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THEORETICAL BIAS IN EVIDENCE : A HISTORICAL SKETCH

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0. An Introductory Apologia

All my efforts to present the following historical material without any complaint made friends and colleagues misread and express puzzlement at what I intended to say. The kind comments from the editor of this volume on the final draft finally made me decide to declare my hand clearly as follows.

The studies of theoretical bias in evidence are these days developed by many clever psychologists, social psychologists, and philosophers. It therefore comes as a surprise to realize that most of the material one can find in the up-to-date literature repeats discoveries which are due to the heroes of the present sketch, namely Galileo, Bacon, and Boyle; Whewell, Duhem, and Popper. We may try to raise scholarly standards by familarizing ourselves with their ideas and studying them with a little appreciation.

A little familiarity and a little appreciation, not consent or assent or agreement, is what I seek : my disagreements with each and all of these writers are to be found in other writings of mine, but here I wish to direct the attention of the learned reader to the overlooked classical writings and invite her or him to throw a new glance at them (see bibliographic note at the end).

The main hero of this sketch, however, is Sir Francis Bacon. The status of Bacon as a leading thinker of the eighteenth century was quite exaggerated and invited the debunking he received in the nineteenth century. The chief editor of his works, Robert Leslie Ellis, began his work as an act of hero-worship and ended by condemning Bacon as an unoriginal thinker, a plagiarist, and an author who violated his own principles when he described the process of induction (since he permitted the formation of hypotheses). Justus von Liebig exposed his plagiarism, ignorance, gullibility, and scientific incompetence. Severe as Liebig's judgement was, his strictures were just and unanswered, and so his is the last word, all the many later works on Bacon notwithstanding. And though it is dangerous to cite Bacon to support any interpretation of his philosophy — since he was so often flagrantly inconsistent — there is little doubt that he made many discoveries concerning perception. In particular, he knew the difference between sense illusion and theory-laden observation whose error is theory-based; he knew the difference between theory-ladenness on account of some very general features of our perceptual-cum-cognitive apparatus and theory-ladenness on account of a specific theory, such as Aristotle's or Gilbert's. And he observed both the impact of a specific theory which is a metaphysics, which makes one observe everything in its terms, and the impact of specific local hypotheses which refer to a small sector of our experience, and yet which make one see there only evidence corroborating it and ignore or dismiss all evidence to the contrary. When one notices that these facts still occupy the writings of the latest commentators on the matter, one cannot but gasp in admiration.

Nor is it a matter of sheer historical curiosity. Whewell refuted Bacon's hypothesis that we are captives of our hypotheses, by arguing that critically minded science is the critical test of theory, so that we can employ hypotheses without being imprisoned within their frameworks. This way a new vista opened for philosophy. And it was, I think, the evolution of Whewell's philosophy that has led to Duhem's conventionalism and instrumentalism and to his claim that a new framework does not supersede the old one, which claim is these days hotly debated and is known by an oxymoronic anachronistic label, as the Kuhn-Feyerabend incommensurability thesis.

The rest of my complaints are not important for the avoidance of confusion, so I will drop them. Let me repeat, my aim is to present the material, which is still topical, with a historical perspective; complaints are better overlooked whenever possible.

1. The legitimation of science : Bacon versus Galileo

The claim that empirical evidence carries with it theoretical bias was published, more or less simultaneously, by Sir Francis Bacon and by Galileo Galilei, though priority should go to Bacon for whom it was a very central point which he elaborated upon in all of his writings. The claim was made for the purpose of debunking the inductive basis of traditional theories. Every theory can be inductively based on evidence which is biased in its favour. The bias in favor of a theory is given both in the choice of evidence as significant and in the interpretation of the evidence in the light of the theory. The claim is dual. First, we use a theory both to decide which facts are significant, and to interpret these facts. Second, presenting a series of such interpreted facts amounts to no more nor less than a round-and-about way of presenting that theory. It is intuitively obvious that this can be done and it is both intuitively and logically clear that the support a theory received from such evidence is only circular and so not very convincing: take two competing theories and a given pool of information and use the facts twice, once to support the one theory and once to support the other. You will see that competitors disagree as to what facts are significant and what not, that their disagreement is rooted in their initial choice of theory, that each of them supports his own theory by facts, and that they have achieved a stalemate. This fact will convince you of the truth of the Bacon-Galileo thesis that facts are theory-laden and so are biased testimony and so are invalid.

