoted here — but "did not understand the significance of the hypothesis which was suggested at the end of the experiment." See also note 26 below.

It is odd that Boyle wishes to "make mention" of Power but only refers to him as "the same person I took notice of in the former Chapter, as having written something about rarefaction". One may also notice that we have reference to two different manuscripts by Power here ([Webster, (b)pp3,481].

Cohen says (p. 6l9a), "Boyle mentions Townley but not Power. This is explained by Webster" as an oversight and as a result of the fact that the title of Power's manuscript includes Townley's name, and others', but not Power's. Further, Cohen takes it for granted, though he is clearly in error, that Boyle's reference to "the same person I took notice of in the former Chapter" is Robert Hooke. Now, Gerland says ([b] p. 500) that the person to whom Boyle refers is presumably Power; since Cohen views Gerland's study as careful, he might, at least, not have considered Hooke to be that person as a matter of course.

matter? To this, he said, the empirical answer is evidently yes. The remaining metaphysical question was, is there in the vacuum non-gross matter? To this he said, not necessarily. He thus was a vacuist both in physics and in metaphysics; he took pain to distinguish his two positions and stress that he was demolishing plenism in physics, leaving the metaphysical question open.

All this must have been known to both Galileo and Torricelli, not to mention Descartes and Mersenne; but it took Boyle to formulate and explain it. Once this was done, much of the initial interest in the vacuum and related phenomena disappeared. Perhaps this was noticed by Boyle from the start, when he libeled his original book with reference to the "spring and weight of the air", not to the vacuum. But perhaps he thereby merely showed tact in the face of the immense popularity of Cartesianism.

It was Boyle, in any case, who raised interest in the elasticity of air, in the first place; and it proved to be less interesting than Boyle had hoped when he suggested that his readers would take his broad hints and repeat his experiments while varying the temperature of the air; his hopes led to a deep disappointment (III, A 209a; B 505). The study of this field, as of any other field, must be connected with some interesting problems. The following seems to me to be the story of the initial seventeenth-century in elasticity.

Descartes' theory of matter as extension explained, at least so he thought, why matter is impenetrable (since inter penetrability of the pieces of matter, would diminish their total volume, and on the assumption of the identity of matter with extension, this would violate the geometrical law of invariance of volume.) It is a strange fact that the vacuists accepted the thesis of impenetrability of matter just as much as the Cartesians; among those was Locke, who denied the validity of Descartes' deduction for empirical reasons, as well as Leibniz who denied it for *a priori* reasons — they all accepted impenetrability of matter quite axiomatically. So did Laplace. So did even Kant, — in his own peculiar way, need one add — at least in his *Critique of Pure Reason.*[23] Perhaps they all did so on Democritus' or Plato's authority, but I have no evidence for this. There exist many known cases of matter seemingly penetrating matter, of course: these cases, as Kant has noted,[24] are not allowed to refute the thesis of impenetrability. Indeed, a simple and surprising example of penetration confirms it: a ball of metal full of water can be compressed with a hammer, with the result that water penetrates its

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[23] Kant says, *Critique of Pure Reason,* 8278, p. 247 of Norman Kemp Smith translation, Macmillan, London, 1929, 1961, *" ... impenetrability* serves in our *empirical* intuition of matter'; and, again, A618, 8646, p. 516:

> In fact extension and impenetrability (which between them make up the concept of matter) constitute the supreme empirical principle of the unity of appearance; and this principle. so far as it is empirically unconditioned, has the character of a regulative principle.

Notice that "empirically unconditioned" means, neither verifiable nor refutable. The status of impenetrability as that of a regulative principle seems to me to conflict with Kant's *Metaphysical Principles of Natural Philosophy.* See my "Kant's Program", in *Synthese,* 23 (1971), 18-23, and in my *Faraday As a Natural Philosopher* (University of Chicago Press, 1971), pp. 86-91.

[24] For more detail, see my doctoral dissertation, mentioned in note 20 above.

invisible (hypothetical) pores and appears on the surface. What is conserved here is the *total volume of matter,* as the law requires. Similarly, a sponge absorbs water but expels air from its pores, as shown by immersing the sponge in a bucket of water. But the case of a football which is blown to its *maximal size* by *n* blows of air, and into which you can still push another *n* blows or so, this case does seem to refute the thesis of impenetrability. This last example is Boyle's (Proëmial Essay). It is strange to note that this fact probably moved no one until Boyle made his studies, and even afterwards the thesis of impenetrability remained axiomatically accepted in spite of Leibniz's criticism and his suggestion of explaining impenetrability as the result of repulsive forces, until Boscovich and Kant presented, each in his own way, a Leibnizian theory of matter as expansive due to repulsive forces.

The explanation of this fact is the same as the explanation of the fact that palmists stuck to their view in spite of the discovery of the vacuum: it was quite possible that space is filled partly by air and partly by anther which can slip through the pores of glass containers and of footballs, and if so, impenetrability and plenism is reconciled with our vacui and with our footballs. This is, again Boyle's reasoning.

Why, then, did Boyle keep his interest in the football? What was its significance? Will this shed light on the great value placed by contemporaries on Boyle's law? My answer is that he had a particular dislike for the dogmatism with which the views on the matter were held, and that his success was the success of scientific and metaphysical liberalism ─ a liberalism that was essential to the success of Newton's deviation from Cartesian physics. The beginning of the story of Boyle's advocacy of intellectual toleration lies in a sad incident in his own life. In 1655 he published anonymously a medical collection plus a plea to publish all medical secrets; it was a complete failure. (This was discovered only recently; see Margaret E. Row bottom, 1950; See also Fulton [b] p. I.) (Boyle's plea for openness and his demand for accepting only repeatable experiments were linked in his mind, as he explained at great length in his *Skeptical Chymist,* especially in his preface to it.) In his already mentioned Proëmial Essay to *Certain Physiological Essays,* published in 1661 and written a short time before he started the work on the elasticity of air, he complains that works of 'learned men, especially physicians' were often undervalued because they were not cast in the Cartesian system, as if Cartesianism was a basic requirement for rational thinking. In opposition to this dogmatism Boyle suggests that even if Cartesianism is true, there is no reason to oppose the presentation of a non-Cartesian explanation of facts, as it may always be hoped that a Cartesian explanation of that (seemingly) non-Cartesian explanation may later be found. I do not wish to discuss here the drastic deviation of this idea from those of Galileo and Descartes, nor to indicate its significance with relation to Newton's introduction of forces in the hope of finding a Cartesian explanation of them. This is a vast topic indeed.[25] I suggest here that Boyle saw an opportunity to study the elasticity of air in this fashion, and that Pascal's loosely blown football, which expanded fully blown, was a chief reason attracting him to the study of the vacuum. This, I think, is why his *Spring of the Air* concerns elasticity, not vacuity; in it he introduced an hypothesis which evidently had nothing to do with Cartesianism. In it he designed a non-Cartesian model of the elasticity of air: he viewed it as a heap of minute springs. Now, if Cartesianism will include a model to explain an ordinary spring, then it will also automatically incorporate an explanation of Boyle's (seemingly) non-Cartesian law. This, it seems, is quite a neat example for Boyle's new methodological principle. The example of a football occurs in the Proëmial Essay and was thus written as an example of Boyle's methodology *before* his reading of Pascal's football, and his experiments with the vacuum pump which led to Boyle's law. Now the *Spring of the Air* contains the qualitative law of monotony between elasticity and pressure. Boyle needed it, I conjecture, in order to present his spring model of the air. The quantitative law is but a part of the *Defence of a Doctrine Touching the Spring … of the Air;* it was of no particular significance for Boyle, and he never laid too much stress on it.

