GOING AMISS IN EXPERIMENTAL RESEARCH
Acknowledgments

Most of the essays comprising this volume grew out of presentations at the conference, Going Wrong and Making it Right: Error as a Crucial Feature of Concept Adjustments in Experimental Contexts, which we, the editors of this volume, organized in Aegina, Greece, in Spring 2003. The three of us had earlier met at the Max-Planck Institute for the History of Science, Berlin, where we had ample opportunity to discuss our common interest in concept formation and the pivotal role of error in experimental practice. We are most grateful to Hans-Jörg Rheinberger, one of the directors of the MPI, for constant encouragement and generous support of our research projects. Our conference proposal received the financial backing of the Dibner Institute whose then acting director, George Smith, responded enthusiastically to our suggestion to have an international meeting on the theme of going amiss. The conference had been originally planned to take place in Israel, but circumstances were such that it had to be moved to a different location. The meeting proved successful due in large measure to the invaluable support of Kostas Gavroglu, who graciously offered to act as the local organizer for the event. We are most grateful to him for hosting our conference as well as to Dina Dalouka for her assistance. We thank the conference participants for lively and fruitful discussions, and we are particularly grateful to our authors for their contributions and for their patience throughout the volume’s long gestation period. The bulk of the editorial work on the volume was carried out while the three of us were members of the German-Israeli Foundation (GIF) project: Generating Experimental Knowledge: Experimental Systems, Concept Formation and the Pivotal Role of Error—a cooperation between the Philosophy Department at the University of Haifa (Giora Hon), the Max Planck Institute for the History of Science, Berlin (Hans-Jörg Rheinberger) and the Department of History at the University of Wuppertal (Friedrich Steinle). The project was funded by the Foundation Grant No. G-767–217.4/2002. We thank the foundation for its generous support.

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It is a common human trait to wish to disown one’s errors. While it is a truism that one can learn from one’s failures, no one wants to be remembered for them, best to forget one’s faults, left buried in layers of history. Philosophers are concerned with warranted knowledge—error is simply everything that is excluded from the domain of accepted claims to knowledge. It is the historians’ task to uncover the past, but they too prefer to leave failures hidden away. Their worries, however, are more concrete. Historians fear that the study of past errors is intrinsically Whiggish and inadvertently produces anachronistic historical accounts. We take these worries seriously and transform them productively. We are convinced that it is fruitful to uncover forgotten and lost failures, subject them to analysis and learn from their moral. The central tenet of this volume is that failures count; they are quarries for knowledge. To be sure, failures should not be considered knowledge. Strictly speaking, they have proven to be false claims to knowledge, or, alternatively, the ground for a claim to be formulated could not be provided. We argue, however, that the study of failures, errors, pitfalls and mistakes shed light on the way knowledge is pursued and indeed generated, and we substantiate this position with historical accounts and philosophical analyses.

Science is a field of inquiry in which failures assume specific characteristics. If there is a method to scientific pursuits, their principles and features determine the scope and nature of the failures. We propose to examine the failures of scientific claims like an engineer who studies the breakdown of a certain technological system. However, unlike the engineer who knows well the expected performance of the technological system he or she has helped design, the historian and the philosopher of science are not privy to the original design; hence the inherent vagueness in the determination of characteristics of scientific failures. This is reflected in the title of this volume, Going Amiss in Experimental Research. “Going amiss” comprises two related themes: first the experimental results that proved wrong, and secondly the challenges that practitioners are facing in their everyday endeavors to generate experimental knowledge. The notion

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“going amiss” reminds us of the fact that even in those cases where everything turns out right, numerous pitfalls and confusions had to be overcome. We are dealing not only with errors but also with misguided conceptions, dead ends, and reorientations.

**Approaches to Error and Going Amiss**

One specific aspect of our enterprise has already received some attention in recent years—the problem of error. Philosophers of science have begun exploiting error as a probe into scientific practice. Deborah Mayo and others have provided detailed analyses of the conceptual tools of error statistics (Mayo 1996). According to Giora Hon, an experimental setting comprises an ensemble of materials, instruments, measuring devices and of course experimenters. The setting might be devised according to explicit clear-cut questions and goals and thus employ specific background assumptions and auxiliary theories. The instances of error are shaped then by the procedures that the setting stipulates and by the underlying methodological assumptions. Bringing to light those elements that were most prone to error or failure and characterizing the sources of these problems could elucidate the structure of the experiment at stake (Hon 1989). Jutta Schickore has presented an analytical map of the field while arguing further that error can play epistemologically productive roles. She shows that “arguing from error” took center stage in the early nineteenth century, and that the scientists’ encounters with the possibility and diversity of error gave rise to epistemological optimism (Schickore 2005). Here we see how the theme of error provides a fertile ground for dovetailing philosophy of science with history of science.

Like philosophers, historians too begin to see the riches of failures. Jed Z. Buchwald and Allan Franklin state their claim in the very title of their book: *Wrong for the Right Reasons*. They remark that “there is more to scientific error than merely noting that \( x \) … working in a particular locale, just believed that \( y \) was wrong. Of course, one might argue that it is hardly of any compelling interest to know that \( x \) was right and \( y \) wrong, at least for matters long gone. So what?” And they respond vigorously by claiming that “it’s not a question of handing out report cards on the past”; rather, the aim is to understand “thoroughly” what was done. This, they contend, “can be achieved only by means of a mastery of contemporary technique … that uncovers apparent *lacunae* and problems” (Buchwald and Franklin 2005, 1). As historians of science, Buchwald and Franklin seek understanding of past practices and claims to knowledge through the examination not only of successful but also of failed attempts at gaining knowledge. The determination and identification of error in past scientific studies thus become a historiographical means of conveying coherently not only the “state of the art”, but also its shortcomings. Buchwald and Franklin imply that more general lessons can be drawn from this endeavor. They maintain that “meaningful statements can be made about mistakes and errors in science, and that these statements reach beyond the momentary and the local” (Buchwald and Franklin 2005, 3). By
examining how past scientists mastered the problem of error, they hope to recover enduring standards of scientific practice.

