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Observation and Experiment in the Natural and Social Sciences

Edited by
Maria Carla Galavotti

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AN ATTEMPT AT A PHILOSOPHY OF EXPERIMENT

INTRODUCTION

"What exactly is an experiment in physics?" Pierre Duhem posed this question almost a century ago. Apparently, he was concerned not only with the content of the question but also with its reception, since he added forthwith: "This question will undoubtedly astonish more than one reader. Is there any need to raise it, and is not the answer self-evident?" (Duhem 1974, p. 144) Recent attention to the issue of experimentation illustrates that the answer to this question is not self-evident and that there is a philosophical and historical interest in raising the question: what exactly is an experiment in physics and indeed in science generally? The study of experiment has constituted one of the principal forces in reshaping history and philosophy of science during the last two decades. We therefore can assure Duhem that today his question has finally struck a responsive audience. We now consider experiment a central issue in history and philosophy of science, a concept that needs explication and elucidation.

However, the rich studies of experiment indicate that the philosophy of experiment is lagging behind the extensive historical studies of experimentation and the many facets that historians of experimentation have addressed, facets such as technological, cultural, sociological and anthropological. It appears indeed that a divide separates the historians of science from the philosophers of science as to experimental practice. The divide may be clearly discerned in the collection, *Experimental Essays – Versuche zum Experiment* (Heidelberger and Steinle 1998). It appears that a stronger case for the philosophy of experiment should have been made (Radder 1998). To be sure, there have been attempts at such philosophy and I shall outline a few of them shortly. However, these attempts have not cohered into a forceful and cohesive philosophical analysis of experiment, incisive at once for epistemology and for the historiography of experimentation. My objective then is to contribute towards a philosophy of experiment. I grope to bridge the divide between history and philosophy of scientific experimentation by developing a historically informed philosophy of experiment.

M.C. Galavotti (ed.), Observation and Experiment in the Natural and Social Sciences, 259–284.

In this study I focus, as it were, on the inner working of experiment. There are quite a few philosophical discussions of experimentation that are concerned with the paramount relation between experiment and theory, that is, the role of experiment in the overall framework of the scientific method – in a word, all what is external to experiment be it analytical, logical or methodological. For example, the notion of error statistics in experimentation that concerns directly with the relation between theory and experimental results (Mayo 1996; but see Hon 1998b), or for another example, the experiment as an interrogative procedure that executes some kind of erotetic logic (Hintikka 1988) – these external issues do not constitute the theme of my paper. Rather, my interest lies in the internal elements that comprise experiment, their physical and logical interrelationships, their governing principles – in sum, the internal "working", as it were, of experiment which brings about a result, that is, a feature of the world we have come to know.

As a preliminary step, I shall identify and characterize what appear to me the principal obstacles to the construction of a philosophy of experiment, obstacles that have proved quite recalcitrant. An outline of the tension between history and philosophy of experiment will serve as a background.

HISTORY VS. PHILOSOPHY OF EXPERIMENT

The position of the historians of science may well be represented by the view of Jed Buchwald. He claims succinctly and bluntly that

> living sciences cannot be corralled with exact generalizations and definitions. Attempting to capture a vibrant science in a precise, logical structure produces much the same kind of information about it that dissection of corpses does about animal behavior; many things that depend upon activity are lost.

Indeed, according to Buchwald, "axiomatics and definitions are the logical mausoleums of physics" (Buchwald 1993, pp. 170–171). The position of the contemporary historian of science is then to regard science as an activity, not an end result but a process, a "living" and "vibrant" process. The historian's claim is that any generality in the form of, say, logical structure, simply kills this lively activity. The metaphor of the living and the dead appears to be crucial to Buchwald and to historians of science at large. They follow Kuhn's directive, which he formulated right at the beginning of his *Structure of Scientific Revolutions*. According to Kuhn, the aim of history of science "is a sketch of the... concept of science that can emerge from the historical record of the research activity itself." "Activity" appears to be the key feature as distinct from "finished scientific achievements" (Kuhn 1970, p. 1).

The historian may well be happy therefore with a detailed description and a thorough analysis of the activity – the "living" particular; but the philosopher must strive, as Hacking put it simply and directly, for "both the particular and the general" (Hacking 1992, p. 29). There is no escape. If we want to do philosophy, that is, if we believe that philosophy has a bearing on a certain kind of activity, we have then to seek its general features, its underlying principles. In other words, we have to uncover logical structures and characterize methodological principles that govern this activity, without however losing sight of its particulars, namely, its "living" execution. Now, as to experimentation, it is unquestionable that philosophy ought to have a bearing on this activity – it being one of the chief methods of obtaining scientific knowledge. We have then no choice but to analyze experiment *in vitro* as it were, keeping a wide eye on its features as an activity *in vivo*. Buchwald's claim should serve as a warning rather than a condemnation. We should give heed to this warning and follow Whitehead's cautious dictum: "Too large a generalisation leads to a mere barrenness. It is the large generalisation, limited by a happy particularity, which is the fruitful conception" (Whitehead 1929, p. 39).

Thus, a well-developed philosophy of experiment should bring together in a consistent fashion both the normative aspect of the experimental activity – its descriptive as well as prescriptive dimensions, and a comprehensive theoretical conception of experiment that throws light on its internal features, features that underwrite the reliability of the knowledge thus obtained. I propose the notion of experimental error as an efficient vehicle for attaining this objective.

A claim to knowledge in the form of a proposition may be found in time, by various means, to be either true or false. A conceptual system contains by its very nature such claims of which some are found, whatever the system, erroneous. It is commonly expected of the proponent of such system to address the problem of error and to explain the failed attempts at knowledge. The most habitual approach is to analyse errors in terms of the system itself. By doing so, the entire structure of the system – its constituting elements and governing principles – becomes exposed.

Consider for a very brief example Descartes' system of philosophy. As expected, he conceived of the notion of error in the very terms with which he constructed his philosophical system. In Descartes' system error is associated with the cleavage introduced between will and reason. When free will is not restrained and it cajoles successfully the intellect to assent to a proposition that is neither distinct nor clear then, according to Descartes, an error occurs – an indication that the God given faculty of free will has been misused (Hon 1995, pp. 5–6). Thus, a study of Descartes' conception of error reveals immediately the central elements of his philosophical system and its governing principles. In this vein, I present in this essay an outline of Bacon's theory of error; it serves as a background to the philosophical analysis of experiment that I develop (for further clarification of this method of inquiry see Hon 1998a, § 2, 3).

I consider experiment a philosophical system that aims at furnishing knowledge claims about the world, be the world physical or social. Like any philosophical system, experiment comprises elements and governing principles. Given the above method of inquiry, I propose that a study of sources of error arising in this system will throw light on its working. Thus I seek generalizations of the experimental activity that emerge through a study of the notion of experimental error. I claim that while capturing a central feature of the experimental activity, namely, seeking to minimize if not eliminate errors, the notion of experimental error also reflects, albeit negatively, principal conceptual features of experiment. To be more specific, the thesis exploits types of experimental errors as constraints by which one may uncover general features of experiment. It may be seen that the articulation of the notion of experimental error originates in the normative dimension – how to address, rectify and indeed avoid errors in the execution of experiment. However, this articulation reflects at the same time structures and governing principles of experimentation. The attempt then is to capture at once, via the notion of experimental error, both the normative aspect and the theoretical conception of experiment. I shall be concerned in this essay only with the theoretical conception of experiment.

SETTING THE PHILOSOPHICAL SCENE: TWO CLUSTERS OF PROBLEMS

To set the philosophical scene, it is useful first to identify the obstacles that obstruct the way to a viable philosophy of experiment. I discern two principal clusters of obstacles to the construction of such philosophy. Not surprisingly, both clusters have to do with the transition from the particular to the general. For reasons that will shortly become clear, I call the first cluster epistemological and the second methodological. As it happened, right at the beginning of the last century two physicists-cum-philosophers published pioneering, influential works that bear on these issues. Ernst Mach published in 1905 his *Knowledge and Error.* In this collection of essays he addresses problems pertaining, in his words, to "scientific methodology and the psychology of knowledge" (Mach 1976, pp. xxxii). Mach dedicated one essay to the analysis of physical experiment and to identifying its leading features (Ch. XII). A year later, in 1906, Pierre Duhem published his book entitled *The Aim and Structure of Physical Theory*. It is in this book that Duhem posed the question to which I referred at the outset of my talk: "What exactly is an experiment in physics?" (Duhem 1974, p. 144) While Duhem focuses on the epistemological problem, Mach is concerned with methodological issues.

1. The epistemological cluster: the transition from matter to argument

The first cluster of obstacles to a philosophy of experiment is in my view the transition from the material process, which is the very essence of experiment, to propositional knowledge – the very essence of scientific knowledge. As Duhem sees it, the experimental physicist is engaged in "the formulation of a judgment interrelating certain abstract and symbolic ideas which theories alone correlate with the facts really observed." The conclusions of any experiment in physics, and for that matter in science, are indeed "abstract propositions to which you can attach no meaning if you do not know the physical theories admitted by the author" (*ibid.*, pp. 147–148). The end result of an experiment is not, to refer once again to Duhem, "simply the observation of a group of facts but also the translation of these facts into a symbolic language with the aid of rules borrowed from physical theories" (*ibid.*, p. 156). In other words, the obstacle I wish to identify is the problematic passage from matter that is being manipulated and undergoes some processes, via observations to propositions – a language expressed in interrelated symbols – whose meaning is provided by some theory.