The history of the study of elasticity changed radically with the introduction of Newtonian mechanics. Prior to that there are Hooke's studies25 and young Newton's private notes that are relevant to our topic. I shall discuss Hooke's claim concerning Boyle's law, as well as young Newton's comments on Hooke, in the final section of this study. This section is devoted to elasticity in general; I shall here briefly discuss Hooke's contribution to the theory of elasticity before concluding with a sketch of eighteenth- and nineteenth-century interests; I shall rely on the most scholarly and esteemed study [Truesdell, 53-58].

Truesdell reads (p. 54) Hooke's law to say, elastic force is proportional to displacement, where displacement can be caused by a weight hanging from a spring, a wire, or even a string. This sounds very much like Hooke's law out of the standard elementary physics textbook, which is evidently a historical. And, indeed, later on (p. 56), Truesdell makes observations which prevent this gross a reading: "it was not yet customary" in Hooke's time, and before Newton's *Principia* was absorbed, he says, "to think of motions as determined directly by assigned forces." And still later he added (p. 57), that the standard treatment of Hooke's law as the law of harmonic motions, "seems first to have been given many years later by John Bernoulli." But, no matter how well- grounded is Truesdell's reading of Hooke to say force is proportional to displacement, I still face quite a few difficulties regarding it.

Hooke says, *"ut tensio sic vis";* That is, the power of any spring is in the same proportion with the tension thereof" (p. 54) where both "*tensio*" and "tension" should mean displacement, of course. Hooke continues, concluding or expanding, to say that the force or power of an elastic body "to restore itself to its natural position is always proportionate to the distance or space it is removed

[25] See my *Science in Flux.* mentioned in note 16 above, chs. 8-10.

therefrom, whether it be by rarefaction … or by condensation", *i.e.,* whether by strain or stress (p. 55). One may wonder, what strain or removal from "natural position ... by rarefaction" air can suffer: perhaps my claim that Hooke was following Boyle's analogy (of air particles with springs) breaks down but I do not see how. Moreover, I do not know why Hooke translates "*tensio*" to "tension" but in examples (p. 55) uses "extension" as usual.

Nor is this the only difficulty. For, Hooke means, first, force is proportional to tension and tension to extension; second, force is proportional to stress, and, third, stress to compression; and he may also mean to say that the proportionality in extension is the same as that in compression. Truesdale says (p. 55),

> While Hooke does not say explicitly that *the moduli* [*i.e.* the factors of proportionality] *of extension and contraction are the same,* this seems to be his opinion; in the case of air, the only material for which he says he has measured condensation, this is true.

It is surprising that of all materials to which Hooke generalizes Boyle's law, he only tested his view on the original material, namely air. Nor is the experiment so hard to perform: a spring may be both stretched and compressed by the same weight, once hanging from the ceiling with weight hanging on it, once resting on the floor with the weight resting on it ─ it is really very easy to measure the expansion and the compression and see whether they are equal or not. And, no doubt, for displacements small enough the results are nearly the same.

But in the two experiments just described the displacements at1e from the "natural position" of the spring, which is the equilibrium position it has when not under external force so called (nearly; gravity is external to the spring). And, as I say, air has no such "natural position". Of course, air particles do vibrate in sound around equilibrium positions, but these equilibrium positions are not "natural ", *i.e.* not in the absence of external force.

Hooke's (alleged) claim that strain equals stress was replaced by Young's modulus so-called, which says, strain is proportional to stress (and which Truesdell says, should be credited to Euler, nor Young). And Young's modulus is different when the equilibrium is "natural" than otherwise.

It seems clear to me, that the only straight reading of Hooke is to take him to assume Boyle's model of the air particle as a spring, and one atmosphere as the condition of the "natural position" of air particles. We can then read Boyle's law to hold for all elastic bodies,[26] and thus read Hooke to say, force equals tension equals stress, and tension equals displacement in one direction and stress in the opposite direction. With this reading all difficulties disappear, on condition that we allow Hooke to have confirmed his false view with somewhat inaccurate experiments.

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[26] Hooke says in 1678, "It is now about eighteen years since I first found it, but designing to apply it to some particular use, I omitted the publishing thereof" ([Truesdell) p. 54); I suppose sixteen or seventeen is more accurate than "about eighteen".

The *Encyclopedia Britannica* Article "Elasticity" translates "*tensio*" to mean at times, tension, at time displacement; so do other writers, particularly Andrade [b] unless he interprets *'tensio*" to mean systematically both tension and extension. This puts Hooke in the right at the expense of destroying our ability to understand the history of elasticity.

Interest in elasticity in the eighteenth century was still too limited. The one somewhat pressing empirical case was the study of water which, acoustics informs us, is elastic, yet we know from experiment to be incompressible. Still this was not a very interesting point, and not within the range of existing experimental accuracy. Euler's interest was rooted in his plenism. The interest was given new life in the early nineteenth century when Young imposed the aether theory on all Newtonians, for, in order to test it, more knowledge of elasticity was needed ([Love] p. 7). Interest in the pressure of airs was revived at about the same time by Dalton who faced the problem of diffusion (which arose from Lavoisier's theory of air as a mixture) and tried (in vain) to solve it with the aid of Newton's explanation of Boyle's law. But meanwhile Boscovich's study of elastic collisions and his consequent atomic theory changed the whole scene drastically; the connection between gases and elasticity was soon lost. Boyle's law was generalized and used as basis for the kinetic theory of gases in which atoms of gases were assumed to be perfectly elastic with no further ado.

9. **Webster's defense of Townley's priority**

The first paper on our topic after Gerland's (1909, 1913) is by W. S. James (1928). In many ways it is an enjoyable paper; it is clear and refreshing in that it contains an explicit statement of quite a few difficulties; the author was also very clear as to what he was asking and why he used which documents, and when he used his own judgment. He also brought together most of the then available documents, including records of the Royal Society and Hooke's testimony. I shall not discuss James' view in detail, since it is almost fully reflected in those of his successors which I shall discuss soon; I shall only use details from his presentation while discussing works of later writers. Let me merely present here a few general points of information. James identifies Boyle's law with Townley's hypothesis. He identifies the experiments reported in the *Defense* as those which Boyle reported to the Royal Society on September 11th, 1661 (see below). He notices that Hooke claims having performed them on August 2nd, 1661, and dismisses Hooke's testimony, first, since Hooke was unreliable, especially when staking a claim and second since Hooke was an assistant anyway. Mariotte is declared a plagiarist.