However, scientists must grapple with many kinds of pitfalls and not all of them are best described as errors and mistakes. Confusion, for example, indicates that not everything is going right. But this does not necessarily mean that an error has occurred. In this volume we highlight the diverse ways in which practicing scientists may go amiss. We revisit the historiographical worries arising from the study of past failures and suggest ways of alleviating them. We develop a nomenclature that is designed to capture different ways of going amiss. It provides a framework for the analysis of the specific epistemic roles of going amiss in scientific practices. The proposed nomenclature reflects our conviction that while there are countless ways of going amiss in experimental research, a viable taxonomy, if not a systematic analysis, of this intriguing phenomenon may be given. Finally, we demonstrate the productivity of going amiss.

**Historiographical Challenges**

One of the major worries that historians have about the study of past failures is that applying “failure” or related notions to past scientific endeavors may encourage anachronistic history. This is because such an approach often assesses whether past scientists went amiss according to current theories and today’s standards, or explains with hindsight the reason for going amiss, knowledge to which the contemporary scientist had no access.

But we are not forced to adopt this perspective. We have a variety of options. We may for example seek to adopt the *perspective of the past practitioners*: How did they identify failures and other pitfalls in their work? What kinds of terms did they use? How did they cope with the problem? What standards of evaluation and assessment did they apply? We can trace this notion of *failure over time* and follow the *changing discourse on error*. What factors shaped the course of these debates? How did technological, socio-cultural, institutional, metaphysical, and other conditions influence the scientists’ discourses and practices surrounding the phenomenon of going amiss? We may also utilize this analysis of past failures to shed light on the *metaphysical and epistemological commitments* of the practitioners (Hon 2004). In this perspective, the examination of things that went amiss can serve as a tool for clarifying modes of obtaining experimental knowledge. Alternatively, we may seek to establish whether the scientists went amiss according to the *standards of their own time*. This is indeed the perspective that Buchwald and Franklin offer (see especially Buchwald’s contribution). Finally, to assess the range and limits of past tools and evaluate the practitioners’ judgments about them, we may also investigate the *performance of extant historical instruments* with modern means. None of these enterprises is intrinsically Whiggish, but all of them are extremely fruitful for understanding the role of failure in the generation of experimental knowledge.
The Nomenclature of Going Amiss in Experimental Research

While the problem of failure and error is pertinent to every research activity, even in social science and humanities, we restrict our analysis to domains that rely on experiment and observation. We group terms in the semantic field of “going amiss” with a view to providing an analytic tool for distinguishing different kinds of failure. We seek to capture the richness and diversity of scientists’ use of such terms, which varies widely over periods and fields of study. At the same time we also attend to the analyst’s demand for tools of inquiry into failed experimental enterprises.

To organize the multitude of terms, we distinguish analytically between three main aspects of experimentation: (1) the agent’s reasoning, perceptions and actions; (2) the tools of research, and (3) the object of investigation. In each of these respects, things can “go amiss”.

First, what can go amiss with the agent’s reasoning, perception, and action? The most straightforward case is the failure to apply an acknowledged convention, that is, an accepted rule or standard; this results in a mistake. Once a mistake is detected, rectification is immediate: applying properly the known rule or standard, and using rigorous checking procedure. Common examples of mistakes are typos and miscalculations. In the case of mistakes the standard is already established (Hon 1995). The agent may also be misled with respect to a matter of fact and we call this phenomenon deception. An example for a deceiving phenomenon is atmospheric refraction in astronomical observations. Optical illusions belong to this category because in such physical circumstances the apparent position or shape of the object is different from its true location and form.

A fundamentally more complex situation arises when knowledge of the standard is vague or missing altogether. The practitioners’ language, concepts, perceptual skills, and basic theory may fail to provide an appropriate frame for analysis because they yield only ill-formed questions and categories, or misguided expectations that may result in misinterpretation. Similarly, the process of investigation may go amiss because of absent, overabundant, or vague, ideas of how to proceed. The agents find themselves in a situation where they do not know or are uncertain what to look for. A plethora of possibilities may present itself and nothing seems to give a lead.

There are cases where the mismatch is between experimental findings and theoretical expectation. Anomalies occur when experimental outcomes do not fit the relevant conceptual framework. Discordance is a disagreement between theoretically derived and observed findings in which the difference exceeds the error bounds on the sides of both theory and observation. And in these cases it is more often a matter of pragmatic concerns rather than of epistemology whether to look for the failure on the part of the theory or the experiment. In contrast, there may be clearly identifiably inconsistencies within the theory itself.

It is important to keep in mind that these categories are usually applied retrospectively, i.e. after the failure has been located or even removed. The experimenters’
actual situation when faced with a failure typically looks less clear. The practitioners speak of surprise, puzzlement, confusion, and the like. They try to determine the epistemic status of their claims and to track down the precise location of the failure, but they are not always successful. There may be situations in which it is impossible to decide whether the devil is in the agent, the tools, or the object. Sometimes experimenters give up and acknowledge the fact that they are stuck in a dead end—a situation that often leads to abandoning the original problem and taking up different questions, and at times to completely reorganizing the research field.

