Can the monster Errour be slain?

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Can the monster *Errour* be slain?

Error—a deep rooted feature of the method of experimentation that has been ignored

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Abstract *One cannot discount experimental errors and turn the attention to the logico-mathematical structure of a physical theory without distorting the nature of the scientific method. The occurrence of errors in experiments constitutes an inherent feature of the attempt to test theories in the physical world. This feature deserves proper attention which has been neglected. An attempt is made to address this problem.*

In his *An Apologie for Poetrie* written around 1583, Sir Philip Sidney claims passionately that poetry is superior to the teachings of history and philosophy. Sir Philip argues that poetry presents these teachings in a language which moves the reader by beauty of expression as well as by substance of thought; but above all poetry aims at a moral instruction (Sidney, 1967, p. 153; Hankins, 1971, p. 298). I shall not argue with this claim; I rather concede to Sir Philip that poetry is indeed superior to history and philosophy. Sidney's *Apologie* may be considered a manifesto whose theory of poetry is illustrated magnificently and at length by his contemporary Edmund Spenser who published his epic poem *The Faerie Queene* in 1590 (Winstanley, 1928, pp. lxvi–lxvii).

Spenser intended this epic poem to comprise twelve books, each of them setting forth the deeds of a knight who seeks a certain moral virtue in honour of the Faerie Queene. The poem opens with an isolated visual image for our contemplation: the picture of a lonely knight riding along a plain. The description of his armour forces us to dismantle our image of the knight and distinguish between the arms and the man: the arms are mighty, the man unproved; this is the Redcrosse Knight. Armed and eager, but untried, Redcrosse rides forth and a 'lovely Ladie' (I, 4) whose names is Una, that is, Truth and Oneness, joins him.

Though Spenser usually moves with abundant leisure, he loses no time in opening the first book. No sooner are the knight and the lady before us than (I, 6)

> The day with cloudes was suddeine overcast

and 'an hideous storme' drives the pair to take refuge in a wood where they find a confusion of paths. Their delight and innocence as they move through the wood are suggested by the variety and picturesqueness of the description of the trees; but the catalogue includes a sour note: (I, 9)

> . . . the Maple seeldom inward sound.

Indeed, nature may be misleading: fair without, but unsound within, and the habitat for illusions and perturbations is the forest.

Like Dante who finds himself at the start of the *Inferno* lost in a dark forest,[1] so the Redcrosse Knight and his lady Una lose with 'diverse doubt' (I, 10) their way in the wood. Lost in a labyrinth, they stumble on a cave. (I, 11) The 'little glooming light', (I, 14) shed by Redcrosse's armour, illumines the darkness of the place to reveal a creature: (I, 14)

> Most lothsom, filthie, foule, and full of vile disdaine.

This is the monster *Errour*. *Errour* not only dwells in the heart of the forest, it is the heart of the forest—the ugly and dangerous principle at its core: (I, 13)

> This is the wandring wood, this *Errours* den.

Half-serpent, half-woman, the monster lies in her den. She rushes at the knight and attacks him ferociously. The description of the fight which ensues is given with graphic freshness: the mingled fear, the rancour of the monster and her desperate leap at the fatal moment upon the knight's shield—the shield of faith. Suddenly, *Errour* winds herself around Redcrosse: (I, 18)

> That hand or foot to stirre he strove in vaine:
> God helpe the man so wrapt in *Errours* endlesse traine.

At this crucial moment, Una encourages Redcrosse vigorously: (I, 19)

> Add faith unto your force, and be not faint:
> Strangle her, else she sure will strangle thee.

Mustering his force, the knight strangles *Errour* with such strength that she brings forth the horrible and loathsome contents of her stomach. The monster *Errour* vomits, and it is crucial that the reader should first of all respond to the ugliness of the black flood of stinking 'lumpes of flesh and gobbets raw'. (I, 20) However, when a few lines later Spenser tells us that

> Her vomit full of bookes and papers was,

we receive a clear indication what is the allegory all about. The view is strengthened when the monster's spawn of small, deformed serpents is described as 'fowle, and blacke as inke'; (I, 22) the deformed serpents are, in effect, letters. In this allegory the 'vomit full of bookes and papers' is certainly an explicit reference to what Spenser considered false Catholic doctrine and anti-Protestant propaganda.