The Bacon-Galileo thesis is repeatedly discovered by a number of philosophers and social scientists from different disciplines. Each generation sees the thesis ascribed to some different thinkers. These days it is most often ascribed to Festinger, but things are changing. The ascription is often to slight variants of the Bacon-Galileo thesis. We may therefore prefer to leave the thesis and look at the facts of the matter, as was done in the end of the previous paragraph. Except that the fact, as presented in the previous paragraph, is also theory-laden. Hence, we may have to live with the existence of different variants of the Bacon-Galileo thesis and only attempt to observe the significance of the differences, so as to be able to ignore variants whose difference do not make much of a difference, to echo a dictum by William James.

The major difference in variants of the Bacon-Galileo thesis is the one between Bacon and Galileo. Bacon and Galileo said, if one has a theory it biases one's perception; hence, they said, one should take care to approach the facts with the right theory. But Bacon was convinced that the right theory must be properly based on facts. He therefore claimed that one's very first scientific act should be the observation of facts with no theory in mind, the uninterpreted observations. These, of course, would be unordered as to their significance and unclassified — just a heap of observations. This looked to Galileo to be a monstrosity and he was convinced that without geometry one cannot observe facts — one might as well see the moon jump from one roof-top to another like a cat while one walks in a moonlit city street — and geometry must, therefore, precede facts and thus not be founded on them, but on a priori intuitions. The idea that space-time intuitions and the law of causality are the framework preceding all experience was also Kant's strongest case against empiricism.

The discussion of science, between the early seventeenth century and the early nineteenth century was very general and so limited to more or less this point. The center of debate was epistemological : how is knowledge justified. The apriorists began with the justification of the most universal intuitions and the empiricists with sensations as the most basic observations. These basic observations — sensations or sense data — were deemed not biased by any theoretical basis. In particular John Locke and his followers attempted to present sensations as not dependent in any way on the validity of Euclidean geometry, and one of them — David Hume — even questioned this validity. The apriorists, on the contrary, insisted on the need for an a priori valid framework to insure that the theoretical bias of our observations is innocuous. Science, as usual, lies in between the two extremes : in science sensations are seldom mentioned and the framework of science is taken for granted when experiments and observations are reported in the literature of empirical science.

2. The scientific tradition since Robert Boyle

The tradition which was most strongly represented in the literature of empirical science was based on opinions of neither empiricist Bacon nor apriorist Galileo, but skeptical Robert Boyle. Robert Boyle's philosophy was elaborate, detailed, eclectic, and very very famous. Most of it is intentionally not relevant to the point at hand, which concerns techniques of reporting scientific information in the learned press.

Boyle decreed a few very simple rules. They were endorsed by the Royal Society of London and its daughter societies and so were absorbed into the ideology and the practice of the scientific tradition — though the traditional standards are not always rigorously applied, with results that are at times happy and at times regrettable.

The first claim of Boyle was that it is only dogmatism to ignore data only because they are interpreted in the light of an objectionable theory, and that the dogmatist is the loser. It is a challenge for one who deems data biased to couch them differently. This is Boyle's principle of methodological tolerance. In particular, said Boyle, when he interpreted the elasticity of air as caused by springs, he was not using the established theoretical framework. But since from the established theoretical framework one has to explain the elasticity of springs, the reduction of the elasticity of air to that of springs is progress even from the viewpoint of the establishment (the reduction of two difficulties into one).

Once theoretical bias is so legitimized, the problem arose, what is theory and what is fact ? To emphasize the import of this question, let us notice that to Pierre Simon Laplace the certitude attained by Newtonian mechanics seemed so perfect that he unhesitantingly ascribed to it the status of a fact of nature. True or false, certain or doubtful, we feel, it is a theory, not an observed fact. If we insist it is a fact, then we still wish to know what fact is observed, what not.