Andrew Pickering, to turn to a contemporary author, addresses this problem as a substantial element of the issue of realism. Pickering writes that he is concerned with the process of "finding out about" and "making sense of"; that is, he inquires into the relation between articulated scientific knowledge and its object – the material world (Pickering 1989, p. 275). He conceives of a three-stage development in the production of any experimental fact: a material procedure, an instrumental model and a phenomenal model (*ibid.*, pp. 276–277). These three stages span according to Pickering the material and conceptual dimensions of the experimental practice. It is in the arching of these two dimensions that the passage from matter to knowledge should be forged. Pickering is of the opinion that this passage "is one of made coherence, not natural correspondence." In other words, the coherence between material procedures and conceptual models is an artificial product due to actors' successful achievements in accommodating the resistances arising in the material world (*ibid.*, p. 279).

In a different vein, I have had recourse elsewhere to a concept that I called "material argument" (Hon 1998a, especially §4). I was trying with this concept to bring together in a philosophical context all the elements which are involved in experimentation: the theoretical context and the scheme of manipulation, the material processes and the resulting scientific knowledge which is essentially propositional. I introduced the notion of "material argument" precisely for the purpose of rendering intelligible the transition from the process of manipulating matter to the process of inferring propositions that characterize experimental knowledge, namely, the declared end result of experiment. Experiment, I claimed, is a procedure, a physical process, which can be cast into an argument of a formal nature (*ibid.*, p. 233). But this discussion should not detain us further. Suffice it to remark that the transition from matter to proposition presents the first set of difficulties for a philosophy of experiment. I call this cluster of obstacles the epistemological issue.

2. *The methodological cluster: transcending the list of strategies, methods, procedures, etc.*

The second cluster of obstacles is at the level of manipulation of matter – the very essence of physical experiment; I refer to this cluster as methodological. Here we are concerned with the transition from the myriad of strategies, methods, procedures, conceptions, styles and so on, to some general, cohesive and coherent view of experiment as a method of extracting knowledge from nature. From a philosophical perspective it would have been fruitful had we obtained a general yet fundamental scheme of experiment that captures in a tight economic fashion this myriad of facets and features. This goal may be anathema to Buchwald's historical view of experiment, but in my opinion it is crucial for a philosophical understanding.

A convincing historical account that exhibits the enormous variety of facets and features which experiment possesses is Darrigol's notion of "transverse principles" which he applied to nineteenth century electrodynamics. These principles are not general rules of scientific method; they are rather methodological precepts that regulate at once theory and experiment, hence "transverse principles". Guided by tradition or one's own ingenuity, the physicist follows a transverse principle that links one's theoretical conception of the physics which one studies, to actual experimentation. Clearly, the application of the principle contributes much to the formation and definition of the physicist's methodology (Darrigol 1999, pp. 308, 335).

Consider Faraday for an example. According to Darrigol, Faraday's theories "were rules for the distribution and the interplay of various kinds of forces." Faraday dispensed with the Newtonian distinction between force and its agent. In Faraday's view, "an agent could only be known through actions emanating from it" (*ibid.*, pp. 310–311). Thus, the best course to take in the study of body acting on anther body consisted in mapping the various positions and configurations of the body acted upon. This position called for a principle of contiguity. It is this principle that regulated, according to Darrigol, both the theoretical and experimental practice of Faraday:

> On the theoretical side, this principle entailed his concept of the lines of forces as chains of contiguous actions and his rejection of the dichotomy between force and agent. On the experimental side, it determined the emphasis on the intermediate space between sources and the exploratory, open charactèr of his investigations (*ibid.*, p. 312).

When Darrigol juxtaposes this approach of Faraday to the studies of other nineteenth-century electrodynamicists, the variety and richness of conceptions of theory and experimental practices become apparent.

Darrigol argues persuasively for a close connection between theory and experiment in nineteenth-century electrodynamics. As it is so tightly connected to theory, the conception of experiment and its actual procedures become, at least in this historical episode, enormously varied and complex. The question immediately presents itself as to how should one, as a philosopher, capture in general terms this enormous variety of conceptions of experiment and the concomitant practices of material procedures?

To take another example, Rom Harré analyses experiments by their assigned goals: spelling out the formal aspects of the method involved (e.g., finding the form of a law inductively); developing the content of a theory (e.g., finding the hidden mechanism of a known effect) and technique (e.g., the power and versatility of apparatus). Like Darrigol's "transverse principles", Harré's principle of organization of kinds of experiment according to their goals also demonstrates the enormous variety of facets and features which experiment possesses (Harré 1983).

In his essay on the leading features of physical experiment, Mach realizes that these features may not be exhausted. It seems then that a generalization may not be attained. The formative features of experiment, which Mach describes, have been abstracted, so he writes,

> from experiments actually carried out. The list is not complete, for ingenious enquirers go on adding new items to it; neither is it a classification, since different features do not in general exclude one another, so that several of them may be united in the experiment (Mach 1976, p. 157).

Is the list indeed open or is it in fact in the final analysis constrained? If no constraints were to be imposed on this method of inquiry, then no classification and indeed no generalization would be obtained. The approach, in a word, would be eclectic and *ad hoc*.

A good illustration of a detailed list which goes beyond Mach's preliminary list and yet remains *ad hoc*, is Allan Franklin's list of "epistemological strategies" which he convincingly buttresses with elaborated case studies. Here is the list of strategies which Franklin has drawn:

1. Experimental checks and calibration, in which the apparatus reproduces known phenomena.
2. Reproducing artifacts that are known in advance to be present.
3. Intervention, in which the experimenter manipulates the object under observation.
4. Independent confirmation using different experiments.
5. Elimination of plausible sources of error and alternative explanations of the result.
6. Using the results themselves to argue for their validity.
7. Using an independently well-corroborated theory of the phenomena to explain the results.
8. Using an apparatus based on a well-corroborated theory.
9. Using statistical arguments (Franklin 1990, p. 104; *cf.* also 1986, chs. 6, 7, and 1989).

Franklin argues that these strategies have been designed to convince experimenters that experimental results are reliable and reflect genuine features of nature. The list of strategies demonstrates according to Franklin the different ways experiments gain credibility. Practising scientists pursue such strategies to provide grounds for rational belief in experimental results

(Franklin 1989, pp. 437, 458). For Franklin the use of these strategies has then the "hallmark of rationality" (Gooding et al. 1989, p. 23) and in that sense he is seeking to contribute to a philosophy of experiment.

However elaborated and complex, the list of strategies which Franklin puts forward, is essentially similar to the list which Mach presents in his essay on the leading features of experiment. Like Mach, Franklin is aware of the limitation of this approach – the account is *ad hoc*. Franklin indeed states that the strategies he documented are neither exclusive nor exhaustive. Furthermore, these strategies or any subset of them do not provide necessary or sufficient conditions for rational belief. "I do not believe", he states, that "such a general method exists" (Franklin 1989, p.459). Nevertheless, Franklin is convinced that scientists act rationally. According to the unfailing optimism of Franklin, scientists use, as Gooding, Pinch and Schaffer aptly put it, "epistemological rules which can be applied straight-forwardly in the field to separate the wheat of a genuine result from the chaff of error" (Gooding et al. 1989, pp. 22–23).

Franklin is much concerned with the working scientist, or rather the practising experimenter, and it appears that the strategies he lists have been in fact abstracted from actual experiments, precisely as Mach did a century earlier. As such his list, although rich and varied, remains eclectic and *ad hoc*. While each item on the list provides a thorough and detailed illustration of an experimental procedure that is designed to give grounds for rational belief, there appears to be no overall guiding principle to govern the list itself. Such a list cannot be completed since no constraint is being imposed. A coherent generalisation appears therefore impossible.

This is then another problem that is posed to the philosopher of experiment, namely, how to transcend "the list"? How to generalize the various items that comprise the list? In attempting an answer to this question we should give heed to Hacking's warning and be careful not "to slip back into the old ways and suppose there are just a few kinds of things, theory, data, or whatever" (Hacking 1992, p. 32; *cf*., p. 43).

THE "'ETC.' LIST"

Following Hacking, I call this problem the "'etc.' list". In his "Self-Vindication" paper, Hacking refers to several authors and in particular to Pickering and Gooding, identifying in their writings lists of items. So, for example, what Pickering calls "pragmatic realism" is the co-production of: "facts, phenomena, material procedures, interpretations, theories, social relations etc." (Hacking 1992, p. 31). Similarly, Hacking portrays Gooding as having another "'etc.' list." According to Hacking, Gooding "speaks of an 'experimental sequence' which appears as the 'production of models, phenomena, bits of apparatus, and representations of these things'" (*ibid.*, p. 32). We agree, Hacking continues, "that the interplay of items in such a list brings about the stability of laboratory science" (*ibid.*). On his part, Hacking gives the *matériel* of an experiment a crucial role to play in the stabilization process of experimental science. By the *matériel* he means

> the apparatus, the instruments, the substances or objects investigated. The matériel is flanked on the one side by ideas (theories, questions, hypotheses, intellectual models of apparatus) and on the other by marks and manipulations of marks (inscriptions, data, calculations, data reduction, interpretation) (*ibid.*).