When error is realized in the form of a dragon, the knight can attack it and aims a good stout blow at the monster. Redcrosse slays the dragon *Errour*, and he and Una emerge victorious from the wood. Victorious, that is, until they fall victim to the illusions which *Archimago*, the arch image-maker, creates with the aid of evil spirits (Brooks-Davies, 1977, pp. 16–22; Greenlaw *et al.*, 1961, pp. 175, 422, 425–426; Hamilton, 1984; Heale, 1987, pp. 33–37; Nohrnberg, 1976, p. 202; Rose, 1975, pp. 1–23; Weels, 1983, pp. 43–47).

The victory over the monster *Errour* determines that when evil returns, it returns in a compounded and aggravated form.

> But subtill *Archimago*, . . .
> He then devisde himselfe how to disguise;

For by his mightie science he could take
As many formes and shapes in seeming wise, . . .
Sometime a fowle, sometime a fish in lake,
Now like a foxe, now like a dragon fell,
That of himselfe he oft for feare would quake,
And oft would flie away. (II, 9–10)

The success in eliminating an explicit error blinds Redcrosse to more cryptic and subtle kinds of error. The trial is not over; the knight has yet to learn that error cannot be defeated once and for all like a monstrous dragon. The advice that the knight receives from Una, the 'lovely ladie', when he encounters the monster *Errour*, should be heeded: (I, 12)

The danger hid, the place unknowne and wilde,
Breedes dreadfull doubts: Oft fire is without smoke,
And perill without show: therefore your stroke
Sir knight with-hold, till further triall made.

The lady has perceived what the knight misunderstands: error is a universal phenomenon that cannot be slain and as a consequence a moral vigilance has to be sustained throughout.

Can we then accept Wilfrid Sellars' suggestion and slay, well, he said, 'discount', errors of measurement and other forms of experimental error, and then turn our attention to the purely, one is tempted to say holy, logico-mathematical structure of physical theory? (Sellars, 1961, p. 73). My reply is a categorical no, and I would advise the practice of vigilance also in physics, not to mention philosophy of science.

The tradition of theory analysis which ignores the practical aspects of experimentation in scientific research has dominated the scene in philosophy of science far too long. Recent developments in this field have originated in the attempt to redress the balance: practice—the conduct of experiment, the actual testing of theory—is increasingly given its due weight.

This criticism of Sellars' suggestion of ignoring the problem of experimental error and discounting praxis may be viewed from a philosophical perspective. We leave then the poetic alley but remain in the period in which Spenser was writing, namely, the end of the sixteenth century, the threshold of the scientific revolution.

No great stretch of imagination is required to construe the vomit of *Errour*: these books, papers and letters, as a reference to types of misleading dogma. An intellectual saviour who is intent on liberating mankind from errors of the past and set it on a path in which the attainment of knowledge would be unobstructed by false dogma, would certainly try to emulate Redcrosse's valour deed and slay *Errour*. It has been suggested that Francis Bacon did retain from his puritan upbringing a sense of the fallen nature of man and transposed it from the moral to the epistemological realm (Quinton, 1980, p. 37). Bacon was such a saviour or that is at least how he perceived himself.

In his celebrated *Novum organum*, Bacon argues that Aristotle "has corrupted Natural Philosophy with his logic; . . . he has made the Universe out of categories" (I, lxiii). In Bacon's view, the Aristotelian dogma has rather the effect of confirming and rendering permanent errors which are founded on vulgar conception, and obstruct the search for truth. The student of nature should get rid of all the prejudices and preconceived ideas, and as Bacon put it: "the whole work of the mind should be recommenced a new" (Preface). Thus, the first task of the scientist is to eliminate error from his or her cognition by the "expiation and purgation of the mind", and only then can he or she enter "the true way of interpreting Nature" (I, lxix). Clearly, the religious undertones should not be ignored.

Bacon, therefore, finds it necessary to expound in considerable detail the subject of the obstacles to truth before proceeding to unfold his own method. He devotes nearly the whole of the first book of *Novum organum* to the study of these obstacles which he calls idols. This name reflects the Platonic concept of *eidolon*; it refers to a fleeting, transient image of reality, in contrast to the concept of *idea*, which represents reality in the Platonic sense.