The intuitive tendency is to say, sense data are observed facts. Let that be so. It is irrelevant to our purpose. Sense-data may be the ultimate basis of all scientific theory, since theory is based on fact, and fact may be uninterpreted or part theory part uninterpreted fact, with the theory part based on facts which are either uninterpreted or partly interpreted and so in need of further foundations. But once we agree that the scientific empirical literature reports interpreted observations but not theories and not sense-data, we want to demarcate them.

Boyle demarcated them as follows :

(B1) Observation reports are statements which eyewitnesses can report on the stand.

(B2) To count as scientific they must be reported at least in two independent reports and must be declared repeatable. And the bonus of having the status of a scientific observation is declared as follows :

(B3) In a conflict between theory and observation, observation always wins.

It may be observed that the demand for repeatability was made by both Bacon and Boyle as a part of their hostility to esotericism — especially the esotericism of alchemy; it was also made by Galileo as a part of his demand for credibility. Yet Galileo explicitly rejected Boyle's Rule (B3) and expressed his profound admiration for Copernicus who refused to accept the evidence from Mars's brightness which failed to fit into his system. Clearly, Boyle's Rule (B3) was essential for him as an expression of his empiricism : hypotheses are doubtful but observations are not. Yet he knew that this means theoretically unbiased observations, so that he granted ordinary scientific observations no more than moral certainty and characterized them morally, not philosophically, by relying on court procedures. He also knew that the claim for repeatability can never be made by an eyewitness.

Court procedures in Boyle's time were not sufficiently clear to warrant Boyle's reliance on them, since in his days witchhunts were quite common and he opposed them as a matter of course. Yet his idea was adopted by courts all over the civilized world, so that eye-witness reports were supposed to be not theory-free but as straightforward as to count as unproblematic, and courts also demand, to this day, that when emphasis on repeatability is essential, witnesses count as expert witnesses, not as eye-witnesses, so that their status is different. (They can be countered by experts testifying to the contrary.) This seems to settle matters for most court procedures, but not for science. At least the generality of a generalized observation must remain clearly hypothetical. Hence, Newton felt the need to add to Boyle's rules one more :

(N) When refuted, a generalization of an observation should be qualified and endorsed in its new qualified form.

This is a very important rule, which does indeed give a sense of completeness to scientific procedure. Yet, like Boyle's rules, it was hardly noticed by philosophers. The reason is apparently no more than a historical accident.

As long as the controversy between philosophers centered on the means of justification of science in general, neither Boyle's nor Newton's practical legislation mattered much, since the debate was on a general matter of principle whereas the rule came to distinguish in practical scientific affairs between the admissible and inadmissible. For a simple instance, Boyle demanded that every new fact be published with no further ado — if it passes his criteria, of course. As to theoretical papers, how much they had to be based on fact was never determined, but which facts may be used for or against a theory was determined by Boyle and Newton.

3. The rise of modern methodology : William Whewell

The picture altered when Newton's theories received the status of established unalterable truths. And with that came their empirical justification and thus, as Laplace observed, empiricism won over apriorism. The picture altered again when Newton's optical theory, his corpusclarian theory of light, was deemed superseded. The date for this event is usually declared to be 1818, though it is hard to see how at all this can be precisely determined since throughout modern history some significant thinkers sided with waves and some with particles.

When the Newtonian optical theory was deemed rejected and the Newtonian mechanical theory, especially his theory of gravity, was upheld, better criteria than either empiricism or apriorism were urgently required and had to be devised; the old ones were too general. In 1830 Sir John Herschel tried to sharpen Bacon's ideas so as to be able to show that the data on which one of Newton's theories rested were uninterpreted and those on which the other did were interpreted : and, we remember, according to Bacon only uninterpreted data were kosher. And Herschel's work was not taken to be a success.