Turning to the tools we identify malfunctions. These failures result in divergence from the desired performance of the instruments as well as the setup. Epistemologically, the case is not too troubling, since the standard against which the instrument can be calibrated is (at least in principle) known. By contrast, much deeper problems arise with the central and omnipresent problem of instrumental artifacts, the unintended, and often unrecognized, (side-)effects that are generated by instruments and setups. The possible occurrence of systematic error is a veritable challenge: in experimental analysis one can never be absolutely sure that no previously unknown element interferes with the functioning of the instrument. Such artifacts cannot be avoided or eliminated altogether. Similarly, noise and random errors are intrinsic features of the experimental procedure. We call instrumental discrepancies the specific, identifiable divergence between two or more sets of generated output.

Thirdly, we consider the object under study. Faulty experimental objects may be due to unintended modifications. Artifacts of preparation may be produced through the scientists’ inadvertent intervention. And again, the consequences of this intervention may remain unrecognized. Alternatively, the object may have certain unrecognized features that make it inappropriate for the intended study. A well-known example for such an object is the evening primrose in mutation experiments. Early theories of large-scale mutations were based on investigations of an organism whose chromosomes behave in a very peculiar way, and this unusual behavior of the plant misled the researcher.

Of course, in proposing such a set of terms, the historiographical perspectives outlined above are of critical importance. The difference between modern, analytical categories and those used by the historical actors may be highlighted with the use of this scheme of terms. Furthermore, it is important to realize that several of the terms of the conceptual framework have a historical dimension in the sense that they were introduced at one point and in a specific investigative context. These concepts may change their meaning over time. For example, the term “discordance” has a very precise meaning in celestial mechanics. Another case is the deviation from what is considered the normal, namely, the pathological of which one instance is monstrosity. Monsters were once conceived as errors of nature. From the 18th century onward, however, hardly any natural or experimental philosopher would have acknowledged that nature may err.

When failures are successfully identified and eventually removed, categories like those that we have listed in this section typically come into play. The set of terms that we have proposed serves both as an analytic tool and as a means of capturing the experimenters’ own terminology. We hope that our nomenclature can help trace
the scientists’ attempts at recognizing what exactly it is that is amiss. Moreover, as our Epilogue shows, this glossary prepares the grounds for discussions about the epistemic roles of going amiss.

The contributions to this volume indicate that failures, errors, confusions, and related epistemic phenomena such as dead ends are more than mere obstacles to scientific advancement and inconsequential curios for history and philosophy of science. In fact, this volume demonstrates that accounting for going amiss is productive both in the scientific domain and in reflecting on scientific practices and the knowledge thus gained. For the scientist the recognition of going amiss is an enticement for further work that may result in putting it aright or even in producing novel experimental knowledge. Similarly, for the historian and philosopher of science the historical account of going amiss and its philosophical analysis present a rich source for reflection on the nature of scientific practice and the knowledge that it generates.

References


Further Reading


Part I

Error as an Object of Study
Error: The Long Neglect, the One-Sided View, and a Typology

Giora Hon

To avoid errors... one must seek to disclose and to explain their source, illusion. Very few philosophers have done that, however. They have only sought to refute the errors themselves, without indicating the illusion from which they arise. This disclosure and breaking up of illusion is a far greater service to truth, however, than the direct refutation of errors, whereby one does not block their source and cannot guard against the same illusion misleading one into errors again in other cases because one is not acquainted with it.

Kant (1800/1992, 562)

The long neglect

“The essays and lectures of which this book is composed are variations upon one very simple theme—the thesis that *we can learn from our mistakes*.” Thus prefaced in 1963 Sir Karl Popper (1902–1994) his book, *Conjectures and Refutations: The Growth of Scientific Knowledge* (Popper 1963/1974, vii, italics in the original). Popper characterizes his philosophical work as an attempt at developing a theory of knowledge and its growth. Both reason and experience function in this theory as means of exposing errors from which one may learn and thereby advance knowledge. A few years after the publication of this volume, when the second edition was ready for publication, Popper appeared to have had some additional thoughts concerning his “one sentence” summation of his thesis. He now assures the reader that “all our knowledge grows *only* through the correcting of our mistakes.” He states that the general method of learning from mistakes is the method of trial and error and he further clarifies that in order to apply this method “we must already have some aim: we err if we stray from this aim” (Popper 1963/1974, ix, italics in the original). This appears to be an afterthought that is indicative of the situation:

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Popper did not develop a sustained and full-fledged theory of error. We may surmise
that Popper exploits the issue of error as a rhetorical means for promoting and
enhancing his philosophical position which is indeed well captured by the title of
the book, Conjectures and Refutation.

It should not therefore surprise us that in Popper’s earlier seminal work, The
Logic of Scientific Discovery, no theory of error is developed and error is discussed
incidentally and briefly, only with respect to the problem of measurement and issues
elimination” is a key feature in Popper’s philosophy. It is the means by which his
notion of critical rationalism functions in advancing knowledge.

The proper answer to my question “How can we hope to detect and eliminate error?”
is, I believe, “By criticizing the theories or guesses of others and—if we can train
ourselves to do so—by criticizing our own theories and guesses (Popper 1974, 26, italics
in the original).