Bacon demands that all the four types of idols, which he has classified,[2] "must be renounced and abjured with a constant and solemn determination" (I, lxviii). He insists upon freeing and purging the intellect from them, so that "the approach", as he describes this quest, "to the Kingdom of Man, which is founded on the sciences, may be like that to the Kingdom of Heaven" (I, lxviii). Bacon informs the reader that with his new method "we are building in the human Intellect a copy of the universe such as it is discovered to be, and not as man's own reason would have ordered it" (I, cxxiv).

It is striking to observe with what ease Bacon rejects possible errors which can render his own doctrine erroneous. "It will doubtless occur to some", Bacon remarks,

> that there is in the Experiments themselves some uncertainty or error; and it will therefore, perhaps, be thought that our discoveries rest on false and doubtful principles for their foundation. (I, cxviii)

This is, indeed, an important observation, but Bacon dismisses the problem forthwith: "this is nothing", he exclaims, "for it is necessary that such should be the case in the beginning". Using the analogy of printer's error, Bacon explains that

> it is just as if, in writing or printing, one or two letters should be wrongly separated or combined, which does not usually hinder the reader much, since the errors are easily corrected from the *sense itself*. And so men should reflect that many Experiments may erroneously be believed and received in Natural History, which are soon afterwords easily expunged and rejected by the discovery of Causes and Axioms. (I, cxviii; my emphasis)

Finally, Bacon assures the natural philosopher that he should not "be disturbed by the objections which we have mentioned".[3]

With no difficulty we can imagine Bacon in the role of the Redcrosse Knight charging the monster *Errour* and slaying it with a well directed blow. He could have well presented the Faerie Queene with a great moral deed: freeing mankind, to use Spenser's poetic language, from "*Errour* endlesse traine".

Notwithstanding Bacon's resolute assurance, the objections are disturbing; for it is precisely this very sense—the contextual sense which according to Bacon's analogy is given—that science lacks. Bacon would have us believe that the analogy between a printer's error and an experimental error is faithful; that, for example, H. Hertz's error in concluding his cathode-ray experiment that "the electrostatic and electromagnetic properties of the cathode rays are either *nil* or very feeble", (Hon, 1987) is akin to a printer's error and, therefore, could have been rectified by referring back to the 'sense itself'.

Undoubtedly, some sources of errors can be identified as either prejudices or preconceived ideas. However, to claim that the sources of all errors are prejudices and preconceived ideas, is an error in itself, which in Bacon's case has, indeed, become a prejudice (Agassi, 1975, p. 67).

It is clear that I side with Una, the 'lovely ladie', and I am not so much impressed by the impatient bravery of the Redcrosse Knight:

Oft fire is without smoke,
And perill without show: therefore your stroke
Sir knight with-hold, till further triall made.

My claim is that errors—be they erroneous theories, errors of measurement or other forms of experimental error—are part and parcel of the scientific quest and, to borrow the current poetic expression, cannot be slain. Any attempt to discount errors, even for the purpose of idealization (as is the case with Sellars), misses an essential feature of the physical sciences: the gulf that persists in the intertwined connection between a certain physical condition and its conceptual understanding.

The experimenter constantly encounters in the laboratory discrepancies between theories and observational results. Indeed, part of his or her daily routine consists, as Polanyi intimates, in explaining away these discrepancies. "In my laboratory", Polanyi writes (1964, p. 31),

> I find the laws of nature formally contradicted at every hour, but I explain this away by the assumption of experimental error. I know that this may cause me one day to explain away a fundamentally new phenomenon and to miss a great discovery. Such things have often happened in the history of science. Yet I shall continue to explain away my odd results, for if every anomaly observed in my laboratory were taken at its face value, research would instantly degenerate into a wild-goose chase after imaginary fundamental novelties.

The origins of these discrepancies can be of different kinds: occurring in different contexts and arising from different causes. To clarify this complex array of different causes it is useful to have, as a heuristic device, a system of classification of experimental error.

The most common classification of experimental error is the classification which distinguishes between two categories of error, namely, systematic and random errors. As we shall see, this is not the kind of classification needed for our purposes.

In scientific and technical writings it is common to find different usages of the word error. One school of thought considers error the difference between the experimental result and the 'true' value; another usage is that error is the number placed in the statement of the result after the plus-or-minus sign, irrespective of the 'true' value. It has been suggested—in a *Code of Practice* addressed to the National Physical Laboratory in England—to use the term uncertainty to cover this multiple usage of the word error. In this *Code* the uncertainty of a measurement is divided into two categories: the random uncertainty and the systematic uncertainty. The estimation of random uncertainty is derived by a statistical analysis of repeated measurements while the estimation of systematic uncertainty is assessed by non-statistical methods and much depends on the personal judgement of the experimenter in allocating limits to this uncertainty (Campion *et al.*, 1980).