Enter Dr. William Whewell. Under the influence of Immanuel Kant he declared all data interpreted, since they are couched in the language of space, time, and causality. Also, Whewell himself performed observations to test Newton's theory of gravity on earth, and he knew how sensitive the outcome of an experiment is to the assessment of space-time coordinates. Nothing is easier than to secure success in such experiments than by the use of the tested theory in order to assess coordinates. Hence, Bacon's strictures were certainly valid.

How then do we distinguish valid and invalid data ? Why was the empirical support of Newtonian optics non-kosher yet that of Newtonian mechanics quite kosher ? This was Whewell's chief question.

Given that in every stage of scientific progress there are facts and theories, Whewell claimed the following :

(W1) All the facts are theory-biased, but not all are deductively explained.
(W2) Science attempts to invent new theories which explain some facts and some theories.
(W3) Tests subject theories to risk of refutation, and usually they refute them.
(W4) When a theory withstands a test, both new data — the result of the test — and the validity of the interpretation of the data obtain : the theory is verified.

It is clear that theory-bias is here a matter of degree. It is one thing to say that no observation is free of theoretical bias, and another thing to say that an observation is generated by a theory. We may be using a theory when we observe a fact in an unscientific context, and this may well be the reason for the invalidity of our observation. But we do not usually attempt to see the facts we see, and least of all do we make intellectual efforts when observing. Nor are we aware of the theoretical bias we employ unless it is pointed out to us. In science things are slightly different. The stars we see with our naked eyes with no effort are described in a star catalogue in a manner not available to us unless we are scientifically trained. The more advanced observations invite more intellectual effort. The importance of the claim that we interpret our observations whether we like it or not is that their use as empirical foundations of theories is suspect. The importance of the claim that the more advanced theories are more interpretative makes their empirical foundation all the more suspect. According to Whewell, only by severe tests leading to new facts can we allay this suspicion.

The crowning success of Whewell was his ability to show that Newtonian optics was not risked by tests, that it was repeatedly modified ad hoc in order to accomodate new facts; whereas Newtonian mechanics was severely tested and came out of the tests most successfully, enriching our stock of empirical knowledge in the process.

4. The end of finality in science : Pierre Duhem

Whewell's marvellous edifice collapsed when Newtonian mechanics was superseded. Before that it was found wanting by Pierre Duhem. Before the end of the century Duhem argued that all scientific evidence is theory-laden and that therefore the confirmation it offers to theories is useless. Duhem inverted every point Whewell had made.

(D1) Theories serve as classifications of diverse items of factual information by deductively incorporating them; but theories do not explain, since explanations are realistic and thus have metaphysical import.
(D2) Classifications are improved so as to accommodate ever increasing numbers of items of factual information.
(D3) Classifications are not risked by tests and so cannot be confirmed.
(D4) The incorporation of a new prediction into an old classification is done tentatively, and finally reaffirmed only when the prediction is verified. Otherwise the incorporation is deleted and instead of it a limit to the applicability of the classification is recorded. A modification is invited to the existing classification with the aim of incorporating the new recalcitrant item of information.

The fact that a piece of scientific evidence is theory-laden and that the theory is open to modification meant, to Duhem, that scientific evidence, too, is open to modification. This naturally incorporated and extended Newton's rule (N) : a refuted generalization is not rejected but modified. Since evidence is theory-laden, all theories we have are operative in any new prediction. Hence, when the prediction is refuted we do not know which of the various theoretical items we employ is limited. Hence we do not know which of them invites modification. In particular, at times we may want to modify different parts of our theoretical apparatus in order to overcome a given limitation.

The refutation of a prediction does not refute a theory; hence it cannot confirm its competitor. The experiment which refutes a given theory and confirms another is known as a crucial experiment. Whewell taught that by proper confirmation we verify a theory. Duhem denied that. Hence a crucial experiment — as a verifyer — is impossible.

(Duhem was aware of the fact that crucial experiments were performed repeatedly; what he denied is not the fact but its theoretical bias in favor of verification and refutation; he rejected both. This is regrettably often ignored these days.)