It looks then as if Hacking presents us with an "'etc.' list" of his own. Hacking however is not content with "lists and etc.'s" (*ibid.*), and he ventures a taxonomy of elements of experiment which takes him further afield, beyond Mach and Franklin.

The conception that in experiment the *matériel* is flanked on one side by *ideas* and on the other by *marks* is the clue to Hacking's proposal for making the open list converge onto three groups of elements of experiment, namely, "ideas, things, and marks" (*ibid.*, p. 44). "Ideas" are the intellectual components of experiment; "things" represent the instruments and apparatus, and finally "marks" comprise the recording of the outcomes of experiment. Apparently, Hacking is not worried by Mach's claim that classification will not do, "since different features do not in general exclude one another, so that several of them may be united in the experiment" (Mach 1976, p. 157). In fact, Hacking delights in constructing a flexible taxonomy, since in his view the stability of experimental results arises from precisely the very interplay of elements – whatever the case may be the taxonomy should *not* be rigid (Hacking 1992, p. 44). With this taxonomy Hacking seeks at once to demonstrate, in his words, the "motley of experimental science", and to contribute towards a philosophy of experiment so that one would not meander, as he puts it, "from fascinating case to fascinating case" (*ibid.*, pp. 31–32).

In what follows, I wish to address this second cluster of problems, that is, the methodological issue – the "etc." list. My objective is to transcend the list much in the spirit of Hacking but based on a different line of argumentation, then reach the taxonomic stage and aim beyond it to experimental principles.

THE GUIDING IDEA: APPROACHING KNOWLEDGE FROM THE PERSPECTIVE OF ERROR

As I have indicated, my guiding idea is to study experiment by the nature of its possible faults. I suggest that light may be shed on experimentation by examining and ordering possible sources of error in experiment. My approach takes then a different route altogether from that of Franklin. I am not seeking epistemological strategies that are designed to secure reliable outcomes that may in turn provide basis for rational belief. As I have argued, this approach results in an open, *ad hoc* list. I am looking rather for general characterizations of classes of possible sources of error. We shall see that in many respects the emerging typology of classes of experimental error reflects, albeit from a negative perspective, Hacking's typology. There will be however some crucial differences. It is hoped further that the resultant typology would serve as a framework for developing a theory of experiment out of which general principles may emerge.

By way of clarification, here is a brief account of how sources of experimental error may be broached. Consider the standard approach to experimental error, that is, the dichotomy of systematic and random error. Clearly, this dichotomy reflects an interest in the mathematical aspect of error: does a deterministic law govern the error? Or is it a statistical law? In the former case, as is well known, the error is systematic and in the latter it is random. The dichotomy is very useful and much in use in the practice of experimentation, especially in the analysis of the results by introducing correction terms and reducing the data. The dichotomy could therefore be included in the list of strategies. However, the distinction throws no light on the source of the error; in other words, philosophically it is not useful. Error that may originate in the presupposition of incorrect background theory is classified together with an error that has originated in a faulty calibration – both being systematic. For another example, small error in judgment on the part of the observer in estimating the scale division and unpredictable fluctuations in conditions such as temperature or mechanical vibrations of the apparatus, are classified together since these errors are all random in nature (for a detailed analysis see Hon 1989b, pp. 474–479).

I maintain that for philosophical purposes analysis should be focused on the source of error while clear distinctions should be drawn among different kinds of possible sources. From an epistemological perspective, one is interested in the source of error and not so much in the mathematical features of the error and the means of calculating it away – the causal feature being of a higher interest than the pragmatic one. Thus, for example, errors that have originated in the use of the apparatus should be set apart from errors that pertain to the interpretation of the data. It is hoped that once distinctions among the different kinds of source of error are being introduced, retained and elaborated, the structure of the method at stake would come to light. Specifically, as we shall see, the features of the different kinds of source of error reflect the various elements that are involved in experimentation.

The approach to knowledge from its negative perspective, that is, from errors and faults, is not new. In fact, "the first and almost last philosopher of experiments" – to use Hacking's characterization of Francis Bacon (Hacking 1984, p. 159) – employed a similar methodology. Bacon was philosophically aware of the problem of error and explicitly addressed it. Indeed, he deployed the notion of error as a lever with which he hoisted his new program for the sciences. As expounded in the *Novum Organum* (Bacon [1620] 1859; 1960; 1989; 2000), his programmatic philosophy consists of two principal moves: first, the recognition of error and its rebuke if not elimination, and then the commencing anew of the true science based on experiment and induction. I shall presently argue that Bacon's conception is found wanting especially when experiment, the very instrument of his research, is in question. The shortcomings of his approach would be the key to my move. So here is a précis of Bacon's theory of error.

BACON'S TYPOLOGY OF ERRORS: THE FOUR IDOLS

Bacon argues in his celebrated *Novum Organum*, that Aristotle "has corrupted Natural Philosophy with his Logic; ... he has made the Universe out of Categories" (Bacon 1859, p. 39 (I, lxiii)). In Bacon's view, the application of Aristotle's doctrine has rather the effect of confirming and rendering permanent errors which are founded on vulgar conceptions, than of promoting the investigation of truth (Bacon 1859, pp. 13–14 (I, xii); *cf.*, Bacon 2000, p. 10 (Preface to "The Great Renewal") and p. 28 (Preface)).

Bacon builds his program on the doctrine that truth is manifest through plain facts, but for this claim to be valid the student of nature has to get rid of all prejudices and preconceived ideas – "freed from obstacles and mistaken notions" (Bacon 2000, p. 13 (Preface to "The Great Renewal")). As Bacon instructs, "the whole work of the mind should be recommenced anew (*ut opus mentis universum de integro resumatur*)" (Bacon 1859, p. 4 (Preface); 1989, p. 152); only then would the student experience things as they are. "Our plan", he explains, "consists in laying down degrees of certainty, in guarding the sense from error by a process of correction ... and then in opening and constructing a new and certain way for the mind from the very perceptions of the senses" (Bacon 1859, p. 3 (Preface)). In this way, Bacon concludes, "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 (*Etenim verum exemplar mundi in intellectu humano fundamus; quale invenitur, non quale cuipiam sua propria ratio dictaverit*)" (*ibid.*, p. 120 (I, cxxiv)). Thus the first task of the scientist is to eliminate errors from his or her cognition by the "expiation and purgation of the mind (*expiationibus et expurgationibus mentis*)", and only then can the scientist enter "the true way of interpreting Nature (*veram interpretandæ naturæ*)" (*ibid.*, p. 51 (I, lxix)). Bacon states explicitly this objective in the full Latin title of the book: *Novum Organum, sive indicia vera de interpretatione naturæ*, that is, *The New Instrument, or True Directions for the Interpretation of Nature* (Bacon 2000, p. 11, fn 8). The project then is to put an end to an unending error – *infiniti erroris finis* (*ibid.*, p. 13 (Preface to "The Great Renewal"); 1989, p. 133) and to seek "a true and lawful marriage between the empirical and the rational faculties (*Atque hoc modo inter empiricam et rationalem facultatem ... conjugium verum et legitimum in perpetuum nos firmasse existemamus*)" (Bacon 2000, pp. 11–12 (Preface to "The Great Renewal"); 1989, p. 131).

Bacon therefore finds it necessary to expound in considerable detail the subject of the obstacles to the true interpretation of nature, before proceeding to unfold his positive program: the method of inductive inquiry based on experimentation. He devotes nearly the whole of the first book of *Novum Organum* – "the destructive part" (Bacon 2000, p. 89 (I, cxv)) – to the examination of these obstacles: "the signs and causes of error (*signis et causis errorum*) and of the prevailing inertia and ignorance" (*ibid.*, p. 89 (I, cxv); 1989, p. 210) which he calls idols, idols of the mind. The term "idol" conveys at once the meaning of the Platonic concept of *eidolon* – fleeting, transient, image of reality as well as religious undertones. *Eidolon* stands as an antithesis to the concept of *idea*: "*humanae mentis* idola" vs. "*divinae mentis ideas*" (Bacon 1859, pp. 16–17, fn (I, xxiii)).

Although Bacon claims that "to draw out conceptions and axioms by a true induction is certainly the proper remedy for repelling and removing idola" (*ibid.*, p. 21 (I, xl)), he still finds it of great advantage to explicitly indicate the idols and expound them in detail. For, as he explains, "the doctrine of idola holds the same position in the interpretation of Nature, as that of the confutation of sophisms does in common Logic" (*ibid.*, p. 21 (I, xl)). In other words, to use Jardine's formulation, "the idols ... bear a relation to the inductive method analogous to that which cautionary lists of fallacious arguments bear to syllogistic" (Jardine 1974, p. 83). As I have indicated, I wish to advance further from mere "cautionary lists" and to obtain a conceptual scheme of experiment based on a typology of sources of error. Bacon's theory of error, his typology of idols and its critique, serves as a philosophical illustration of the approach I am taking.