Next comes the paper by D. McKie, 1948, which for all I can judge, says nothing more than James', but which, for some reason I am unable to discover, has fared more fortunately than James' paper: it has been cited approvingly, one way or another, by Sarton, Fulton (who calls it "important", ([b] p. 11n), I. B. Cohen (who calls it "convincing", p. 618), and others.

McKie argues that the law should not be called Mariotte's but Boyle's since it was found by Townley. I have quoted him on p. 220 above. I shall not discuss his work further. In 1950 Andrade claimed ([b], especially p. 459) priority for Boyle’s law to Hooke, since, Andrade says, Hooke claims to have anticipated Townley’s hypothesis. Andrade’s claim has no leg to stand on, of course, since Townley’s hypothesis was published in Townley’s name prior to Hooke’s publication, and priority goes to the first published. The final touch was given by C. Webster and by I. B. Cohen, especially since Webster’s second paper is very detailed, and covers all known material and some new material discovered recently and first discussed in print by himself. I shall report this study in detail now since it contains abundant historical material misread in a complex manner.

Webster’s general thesis is one which is becoming increasingly popular these days, and it is the thesis of multiple discovery of Usher, Merton and Kuhn: every discovery is made by a few individuals. This thesis is very easy to support by multiple evidence: all one has to do is ignore differences between contemporary researchers, and they look identical. In other words, unless a historian provides a criterion by which to identify or differentiate works of different writers, his study may be safely ignored. Of course, multiple discovery is possible, especially when a problem hits the public fancy ─ even when an experiment, or even an instrument, does. And, in order to explain a specific multiple discovery, then, we must say what problem, or other factor, became a focus of interest and why.

Webster merely states that the elasticity of air did take public fancy, and illustrates this by the number of students of the topic from Torricelli to Boyle and his associates. But he does not explain this phenomenon; although he notes for the first time the Cartesianism of Townley and discusses its relations to vacuism, he does not relate it to spring or elasticity. On the whole, he barely refers to theories of elasticity except in the case of Boyle, where he links Boyle’s view with scholasticism in order to belittle him so as to make room for Townley, as I shall soon explain.

Webster finds ideas similar to, and reminiscent of, Boyle's law in Roberval (1648) and a more succinct version of it in J. Pecquet (1651); Boyle's progress (1660), Webster says, was in his groping - perhaps towards more precision, perhaps towards more clarity (see below). Also Webster indicates the difficulties on the way towards Boyle's law, particularly as seen from the failures of Power and Townley.

Webster also quotes (p. 468) Boyle's summary of his unsuccessful attempt to record quantitative observations, made in 1660, which ends with a plea to others to try it out again, and he quotes (p. 469) Boyle's report of his plea to mathematicians to take up matters as well - adding that commentators had been in error when they took this to mean that Boyle was weak in mathematics. Webster notices as well Power's and Townley's reluctance to publish, and he also notices that Boyle's reference to his colleagues was often both highly encouraging and quite cursory - so as to let them do their own publishing.

All this is very interesting, and also very charming; from now on, however, things start downhill.

Webster's attitude towards Boyle is very ambivalent. Already when discussing Boyle's debt to Guericke, though he is careful in his report of the degree of Boyle's dependence on his predecessors, he is unfair to Boyle in his over- generosity to Hooke (see p. 206 above). He then simply speaks of "Hooke's pump" or "Boyle's pump" in a rather indiscriminate fashion.

Webster wishes to ascribe the law to Townley; but he ascribes the law, in effect, to Power and Townley. For, he claims (see below) that Townley's hypothesis comes no earlier than September 1661, yet he reads the description of the experiment of April 1661, performed by Power and Townley to be an expression of Boyle's law (p. 482, also [a] p. 227a)! He admits that Power is rather "cryptic", but he still reads the law there. Also, he does not mention the possibility that though the Power-Townley experiments were concluded in April, 1661, the tables may have been included in the manuscript later on, and even the text may have been updated later on to show the influence of Boyle's writing of late 1661 and early 1662.[27] Yet, on Webster's own claims, one has to conclude, as I. B. Cohen concludes, that the law was discovered in April 1661 by Power and Townley, not in September 1661 or later by Townley! This, incidentally, covers entirely Hooke's claim to have discovered the law on August 2nd, 1661.

But even on Webster's understanding we must ignore Power's manuscript ─ for a while at least ─ as quite puzzling. Webster's most important section is his tenth, on "Boyle's experiments on the compression and dilation of air"; his major point (pp. 481-482) is contrary to his claim for Townley and Power; it is one which I would gladly endorse, and can only regret that later he rejects:

> It is probable that Boyle derived the law from his experiments on the compression of air, whereas Townley pointed out that it also applied to the experiment on expansion.

I agree, except for the expression "derived the law from his experiments", which at worst merely reflects a widespread prejudice (which on page 492 Webster accepts in part and rejects in part, but with no discussion), and can easily be translated into "confirmed his own law by experiment", or "conjectured and confirmed", or some such. But it seems that Webster himself cannot accept his own view, as it amounts to saying that it was Boyle who both first stated Boyle's law, and confirmed it for pressures above one atmosphere.

Webster ascribes the law to Townley on three conflicting grounds: first, that (Power and) Townley deduced the law from his (their) experiment ([a] p. 227(a), [b] p, 488, lines 6-8), second, that he made the hypothesis on the extension of Boyle's law to expansion and coaxed Boyle and Hooke to confirm it ([b] p. 488,

[27] Webster cites ([a], 227b) from Power's book a formulation of Boyle's law which amounts to saying, $p_1\ v_1 = p_2v_2$,. He takes it for granted that the tables and formulae from the book occur in the 1661 manuscript which Boyle saw. He has the choice of accusing Boyle of plagiarism. stupidity, or oversight, and suggests mild doses of each of these remedies. Evidence clearly indicates the opposite, no less Webster's own crediting Townley with incompatible contributions. Even the link between the tables Power produces and the version of Boyle's law which he concludes, has to be provided by Webster, by streamlining and correcting some "profound typographical errors" [Webster. (a)1 p. 227b), last paragraph.

lines I and 2), and third, that he clarified the meaning of Boyle's results, ([b] p. 487, line 19). There is no hint of a clarification anywhere, and no one before Webster ever made such a claim. But Webster even thinks "in Boyle's mind there still lingered the scholastic notion that condensation and rarefaction were qualitatively different" (lines 1 and 2 of the same page); needless to day, this scholastic view is still upheld in all standard texts on elasticity; what is of more concern here is that both Boyle and Power ascribe the same "scholastic"

notion to everybody, including Townley. Webster quotes ([b] p. 484), from the records of the Royal Society (Birch, ed.), the passage referred to by James and McKie; and quite a remarkable passage it is: it begins with a report of two experimental demonstrations of a weekly meeting of the Society on September 11, 1661, and ends thus:

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

In other words, on September I, 1661, Boyle already knew that the pressure of three atmospheres reduces volume to one third; hence, one might assume . with James (p. 226) that the experiment, reported in Boyle's *Defense,* when Boyle reports his first "delight and satisfaction" in confirming his hypothesis, came prior to September, 1661. I assume that Boyle had put together the results of his two experiments; if so, he had before him something like the following table:

| **Observations:** | **First** | **Second** | **Third** |
|---|---|---|---|
| **pressure, approx.** | **1 atm.** | **2 atm.** | **3½ atm.** |
| **volume, approx.** | $V_o$ | $\frac{1}{2}V_o$ | $\frac{1}{4}V_o$ |

(where observed pressure is the differential of height between the two columns, and observed volume is the height of air column in the closed tube). (The inaccuracy in one direction may be explicable by. reference to the escape of air which we know Boyle was sensitive to, but not in the other direction; naturally, the inaccuracy is in the right direction.) The table looks suspiciously like a confirmation of Boyle's law. One might ask, however, why didn't Boyle scoop and pronounce that hypothesis there and then?