Another defect in Whewell's theory was bridged by Duhem. Whewell never explained the presence of unexplained facts. That some facts could only be discovered by tests was well accounted for by him, and he emphasized that but for these theories these facts would remain undiscovered. But how can there be facts not due to tests ? Duhem had two kinds of facts, those given to common sense and those which are part-and-parcel of science. Commonsense facts are crude, free of theory, and final. They are, however, for ever extra-scientific, he said. Scientific facts are precise, theory-laden, and therefore modifiable. Was this remark, however, theory or commonsense ? Duhem's view of commonsense is not commonsense : we all know that commonsense is never so stubborn. Duhem's view is a theory, and it cannot stand as it is : indeed it has been subject to quite a few modifications.

The hardest aspect of Duhem's theory, however, is its place along with classical empiricism and apriorism. Whewell, we remember, was an empiricist, whose chief merit is that by stressing hypothetico-deductivism he moved from the generality of the empiricist philosophy of science to specific historical examples of progress in the empirical sciences. His major modification of empiricism was his rejection of the standard empiricist claim that empirical evidence not theoretically biased is possible. He thus sounded problematic, and, indeed, following him Duhem declared no empirical foundation of science possible. Nor was Duhem ready to permit a priori justification to any scientific theory, viewing the domain of apriori thinking to be logic and mathematics alone. How, then, did he think science could be justified ?

Duhem denied total justification, as he demanded that both theory and evidence be regularly modifiable. But he felt that after its modification any theory is improved and deserves a higher level of justification then before its modification. And thus the breadth of scope of a theory is its partial justification, where the scope is broadened with the increase of the number of facts covered and for the simplicity of the classification. Both these factors are theory-laden, of course, yet we can easily see if and when a modification is an improvement or not. Once we omit commonsense from Duhem's theory, its consistency and success are truly imposing.

The weakness of Duhem's philosophy is in the difficulty one has in viewing science in its light. In addition, we may observe that it was empirically refuted by evidence which Duhem had only a glimpse of — the scientific revolution of the early twentieth century.

5. The Duhem–Quine thesis

The weakness of Duhem's view can best be illustrated by contrasting Duhem's image of science with that of the contemporary empiricist followers of F.P. Ramsey. Science may be viewed as a set of statements of three or four kinds : logic and mathematics, theories, theory-free observations, and a few correspondence rules. These rules are necessary because to be theory-free the observation statements in the Ramseyan system should not include theoretical terms, and vice versa. Duhem, on the contrary, declared that scientific observation statements always include theoretical terms and so the revision of theory immediately revises also observation statements couched in its language. Also, when an observation statement clashes with a theory, then in the Ramseyan system it is possible to present a complete set of theoretical statements which the standard correspondence rules make conflict with the observation statement. Quine goes so far as to claim that in each case of conflict our whole theoretical system was tested as a unit and then we cannot know which part of the premises is refuted when an empirical conclusion based on it is refuted : we do not, therefore, know a priori which part of our theoretical system invites modification. Duhem saw a greater difficulty in the situation than Quine. He considered the fact — and it is a fact — that only a part of the theory is explicitly stated, whereas another part may well be expressed as the theoretical bias of the observation, not as a premiss.

To take an example, a researcher tries to extend an astronomical theory to a new prediction. Suppose the venture turns out unsuccessful. There will be then a straightforward contradiction between the astronomical theory and the observation report. Nothing can make us ignore this contradiction and stay scientific. Yet it will be rash to conclude that either theory or observation is false, since the error was in the excessive application. It will also be rash to conclude that the elimination of the contradiction from the application to this new case necessarily requires the modification of the astronomical theory. Since the observation was attained with the aid of optical theory and with the aid of optical instruments whose design embodies optical theory, there is a wider choice here.

The label Duhem–Quine argument is not in itself objectionable, but one may well be advised not to confuse the two variants, Duhem's, where some theory is declared implicit in the situation, and Quine's, who sees no need for an implicit hypothesis. Or perhaps it is not Quine but his teacher Rudolf Carnap and other followers of Ramsey who would not put the argument the way Duhem has put it.