Bacon classifies four types of idol that, as he puts it, "block men's minds (*mentes humanas obsident*)": idols of the tribe (*tribus*), the cave (*specus*), the marketplace (*fori*) and the theatre (*theatri*) (Bacon 2000, p. 40 (I, xxxix)).

I) Idols of the tribe

The first type of idols consists of idols of the tribe; that is, errors incidental to human nature in general. The most prominent of these errors are the tendency to support a preconceived opinion by affirmative instances, whilst neglecting all counter examples; the tendency to generalize from a few observations, and to consider mere abstractions as reality. Errors of this type may also originate in the weakness of the senses, which affords scope for mere conjectures (Bacon 1859, pp. 21, 24–29 (I, xli, xlv-lii)). Bacon warns the student of Natural Philosophy against the belief that the human sense is the measure of things. For Bacon, "the human intellect is like an uneven mirror (*speculi inæqualis*) on which the rays of objects fall, and which mixes up its own nature with that of the object, and distorts and destroys it" (*ibid.*, p. 21 (I, xli)). To obtain the true interpretation of nature, the human mind should function, according to Bacon, like an even mirror.

II) Idols of the cave

The second kind of idols consists of idols of the cave. These errors are incidental to the peculiar mental and bodily constitution of each individual (the cave is a direct reference to Plato's simile in the *Republic*). These errors may be either of internal origin, arising from the peculiar physiology of the individual, or of external origin, arising from the social circumstances in which one is placed by education, custom and society in general (*ibid.*, pp. 22, 29–30, 32–33 (I, xlii, liii, lviii)).

III) Idols of the marketplace

The third class of idols comprises idols of the marketplace, that is, errors arising from the nature of language – the vehicle, as Bacon puts it, for the association of men, their commerce and consort (*ibid.*, pp. 22–23, 33–35 (I, xliii, lix, lx)). Language, according to Bacon, introduces two fallacious modes of observing the world. First, there are some words that are merely "the names of things which have no existence (as there are things without names through want of observation, so there are also names without things through fanciful supposition)." Secondly, there are "names of things which do exist, but are confused and ill defined" (*ibid.*, p. 34 (I, lx)). Bacon is aware of the opaqueness of language to nature and that may lead the researcher astray. He therefore cautions the researcher of the faults of language.

IV) Idols of the theatre

Finally, the fourth class of idols consists of idols of the theatre. These are errors which arise from received "dogmas of philosophical systems, and even from perverted laws of demonstrations" (*ibid.*, p. 23 (I, xliv); *cf.*, pp.35–49 (I, liv, lxi-lxvii)). Here Bacon refers mainly to three kinds of error: sophistical, empirical and superstitious. The first error corresponds to Aristotle who has, according to Bacon, "made his Natural Philosophy so completely subservient to his Logic as to render it nearly useless, and a mere vehicle for controversy" (*ibid.*, p. 30 (I, liv; *cf.*, lxiii)). The second error, the empirical, refers to leaping from "narrow and obscure experiments" to general conclusions. Bacon has in mind particularly the chemists of his time and Gilbert and his experiments on the magnet (*ibid.*, pp. 41–42 (I, liv, lxiv; *cf.*, lxx)). The third error, the superstitious, represents the corruption of philosophy by the introduction of poetical and theological notions, as is the case according to Bacon with the Pythagorean system (*ibid.*, pp. 42–44 (I, lxv)).

Concluding his discussion of the idols, Bacon demands that all of them "must be renounced and abjured with a constant and solemn determination" (*ibid.*, p. 49 (I, lxviii)). He insists upon purging (*expurgandus*) and freeing (*omnino liberandus est*) the intellect from the idols, so that "the approach to the Kingdom of Man (*regnum hominis*), which", as Bacon conceived of his quest, "is founded on the Sciences, may be like that to the Kingdom of Heaven (*regnum cœlorum*)" (*ibid.*). Thus, having performed these "expiations and purgations of the mind", one "may come to set forth the true way of interpreting Nature" (*ibid.*, p. 51 (I, lxix)). The religious connotation is explicit and should be underlined.

Clearly, Bacon's doctrine of the idols is systematic and methodical if somewhat contrived. He neatly classifies the idols as "either adventitious or innate. The adventitious," Bacon explains, come into the mind from without – namely, either from the doctrines and sects of philosophers or from perverse rules of demonstration. But the innate are inherent in the very nature of the intellect, which is far more prone to error than the sense is. (Bacon 1960, p. 22 (The Plan of the Great Instauration); on the history of Bacon's scheme see Spedding, Note C, in Bacon 1989, pp. 113–117, and p. 98 fn 1).

The classes of idols proceed progressively from the innate to the adventitious, from the most persistent to the easiest to discard. They reflect as much as "they are separable or inseparable from our nature and condition in life", to use Spedding's formulation (Bacon 1989, p. 91 fn 4; 98 fn 1 and Note C, pp. 113–117). The idols commence with the general character of human beings – the tribe – move on through the features of individuals that comprise the tribe – that is, the cave – further on to the daily intercourse of common life: negotiations and commerce between individuals – the marketplace – and reach finally the doctrines that individuals conceive and believe in – the theatre. Bacon is aware of the fact that the innate features are hard to eradicate, so that these idols cannot be eliminated. "All that can be done", he instructs, "is to point them out, so that this insidious action of the mind may be marked and reproved (else… we shall have but a change of errors, and not clearance)…" (Bacon 1960, p. 23 (The Plan of the Great Instauration)). By contrast, the adventitious idols, principally those of the theatre, could and should be eliminated (Bacon 2000, p. 49 (I, lxi)). Having undergone these epistemological ablutions, and "clarified the part played by the nature of things (*rerum natura*) and the part played by the nature of the mind (*mentis natura*)", one is ready according to Bacon to commence anew the true interpretation of nature (*ibid.*, p. 19 (Plan of "The Great Renewal"); 1989, pp. 139–140. *Cf.* I, cxv).

Bacon designed the typology to shed light on the nature of sources and causes of error (*causas errorum*) (Bacon 1989, p. 186 (I, lxxviii)). The scheme of idols presents a systematic and methodical view of the elements involved in the obstruction of knowledge: the interplay of sources of error pertaining to the nature of the mind in general, to individuals and their community, to language and doctrines. The scheme may appear somewhat artificial, but it constitutes an essential element of Bacon's comprehensive conception of the emergence of new knowledge and its impediments. In many respects the scheme of idols anticipated new disciplines, namely, the study of anthropology, ethnology, psychology, linguistic and cultural, political and religious ideologies (Coquillette 1992, pp. 233–234; for references see p. 300, fn 24).

A CRITIQUE OF BACON'S SCHEME

The question naturally arises whether or not this all-embracing typology of sources of error is applicable to the very method of research that Bacon advocates for use, that is, experimentation. "It will doubtless occur to some", Bacon acknowledges the question, 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 (Bacon 1859, pp. 111–112 (I, cxviii)).

This appears to be a surprising remark. Could it be that Bacon's proposed method of research is open to objections and that all the cleansing and ablutions were for nothing? No! Bacon dismisses the threat right away; "this is nothing", he exclaims, "for it is necessary that such should be the case in the beginning." By way of an analogy he 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 afterwards easily expunged and rejected by the discovery of Causes and Axioms (*ibid.*, p. 112 (I, cxviii)).

Bacon assures us that we should not be disturbed by these objections and he reiterates this confidence in his outline for experimental history (Bacon 1960, p. 280 (viii)). However, he admits 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 (Bacon 1859, p. 112 (I, cxviii)).

Thus, if there lurked at times "something false or erroneous" in Bacon's Natural History which have been proved with "so great diligence, strictness, and", Bacon adds, "religious care", what then must be said, he asks rhetorically, "of the ordinary Natural History, which, compared with ours, is so careless and slipshod?, or of the Philosophy and Sciences built on ... quicksands?" (*ibid.*)

Notwithstanding Bacon's resolute assurance, the objections are disturbing. Bacon appears to be waving his hands, so to speak, rather than providing convincing arguments in defence of his position. He would have us believe that the analogy between a printer's error and an experimental error is a faithful one. However, it is precisely the sense of the context – the meaning which is given according to Bacon's analogy – that the experimental sciences lack and in fact seek to discover. The two types of error, namely, the printer's and the experimental, are categorically different. (I distinguish elsewhere between these two possible faults. I call the former mistake and the latter error, see Hon 1995.)

Surprisingly, it appears that Bacon did not apply consistently his critical scheme of errors to the very instrument of his inquiry – experiment. Admittedly, he was concerned with errors that beset the mind: once one had purged one's mind from the idols and, to use Bacon's mirror metaphor, smoothed away with religious fervour every protrusion and cavity in one's intellect so that it became an even surface reflecting genuinely the rays of things (Bacon 1960, p. 22), one was then ready to embark on the true way of interpreting nature. At issue here is not whether this instruction to cleanse one's mind is practicable or not, but rather can the instrument of one's inquiry be itself an object of critical scrutiny. Indeed, as we have seen, it had taken some time before the question: "What exactly is an experiment in physics?" was explicitly raised and addressed (Duhem 1974, p. 144).