Thus his view of the scientific method is summed up in four steps:

1. We select some problem—perhaps by stumbling over it.
2. We try to solve it by proposing a theory as a tentative solution.
3. Through the critical discussion of our theories our knowledge grows by the
elimination of some of our errors, and in this way we learn to understand our
problems, and our theories, and the need for new solutions.
4. The critical discussion of even our best theories always reveals new problems
(Popper 1997, 159, italics in the original).

Popper singles out the third step as the most characteristic of the scientific practice,
the error-elimination through criticism. For Popper the objectivity of science, and
indeed its rationality are aspects of this stage, namely, the critical discussion of
scientific theories (Popper 1997, 159). Here lies the difference, according to Pop-
per, between an amoeba and a great scientist like Newton or Einstein: “the distinc-
tive feature of science is conscious application of the critical method; in Stage 3 of
our model, the stage of error elimination, we act in a consciously critical manner”
(Popper 1999, 7, italics in the original).

It is in fact from the domain of biology that Popper takes the view of the crucial
role which error correction may play in a theory of knowledge. He claims that “in
biological evolution, . . . [error correction] appears to be the only means of progress”
(Popper 1997, 100). The issues whether evolution is progressing or not and whether
the concept of error is meaningful at all in this context should not detain us here.
The point is however clear, the occurrence of error and the search for its elimination
is central to Popper’s philosophy. Yet Popper did not develop any theory of error,
nor did he discuss this crucial epistemic phenomenon at length.

Popper is of course aware of philosophical theories of error. He refers to the
“epistemological optimists”—Plato, Bacon and Descartes for whom truth is man-
ifest—as being responsive to the fact that we sometimes “mistake error for truth”,
and he points out that, “in order to save the doctrine of manifest truth they were
forced to explain the occurrence of error” (Popper 1997, 203). Having sketched
very briefly and in a most schematic way these theories of error, he concludes, “Bacon’s theory of error is, in spite of its desirable consequences, untenable” (Popper 1997, 203). What would be a tenable philosophical theory of error, Popper does not say, let alone develop. This is rather disappointing. The reader of Popper’s philosophical writings would be hard pressed to find discussions of theories of error, conditions of error, kinds of error, and in general the nature of error, for the simple reason that the theme of error is not developed in this philosophy. We may conclude that Popper presents the problem for purely rhetorical purposes. His claim then that,  

nobody is exempt from making mistakes. The great thing is to learn from them. And this is done by criticism, and by the discovery of new problems brought forth by criticism (Popper 1997, 144),

is more a slogan than a thorough, well argued philosophical position.¹

Turning our attention to another contemporary influential philosopher, we may record that Willard van Orman Quine (1908–2000) opens his book, *From Stimulus to Science*, with the very problem of error.

We and other animals notice what goes on around us. This helps us by suggesting what we might expect and even how to prevent it, and thus fosters survival. However, the expedient works only imperfectly. There are surprises, and they are unsettling. How can we tell when we are right? We are faced with the problem of error. These are worries about our knowledge of the external world. To deal with them we have had to run inward and seek knowledge of knowledge (Quine 1995, 1, italics in the original).

The reader soon realizes, however, that the problem of error is being used here as a literary device. Quine does not consider error a serious philosophical issue worthy of further consideration; he does not discuss the problem of error in the remaining parts of the book. One may therefore infer that by developing a theory of knowledge one implicitly takes care also of the problem error.

This implied view that the problem of error is some sort of a mirror image of the problem of knowledge is misleading if not mistaken. To be sure, error is an epistemological phenomenon that in the final analysis has to be analyzed with the tools of a theory of knowledge. However, nothing in such a theory reflects directly the phenomenon of error and it is clear that a special inquiry has to be undertaken.

I now turn from philosophy and philosophy of science to history of science where the problem of error is acute, pertaining to scientific knowledge and its historiography. It quickly transpires that here too the problem has been neglected and attention to its occurrence has been either superficial or uneven, stressing its importance in the historical context but performing no analysis. I proceed then to another example in which it appears that the author, though much appreciative of the problem, makes a rhetorical use of error and does not attempt a proper analysis.

Alexandre Koyré (1892–1964) is amongst the historians of science worth considering with respect to the problem of error. He expresses explicit interest in the problem and appears to be convinced of its importance. In Koyré’s view, as Biagioli writes,
“mistakes played fundamental role in the production of scientific knowledge.” Koyré’s fascination with error, Biagioli remarks further, went so far as “to attribute an almost providential role to error.” Thus, according to Biagioli, for Koyré “the fact that the mind can develop wrong scientific theories was not a symptom of its biases, idols, or conceptual shortcomings, but rather a sign of its freedom” (Biagioli 1987, 169). Whether the mind is indeed free and the occurrence of error is an expression of this freedom or, conversely, that error has an objective, ontological status and the mind stumbles upon it, are in fact issues for a theory of error to address and analyze. Whatever is the case, the road to truth—*itinerarium mentis in veritatem*—as Koyré writes, is not straight:

The road to truth is full of traps, strewn with errors, and the failures there are more frequent than the successes…. But we would be wrong to neglect the study of errors (*l’étude des erreurs*)—it is by way of them that the mind progresses towards truth (Koyré 1966, 361; quoted by Jardine 2000b, 363).