Clearly, we have no evidence that he did not. And no evidence has been misread as evidence of absence of such a pronouncement ─ by James or anyone else. The records of the Royal Society, according to the statutes of 1663, clearly indicate preference for facts over theory and a taboo on reporting fact and theory in juxtaposition ─ indeed, they were put into two different books by 1663 (Weld p. 527). It is very likely that this is a compromise, that in 1661 they recorded facts alone.

Webster reports the three-atmosphere experiment of September 1661, adding (p. 484), "Already Boyle had an intuitive understanding of the nature of the relationship between the elasticity of air and its pressure ... "; he continues with the experiment with the tables of October 1661; he digresses to the details of the apparatus, and then plunges into the two-atmosphere experiment with

"The observation which Boyle notes with the greatest pleasure", concluding with "He now arrives at the following hypothesis". There is about one page between "Already Boyle ... " and "He now ... ", and I do not quite know if and how they connect.[28] If they do, Webster is mistaken; if they do not, he is bizarre. One way or another, it cannot be said that he shares his difficulties with his readers frankly, which is a conduct not uncommon amongst historians of science, and of which I have already complained extensively (Agassi, [a] section 3 and notes; where examples for this very misuse of "now" are given). Before leaving the chronological difficulty, let me only mention in haste, that on Webster's chronology, but not on mine, there is the problem of priority of Power and Townley of April 1661. Also, on Webster's chronology, and on Cohen's ascription of priority to Power and Townley, Hooke's claim (see below) that he worked independently on Townley's hypothesis in August 1661 is problematic, but not on my chronology. I shall come to this later.

Webster now arrives at Boyle's hypothesis following his experiment with the pressure of two atmospheres. The hypothesis is, the spring of air is proportional to its density. In Section 2 I have discussed in detail the relation between

$$\text{Pressure x Volume = constant} \qquad (2)$$

and

$$\text{Pressure = constant x density} \qquad (4)$$

and shown (4) to be slightly more general than (2). Since I consider my discussion on that point quite a trivial discussion of very pedantic elementary physics, containing only an elementary deduction and a trivial law

the weights of equal volumes of air under equal pressures are equal, (3)

I naturally did expect Webster at this junction of his discussion to congratulate Boyle on his statement (4) in preference to (2), especially since Boyle was ─ beyond any measure of doubt ever entertained by a historian of science ─ quite familiar with the definition of density, and since Boyle asserted (3) quite explicitly. Instead, Webster claims (p. 486) that

> it is by no means certain that Boyle realized at this time that (4) implied (2) ...

and after some further exposition Webster concludes (p. 486),

> Perhaps in Boyle's mind still lingered the scholastic notion that condensation and rarefaction were qualitatively different.

This is on the right track but nonetheless a howler and an injustice unusual even in the annals of the history of science. It is not a mere scholastic notion that strain and stress are different qualities. Moreover, Boyle did not have to assume difference, but merely avoid assuming identity! Strangely, it is because the above quotation is so unjust to Boyle that Webster who is usually well- disposed towards him, does not pursue the idea more seriously. He almost arrives at the idea that Boyle speaks of high pressures (above one atmosphere) and Townley

[28] To prevent sidetracking let me mention that Webster explicitly assumes, as I do too, that the relevant part of Boyle's narrative in his appendix is chronological; see [Webster, (b)1 p. 486, note.

of low pressures. He takes Boyle’s *Defence* to be chronological and puzzles that Boyle uses “expansion” only in the later part ─ yet he still claims (p. 486) that “expansion” means volume (rather than the opposite of condensation), even though in the passage in which Boyle first introduces the word (see above p. 219), he introduces the contrast between . "compression” and “spontaneous expansion”, inserting Townley’s contribution between them). All this Webster does merely in order to identify Townley’s hypothesis with Boyle’s law. He even notices (pp. 487-8) that

> Boyle derived the law from his experiments on the compression of air, whereas Townley pointed out that it also applies to the experiments of expansion.

And he corroborates Boyle’s testimony by Townley’s manuscript. Yet he denies that it was Boyle who discovered Boyle’s law. I must admit, however, that he has one piece of evidence: he quotes (p. 498, n.) a letter from Townley to Oldenburg, of 1672, where Townley too misreads Boyle.

> It was some satisfaction to me [says Townley] to find in the *Transactions* of July that the hypothesis (which Mr. Boyle was pleased to own as mine) about the force of air, condensed and [*sic!*] rarified, both succeed as well in deep immersions, as in those I made trial, and that it both administer now to the learned matter of further speculati6n, as formerly it did to me of writing some few things, (of which I then showed Mr. Boyle) ...

Clearly, however, this letter is a moving nostalgic reminiscence of a person who is already out of it all, not any careful testimony; no court, and no political historian, would accept such evidence; but historians of science often do, and show even worse credulity. Whatever is the case and the value of this letter, doubtlessly it must have been soon forgotten.

Finally, Webster views Townley’s letter just re-quoted as confirmatory of his own “interpretation of Towneley’s part in Boyle’s experiments”. And yet Webster rightly concludes (p. 492) (italics mine) that

> Boyle [said] that *the spring of air was proportional to its density*

whereas later on

> Towneley [said] that *the pressure of air was reciprocally proportional to its expansion*

which two quotes are inconsistent with Townley’s claim in his letter to Oldenburg. But I suppose I do injustice to Webster, because, finally, he attributes the law neither to Boyle, nor to Townley, much less to Townley and Power. He closes by saying that this summery

> leaves unanswered the question of priority of discovery and the correct title of the law, problems which are of limited importance compared with that of obtaining an accurate historical account ... ,

with which I fully concur.

Thus, Webster claims to have shown with his massive erudition that Boyle's achievement was "the climax of a cooperative enterprise" (p. 490); but I am afraid collaboration is not the same as multiple discovery; and "cooperation" is here ambiguous. Even with obviously collaborative and other interdependent enterprises going on in front of our own eyes, we go on attributing priorities and rewards for them of all sorts. To argue for the priority of one and then credit another's contribution is not to credit and then withdraw credit.