The persistent impediment that the occurrence of errors poses knowledge resulting from experimentation is not covered by Bacon's scheme of idols of the mind. Bacon's trust in his method of inquiry, which he expressed with his off-hand dismissal of experimental errors, is objectionable. I follow up this criticism and propose to examine the different idols that beset experiment.

THE IDOLS OF EXPERIMENT: SCRIPT, STAGE, SPECTATOR AND MORAL

The construction of a scheme of idols that beset experiment has a similar objective to Bacon's scheme, but the analysis goes further in that it explicitly argues that the scheme reflects underlying principles of experimentation, that is, the principle of classification reflects the elements that comprise experiment and their interrelations. My intent, to repeat, is not to seek strategies in an *ad hoc* fashion following Mach and Franklin. That is, to refer once again to Gooding, Pinch and Schaffer's well phrased remark that, in Franklin's view "there are epistemological rules which can be applied straightforwardly in the field to separate the wheat of a genuine result from the chaff of error" (Gooding et al. 1989, pp. 22–23). The objective is not to list such rules in an eclectic way, but rather to construct a constrained scheme of "the chaff of error" that reflects the structure of experiment as an instrument of inquiry designed to secure knowledge.

In the spirit of the metaphorical language of Bacon and following his idols of the theatre, I suggest to discern four kinds of idol that beset experiment: idols of the *script*, the *stage*, the *spectator* and the *moral*. The image of theatrical play constitutes a convenient and useful metaphorical setting for experiment since, like a play enacted on stage, an experiment is the result of an activity that has truly "a show" at its centre (Cantor 1989, pp. 173–176). In an experiment, nature is made, if you will, to display a show on a stage conceived and designed in some script. The show is observed and registered by a human or automated spectator and, finally, interpretation is proposed with a view to providing a moral – that is, the outcome of the experiment as knowledge of the physical world.

Error is a multifarious epistemological phenomenon. It is an expression of divergence whose mark is discrepancy – a discrepancy which emerges from a procedure of evaluation against a chosen standard. The nature of this discrepancy, the reason for its occurrence, how to treat it and what can be learnt from it once it has been perceived and comprehended, constitute the vast subject of the problem of error. Each of the four different idols depicts different kinds of cause of discrepancy that may arise at different stages of the process that makes an experiment.

Experiments proceed essentially in two stages: *preparation* and *test*. In the preparation stage the experimenter sets up the initial conditions of the apparatus and the system within which the experiment is designed to evolve – this is the theoretical and the material framework of experiment. Once the experimenter sets the framework, the experiment may commence its runs: the testing – the evolution of the system within the designed framework. I should underline that I use the term "test" in a very loose sense: an experiment is not necessarily a test of some theory. In fact, many experiments (e.g., in physics) have to do with determining some constant of a certain material or a system. However, the dichotomy between these two distinct stages: the *preparation* and the *test*, is crucial in the sense that experiment always exhibits the evolution of a prepared system. (For further analysis see Hon 1998a, §6.)

Constituting a typology of sources of error, the idols reflect the roles that faulty elements would play in the overall structure of experiment. It may be seen immediately that the idols of the script and of the stage are associated with the *preparation*, whereas the idols of the spectator and of the moral pertain to the *testing*. In this way the idols cover all possible faults in terms of the different contexts in which sources of error may crop up in experiment. The claim then is that possible sources of error arranged as they are in four different idols, illuminate the structure of experiment.

A distinct characteristic of the proposed taxonomy is its focus on the source rather than on the resultant error. By concentrating on the definitions of different classes of source of error, the typology illuminates from a negative perspective the elements which are involved in experiment and their inter-relations. Thus:

- An incorrect or ill-suited background theory (e.g., the application of Stokes' law to the very tiny and irregular, jagged metal dust particles in Ehrenhaft's alleged discovery of subelectrons (Hon 1989b)) – an idol of the script, is different from
- Assuming erroneously that certain physical conditions prevail in the set-up (e.g., technical difficulties in establishing and continually maintaining in a systematic fashion the physical conditions required for the determination of the Hall effect: a metal specimen kept in very high temperatures and subject to a strong magnetic field) – an idol of the stage.
- Physical, physiological and psychological elements interfering with the depiction of the displayed phenomenon or with the reading of a measuring device (e.g., Blondlot's auto-suggestive perception of N rays (Nye 1980)) – an idol of the spectator, is different from
- Conferring an erroneous interpretation on experimental results (e.g., Franck and Hertz's interpretation that the first critical potential they measured was an ionisation potential (Hon 1989a)) an idol of the moral.

Which way we look at them, errors – that is, experimental errors – would be covered, I submit, by one of the four idols. (For an elaboration of the account of the four idols as classes of experimental error together with historical illustrations see Hon 1989b.)

An important feature of the typology is that it characterises "the script" – the conceptual, theoretical guiding lines of apparatus and instruments, that is, the background theories – as analytically distinct from "the moral": theories that provide the basis for the interpretation of the outcome of experiment. This distinction is logically crucial since it keeps apart the theories that constitute the conceptual framework of experiment and the theories that render the outcome of experiment meaningful. One of the crucial features of the modern method of experimentation, namely, procedures of correction and reduction of data, was recognised at the outset by Galileo. The experimenter should be, as Galileo demands, a good accountant:

Just as the computer who wants his calculations to deal with sugar, silk, and wool must discount the boxes, bales, and other packings, so the mathematical scientist (*filosofo geometra*), when he wants to recognize in the concrete the effects which he has proved in the abstract, must deduct the material hindrances, and if he is able to do so, I assure you that things are in no less agreement than arithmetical computations. The errors, then, lie not in the abstractness or concreteness, not in geometry or physics, but in a calculator who does not know how to make a true accounting (Galileo 1974, pp. 207–208).

Clearly, to conduct successfully this true accounting the experimenter would need to resort to a theory. This theory should be provided by "the script" and not by "the moral", lest the argument would be circular.

Duhem's insightful logical analysis of the correction procedure of systematic error is rightly based on theories that belong to "the script" and not to those that belong to "the moral" of experiment. Duhem observes that a physical experiment is not merely the observation of a group of facts produced under some controlled constraints. If it were so, it would have been absurd to bring in corrections,

for it would be ridiculous to tell an observer who had looked attentively, carefully, and minutely: "What you have seen is not what you should have seen; permit me to make some calculations which will teach you what you should have observed" (Duhem 1974, p. 156).

Following Duhem, observations in experiment have to be capable of translation into a symbolic language, e.g., an equation, and it is physical theories that provide the required rules of translation. The experimenter has constantly to compare, to continue Duhem's line of argumentation, two objects: on the one hand, the real, concrete object which is being physically manipulated – the apparatus, and on the other hand the abstract, symbolic object upon which one reasons (*ibid.*, p. 156). This crucial comparative activity in experimentation, which allows for the introduction of necessary correction terms, depends entirely on "the script". By contrast, the theories that provide the basis for interpretation, that is, "the moral", are brought as it were from without; they are not involved in the process of correcting systematic errors. They are however crucial for correcting errors of interpretation. However, this analytical purism of separating the script from the moral is not strictly adhered to in the laboratory. In the actual practice of experimentation one encounters frequently the toing and froing between the script and the moral in an attempt to stabilise the result. As philosophers we should caution the practicing experimenter of this shoddy logic.

I am now in a position to look critically at Hacking's typology. Hacking, it may be recalled, has grouped experimental elements into three classes: "ideas, things, and marks" (Hacking 1992, p. 44). As I have indicated, my proposed scheme of idols that beset experiment reflects, albeit negatively, Hacking's typology. The scheme of idols diverges however from the typology which Hacking has proposed on two important points. Roughly, "ideas" correspond to "idols of the script", "things" to "idols of the stage" and finally "marks" relate to elements of "idols of the spectator". There remains the class of "idols of the moral" which Hacking's typology appears not to cover; or, alternatively, in his typology "ideas" cover both the background and the outcome of experiment without distinguishing between these two sets of elements. I agree with Hacking that flexibility and interplay of elements are crucial to the stability of experimental results, and so one may cover the fourth set of idols, "idols of the moral", by "ideas". This is, as I have pointed out, a realistic view of experimental practice since "the script" – "ideas" in Hacking's terms – often informs the interpretations of experimental results.

Nevertheless, I do hold strongly that for analytical, logical reasons there should be a clear separation between "the script" and "the moral". Hacking's taxonomy eliminates the crucial difference between these two sets of idols. Again, the "script" consists of theories that are presupposed to govern and shape the experiment – both the working of the apparatus and the application of instruments. The experimenter does not put these theories to the test; they are presupposed at the preparation stage for the purpose of setting up the initial conditions of the experiment and therefore considered correct. These theories provide the framework for the execution of experiment. By contrast, theories that belong to the "idols of the moral" are being tested and may be dispensed with, replaced or rejected and indeed proved false without affecting at all the overall experiment, its argument and the body of its accumulated data.