This appears to be a promising line of inquiry. It is an expression of a view that seems to take a keen interest in the concept of error. Here I am in complete agreement with Koyré’s claim that the study of errors can teach us about the working of the mind as much as the study of truth. Knowledge and error, as Ernst Mach (1838–1916) observes, flow after all from the same mental sources, and it is only success that can tell the one from the other. Thus, “a clearly recognized error, by way of corrective, can benefit knowledge just as a positive piece of knowledge can” (Mach 1905/1976, 84).

In the Introduction to his study of the law of falling bodies, Koyré expresses his fascination with the fact that both Galileo and Descartes made similar errors. The passage is worth quoting at length:

For the historian of scientific thought, at least for the historian-philosopher, failure and error, especially the error of a Galileo or a Descartes, can sometimes be just as valuable as their successes. They can, perhaps, be even more so. They are, in fact, very instructive. They sometimes enable us to grasp and to understand the hidden processes of their thinking.

No doubt the objection could be made that one should not look for a rational explanation of error. Error is a consequence of the weakness and limitations of the human mind, a function of its psychological, and even biological, conditioning. Everyone is capable of falling into error. Anyone can make mistakes. Nobody is an exception to this. It is enough to explain error by a lack of attention, by distraction, or by “inadvertence”. We cannot accept this objection, or at least not entirely. No doubt any mistaken reasoning is inadvertent. And when Galileo and Descartes made mistakes they were guilty of this. But that this duplicated inadvertence (this duplication being in itself already an extremely curious fact) should lead them to exactly the same error, it is this… that cannot have been the result of pure chance… Nevertheless it is far from plausible. There must be some reason for similarity in error (Koyré 1966/1978, 66).

Yet Koyré’s exuberance of the reveries of error in history of science (here specifically the case of Galileo’s and Descartes’s duplicate error) does not hold good, for in this study of the law of falling bodies, and indeed in any other study of Koyré, no detailed analysis of the problem of error is forthcoming and no general theory of error is developed. Koyré considers error an unproblematic, primitive concept,
basic and well understood, namely, what is not the case vis-à-vis current theories and hindsight knowledge.

Descartes himself... though he was a mathematician of genius, was never able either to recognise the mistake he had made, nor even, when he came across the correct formula in Galileo, to recognise that it was different from the one that he had put forward earlier himself. From which we can see once again just how difficult it was to isolate and grasp those simple and clear ideas with which we are so familiar from classical physics and from Cartesian philosophy. Even for Galileo. Even for Descartes (Koyré 1966/1978, 86).

I note that at issue is not a historiography that involves both judgmental evaluations and elements of anachronism (see Jardine 2000a). Rather, I stress the unproblematic meaning of the concept of error to which Koyré resorts. It is indeed based on the success criterion of which Mach has spoken. Koyré does not accompany his appeal to the concept of error with an appropriate analysis of the concept and therefore one is left with rather picturesque metaphors of errors as obstacles on the itinerarium mentis in veritatem. In sum, no insight into the epistemic phenomenon of error is offered in Koyré’s historical studies.

Consider from another perspective Koyré’s return to Plato; the study, Discovering Plato (1945), which he published after his inquiry into the law of falling bodies and in the aftermath of the Second World War. While detailing the arguments concerning the definition of knowledge that Socrates develops in the Theaetetus, at the juncture where Socrates argues that knowledge cannot be just true opinion because it will be then impossible to err, Koyré states that, “the problem of error is one of philosophy’s very serious and crucial problems.” Indeed, the problem of error together with its dour consequences was realized right at the inception of philosophy. Plato acknowledges in the Theaetetus that his theory of knowledge must account for the occurrence of error lest it would collapse altogether. “If [false opinion]... is found not to exist, we shall be forced to admit many absurdities,” Socrates cautions Theaetetus (Plato, Theaetetus, 190E; Fowler 1977, 183). A theory of knowledge which cannot explain error and its occurrence, cannot discriminate it from truth, and so cannot explain that either. Indeed, the mark of a false proposition is that it is indistinguishable in form from a true one.

Attempting an explanation, Plato assumes an object of error as well as an object of knowledge and studies the consequences of this assumption. “It is, then,... possible for the mind to regard one thing as another and not as what it is” (Plato, Theaetetus, 189D–E; Fowler 1977, 179). Error consists then in taking the one object for the other. However, to know this we must know both, and knowing the object of error as such is not an error. Plato reaches thus an impasse. Error involves the contradiction that we must simultaneously both know and not know in the same cognitive reference, “to know what one does not know; not to know what one does know”, to use Koyré’s formulation (Koyré 1945, 50)—a consequence that may threaten the coherence of any theory of knowledge that takes up seriously the challenge of error (Schiller 1908).

Koyré’s assessment of the status of the problem of error is thus most appropriate. Notwithstanding, he states this important observation in a footnote (Koyré 1945, 40 n. 9). The observation epitomizes the state of the problem of error; the problem is
indeed “very serious and crucial”, yet the considerations it has received have generally been scanty and peripheral, that is, metaphorically they amount to a footnote not only to philosophy but to science as well.