It thus appears that whatever the terms and the definitions, the mathematical methods used for estimating uncertainties constitute the underlying criterion of the dichotomy between systematic and random error. The standard view is that, apart from random error, all experimental errors can be eliminated, and that the distribution of random errors can be captured by a simple law, namely the Normal law.

However, in practice it is very rarely, if ever, the case that the experimenter can remove the systematic errors altogether. Furthermore, the distribution of errors follows the Normal law only approximately, even when the quantity to be measured is steady. Consequently, the application of the Normal law as applied to actual observations is justified, in the last resort, by comparison with the spread of the observations themselves. This is a vicious circle; it is the result of justifying the treatment of observations by exclusively referring to the observations themselves.

This state of affairs has led to much confusion with respect to the validity of the Normal law; a confusion to which Lippmann wittily referred in his remark to Poincaré:

> Everybody believes in the exponential law of errors: the experimenters, because they think it can be proved by mathematics; and the mathematicians, because they believe it has been established by observation. (Whittaker and Robinson, 1924, p. 179)

There is, of course, no reason for experimental observations to follow the Normal distribution exactly. This distribution is a convenient mathematical expression which fits most of the experimental observations and it should be recognized that this is an assumption which may not always be justified. Indeed, as Margenau critically remarks:

> experience presents the scientist with innumerable skew distributions, differing perceptibly from the normal law. These he often dismisses or corrects, because for some hitherto unstated reason he objects to them. He uses the normal distribution both as an inductive generalization from experience and as a criterion for the trustworthiness of that experience. Thus he is lifting himself by his bootstraps unless an independent argument can be given for the normalcy of that distribution. (Margenau, 1950, p. 114)

The correct approach is to regard the number following the plus-or-minus sign as an estimate of the width parameter of some, and it should be stressed, *some* statistical distribution of observed values which would be obtained if the measurement were replicated a number of times (Cohen and DuMond, 1965). Clearly, the appeal to probability is an attempt to break the vicious circle.

The upshot of such analysis is that the experimenter gets a mathematical insight into his or her collection of data. Indeed, the object of the mathematical theory of error is to work out methods for estimating the numerical values of the required magnitudes by means of a given set of observations. The theory makes it also possible for the observer to arrive at some conclusion with respect to the degree of precision of the estimates obtained. In accordance with that theory the experimenter identifies the unknown 'true' value of the observed magnitude with the mean of the corresponding Normal distribution, and the degree of precision with the standard deviation of the distribution (Cramér, 1966). Commenting on this theory of error, Margenau remarks that:

> the philosopher of science is obliged to take note of this remarkable fact: both 'truth' and 'tolerance' must be fished out of the uncertainties of the immediately given by more or less arbitrary rules not immediately presented in Nature. (Margenau, 1950, p. 113)

The experimenter thus remains in the dark as to the conceptual and physical circumstances in which the errors have originated. The mathematical criterion which underlies the dichotomy between systematic and random error does not illumine the source of the error; it only expresses the way the error is supposed to behave mathematically. Most scientists and philosophers of science regard error as a mathematical abstraction—essentially, a probabilistic phenomenon resulting from some stochastic process. Although the probability approach is of considerable importance in coping with inaccuracy and imprecision, I am concerned here with error as an epistemological concept, and this view calls for a different classification of experimental error than the standard dichotomy of systematic and random error.

I consider experimental error a two-fold phenomenon consisting of conceptual and physical elements. Indeed, it appears that these two elements are invariably interwoven: there is always a certain physical condition and the experimenter's conceptual understanding of it. To arrive at a classification which focuses on the epistemological nature of experimental error, one has to distinguish between the different contexts in which error may arise, and within each context to determine the kind of possible reasons for an error to occur.

I discern four distinct stages in the execution of an experiment:

1. Laying down the theoretical framework of the experiment.
2. Constructing the apparatus and making it work.
3. Taking observations or readings.
4. Processing the recorded data and interpreting them.

Corresponding to these four stages we may classify experimental errors as arising in:

1. Background theory.
2. Assumptions concerning the actual set-up and its working.
3. Observational reports.
4. Theoretical conclusions.