A fine historical example is the Franck-Hertz experiment. It required just a change of interpretation to render the experiment worthy of the Nobel Prize. Franck and Hertz interpreted their measurement of the initial critical velocity of slow electrons colliding with gas molecules as corresponding to the energy required to initiate the ionization process. Thus, the very first experiment that demonstrated directly and graphically the existence of quantum energy levels in an atom – a curve exhibiting distinct peaks that indicated a stepwise transfer of energy within the atom – was interpreted not as a quantum but as a classical phenomenon. Only in light of a suggestion by Bohr in 1915 did Franck and Hertz reinterpret their experimental results. They resisted Bohr's suggestion for a while, but once they accepted it, namely, that the critical velocity of the accelerated electrons indicated excitation and not ionization of the bombarded atoms, they recognized their experimental results as strong evidence for the existence of atomic energy levels. The experiment thus contributed to the acceptance of Bohr's atomic theory. This was acknowledged by the Nobel committee, who in 1925 declared that Franck and Hertz had demonstrated the existence of energy levels of the type called for by Bohr's theory of the atom. Franck and Hertz received the Nobel prize in physics, as the citation reads, "for their discovery of the laws governing the impact of an electron upon an atom" (Hon 1989a and forthcoming). Clearly, the moral came as it were from without and it was not part of the script.

Furthermore, the alternative of grouping together the "spectator" and the "moral" under Hacking's class of "marks" should also be objected to. Again, the sources of error and procedures of correction that take place in reading data are distinct from analysing, reducing and interpreting the data and rendering them an experimental result. Thus, from the negative perspective, that is, from the perspective of error, it is instructive to split Hacking's "marks" into two different, distinct classes – "spectator" and "moral". The reader may recall that these two idols comprise the second stage of experiment – the test.

CONCLUDING REMARKS

Against the background of collapse and decline of Scholastic epistemology, a breakdown that led to the proliferation of often conflicting views of knowledge, Bacon conceived of a science in which one seeks "to discover the powers and actions of bodies, and their laws limned in matter. Hence this science", according to Bacon, "takes its origin not only from the nature of the mind but from the nature of things". Bacon developed a new logic, which he had designed in order "to dissect nature truly" (Bacon 2000, pp. 219–220 (II, lii); Solomon 1998, p. xv). This new logic should vouch, in Bacon's view, for the true "Interpretation of Nature" (Martin 1992, p. 147). It consists essentially of two moves. The first, as Bacon put it, is the "expurgation of the intellect to qualify it for dealing with truth" (quoted by Martin, *ibid.*), and the move to follow is "the display of a manner of demonstration for natural philosophy superior to traditional logic" (*ibid.*). Bacon developed the scheme of idols to facilitate the first move; the second move proceeds by founding philosophy on natural and experimental history – the furnishing of the material of knowledge itself (*ibid.*, pp. 146–147).

My proposed scheme of the idols of experiment takes its cue from this Baconian two-tier approach to the true way of interpreting nature. However, the point of my scheme is not epistemological but rather methodological – it is here that the analogy to Bacon's approach ends. The proposed scheme carries the critical, Baconian program over to experimentation itself.

The scheme focuses on the different kinds of possible sources of error that may crop up in experiment. In that sense, the scheme reflects the normative aspect of experiment: the practice of seeking to minimize, if not eliminate altogether, experimental errors. However, once the typology is set up, it may be seen that the different kinds of source of error present four different contexts, which together make experiment. In other words, the four idols: the script, the stage, the spectator and the moral, cover all possible sources of error, each idol characterizing a class of sources of error which arise in the same context, that is, discrepancies of similar origin. The constraints imposed by the scheme with its clear delineation of the classes, provide a comprehensive overview of experiment from a negative perspective that does not depend on open lists. It is hoped that studies of the relations between the elements that comprise the idols could provide an insight into the epistemological underpinnings of experimentation. By transcending the list, the set of idols of experiment provides us with both a normative and a comprehensive, conceptual view of experimentation.

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RAFFAELLA CAMPANER

AN ATTEMPT AT A PHILOSOPHY OF EXPERIMENTAL ERROR A COMMENT ON GIORA HON

Giora Hon's paper aims at presenting a philosophical analysis of experiments and their role in the acquisition of scientific knowledge. In an attempt to answer the question "What is a scientific experiment?", the author suggests what he himself defines as a "negative way" to experimentation: in order to uncover the crucial features of experiments, we shall focus on experimental errors, and, more precisely, on their sources.

Hon maintains that accounts of experiments already presented in the literature, such as those by Ian Hacking and Allan Franklin, are inadequate, insofar as they fail to elaborate a coherent and convincing philosophical view of experiment. Instead of looking for some "principle of organisation of kinds of experiments according to their goals" (this volume, p. 266), as the other authors have been doing, Hon puts forward a restricted classification meant to organise errors according to their sources. He maintains that errors are related to the following:

1) An incorrect or ill-suited background theory;
2) Assuming erroneously that certain physical conditions prevail in the set-up;
3) Physical, physiological and psychological elements interfering with the depiction of the displayed phenomenon or with the reading of a measuring device;
4) Conferring an erroneous interpretation on experimental results.

"Which way we look at them" – Hon claims – experimental errors will fall under one of these four "idols" (this volume, p. 278).

These categories reflect, I believe, a conception of experiment as having a strong theoretical component. As suggested by the literature on the topic over the last fifteen years (*cf.*, for example, Pickering, Galison and Gooding, as well as Hacking and Franklin, already mentioned), it is necessary to take into account a much wider range of aspects having to do

M.C. Galavotti (ed.), Observation and Experiment in the Natural and Social Sciences, 285–293.

with scientific activity and experimental practice to elaborate an adequate philosophy of experiment From the list 1) – 4), there emerges a peculiar, almost exclusive attention for theoretical aspects of knowledge: Hon's concern focuses on background *theory*, *assumptions* about set-ups and *interpretations* of results. The only category which seems to leave wider scope for practical aspects is number 3), although it, too, lays more emphasis on *depiction* of the displayed phenomena and *reading* of devices more than, for example, their manipulation. The list mirrors a rather "biased" conception of possible sources of errors if the target is to identify an overall, well-developed epistemology of the latter. A number of actual, practical skills and, so to speak, "practicalities" do not constitute a secondary, peripheral or accessory aspect, but play a pre-eminent role in the performance of a large number of experiments. Non-verbal or pre-linguistic skills and mastery of experimental apparatus, techniques and procedures have been more and more emphasized by recent attempts to reconstruct the peculiar features of scientific research[1]. To refer to some episodes in the history of science analysed in detail in the pertinent literature, Gooding, for example, describes the experiments performed by the French physicist J. B. Biot around 1820, exploring the interaction between electric currents and magnetized needles:

> Biot reports that when the wire was brought close to a horizontally suspended magnetic needle, there was an immediate deviation of the needle [...] But the possibility of observing anything but chaotic needle behaviour depends on skilful manipulation of the wire, and this takes some time to acquire. [...] As we shall see with Faraday and Morpurgo, would-be observers have to *do* quite a bit in order to see anything at all. [...] Scientists engage nature in the fine structure of their experiments. That is where they gain the practical mastery of a phenomenal domain that enables them to develop the linguistic resources and the demonstrative experiments that they use to establish facts about nature (Gooding 1990, p. 133).

And discussing Faraday's investigation of electromagnetism, Gooding highlights that:

> Recent repetition of these experiments has shown the difficulty of seeing what Faraday recorded he saw, even after considerable practice. [...] skilful interaction with the phenomenal world is needed as well as a concept of what might be elicited. When Biot and Faraday arranged their operations and the outcomes as images or instruments they embodied their experience and associated observational skills which had been *impossible* to communicate in verbal and material representations that were *easy* to communicate (Gooding 1990, p. 134; p. 137).

Experimental activity involves a good deal of manipulation of the entities and phenomena investigated and may require some highly sophisticated skills (let us think, for example, of those necessary for chemical syntheses, or of performances of microinjections in cell cultures in molecular biology), as well as skills in elaborating visualizations and in reading of visual images. Hon criticises Franklin's list of strategies "that provide reasonable belief in the validity of an experimental result" (Franklin 1990, p. 103) for being *ad hoc*, and "neither exclusive nor exhaustive" (this volume, p. 267), not inspired by any general guiding principle. Hon's conceptual scheme seems, though, to be based on an over-general, or perhaps partial, typology of sources of error, which runs the risk of failing to account for observational and procedural abilities. Practical aspects, not strictly theoretical, logical, or linguistic ones, seem to be particularly important if, as it is the case here, an epistemology of *experiment* is the final target of the whole inquiry. Hon's analysis is meant to shed light specifically on the crucial features of experiments. "Practicalities" and skills ought therefore to be given a very specific place among the possible sources of *experimental* errors: it is necessary to ensure that Hon takes into consideration all relevant features to make it illuminating specifically for a theory of experiment, and not simply for any general theory of knowledge[2]. The author himself remarks that capturing the enormous variety of variables involved in experimentation is an extremely arduous and puzzling task. Experiments consist in:

> a play of operations in a field of activity, which I call the experimenter's space. The place of the experiment is not so much a physical location [...] as a set of intersecting spaces where different skills are exercised (Pickering 1992, p. 75).