**10. Cohen's defense of Hooke's priority**

Let us now revert to t. B. Cohen, the contributor of the latest weighty comments on the situation. His paper. "Newton, Hooke, and 'Boyle's law' (Discovered by Power and Towneley)", is probably the last word on the present topic. Cohen ascribes to Webster the establishment of the following facts. Power and Townley postulated the reciprocity of pressure and volume and confirmed their postulate and had their results communicated to Boyle prior to Boyle's postulation of any reciprocity, let alone confirming it (p. 618). Cohen next accepts Webster's explanation of the alleged fact that Boyle omits reference to Power.

From this Cohen moves on to a discussion of Hooke's activities and Newton's comments on them. As it turns out, however, Cohen is in a similar predicament to Webster: whereas Webster wishes to defend the priority of Townley, and does defend that of Power and Townley, Cohen wishes to defend that of Power and Townely and does defend that of Hooke.

To Hooke, then. Cohen quotes Hooke to say

> that the Elater of the Air is reciprocal to its extention or at least very neer

which can be interpreted (correctly) as Townley's hypothesis, or (incorrectly) as Boyle's hypothesis, or as Boyle's law. Cohen interprets it as Boyle's law without hesitation: he goes on quoting Hooke to confirm the above quoted hypotheses by pressures higher than one atmosphere: without hesitation he simply reads both "elator" and "elastic power" as associated with "expantion" as well as with "compression", even though in Hooke "elator" is systematically coupled only with "expantion".[29]

Hooke's essay is the penultimate contribution to his *Micrographia,* 1665. His purpose in that essay is to aid astronomy: to explain the apparent disfiguration of the sun's and moon's figure at the horizon (discovered thanks to the telescope) and the observational errors of stellar locations due to atmospheric refraction. Briefly. the index of refraction of the air is a function of its density which diminishes with height, so that rays of light travel in curved lines ─ except for the zenith, of course. The other interesting aspect of Hooke's penultimate essay is relevant to astronomy and to other topics, but not very much to the present

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[29] Webster (b) has an interesting discussion on the history of "elator" and "elasticity"; section IV and Appendices II and IV. His discussion amply shows that "elator" refers to elastic behavior under low pressures, unlike "elasticity"; which is true for the later period, not for the period when it was introduced and used.

essay: it is the estimate, with the aid of Townley's hypothesis, of the height of the atmosphere.

Now, Hooke introduces his problem in a very straightforward manner; but from his very assault on it, he is strangely devious. To begin with, a definition ([Hooke] p. 219):

> By density and rarity I understand the property of a transparent body, that does either more or less refract a ray of light ... 1 call glass a more dense body than water ... because it refracts light more ... So to the business of refraction, spirit of wine is a more dense body than water …

This is Hooke's definition of density. The expression "I understand" signifies definitions in the purely verbal and arbitrary sense ─ at least in the works of Boyle and his contemporaries. When definitions are introduced as essential (and hence non-arbitrary), whether earlier (say, by Descartes) or later (say, by Newton) there is no use of such expressions as "I mean" or "I understand". And so, Hooke may define any word any way he likes. Yet, his definition is a bit odd, to say the least. After all the words "density" and "rarity" follow a standard use which Hooke employs even in these pages. His conduct may easily be explained, however, as an expression of his ambivalence towards expressing a hypothesis. For, evidently, his unstated but clearly indicated hypothesis is this: the refractive quality of air is a monotonic function of rarity in the ordinary sense; or perhaps even rarity is proportional to rarity. As it is a mere matter of choice of proper units to convert proportionality to equality, Hooke's hypothesis may perhaps be put as, "rarity is rarity". which sounds like a tautology but is not since the word is employed in two senses. Hence, the hypothesis needs a test. The test requires an estimate of the air's rarity. Hence, the interest in Townley's hypothesis which deals with rarity, not Boyle's which deals with density.

Before the detailed discussion, however, Hooke offers qualitative experiments ─ like Boyle before him, and like Faraday after him.[30] First, Hooke shows empirically that a solution whose concentration at bottom is higher than at top yields a curved path for a light-ray passing through it. Then he shows that a change in the density of air changes the air's refrangibility: he heats a glass ball, seals it, lets it cool, and uses i- as a lens.

The stage is now set for the study at hand, namely the employment of Townley's hypothesis for the estimate of the distortion of astronomical observations. We are now coming to Hooke's barometric experiments (p. 222),

> which experiments, because they may be useful to illustrate the present inquiry [*sic*!], I shall briefly describe.

Hooke's relevant texts (pp. 222-6) contain over one page (pp. 222-3) describing his experimental arrangement for the illustration or test of Townley's hypothesis, a page (224) offering the first and poor table, concerning *low*

[30] See Faraday, *Exp. Res. Electy.,* for example, Volume I, §704, where Faraday explains his design of a Voltaic electro-meter as rooted in his desire for increased accuracy, and §738ff., where qualitative experiments are discussed ─ and then reported ─ before quantitative details are introduced.

pressures, a page (225) of about three paragraphs which I shall soon discuss in detail, a page (226) of precise data concerning *low and high* pressures, and a little more ─ including one sentence which Cohen quotes and which has been re-quoted in the beginning of this section.

Hooke's first table of experiments relates to Townley's hypothesis, but it is problematic: when the pressure on a given quantity of gas decreased from 30 inches to 3 inches its volume increased not by 10 but by 15 *!fl.* From today's stand-point, with all the hindsight we normally amass, we can say for sure that the discrepancy was a result of a leak. But if we remember that Hooke wanted to publish responsibly ─ as he says (see below) ─ we may well understand his unease about not gaining enough recognition because of responsibility (as Galileo did before him, see note 2 and note 4 above).

One must, in simple human sympathy, notice the unease Hooke felt when he wrote his report.

> I had several other tables of my observations, and calculations which I then made; but it being a twelve month since I made them; and by that means having forgot many circumstances and particulars, I was resolved to make them over once again, which I did *August* the second 1661. with the very same tube which I used the year before, when I first made the experiment (for it being a very good one I had carefully preserved it:) And after having tried it over and over again ...

What does Hooke report in so much meticulous detail? Perhaps he means this. The experiments I am here reporting are of August 1662, and they are a repeat of experiments made twelve months earlier, *i.e.* August 1661. This is James's and Andrade's and Cohen's reading ot the above quotation. Now, the above quotation follows a table of *low* pressure experiments. All *low* pressure experiments have been introduced previously (p. 222) as merely variants on Torricelli's experiment, but immediately after the above quotation there is a brief paragraph opening with "the other experiment", following with one paragraph describing the apparatus, one final paragraph of explanation, and a table of *high and low* pressures. The final paragraph begins:

> But having (by reason it was a good while since I first made) forgotten many particulars, and being much unsatisfied in others, I made the experiment over again ...

And so, it seems, in August 1661, Robert Hooke made sophisticated quantitative observations of *both high and low pressures* and kept quiet until in step after small step Boyle, Townley, Power, and perhaps others improved their experimental techniques to cover the same ground. So say James and Cohen. Let it be so. Why do they express no puzzlement at such a silence? Is it because they are not puzzled? This is hardly credible.