At this juncture an analysis of each category is required and the assumed distinctions should be argued for. I shall not proceed in this vein (see Hon, 1989a); rather, I would like to turn to the history of physics and outline four experiments which have turned out to be in error. The application of the proposed typology to these cases sheds light on what went wrong.

My first example is concerned with the controversy between Millikan and Ehrenhaft over the atomicity of the electric charge. This case is by now famous due to several studies which, however, concentrate on Millikan's side of the story (Holton, 1978; Franklin, 1986). As in a court hearing we should listen attentatively to Ehrenhaft's arguments even if we were to be convinced, like Dirac, that Ehrenhaft was not a good experimenter (Weiner, 1977, p. 292). Ehrenhaft failed to demonstrate the accepted view that the electric charge has a fundamental unit. The question is then what did go wrong in these experiments of Ehrenhaft?

Following up his 1909 determination of the elementary charge, Ehrenhaft sought to increase the sensitivity of his apparatus and to achieve a greater accuracy in these measurements by observing ultra-microscopic metal particles whose radii were of the order of 10^{-5} cm. He used metal particles obtained from sparks between metals. These particles were much smaller than the spherical oil drops of Millikan (2.5×10^{-5} cm–23×10^{-5} cm) and their homogeneous nature as spheres was open to objections. Assuming that Stokes' law for the resistance to the motion of a sphere in a viscous fluid holds for these particles, Ehrenhaft continually found fractions of the 'atom' of electricity. He considered these findings a demonstration of the existence of what he called subelectrons.

Millikan's principal objections to Ehrenhaft's results was based on the uncertainty as to the correctness of the assumption that Stokes' law is applicable to the motion of these metal particles: their diameters were not negligible in comparison with the mean free path of gas molecules, and their sphericity was doubtful.

Ehrenhaft declined to accept Millikan's objections and saw no reason why he should abandon Stokes' law in its original form. Fifteen years later, he was still holding fast to his results, claiming that if Millikan's criticism were to be valied, that is,

> if . . . the validity of the resistance law for small moving spheres of mercury and other material is to be doubted, then it cannot hold either for other substances, e.g. oil.

However, by the autumn of 1910, Millikan was already aware of the failure of Stokes' law when applied under the conditions of his own experiment and, what is more, he realized that the correction term is a variable depending on the size of the drop and other considerations. Stokes' law is deduced after all from hydrodynamical principles of continuous flow and absence of slip, and it is most probable that it ceases to hold for spheres whose size is comparable with the mean free path of the molecules of the gas.

Perrin concurred with this judgement. In his view, the particles which Ehrenhaft used were really irregular, spongy bodies having an entirely jagged surface; their frictional effect in gases will be very much greater than if they were spheres, and the application of Stokes' law to them has no meaning. Perrin found confirmation for this view in Ehrenhaft's own report that the dust metallic particles have no appreciable Brownian movement, although they are ultra-microscopic. According to Perrin, this very observation of Ehrenhaft indicated an enormous frictional effect.

Ehrenhaft, however, persisted in defending his experimental results. He thought that he had established decisive objections to Millikan's work. These objections can be generally characterized as logical and experimental.

The logical objections amount to the claim that throughout his work Millikan presupposed atomicity and invariably begged the question. Millikan's curious early method of qualifying the data as 'best', 'very good', 'good' and 'fair', and, moreover, the practice of discarding data which seemed not to confirm atomicity as expected, strengthened Ehrenhaft in his belief that Millikan's argumentation constituted an instance of *petitio principii*.

The allegedly fallacious reasoning of Millikan was compounded in Ehrenhaft's opinion by what he considered a crude experimental technique. As late as 1941, Ehrenhaft still stressed the point that Millikan's

> apparatus is not capable of optical observation of particles smaller than 3×10^{-5} cm, and it is just on such particles that charges smaller than the expected elementary quantum are found most frequently.

That was, indeed, the original experimental problem Ehrenhaft had set himself in 1909: to measure the electric charge on the smallest possible individual particles and thereby subjected the foundations of the electric theory to a sharpest test. (For a detailed study and bibliography see Hon, 1985, chapter 5.)

The second example comes from a research on the nature of cathode rays that H. Hertz carried out. In 1883 H. Hertz, the celebrated German experimenter and theoretician, conducted experiments on cathode rays. He concluded these pioneering experiments by stating that "the electrostatic and electromagnetic properties of the cathode rays are either *nil* or very feeble".