It appears however that things have changed. Historians and philosophers of science are increasingly paying attention to the vast and varied problem of error, both as a probabilistic epistemic phenomenon and as an inherent difficulty in the context of observation and experimentation. This growing concern with the concept of error is connected to the shift in attention which the discipline of history and philosophy of science is currently undergoing. The actual nitty-gritty practices of scientific research, the bra s tacks of research in both the natural and the social sciences, including the social context within which knowledge is generated, have become legitimate objects of historical and philosophical studies as much as the conceptual content itself and its cognitive aspect (e.g., Galison 1987; Shapin 1994). In parallel to this development the literature of philosophy and history and philosophy of science has seen a growing interest in the problem of error, its historical background in philosophy (Evans 1998) and its presence in philosophy and discussions in the cultural-literary realm (Affentranget 2000; Almeder 1999; Bates 1996, 2002; Crocker 1953; Kenaan 1999); as a probabilistic phenomenon (Krüger et al. 1989, Section III: Uncertainty; Mirowski 1994, 1995; Sheynin 1983; Stigler 1986), its occurrence in mathematics (Sherry 1997) and in science (Schlich 1993), its focus in engineering (Pool 1997) and generally as an acknowledged theory (Swijtink 2000, for an annotated bibliography of “Error Theory”). Gone are the days when one could flip casually the remark that “once [errors of measurement and other forms of experimental error] . . . have been discounted, our attention can turn to the logico-mathematical structure” (Sellars 1961, 73; cf. Mellor 1965, 106). The occurrence of errors, especially in observation and experimentation, constitutes a permanent feature which deserves proper attention. The problem of error is not incidental to the pursuits of science, it deserves the attention of philosophers and historians of science. As Mellor pointed out, error should not be treated as “a tiresome but trivial excrescence on the neat deductive structure of science” (Mellor 1967, 6).

The One-Sided View

In this vein, Deborah Mayo (1996) has made an attempt in her book, Error and the Growth of Experimental Knowledge, to bring the problem of error to the center of discussion. The ambitious title takes its cue from the subtitle of Popper’s book, Conjectures and Refutations: The Growth of Scientific Knowledge, which as we have seen evolves around the seemingly simple theme that we can learn from our mistakes. Mayo’s reference to experimental, rather than to scientific, knowledge has to do with her claim that experimental knowledge is knowledge grounded on argument from error (1996, 7). Clearly, we expect here to find the missing Popperian insights into how to learn from mistakes.
Mayo is vigorously critical of the orthodoxies of Popper and Kuhn and vehemently rejects the popular Bayesian approach in philosophy of science. Building on a classical statistical tradition (1996, x, 10, 337; see however Mirowski 1995, 547 n. 10), she develops a sophisticated technical machinery with which she underpins a non-Bayesian philosophy of science. Mayo sides with Peirce, Neyman and Pearson and stands against their common opponents—the Bayesians. She christens her position “error-statistical philosophy of experiment” (1996, 410, 442, 457, 464), because the chief feature that her approach retains from the Neyman-Pearson statistical methods is the centrality of error probabilities (1996, x–xi). The demand that it is necessary to take into account the error probabilities of experimental procedure in order to determine what inferences are licensed by data, is the principal element that fundamentally distinguishes, according to Mayo, her approach from others (1996, 442).

The statistical methods which Mayo offers are designed not only to stabilize experimental knowledge and explain its growth but also to grapple with other issues pertinent to philosophy of science, such as the Duhem problem (1996, 103, 106–109). Indeed, in referring to an error-statistical philosophy of science, she has in mind the various ways in which statistical methods based on error probabilities may be used in philosophy of science generally. Mayo seeks to convince the reader that the error-statistical philosophy of science that she has developed has a structure and a logic, so that its parts hang together to provide a full-bodied philosophy. In her view, this philosophy presents a viable alternative to the Bayesian Way (1996, 442–444). She thus concludes that,

the ability to make successful inductions, our success in obtaining experimental knowledge, is explained by the error-statistical properties of our methods. We make progress in experimental knowledge—experimental knowledge grows—because we have methods that are manifestly adequate for learning from errors (1996, 464).

On this account, errors and the statistical methods for treating them have become the tools for building the body of knowledge we call science. Has error then gained in this rejuvenated classical statistical approach the attention it deserves?

Error covers multiple sins. It is a multifarious epistemological phenomenon of great breadth and depth. To be sure, error-statistical analysis is a powerful tool which is much needed in the technical domain of the reduction of data. As such, it can undoubtedly throw light on methodological issues, but it cannot do philosophical justice to such complex concepts as error and experimental knowledge. Mayo’s book is not about error but about error probabilities, and the notion of experimental knowledge it develops is rather the knowledge of the probabilities of specified outcomes in some series of experiments (1996, 12).

The central problem which Mayo addresses is how to link experimental data to primary theoretical hypotheses. It is commonly known that data gathered from experiment are corrupted by various kinds of error introduced by intermediary processes of observation and measurement. Moreover, data are finite and discrete, while primary hypotheses may refer to an infinite number of cases and involve continuous quantities such as weight and temperature (1996, 132). Nevertheless, since the mandatory linkage between data and hypothesis is the only game in town...
which deserves the appellation scientific, one must use experimental data to assess
the values of theoretical quantities. How is this done? How are we to proceed?

Mayo perceptively points out that the study of the relation between evidence and
hypotheses solely in terms of logical relationships ignores completely all the deliberate
and active intervention in which the experimenter is engaged (1996, 212). In focusing
too exclusively on the appraisal of global theories, philosophers have overlooked
how positive grounds are provided for local hypotheses, namely, whenever evidence
counts as having severely tested them. By attempting to talk about data and hypotheses
in some general way, apart from the specific context in which the data and hypothesis
are generated, modeled, and analyzed to answer specific questions, philosophers have
missed the power of such a piecemeal strategy, and underdetermination arguments
have flourished (1996, 213). In sum, Mayo instructs not to follow the Bayesian Way,
but rather the path of the classical statistician and to search, as Pearson put it,

_for a way of expressing in mathematical terms what appeared... to be the requirements
of the scientist in applying statistical tests to his data_ (quoted by Mayo 1996, 381, italics
added by Mayo).