There are a few points at issue here. First, the two experiments reported by Hooke, second the dating of them, third, their relations to theory, and fourth Hooke's relation to Boyle. There are two experiments: first a variant of Torricelli's, illustrating Townley's hypothesis and reported in the first and poor table and in the *second* part of the second and good table. The second experiment is referred to as "the other experiment" and is reported in the first part of the second table:

> I made the experiment over again and, from the several trials, collected the former part of the following table. .

So much for the *two* experiments. This is amply clear from the above quotation: Hooke has performed *both* experiments twelve months earlier! The question is, earlier than when? Twelve months between the two experimental sessions, or twelve months between the first experimental session and the writing of the first draft of a text published over two years after the second experimental session? There is even a wrong fullstop in the crucial passage.[31]

In my view, it was the first experimental session, not the second, that took place on "August second l66l.", and the second took place twelve months later. This becomes clear from the end of the paragraph concerning, and immediately following, the first and poor table of high-pressure experiments:

> And after having tried it over and over again; and being not well satisfied of some particulars, I, at last, having put all things in very good order, and being as attentive, and observant, as possibly I could, of every circumstance requisite to be taken notice of, did register my several observations in this following [second] table. In the making of which, I did not exactly follow the method that I had used at first; but having lately [*sic*!] heard of Mr. Townley's hypothesis, I shaped my course in such sort, as would be most convenient for the examination of that hypothesis; the event of which you have in the latter part of this last table.

Hooke relates quite a few interesting facts here. First he relates Townley's hypothesis concerning low pressures. Note, however, that in "this last table" Hooke refers not to the previous table, of course, but to the following table, namely to the next and last one. Yet, even though he is ultimately clear, his presentation is not straightforward and easy to follow.

Now, Hooke says he has "lately" heard of Townley's hypothesis. This "lately" must be between September or October of 1661 and the publication of the *Defense* of 1662. But when, more exactly? There is one indication that indeed Hooke's "lately" is in 1662: We know that Hooke performed experiments before the Royal Society on December 10th, 1662 on *low* pressures and on January 28th, 1662/3, on *high* pressures. It is quite possible that "lately" then means before Hooke was writing the details for the demonstration, i.e. not long before December, indeed not long after he was making his second set of experiments ─ in August 1662, twelve months after the first set of August 1661.

Cohen offers one clear-cut piece of evidence against this: he quotes (p. 618) Boyle's passage already quoted here (on pp. 223-224 above), and adds (p. 619a):

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[31] Note that Cohen corrects the misprint in Hooke's text.

> Boyle refers explicitly to Hooke's claim that when he had first heard Boyle speak of the "proportion" supposed by Towneley, he had stated unequivocally that he himself "had the year before (and not long after my publication of my pneumatical treatise) made observations to the same purpose which he acknowledged to agree well with Mr. Townley's theory".

In other words, Cohen thinks that here Boyle repeats Hooke's claim that in August 1661 Hooke had invented and tested Townley's hypothesis! Cohen even says "explicitly" and "unequivocally", which is not fully documented to everyone's satisfaction. And he accepts the "agree well" which Hooke does not accept about his own first set. Between James's calling Hooke a liar and Cohen's trusting a vague report as "explicit" and "unequivocal", there is a lot of room for maneuver. Why does Cohen stress this so much? How could it escape his notice that Boyle was talking, not about Hooke, but about Dr. Power, and that Gerland said so explicitly (see note 22). Perhaps, his misreading fits the generally accepted misreading so well that he could not doubt it; but I really do not know.

Finally, as James has noted, Hooke makes it clear that the first table is not presented as a verification of any hypothesis, that it was performed before he had heard Townley's hypothesis. Hooke also says that his second table is a test of that and of another hypothesis ─ the other hypothesis remaining both unstated and unnamed! Also Hooke tells us that his oldest data concerning the other hypothesis ─ "the other experiment" ─ were good from the start, yet he repeated them now! Moreover, Hooke presents the experiment to test Townley's hypothesis as a mere variant on Torricelli's but stresses that "the other experiment" stands by itself. That is where the action lies.

To put it differently, I think that in August 1661 Hooke had both Boyle's and Townley's hypotheses; that his test of Townley's hypothesis was very unsuccessful and so he claims no priority for it, but that his test of Boyle's hypothesis was successful and so he claims priority for it. In August 1662 or so he repeats his test of Townley's hypothesis and now gets excellent results.

In checking Hooke's text carefully, I find very few unclear or inconsistent expressions, and the rest clearly disagreeing with Cohen's reading and agreeing with mine, except for one passage which seems to go the other way. Between the poor *low* pressure table and the good *high and low* pressure table there is the page (225) containing one paragraph on *low* pressure in both tables, ending with Townley's hypothesis, and two paragraphs or so on the *high* pressure part of the second table, one of these describing the instrument and such, and the other commenting on Hooke's experience and on the table. The first of these two paragraphs (225) ends with, .

> and by making several other trials, in several other degrees of condensation of the air, I found them to exactly answer the former *hypothesis.*

At least *prima facie,* the words "answer the former *hypothesis"* refer to "Townley's hypothesis" in the previous paragraph, the word "answer" means agree with or confirm; meaning that Hooke uses this descriptive phrase to name the law "pressure x volume = constant for all pressures, above or below one atmosphere" .

It is not incumbent on an interpreter to offer a view in accord with every word in every document. This may be impossible, at least since records are not all reliable to the exclusion of all slips and errors ─ not to mention confusions and ambiguities. The famous economist and philosopher of science, J. M. Keynes, has declared the proper criterion in a very well-known passage *(Treatise on Probability,* Part III, Chapter 23), in which he wholeheartedly endorsed R. L. Ellis's reading of Bacon's works, in spite of its having left many passages obscure or inconsistent with itself: that interpretation is to be preferred which makes better sense of more available passages. Possibly, however, one may resolve the above difficulty by reading the word "answer" not to mean "confirm", but to mean "in accord with" or "in harmony with", similar to the use of the word "answer" in music, in the analysis of a melody. Possibly, and I am inclined to think so, this is a reference *not* to the expression "Townley's hypothesis" of pressure, which reads:

> The other experiment was, to find what degrees of force were requisite to compress, or condense the air into such or such a bulk.

It may sound strange to say that the table answers this hypothesis rather than this question by an hypothesis, but this is true to Hooke's style;[32] other writers of the same group used even worse styles; see Isaac Disraeli's delightful and thoughtful "Calamities and Quarrels In the Royal Society".

One more discomfort: during the whole discussion there is no mention of Boyle. James asks (p. 269) why, and answers, because no one thought much of it all. As evidence he lists Hooke's flimsy manner of claim staking. One might indeed disagree: the importance of Boyle's work was noticed almost at once. Besides, James is partial in viewing Mariotte, but not Hooke, a plagiarist; all points on which he claims ignorance for Hooke are valid for Mariotte as well. Moreover, Hooke mentions Townley's hypothesis by name, but does not name Boyle's hypothesis. Surely he did not think Townley's work more important than Boyle's. Boyle is mentioned on p. 55, in reference to an insignificant contribution he made in his *Discourse on Colours,* and on p. 227, immediately after the discussion just reported, in reference to an even less significant contribution of his, in his (by no means original or interesting) estimate of the relative densities of mercury and air,

> the most accurate Trials of the ,most illustrious and incomparable Mr. Boyle published in his deservedly famous pneumatic book ... .