It is ironic that the prototype of the oscilloscope—for that is what Hertz's apparatus amounted to—should be instrumental in demonstrating that cathode rays have no closer relationship to electricity than has light produced by an electric lamp. Indeed, Hertz argued that since "cathode rays are electrically indifferent, ... the phenomenon most nearly allied to them is light".

Hertz failed to realize that due to poor vacuum the electric field which he had applied to a beam of cathode rays, did not attain sufficient intensity. Consequently, a distinct deflection of the cathode ray could not be detected; an experimental result which led Hertz to an erroneous conclusion.

It took almost fifteen years to show that Hertz was in error. When J. J. Thomson evacuated a cathode-ray tube more carefully and efficiently, he could see quite clearly—as the pressure was being reduced—how an electric field can deflect a beam of cathode rays. As is well known, the discovery of the electron took place with the advent of this experiment of Thomson. (For a detailed study and bibliography see Hon, 1987.)

Another interesting case is the so-called discovery of the N-rays by Blondlot. Blondlot, a well-known French Physicist who was working at Nancy University, claimed to have discovered in 1903 a new form of radiation which he called N-rays, after his university. The original method of detecting the N-rays was to observe an electric spark which allegedly got brighter when subject to this radiation. Blondlot was confident that he could distinguish visually between the brightness of the sparks. Later on, he developed a special calcium sulphide screen whose phosphorescent glow increased markedly, so it was claimed, when it was exposed to N-rays.

By the summer of 1903 the discovery of this new form of radiation became a major issue and many attempts were made to reproduce it. However, except in France all the attempts failed.

Blondlot argued that the N-ray phenomena lie almost at the limit of what observers are able to discern, and it is only after a certain amount of practice that they succeed in catching them easily, and in observing them with complete certainty. Under the heading, "How the action of 'N' rays should be observed", Blondlot gave the observer the following instructions:

> It is *indispensable* in these experiments to avoid all strain on the eye, all effort, whether visual or for eye accommodation, and in no way to try to *fix* the eye upon the luminous source, whose variations in glow one wishes to ascertain. On the contrary, one must, so to say, see the source without looking at it, and even direct one's glance vaguely in a neighbouring direction. The observer must play an absolutely passive part, under penalty of seeing nothing. Silence should be observed as much as possible. Any smoke, and especially tobacco smoke, must be carefully avoided, as being liable to perturb or even entirely to mask the effect of the 'N' rays. When viewing the screen or luminous object, no attempt at eye-accommodation should be made. In fact, the observer should accustom himself to look at the screen just as painter, and in particular an 'impressionist' painter, would look at landscape. To attain this requires some practice, and is not an easy task. Some people, in fact, never succeed.

It is worth stressing that these instructions came from a *physicist* with a well-established reputation.

As the criticism mounted the *Revue Scientifique* polled leading scientists on their judgements concerning the existence of N-rays. The editorial board announced that:

> It is time for French science to settle this question definitely ... Science has demonstrated its power sufficiently ... [and its] business ... [is] not only to

> discover the true but to reveal the false. It has been said over and over that the 'N' rays are the product of the imagination. Reason should control this assertion and make a final decision.

Responding to this call, Poincaré visited Blondlot and was impressed by the results. To be sure, he saw no effects; yet he believed that the N-ray radiation exists. Poincaré expressed great confidence in Blondlot who was according to him a distinguished and competent physicist. He attributed his failure to observe the effects of the alleged radiation to involuntary accommodation of his eyes (Poincaré, 1904).

The strongest, and as it happened fatal, objection came from the American physicist, R. W. Wood. He paid a visit to Blondlot at his laboratory in Nancy, where he exposed Blondlot and his co-workers to be the victims of autosuggestion (Wood, 1904).

It is misleading to say with the critics of Blondlot that he was wrong in that the ultimate test was subjective. By definition, an observation is a subjective process; the problem lay rather in the nature of the observation: it was simply unamenable to objective, or rather inter-subjective criteria such as pointer reading. To observe the N-ray effects one had to look for a small change of brightness which, of course, required remembering brightness—a very imaginative task indeed. (For a detailed study and bibliography see Nye, 1980; Hon, 1985; chapter 5.)