The application of statistical tests is the key idea, and as Pearson reported,

_from the start we [Neyman and Pearsons] shared Professor Fisher’s view that in scientific
enquiry, a statistical test is “a means of learning” (quoted by Mayo 1996, 382)._

On this account one learns in science not from how much the evidence confirms the
hypothesis tested, but rather from _how_ discordant evidence shows a given model to
be in a specified respect. Learning from experiments requires not some update of
the probability assignment that one starts out with, but deliberate and often devious
methods of testing with which one builds, corrects, and fills out a model (1996, 212,
433).

The theme of learning from error thus plays a central role in the experimen-
tal program which Mayo develops. She demonstrates how the famous Popperian
thesis of “conjecture and refutation” does not stand up to criticism. According
to Mayo, Popper’s account falls far short of showing how reliable knowledge is
obtained from experiment or how that knowledge grows. Mayo finds in the Peircean
error-correcting justification of induction, the very justification she needs for her
error-statistical methods of science.

The justification for these methods lies in their ability to control error probabilities, hence
sustain learning from error, hence provide for the growth of experimental knowledge
(1996, 413).

Mayo’s central thesis is that the argument from error, that is, learning from error,
may be described in terms of a test of a hypothesis, $H$, that a given error is absent.
The evidence indicates the correctness of hypothesis $H$, when $H$ passes a severe
test—one with a high probability of failing $H$, if $H$ is false. An analogous argument
is used to infer the presence of an error (1996, 64; cf., Giere 1997, S183–S184;
Howson 1997; Mayo 1997; Chalmers 2000, 198ff.). This then is the framework of Mayo’s error-statistical philosophy of experiment.

Mayo’s philosophy of experiment relies neither on scientific theories nor on a theory of experiment; rather, it relies on methods—statistical methods—for producing experimental effects (1996, 15). This observation is crucial. It explains the limited view of experiment exhibited in this study. In spite of the fact that Mayo speaks voluminously about the need to address the actual practice of experimentation, she focuses her attention solely on statistical calculations. As Mirowski, criticizing both Bayesians and classical statisticians, aptly puts it: “the empirical inquirer cranks through the formulas, assigns the error probabilities and reports an outcome—all as a hermetically self-contained procedure” (Mirowski 1995, 542). This is not what one would expect of, say, a Faraday, a Helmholtz, a Hertz, a Rutherford, or a Kapitza. Consider Peirce’s observations on experimental style:

Of all men of the century Faraday had the greatest power of drawing ideas straight out of his experiments and making his physical apparatus do his thinking, so that experimentation and inference were not two proceedings, but one. To understand what this means, read his *Researches on Electricity*. His genius was thus higher than that of Helmholtz, who fitted a phenomenon with an appropriate conception out of his store, as one might fit a bottle with a stopper (Peirce 1966, 272).

Mayo’s “full-bodied experimental philosophy” (Mayo 1996, 444) is not attuned to the act of experimenting; it focuses rather on the end result: data and their statistical tests. For example, questions as to the interpretation of the experimental result do not arise in this framework. For another example, no theory of experiment is forthcoming in this approach (see, e.g., Radder 1995, 2003). Mayo indeed admits that her philosophy of experiment is limited:

the statistical theory of experiment deals only with certain kinds of experiments insofar as their behavior may be characterized by certain parameters. A characteristic of key interest is the relative frequency with which an outcome occurs, or would occur, in a sequence of applications of the experiment in question (Mayo 1996, 161–162, italics has been added; cf., 164, 173).

She does therefore state that, “any planned inquiry in which there is a deliberate and reliable argument from error may be said to be experimental” (1996, 7). In Mayo’s philosophical framework, experimental knowledge becomes completely statistical:

experimental knowledge is knowledge of the probabilities of specified outcomes in some actual or hypothetical series of experiments. Its formal statement may be given by an experimental distribution (a list of outcomes and their associated probabilities), or by a standard “random” process such as a coin-tossing mechanism (1996, 12, italics in the original; cf., 162, 461).

Thus the errors upon which Mayo builds her error-statistical philosophy of experiment are not error at large but rather a specific and indeed limited kind of error, namely, error probabilities. Error probabilities are not probabilities of hypotheses,
but the probabilities that certain experimental results would occur, were one or another hypothesis be true about the experimental system (1996, 367).

As I have remarked, error covers multiple sins. What kind of error did Franck and Hertz commit in their Nobel winning experiment? (Hon 1989a) What happened in Ehrenhaft’s experiments which made him conclude that there are sub-electrons? (Hon 1989b, 485; Franklin 1981) What went wrong in Kaufmann’s experiments so that he could speak decisively against the correctness of Einstein’s special theory of relativity? (Hon 1995) How did Blondlot discover the existence of a new form of radiation—the N rays—which does not exist? (Nye 1980 and Hon 1989b, 493–494) Why was Lowell so convinced that the visible lines on the surface of Mars are in fact artificial canals for irrigation purposes? (Hon 1989b, 492–493) “The history of science,” Maxwell observed, “is not restricted to the enumeration of successful investigation. It has to tell of unsuccessful inquiries, and to explain why some of the ablest men have failed to find the key of knowledge” (Maxwell 1871, 251). The probabilistic approach to the study of error is undoubtedly of considerable importance; of no less importance is the study of conceptual and physical circumstances in which errors in experimentation and generally in the search for knowledge may originate. “One must,” as Wittgenstein demands, “reveal the source of error” (Wittgenstein 1979, 61). And to follow Kant’s dictum, one must seek to disclose and explain sources of errors. Indeed, for Kant the disclosure of the source of error is of greater service to truth than the direct refutation of errors (Kant 1800/1992, 562). It is apparent that Maxwell, Wittgenstein and Kant speak of different kinds of error than the error probabilities of Mayo; theirs is the general phenomenon of error, not the one-sided account of error which Mayo expounds (cf. Hon 1998b).