If practical, concrete elements are maintained to be such an essential component of experiments, it seems reasonable to acknowledge their primary role also within an attempt to identify sources of experimental errors.

Hon faces the task of elaborating a philosophical map of the complex array of heterogeneous elements experiments consist in.

> The shaping of experimental systems is a contingent process. It is embedded in instruments, apparatus, technical procedures, materials at hand, and model objects, on the one hand, and it is closely linked to local crafts, research traditions, and wider epistemic as well as practical interests on the other. The decisive question is how these particular segments get articulated, how they condense to a structure that finally develops a dynamics that was not *inherent in these parts per se, and therefore serves as a crystallisation point* for unprecedent knowledge (Hagner and Rheinberger, p. 363).

Is it enough to present a categorisation of errors by source to capture such a complex, intertwined set of elements? At least two issues seem to be at stake: on the one hand, it is to be established whether Hon's typology covers all the possible sources of error; on the other hand, whether an analysis of errors as such can accomplish the challenging enterprise of giving a satisfactory insight on experimentation as a scientific activity. Is it possible – for example – to elaborate a good philosophy of experiment without considering at all the social side of experimental practice, its economical components, human intentionality, plans and goals, or even the role of "common sense" in deciding when to consider an experiment concluded?[3] Although these cannot be strictly regarded as "sources of error", a certain social, public and economic dimension should perhaps be given some space. In many cases, dozens or even hundreds of scientists combine a diverse range of resources in a collaborative effort to perform a single, massively sophisticated experiment (*cf.*, for example, modern light-energy and quantum physics). These features are to be taken into account as playing some role in the working of experiments, but it seems unlikely that they can be satisfactorily represented in terms of error source analysis.

Hon's explicit concern is with what he calls the "methodological cluster of obstacles to the construction of a philosophy of experiment" (this volume, p. 260). When addressing such a cluster,

> we are concerned with the transition from the myriad of strategies, methods, procedures, conceptions, styles and so on, to some general, cohesive and coherent view of experiment as a method of extracting knowledge from nature (this volume, p. 264).

It is debatable whether one can formulate an account of experimental error to solve solely the issues raised by the methodological cluster of problems, without also dealing with those raised by the epistemological cluster, namely the transition from material processes to propositional knowledge. Hon views experiments as arguments:

> An experiment – I claim- can be cast into a formal argument whose propositions instantiate partly states of affairs of material systems and partly inference schemata and some lawful, causal connections. In other words, an experiment implies an argument the premises of which are assumed to correspond to the states of the physical systems involved, e.g. the initial conditions of some material systems and their evolution in time. These premises warrant the argument's conclusion (Hon 1998, p. 235).

It might, however, also be necessary to give some more consideration to the move from material procedures to propositional knowledge, from the performance of instrumental devices and their manipulation to their translation in accounts of phenomena. When trying to identify as completely as possible the sources of error, we need to reconstruct the whole performance of the experiment, that is the process of its occurring, or, in other words, its development. While *historical* studies on experiments have recently been flourishing, a *philosophy* of experiments – Hon highlights – has yet to be advanced. Even if we espouse the author's point of view, we will still need an account of the *dynamic* process of which the experiment consists. Scientific practice has an intrinsic real-time structure[4]. If we are content with merely a classification of possible sources of errors, we might get simply a static image, a "photograph" of how experiments *have* worked, or, rather, of how they failed to work, instead of reaching an understanding of effective inner *workings* of experiments[5]. Some reference to the temporal dimension of the experimental activity seems to be particularly important, especially given that the target of Hon's own paper is to "develop a *historically informed* philosophy of experiment" (this volume, p. 259, italics added).

Following Hon's proposal, to provide a satisfactory answer to the original question "What is a scientific experiment?", the question to be raised is then: "What is an experimental error?". If the experimenters have insufficient practical skills or do not possess sufficient manual dexterity with tools and procedures, the theory and the storage of knowledge behind given techniques cannot be of much use. The experimenter may not possess the necessary practical competences. These often cannot be conveyed verbally, but require lengthy practice to be mastered. Not only the apparatus, but also the experimenter may not be working properly. Is a lack of abilities of this sort to be considered an error, or rather as an "oversight", or some sort of "miscalculation", or a still different kind of "fault"?

In another paper, Hon draws a line between the concept of "error" and that of "mistake"[6]. In Hon's perspective, errors are associated with unavoidable ignorance; they come about when one applies techniques to novel phenomena and is therefore groping, so to speak, in the dark. They occur because an exploration of a *terra incognita* is taking place. Mistakes, in contrast, are associated with avoidable ignorance. They occur while we are walking on a *terra firma* and could be avoided since checking procedures are known and available (*cf.* Hon 1995a, p. 6). In this respect, "material aspects" and practical skills should hence be considered particularly important precisely in the light of the innermost, distinctive features of errors. In experimental enquiry, which tries to breach the borders of acquired knowledge and gropes its way in a vanguard position, practical abilities and manipulative techniques have an extremely important discovery role: encounters with bits of the world not anticipated by any theoretical knowledge often occur through material procedures.

In order to turn a classification of errors into an efficient means toward an epistemology of experiment, the analysis needs to include some understanding of how a number of distinct elements happen to come together into that special source of knowledge an experiment is. The problem comes down to whether a fixed classification of sources of error can on its own provide a satisfactory insight in such a complicated concrete process or activity as experimentation. A classification of errors might run the risk of being considered a kind of *a posteriori* operation: only after errors have been clearly identified and attributed to some causes, is it possible to define their sources. The individuation of sources of errors might be suspected to already presuppose a specific, possibly biased, conception of experiments, their structure and their functioning, whereas the real challenge here is to understand what the added value of a negative route towards a philosophy of experiment is with respect to the "standard", "positive" way. We should, in other words, question whether an analysis of errors constitutes a viable access to the essential functioning of experiments, or, rather, whether an epistemology of experimental errors cannot but presuppose an already quite refined view of what experiments are like. The immediacy of this risk is suggested by a different notion of "error" given by Hon. Error is "an expression of divergence whose mark is discrepancy – a discrepancy which emerges from a procedure of evaluation against a chosen standard" (this volume, p. 277). This seems to hint at the existence of some already established standard or datum point in the definition of error, which would not be present, in this case, as science is proceeding towards what is largely a *terra incognita*.

Hon's attempt to build a new philosophy of experiment in terms of errors and their sources no doubt represents an original approach in the literature on the topic. Especially because of its originality, this shifting of perspective is worthwhile of further development, in order to avoid falling victim of some other "idols" and to shed more light on the complex web of elements that makes an experiment: "knowledge can arise only when there is a possibility of being wrong" (Hon 1995, p. 15), or a possibility of lacking material, manual skills and acting incorrectly.

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NOTES

[1] Commenting on Gooding's work, Thomas Nickles, for example, recalls: "Given the highly formal treatment of reasoning by many philosophers, even to speak of experimental reasoning already threatens to impose an overly verbal, rule-based, indeed theoretical, perspective and to ignore the skilled practice and judgmental behaviour which characterise experimental work. While philosophers and other students of science have long debated the theory-ladenness of observation, Gooding point out what we might call the technique- or skill-ladenness of observation [...] He reminds us that at the frontier experimentalists are, in some respects, novices rather than experts, and he is thereby able to backlight the surprisingly large gaps between the initial detection of observation novelty, its eventual cognitive organisation in the work of an individual, and its later articulation as a finished scientific communication" (Nickles 1988, p. 300).
[2] See, for example, the following reflections on the topic: "...grounding rational lines of inquiry in lucky discoveries of improvement in apparatus seems embarrassing to experimenters, who might like to be granted powers of thought, and who might also crave an image of scientific rationality. Therefore, it is not all that frequent that an experimental paper freely admits that a breakthrough occurred when someone tried some 'sticky tape', 'waste plastic material that happened to be at hand' or 'a new kind of oil' to doctor a balky piece of equipment, but such accidents occur. So, there's a bias against sticky tape in the original accounts, and then again in philosophical reflections. In my opinion, we have to work against the temptation to produce smooth symmetric theories of experimentation. Let me come back to Allan Franklin. [...] The only real representation of experiment [...] in his first book is the glorious photo of a mess of a laboratory on the dust jacket [...] Philosophers still need to get sticky tape on their fingers. In short, we need to get down and get dirty before we will have an appropriate understanding of experimentation" (Ackermann 1990, p. 456); and: "The more abstract, theory-based conception of knowledge familiar from earlier socio-historical studies is gradually turning into a more particularistic conception of the material sites, artefacts and techniques of 'knowledge production'. The focus is more intensive and 'internal' [...], as the aim is to identify the pragmatic strategies and informal judgments made at the worksite when researchers sort through 'messy' arrays of data and decide whether equipment is working properly" (Lynch 1990, p. 476).
[3] *Cf.*, for example, Pickering 1995a, especially pp. 17–23.
[4] On the essential temporal dimension of scientific practice, *cf.* Pickering 1995a and 1995b.
[5] As Hon acknowledges, even if "we have no choice but to analyze experiment *in vitro*", we should "keep a wide eye on its features as an activity *in vivo*" (this volume, p. 261).
[6] See Hon 1995a, especially pp. 6–7.