My last example is Franck's and Hertz's experiment which is perhaps the first experiment that demonstrated vividly in a graphic way the quantized spectrum of the atom's energy levels. In 1925, the Physics Nobel Prize was awarded to Franck and Hertz for their experimental work on the laws governing the impact of an electron upon an atom. The Nobel Committee claimed that Bohr's hypotheses of 1913 were no longer mere hypotheses, but experimentally proved facts. The committee stated in its citation that the methods of verifying these hypotheses had been the work of James Franck and Gustav Hertz. However, the original experimental work of Franck and Hertz was in error.

Franck and Hertz regarded the first inelastic impact recorded by their device as an ionization process; they thus believed that the spectral line which they had detected was emitted as a result of the ionization of the mercury molecules. They were wrong in this interpretation; in other words, what they measured was not what they thought they were measuring. In fact, their device could not distinguish between excitation and ionization potentials; it only recorded the occurrences of inelastic collisions. Franck and Hertz measured, it is now believed, a real physical quantity: the first excitation potential of mercury. However, they thought it to be an ionization potential and therefore erred in their interpretation of the observational results.

In fairness to Franck and Hertz, it should be noted that they admitted their error in their Nobel lectures. "It appeared to me to be completely incomprehensible", Franck observed, "that we had failed to recognize the fundamental significance of Bohr's theory, so much so, that we never even mentioned it once in the relevant paper". Hertz on his part stated explicitly that at the time they "erroneously believed that ... [what they had measured] was the ionization potential". (For a detailed study and bibliography see Hon, 1989b.)

Applying the proposed typology of experimental errors to these cases, we can see that Ehrenhaft's error belongs to the first category. Ehrenhaft's measurements of the charge of the electron which allegedly demonstrated the existence of subelectrons, provide an illustration for a false set-up theory that resulted in an error. Stokes' law was not applicable to the physical conditions which Ehrenhaft had created and as a consequence his final experimental result was in error.

In the second example we observe that H. Hertz's error had originated in the assumption that the intensity of the voltage across the condenser's plates was high enough to induce a deflection of cathode ray. Hertz was not aware of the fact that due to poor vacuum the cathode rays ionized sufficient residual gas to permit neutralization of the plates; that in turn reduced substantially the intensity of the electric field. It was, therefore, the conductivity which had been produced in the residual gas that affected Hertz's experiment. This error is then an error of the second category.

The so-called discovery of the N radiation presents us with a case in which the error is directly associated with the method of observation. The technique of detecting this new form of radiation was prone to many errors of observation and, in particular, autosuggestion. I therefore characterize this type of error as error of the third kind which pertains to the process of observation.

Concerning the last example, we can see that Franck's and Hertz's experiment constitutes a case where the error originated solely in the interpretation. It was neither the method nor the various physical approximations, but rather the theoretical considerations in comprehending the observations that gave rise to an error. This is then an error of interpretation and it belongs to the fourth category.

In conclusion, I return to Sidney's *Apologie for Poetrie* whose principal claim is that poetry can convey by means of allegory a moral instruction. Spenser followed this principle and wrote an epic poem of instructive nature: "it aims at laying before the reader an ideal of character and a conception of life to serve as a permanent model". (Winstanley, 1928, lxx.) Spenser's poem not only illumines the pitfalls of moral life, it also shows how to achieve this goal. The typology which I have here presented may not show the way to true knowledge of the physical world, but it does seek to shed light on pitfalls that waylay the experimenter. We cannot succeed in eliminating the monster *Errour*, but we can force it to come into light:

> For light she hated as the deadly bale,
> Ay wont in desert darkness to remaine . . . (I, 16)

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Notes

1. "Nel mezzo del cammin di nostra vita/mi ritrovai per una selva oscura, ché la diritta via era smarrita" (Dante, 1925, I, 1). "Midway in the journey of our life I found myself in a dark wood, for the straight way was lost" (Dante, 1971).

2. I. Idols of the Tribe have their foundation in human nature as such; II. Idols of the Cave have their origin in the peculiar constitution, education, habits, and accidental circumstances of the individual concerned; III. Idols of the Market-Place originate in the misuse of language; and IV. Idols of the Theatre are erroneous systems of philosophy (Anderson, 1971, pp. 97–105).

3. Bacon admits, however, that "it is true, that if the mistakes made in Natural History and in Experiments be important, frequent, and continuous, no felicity of wit or Art can avail to correct or amend them" (I, cxviii).

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