In my opinion Hooke does not label the counterpart to Townley's hypothesis, hoping it be labeled Hooke's hypothesis or law. He hopes so, both because he

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[32] Similarly Hooke's use of "hypothesis" in *Micrographia,* p. 67, is problematic - see Sabra, 328. Note also that the statement of Descartes' rejected hypothesis in *Micrographia* on p. 60 is very clear, as is the one on p. 62; Hooke's hypothesis on p. 64 is labeled unabashedly "short definitions"; the "hypothesis" on p. 67 may well be this "short definitions", but the clarity of the matter is not exactly perfect.

establishes it empirically so much better than anyone else, because he thought about it first, and because his delayed publication was rooted in his seriousness and pedantry. Note that the rule, the first to publish is credited with priority, was not yet entrenched, and even if it were, it was supposed to pertain to the verification of a hypothesis. For example, we call the law Coulomb's which was first announced hypothetically by Franklin and Priestley! And Hooke could claim to be the first careful, and hence proper, verifier! Indeed, Cohen suggests (p. 619b) that this is Hooke's priority.

As to the homage, it shows uneasy feelings; as if to say, I do not wish to belittle Boyle, and his name is secure anyway but mine is not yet, *etc.* The unease about colors, incidentally, on p. 55, is smaller, because it is less clear that Boyle deserves mention or Hooke claims priority (see [Sabra] pp. 321-3 and 328).

The very fact that Boyle's hypothesis is not stated means little: Townley's is not stated either; I have explained here, and elsewhere, how the tradition reigned of alluding to but not stating hypotheses. The very fact that Hooke fails to name Boyle’s law because it ought to be named after Hooke himself indicates humility ─ a humility first shattered by Parkinson who called his own law Parkinson’s. Before that every writer who wished his name immortalized was left to find a device to indicate his pleasure, and the claim-stakers are no exception. Thus Planck staked his claim on his scientific autobiography, not in his original and trailblazing papers.

Perhaps I should not make much fuss about such matters. But perhaps I shall be excused as providing some counter-balance to what Andrade says, and Cohen quotes approvingly,

> Hooke, who always expressed the greatest veneration for Boyle, would never have published his [priority claim] if it was likely to give pain to, or be disputed by, Boyle.

Cohen quotes two arguments from Andrade, one which Andrade quotes from L. T. More (the biographer of Boyle), and one of his own. Andrade’s first argument is that Hooke would not hurt Boyle. The other is that Boyle gives priority to Townley and quotes Hooke to say he had had Townley’s hypothesis before Townley. My view accords with this, but not with identification of Townley’s hypothesis with Boyle’s, of course. L. T. More’s argument is that Boyle’s law is the only quantitative law Boyle ever studied, so it is not in his character. First, this is false: Boyle had the quantitative law of proportionality of heat increment to the increment of pressure times volume ─ quite a quantitative idea ─ and he begged people to work on it. Second, there are few quantitative laws anyway. How many quantitative laws did Ohm produce? Or even the mathematical Fourier? Or even Van der Waals? And was not Edison’s discovery of thermionics out of character? And Faraday’s metallurgy? Galileo’s astronomy or Pasteur’s biology were out of character, the one having had a mechanical and mathematical interest, the other chemical. What do we know about character! Not only did Boyle have exceedingly many scientific commitments, but some of these were pressing and he could not even discharge them. Cohen's own argument is the best of the lot: Hooke's greatest enemy, Newton himself, acknowledged Hooke's priority. But though what Cohen says (p. 620),

> any statement of Newton's giving credit to Hooke for any discovery, is to be taken very seriously,

is unquestionable, he shows not that Newton gives "credit to Hooke for any discovery", but that he credits Hooke with the table of pressures and volumes representing empirical support for Boyle's law. Indeed, I think Newton is correct in crediting Hooke both for unusual precision and for the combination of two tables in one: he, Newton, says ([Cohen] p. 620),

> and Hooke proved by experiment that the double and treble weight compresses air into the half or third of its space, and conversely,

and nobody before Hooke, I agree, did just this just as neatly. But this is a matter of a neat experimental proof.

And yet, the fact is, Newton does not mention Boyle. It is hard to discuss such matters. Clearly, as the quote is from a manuscript one cannot take it as definite. Cohen quotes an even less definite, but more decisive, passage from a manuscript of Newton ─ less definite because "juvenile", and more decisive use it does "credit ... for any discovery", only it credits Townley: he, young Newton, says *(loc. cit.),*

> Mr. Townley's hypothesis is the dimension (or expansion) of air is reciprocally proportional to its spring (or force required to compress it). By Mr. Hooke's experience …

(Why not refer to Newton's mature passage which Gerland mentions (see p. 212 above)?)

So much for Cohen's argument. He offers Newton's remark with his own interpretation. I shall now offer a few alternative interpretations of the same passages. First, one might say, Newton read Hooke here the way Cohen reads him, and entirely obliterates any difference between high pressures and low pressures. In which case Newton is scientifically correct, but historically overlooks a difficulty which once existed but had been overcome. This happens all the time (see [Agassi (a)] Section 16, *The Difficulty of A voiding being Wise After the Event),* and so its being exemplified in a private manuscript of a young man of scientific genius and little historical interest is quite understandable.

Another interpretation rests on the fact that Newton mentions no name, as Cohen notes when discussing Boyle's law in the *Principia* (Bk. II, Prop. 23 and *Scholium)* ─ probably from reluctance to name Hooke. This reluctance Cohen explains as reluctance to credit Hooke with priority. It may also be interpreted as reluctance to enter other people's priority disputes. Still the absence of Boyle's name is puzzling and consistent with the previous manuscript passages. It is quite possible, indeed, that Hooke's second table did so impress Newton that he felt the need to acknowledge to Hooke priority over Boyle's hypothesis (not Townley's) which is more than public opinion would permit; that Newton would not launch a campaign for Hooke's name is quite understandable: much as he wanted public acknowledgment for himself, he was very reluctant to campaign even for his own priority.

One may offer quite a different interpretation, taking the following very seriously. With the discovery of Torricelli and with Pascal's barometry it became clear that the atmosphere is finite. Boyle compares it to a sea, we remember, assuming that it has a surface with waves and ripples and tides. Hooke was the first to use Townley's hypothesis to estimate the height of the atmosphere. He concludes, from simple calculations, that the atmosphere is infinitely high. This, of course, utilizes Galileo's theory of gravity; it assumes pressure to be a constant with respect to height. Newton assumed gravity to diminish according to the inverse square of the distance and elasticity according to the inverse of the distance, thus achieving a theoretical cut-off point, a theoretical limit to the atmosphere, where the two forces balance.