In the Ramseyan system, at least in Carnap's Ramseyan system, each observation report has a fully determined meaning, whereas a theory has only as much meaning as experience warrants. In this way Carnap too, as Duhem before him, can deny theory the status of hypotheses, and he too can grant this status only to every new application of an established theory, and that application can then be tested and either be fully verified and then added to the theory by the extension of its meaning, or else it will be fully refuted and we shall note that the applicability of the theory is limited. In Duhem's system, however, there is a slight problem here : theory gets its meaning from experience and vice versa, which is somewhat unpleasant, since it looks as if meaning is thereby totally absent from the system, which cannot be the case.

6. Poincaré's modification of Duhem's Philosophy

At this junction Henri Poincaré steps in : what he adds to Duhem's system has to do with meaning. The meaning of the axioms of the system, he said, is left open, à la Duhem, by viewing them as implicit definitions. This idea is very important in the history of mathematics, particularly in the theory of the foundation of mathematics. It is of no concern for us here, except to observe that this entrenches Duhem's idea that informative meanings of theories are endowed in them by the empirical information which they are supposed to incorporate. As to that information, Poincaré said, it must be theory-independent. Duhem criticized this point sharply by showing that it does not apply to real science as we know it.

To take a simple modern example, it was deemed highly accurate and reliable that the atomic weight of chlorine is 35.55. This, of course, is a highly theoretically biased statement, a theory-laden observation report. It looks as if it is rejected by physics less than a century after it was very well established. Yet, according to Duhem, the content of observations is certain, only the wording they receive needs alteration when theory is modified. Today the same information is put in modern language otherwise : the terrestrial average atomic weight of chlorine is 35.55.

Poincaré could not elicit instances of observation statements not theory-laden. Hence his defence of Duhem's system failed.

Duhem's system is defective.

7. Popper's theory of science as criticism

The final stage in this history is the system of Karl Popper. All statements of science, he says, are revisable, and hence they are hypothetical. What makes hypotheses scientific is their very revisability, namely their refutability.

If one takes Duhem's system, practically as it is, but reads it realistically contrary to Duhem's expressed demand to deny theory all content, except verified factual content, then one gets the result that when observation contradicts a hypothesis we cannot declare both true, and so they compete for the status of truth, a status which anyway cannot be granted except tentatively, until the next examination. What, then, is the practical methodological difference between Duhem and Popper ? Both recommend deduction of old data and theories à la Whewell; both recommend tests à la Whewell, both reject finality of any statement in science quite contrary to Whewell; both recommend repeated modification of both theory and observation reports. Granted that Duhem is an anti-realist and Popper is a realist, does it make a difference in practical matters ?

Duhem was aware of all this, as was Poincaré. They both stressed that upon a realistic reading of a scientific theory, upon giving it a truth-value straight-forwardly, it is most likely to turn up false. This is what they attempted to prevent. Popper, on the contrary, attempts to present this as unavoidable.

Why, then, the wish to avoid falsehood in science ? Why do we speak of superseded theories as either false and rejected or as not quite false ? The average science teacher, high school or university, insists that Aristotle's theory of gravity, Phlogistonism, and other scientific theories are false and so to be rejected, whereas Galileo's theory of gravity, or Newton's, is not quite false, i.e. true for its domain of applicability. They apply a Baconian standard to some theories and a Duhemian standard to other theories. The reasonable competition, however, is between Duhem and Popper, since the Baconian demand for the absolute truth is out and a compromise between Bacon and Duhem makes no sense and is but a confusion to be explained historically.

Once we admit that false theories are not rejected but taught in universities, then we can also see that in university courses we also teach false observations which we present in the light of refuted theories. Thus, when we teach nineteenth-century atomism we do come up with atomic weights which are today declared false. Likewise we teach Lavoisier and the facts that fit his theory so well, and only later do we tell our students that, contrary to Lavoisier's theory, not all oxydizers contain oxygen. This practice is in accord with Popper's theory. Hence, our teaching is a mixture of Popper, Duhem and Bacon, with Popper dominating the highest echelons, Duhem the middle stages of classical science, and Bacon the early stages of science and its struggle for survival. Is that necessary ? What does Popper offer that Duhem denies ?