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GEREON WOLTERS

O HAPPY ERROR
A COMMENT ON GIORA HON

In Catholic Easter Vigil liturgy at a certain point the joy about the resurrection of Christ finds its solemn expression in the paradoxical declamation: "*O felix culpa*!" – "O happy fault!"[1] – That means that mankind ought to be happy to have been laden with the guilt of original sin and other sins, because only such guilt made possible our salvation through the death and the resurrection of Jesus Christ.[2]

In his very elegant paper Giora Hon pursues the same strategy as the fathers of the church, when they coined the happy fault paradox: "O happy experimental error", he seems to be declaiming, "thou shows us the truth about experiment!"[3]

As Hon maintains, this truth about experiment should consist in "a historically informed philosophy of experiment" (p. 260). He distinguishes two approaches that students of the philosophy of experimentation have developed so far: (a) the *epistemological* approach, which claims to bridge the gap between the "material process, which is the very essence of experiment, and propositional knowledge – the very essence of scientific knowledge" (p. 263); (b) the *methodological* approach, which pertains to the "level of manipulation of matter - the very essence of physical experiment" (p. 264). Hon is dissatisfied with the work done to date in both approaches and wants himself to offer an improvement to the methodological approach. This improvement consists basically in his suggestion that unconstrained lists of "epistemological strategies" (p. 266) that are pursued in experiments should be replaced by a comprehensive typology of possible error sources. There are, according to him, exactly four possible source-kinds of experimental error and they positively enable at the same time – o happy fault! – "four contexts which together make experiment" (p. 282).

M.C. Galavotti (ed.), Observation and Experiment in the Natural and Social Sciences, 295–299.

I would like to question or challenge Hon's view in three respects:

(1) I would like to question in a more general way the approach to truth by way of error.

(2) I am doubtful about Hon's conviction that his typology of experimental error and the resulting "comprehensive overview of experiment from a negative perspective" (p. 282) covers everything that may be rightly designated as experiment. In other words, his typology seems to me to be too narrow.

(3) My skepticism about the completeness of his typology of experiment entails some doubt on Hon's dismissal of methodological approaches like those of Mach and Allan Franklin. Their approaches to experiment allegedly consist of open lists of epistemological strategies that one ought to pursue in order to achieve successful experiments. Such lists, in Hon's view, however, do not provide constraints, and without constraints there cannot be anything like genuine "classification", or "generalization" (p. 266).

I turn now to my first line of criticism. I am of the opinion that Hon's concept of experiment, and consequently both his typology of experimental error and the resulting four basic features of successful experimentation are too narrow for the purpose of exhaustively classifying everything that may be rightly called experiment.

What is an experiment, according to Hon? He cites two different characterizations of the experimental activity. The first – and this is explicitly his own – considers "experiment a philosophical system that aims at furnishing knowledge claims about the world" (p. 262). Here he is obviously thinking in an epistemological context. The second characterization of experiment, which is quoted in the context of the methodological approach, regards experiment "as a method of extracting knowledge from nature" (p. 264). It is not clear to me to what extent Hon himself shares also this second characterization. But I take it, that in a rough way he accepts it.

My thesis now is that there are experiments as exemplifications of methods of extracting knowledge from nature that do not fit into Hon's typology.

This typology consists of four classes that elegantly correspond to Francis Bacon's four sources of error. Hon accordingly calls his four sources of experimental error "idols of the script", "idols of the stage", "idols of the spectator", and "idols of the moral". The "idol of the script" consists in assuming an ill-suited background theory. This means, positively, that the first stage of experiment invokes a background theory, or background theories, respectively. The "idol of the stage" originates from wrong assumptions about the prevailing physical conditions of the material setup of the experiment. The "idol of the spectator" is due to "physical, physiological and psychological elements interfering with the depiction of the displayed phenomenon or with the reading of the measuring device" (p. 278). This means, positively, that correctly registering the outcomes of an experiment is an essential component of a comprehensive theory of experimentation. Fourthly and finally, "the idol of the moral" consists in erroneous interpretations of experimental results, which, in turn, means, positively, that the *interpretation* of experimental results is the final component of a comprehensive concept of experiment.

In addition Hon calls the first two positive components of experimentation – i.e. background theory and physical setup – "preparation" whereas reading the results and interpreting them form a second component in performing experiments, which he calls "test" (p.277). "Preparation" and "test" seem to introduce a temporal ordering, two fundamental stages, to the components of the theory of experimentation.

However, there is, in my view, an important new class of experiments that does not fit well into Hon's four components' scheme. I am referring here to a recent article titled "Equipping scientists for the new biology" by three biologists in the journal *Nature Biotechnology* (Vol. 18, April 2000).[4] This one-page paper mostly deals with science policy, or more exactly with the funding of what the authors call "discovery science." In their conception discovery science is opposed to "hypothesis-driven science." Hypothesis-driven science is roughly science as we know it: you somehow generate a hypothesis that subsequently is submitted to tests. Discovery science, on the other hand, is characterized by what the authors claim to be a "new research method." The prototype of applying this new method is the Human Genome Project (HUGO). They also assert that "discovery science requires large-scale facilities for genome-wide analyses, including DNA sequencing, gene expression measurements, and proteomics." More generally, I would like to characterize discovery science as the collecting and analyzing of gigantic masses of data, in order to find characteristic patterns. Discovery science is, as the authors say, a "technology-driven approach to biology and the biomedical sciences." One could call this approach in more traditional terms also "experimental natural history."[5] The authors of the paper rightly remark the following: "Discovery science ... enumerates the elements of a system irrespective of any hypothesis on how the system functions."

Now here it seems to me that discovery science, first of all, is experimental, in the sense assumed by Hon, in that it "extracts knowledge from nature". In large computer runs discovery science detects patterns that cry out, as it were, for interpretation. What is missing here, however, seems to be the background theory that is the first ingredient of stage one, i.e. "preparation", of Hon's typology. There is no background theory in discovery science, at least not in the sense that one finds it in hypothesis-driven science. The other components of Hon's typology, however, do seem to apply also in the case of discovery science. In this context one should note that Hon is cautious enough not to take the word "test" too strictly, when he says that "an experiment is not necessarily a test of some theory" (p. 277).

My second line of criticism has to do with Hon's dismissal of such somehow "rhapsodic" conceptions of experiment that consist in giving open lists of "epistemological strategies" of experiment or similar devices, as have been provided by Mach and others. Against such open lists of strategies Hon states that without constraints they are *ad hoc* and thus somehow unphilosophical (p. 266). I do not believe that in such lists there is no constraint in the sense of an "overall guiding principle" (p. 267). In my view there is such a principle and it is *success*. By choosing success as a constraint for lists of epistemological experimental strategies one achieves in my opinion two goals. On the one hand, one becomes more flexible: one can easily include basic changes in the overall conception of science itself. For this "discovery science" seems to be an example. Thus, one avoids creating a theory that would become the target of Buchwald's warning, quoted by Hon, that "axiomatics and definitions are the logical mausoleums of physics" (p. 260). On the other hand, by pursuing the success strategy, one achieves – to the delight of the philosopher – a unifying perspective of everything that might be included in the list of epistemic experimental strategies.

My third and last point of criticism concerns Hon's approach for arriving at truth by way of a typology of experimental error. To be sure, one can and should learn from errors, in order to avoid them the next time. But the very expression "the next time" points to a problem. You can learn from error only if those situations in which you have fallen into error are basically of the same type as the ones you have to newly cope with. You are, however, at a loss, when fundamentally new situations occur. So, in a general way, Hon's approach of arriving at a true concept of experiment by way of experimental error does not seem to leave sufficient room for experiments that are of a basically new type. We do not know what the future of science will bring forth. And we should be open for surprises.

I have always found the happy-fault formula rather awkward, logically and theologically. In this commentary I have outlined my difficulties with its analogical transfer to the philosophy of science. In short, I suggest replacing Hon's happy-error approach to experiment by a happy-success approach.

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NOTES

[1] The verse in which "*O felix culpa*" occurs is part of the hymn *Exultet jam angelica turba caelorum.* The full verse is: "O felix culpa, quae talem ac tantum meruit habere redemptorem!" – "O happy fault which we received as its reward so great and good a redeemer!". – The *felix culpa* formula seems to originate from a sermon of St. Augustin. From there it made its way via church fathers like Leo the Great to the *Summa Theologiae* of St. Thomas Aquinas (s.th. 3,1,3 ad 3).

[2] When searching the Internet for "felix culpa" I found besides a German rock band of that name a book by Tom Peters with the title *O Felix Culpa...O Happy Fault: How Bad Guys Keep Good Guys Going*. This title seems to express nicely most of the theological content of the *felix culpa*-formula.

[3] I was very proud to have found this analogy between the early Christian theology of salvation and Hon's approach, but – alas! – Hon himself had used it already years before (see Hon, G. 1991. "A Critical Note on J.S. Mill's Classification of Fallacies". *British Journal for the Philosophy of Science* 42: 263–268, p. 264.).

[4] I would like to thank Eric Kubli (Zurich) for directing my attention to this paper.

[5] I owe this very fitting denomination to a conversation with Michael Friedman (Stanford).