Now, consider what Newton should have acknowledged to Hooke. Today we acknowledge good first shots even when they are misses. Up to the end of last century, this was not acceptable, except for some very eccentric writers. But one could say that Hooke was the first to attempt to apply Townley's hypothesis to the estimate of the height of the atmosphere. Newton did not. His acknowledgment to Hooke, then, is grudging and far from generous. Is it also over-generous in the wrong direction as a compensation? I do not know if and how one can study all this. Nor is it clear how much Hooke's application of Galileo's gravitational theory to any distance provoked Newton to think that the theory needs modification, that the moon may be constantly falling towards the earth, and so forth. Did Newton owe anything to his reading of Hooke? Did he know about his debt? Was he ambivalent about it? This is all open to further exploration.

The last interpretation I wish to mention is perhaps the sharpest — it attributes to Newton high sensitivity, intellectual and psychological. Boyle, we remember (see p. 219 above) saw in air both strain and stress — extension causing stress. Now Hooke's extension of the atmosphere indefinitely puts an end to this and views all extension as expansion releasing prior compression. If so, then there is room only for Townley's hypothesis, none for Boyle's! And Hooke, indeed, has in his second table pressures below and above one atmosphere!

And so, perhaps it was the desire to avoid mentioning Boyle's error and a subsequent absurdity concerning priority which has further complicated the picture!

Newton, then, assuming the elasticity of air to be a result of sheer expansive force, and assuming gravity to be variable, tried to estimate the height of the atmosphere. Here he solved a few interdependent problems quite satisfactorily, as if by miracle. The weakest point in all this complex reasoning is the application of Townley's law to very low pressures, particularly since it leads presumably to the difficult conclusion of unlimited atmospheric height. Here Newton accepts Hooke's experimental evidence as a crucial factor.

Hence Newton's ascription is even historically very accurate and uncontested. Here is the relevant quotation in full:

> In just the same remarkable manner [air] rarefies and is condensed according to the degree of pressure. The whole weight of the incumbent atmosphere by which the air here close to the Earth is compressed is known to philosophers from the Torricellian experiment, and Hooke proved by experiment that the double or treble weight compressed air into the half or third of its space, and conversely that under a half or a third or even a hundredth or a thousandth part of that [normal] weight [the air] is expanded to double or treble or even a hundred or a thousand times its normal space, which would hardly seem to be possible if the particles of air were in mutual contact; but if by some principle acting at a distance [the particles] tend to recede mutually from each other, reason persuades us that when the distance between their centres is doubled the force of recession will be halved, when trebled the force is reduced to a third and so on, and thus by an easy computation it is discovered that the expansion of the air is reciprocal to the compressive force.

This discussion is particularly enlightening when one considers the following facts. If Newton's theory of inverse distance force is true, then Boyle's law is absolutely true for any degree of rarefaction and for any degree of compression-short of the ones involving sub-atomic distances (which are *a priori* excluded anyway). Was Boyle's law considered absolutely true? Newton seems to ascribe to Hooke the affirmative answer. E. Mach, ([b] p. 14) says "already Boyle himself viewed [Boyle's law] as not entirely correct". If both Newton and Mach are historically right, and I think they are, then, doubtless, Newton's ascription to Hooke becomes very powerful, especially in an era when an error was to be overlooked and certainly not to be used for credit.

**11. Conclusion**

My interpretation, then, leaves a few gaps, particularly concerning a sentence or two in Hooke's *Micrographia,* and perhaps young Newton's understanding of Hooke. But tilt: canons of historical interpretation cannot be as strict as those of natural science: there are any number of reasons why Boyle, Hooke, or anyone else may have been unclear. How then should one piece together an interpretation? The usual rule is, one takes the interpretation which makes better sense of more historical material. The question remains, what do we mean by better sense? I have complained before that unlike all other historians, historians of science too often fall prey to the misconception that an interpretation rendering the words of an old master in better accord with today's science text-book make better sense of it. This is a highly un historical criterion.

Yet, by this criterion it is easier to ignore the difference between high and low pressures, and conclude erroneously with Webster and Cohen that priority goes to Power and Townley. Their view is marred by a few difficulties, some of which

Webster glosses over, some of which Cohen discusses. Indeed, I understand that Webster may endorse the criterion in question, and refuse to see the theoretical difference between high pressure and low as anything other than a remnant of scholasticism, and leave it at that. But Webster himself stresses that experimentally high pressures were easier to measure. It is this difficulty that Cohen tries to remove.

Cohen's last paragraphs indicate that he is still not satisfied. He claims there that Boyle had extended the Power-Townley experiments of low pressures to high pressures ─ which is a valid though false conclusion from Webster's faulty chronology, and which solves some difficulties for Webster. But this is not all; for, Cohen parenthetically adds (p. 620b):

> The question whether such an extension [as Boyle's] be significant or not depends on whether the limitation in the first instance to pressures less than one atmosphere resulted from a failure to devise an instrument to make a test of this additional range or from a psychological inability to recognize that the law for the rarefaction of gases might equally be a law for compression of gases.

Cohen kindly refers to the present work in an earlier draft (actually an appendix to my doctoral dissertation) and reports that I discuss this question. I do (except that I would omit the word "psychological"). But the question as Cohen puts it, implies my view, not his. He is here seemingly unnoticingly inconsistent. His seeming inconsistency is easy to repair, but at a very high cost. For, my view rests not only on the historical supposition ─ which even Webster endorses ─ that Boyle's *Defense* is chronological, but also on the historical claim that *high pressures were easier to examine than low pressures* ─ a fact which Webster stresses and explains at length. If Cohen wishes to stick to his view, if he would retain the question he kindly attributes to me, and if he would rectify the impression that he is inconsistent, then he has to deny that the low pressure experiments were harder to observe than the high.

Moreover, as no previous writer has noted, Townley's first idea concerning *low* pressures, of which Webster speaks at length, is the one which Boyle converted to an idea concerning *high* pressures so as to examine the two atmosphere cases ─ which is the easiest, simplest, most obvious, and the first real success. Then Townley (and probably others) reverted to Townley's original idea, and applied to it Boyle's improvement. This inner logic of events is my chief argument. Finally, may I stress, with Mariotte's contribution one major factor in this logic has disappeared: Mariotte's measurement of high pressures and low pressure is of practically the same case (see page 200 above).

And so, my view is of collaboration rather than of simultaneous discovery. I do agree that some measure of simultaneous discovery is necessary. We all rediscover constantly parts of our heritage which have not been articulated to us, and what young learners do in later generations, contemporaries often have to do in the course of their research. But it is the act of collaboration which is both more important to notice, and more difficult to sort out. For example, Mariotte's contribution has thus far not been appreciated by most historians of science.

Boyle, following Bacon, has instituted rules of crediting priority and public recognition of other contributions to science as a reward and an incentive. The infancy of modern science was much more problem-ridden than many would acknowledge, and Boyle's rules may have made all the difference between its survival and infant-mortality. The success of Newton, and his middle-class attitudes (so well discussed by Augustus deMorgan), has led to an exaggeration and a perpetuation beyond reason.[33]

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[33] For further detail see "Fighting the Philistines" *Philosophia,* 4 (1974), pp. 163-201, section II and final paragraph.

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