The answer, in one word, is boldness. Modification was required by Duhem in order to retain continuity and assure that empirical information is modified with the same continuity as theory. He denied that there ever was a scientific revolution. And when Einstein prenounced his revolution Duhem held him in contempt and explained this precisely in reference to Einstein's revolutionary attitude. There is much to discuss in this context, especially the impact of a change in metaphysics on science as revolutionary (as Duhem knew very well when he demanded that science have no metaphysical import). But this takes us away from theory-ladenness.

8. Popper on observations in science

Since Duhem argued that clear-cut refutation is impossible (so that clear-cut verification is impossible too), the question is repeatedly raised these days, how does Popper handle Duhem's argument ? Or rather, the Duhem-Quine argument. And the question is often put in a quasi-Ramseyan way : if we put theory in the premises and a statement regarding observation as the valid conclusion, then the premises include all sorts of hypotheses so that we are never sure any of them is refuted along with the observation. But Popper presents things not in line with Ramsey, Carnap and Quine; rather, his presentation accords with Duhem's : the inference includes only one theory and one observation statement, and we use all sorts of theories to decide that the observation statement is false. Once we have done so, we are in a position of having already decided that the theory is false. The question, then, is, how do we decide that the observation statement is false when we cannot be sure of it ?

This question is absurd : when we are sure we neither can nor wish to decide. Decision is a matter for cases of uncertainty. Query : is there a decision procedure ? Yes, Boyle's. An observation report made twice with the claim for repeatability is generalized, and the generalized observation report has to be admitted — until refuted, Popper adds. He adds, thus, to the canons of science the obvious rule

(P) An observation report can be rejected only when properly replaced by its refutation.

To be precise, Popper does endorse Boyle's rules and is reticent on Newton's rule (N) which demands to reinstate the refuted generalizatin after it is duly modified. But clearly he can endorse Boyle's as well as Newton's rules and add his own. Also, all this is quite in accord with widespread scientific practice[1] .

Popper's system also clearly overcomes the difficulty Whewell's system has : new facts are refutations of old theories. Old facts are either refutations of older theories (often in new interpretations) or survivals from prescience. The facts one observes daily which in a sense are new but not related to new theories are thus, for Popper, outside the domain or empirical science. This is a questionable situation, since we may wish to incorporate them within science. We have no trouble explaining scientifically the blueness of the sky or the greenness of grass -- not scientific facts until recently, since we have inherited them from prescience. Yet there are new facts not scientifically discovered -- not as refutations — such as the mountains on the back of the moon and the atomic weights of new elements, which we regularly incorporate into science. This makes science more than the mere acts of conjectures and refutations since it is also the incorporations of two kinds of facts, refutations of old conjectures and non-scientific facts. How exactly the refutations are theory-laden is clarified by Duhem and more so by Popper in a very satisfactory way. The rest is less clearly explained.

The state of the art today seems as follows. Many philosophers are using Ramseyan methods in the hope of establishing the possibility of theory-free or theoretical-bias-free observations and many empirical psychologists are searching for instances of such observations. Yet these ventures are a priori doomed to failure, at least as long as arguments discouraging them are not answered. The fact that the most advanced empirical information is most theory laden is explained by Whewell, Duhem, and Popper by the observation that such information is the result of tests of new theories. Popper's claim that they are refutations of previous theories makes their value independent of further developments, whereas Whewell's claim that they verify new theories risks their value since allegedly verified theories may be refuted. Yet the theory-ladenness of everyday observations and the novelty of observations not relevant to any known theory — all these are subject to further studies, whether of the empirical theory of perception within psychology or of the methodology of science.

9. A historiographic note

Were the modern thinkers discussed here aware of their important predecessors ? Whewell was certainly aware of all of his predecessors. Duhem was most probably not aware of Boyle's procedure, or even of Newton's -- he dismissed their empiricism. He was probably fully aware of Whewell's ideas and works; if not he must have absorbed them from secondary sources — Claude Bernard is a likely candidate. Poincaré's indebtedness to Duhem is quite a matter of the record. Popper was familiar with their works which he mentions in his own works and he certainly was familiar with Whewell's ideas — whether from primary or from secondary sources and to what extent I cannot say : Whewell is now slowly gaining a revival and a very welcome one, but even when his name was utterly forgotten his ideas were in the air. Presumably Popper had no knowledge of Boyle's rules, which he learned from the tradition of scientific practice. This is no small matter. Except for Robert Boyle and Karl Popper hardly any author about science has noticed that though scientific evidence must contain factual information that makes it bona fide testmony of a bona fide eyewitness, and though it must be stated at least twice, the body of scientific knowledge does not contain this evidence but its space-time generalization. Jacob Bronowski, a follower of Karl Popper, noted with satisfaction, soon after the discovery of the existence of non-parity, that whereas so many philosophers of science are still concerned with the grounds for generalizations in empirical observation and in the reinforcement which repetition lends to this process, within science only one repetition is required and the generalization is fully established at once and with no further ado — until it is successfully questioned anew. This, of course, cannot make Popper's victory over his Ramsayan opponents final. Moreover, some doubts have been thrown on Popper's theory already. But this is another story.

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NOTE

[1] This is not to endorse Popper's theory. I have criticized it elsewhere.

A Bibliographic Note

Since the literature surveyed here is classical, one need hardly mention even names of books. And rather than give page numbers, one should remind the reader that the subject indices to the standard editions of the classical works are often excellent. The following observations, then, have only a limited function.

The works of Galileo are, of course, collected in his impressive *Opera*, but the English-reading scholar may be satisfied to begin even with Stillman Drake's small, popular collection, *Discoveries and opinions of Galileo*, not to mention the two translations of Galileo's major dialogue and his *On Floating Bodies*. I should also draw attention to Michael Segre's study of the role of experiment in Galileo's physics in the *Archives of the History of the Exact Sciences*, 1980.

Bacon's standard *Works*, including the prefaces by Spedding and Ellis, are breath-taking; *Novum Organum* Book I and *Valerius Terminus* — a fragment — will do.

Robert Boyle's *Works* have a detailed index. I will recommend his earliest *Certain Physiological Essays*, first two essays, and his posthumous *Experimenta et Observationes Physicae*, preface, for a start.

William Whewell's philosophical works comprise four volumes, and his *Novum Organum Renovatum*, which emulates Bacon's aphoristic style, will do. But all four, plus his three volumes of the history of science, are just wonderful.

I should not skip Claude Bernard, *Introductory to the Study of Experimental Medicine*, even though the English translation is rather free, and even though it is not discussed here.

Duhem's *Aim and Structure of Physical Theory* suffices to introduce him to the reader in all his glory, and the book is certainly superb. Also his *To Save the Phenomena.* But his historical studies also deserve mention here, and I should observe that Floris Cohen of Twente Technische Hoochschule, Enschede, notices a variant of Duhem's views presented in the introduction to his *Etude Leonardo da Vinci,* Volume 3. The reader interested in the background to this variation will have to wait for Stanley Jaki's forthcoming comprehensive biography.

Poincaré's *Science and Hypothesis* and *Science and Method* do not need any recommendation. With all their deserved popularity they are still unknown : his proof of the metaphysical, unempirical nature of the law of conservation of energy, for example, is still simply unknown. Much verbiage could be saved by a little more attention here.

Popper's *Logic of Scientific Discovery* is not as much to my liking as his original *Logik der Forschung,* of which it is a translation, but as a start it will do amply. Popper's best on the topic, however, is his 'Philosophy of Science : A Personal Report' issued as the first chapter of his *Conjectures and Refutations;* also his 'The Aims of Science' reissued in his two latest books, *Objective Knowledge* and his *Postscript,* volume one.

This bibliography is only of the classics of the field. For more, one has to go elsewhere and take care to avoid all the many many works which at best add nothing to the study at hand.