Three distinct yet related themes of research could be identified in this domain of inquiry: (1) a study of the history of error in science and especially in observation and experimentation (e.g., Hon 1989c); (2) an epistemology of experiment that can inform a history of error via (3) a classification of types of error that reflects this epistemology (Hon 1989b, 2003). I proceed to outline this approach which I call “probing experiment with error” (Hon 1998a). The resulting typology of experimental errors is designed to contribute towards an epistemology of experimentation.

A Typology

Experimentation is a method designed specifically for obtaining physical knowledge. This method can be viewed like any other method which claims to secure knowledge. But what kind of method is it? What do we mean by experiment? I propose to use the concept of error as a means of probing experiment. The method lays bare the structure of experiment by studying its possible sources of errors.

According to Bohr, “by the ‘experiment’ we can only mean a procedure regarding which we are able to communicate to others what we have done and what we have learnt” (quoted in Honner 1987, 159). However, experiment is not just some
procedure by which one can communicate to others what one has done and what one has learnt from the result. Experiment—and this is the central point of the thesis—is a procedure, a physical process, which can be cast into an argument of a formal nature. A crucial characteristic of an experiment is that its result constitutes a claim to knowledge and it is this claim which distinguishes a mere procedure from an experiment. Being a claim to knowledge, the conclusion of an experiment may be seen as the result of a chain of reasoning concerning the behavior of some material system. The conclusion is connected then with a certain mode of reasoning. An experiment can be made to exhibit an inference from premises to a conclusion—the argument of the experiment.

How is it that a physical process we call experiment ends up in a claim to knowledge that may affect a theory? Take for example the following experimental outcome: “The electrostatic and electromagnetic properties of the cathode rays,” concluded Hertz in 1883 his set of cathode ray experiments, “are either nil or very feeble” (Hertz 1883/1896, 254). The fact that this claim to knowledge, the result of a set of experiments carried out by a gifted experimenter, is considered today erroneous shows that there is an additional element to the physical process that makes up the experiment. The error we discern in Hertz’s experiment cannot be associated with the physical process itself, with the course which nature takes within the constraints of the experiment. Rather, errors indicate claims to knowledge. An error reflects the existence of an argument into which the physical process of the experiment is cast. It is this hidden argument upon which the claim to knowledge is based.

The conclusion of experiment, the resultant claim to physical knowledge, is clearly prone to error. What is it for an experiment to result in an error? What is it for an experiment to contain an error? An experiment may be erroneous for all sorts of reasons associated with its different components. But whatever the reason in the final analysis an experiment is erroneous when it does not warrant its conclusion. This, however, is nothing else but saying that an erroneous experiment reflects a failed argument. In contrast to the Aristotelian position, we commonly hold with Newton that “Nature . . . is . . . always consonant to itself” (Philosophiae Naturalis Principia Mathematica, Rules of Reasoning in Philosophy, Rule III). Clearly, an error is ascribed to an argument, not to the actual physical process.

An argument is a sequence of propositions of a special form: each proposition is either an assumption, a premise, or one that arises through inference schema as a conclusion from propositions earlier in the sequence. An experiment, I claim, can be cast into a formal argument whose propositions instantiate partly states of affairs of material systems and partly inference schema 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. This is a fundamental point in characterizing the implied argument of a physical experiment: the experimenter aims at securing premises which correspond accurately to the actual physical situation of the experiment. The requirement of accuracy puts this kind of argument apart from any other argument in which the premises may be conditional, hypothetical or counterfactual.
An experiment therefore is a procedure, a physical process, that has a logical facet of a rhetorical force. Indeed, an experiment is often used as an instrument of persuasion for reaching agreement on certain situations.

An analysis of fallacies would not however suffice for the understanding of erroneous experimental results. What is required is a full fledged exposition of the problem of experimental error in its broadest sense, that is, beyond the mathematical, technical, narrow meaning. In other words, to illuminate the possible failures of an experiment one needs an epistemology of errors in experiment. Such a study will not only shed light on the nature of possible failures of experiment, but it will also reveal the structure of this method of acquiring knowledge—the method of experimentation.

Experimenter perform essentially two different tasks: they prepare a system and then they test it. A preparation is a procedure which is in principle completely specified. This is crucial for the correct completion of the experiment since on the basis of this knowledge rests the belief that the premises of the implied argument of the experiment correspond to its physical conditions. The second task, the test, starts like a preparation: it has a specified procedure which triggers the interaction between the prepared set-up and the object under study. The test includes another step, a crucial one, whereby information, which was previously unknown, is supplied to an observer, that is, the experimenter. This information is not trivial, not only because identical tests following identical preparations need not have identical outcomes (both in classical and quantum physics, though for categorically different reasons), but primarily because this information constitutes, after a suitable reduction, the sought new physical knowledge. Within certain constraints, experimenters are free to choose preparations and tests that they wish to perform—this is their prerogative. However, they are not free to choose the future outcome of a test. They are bound by the acquired information.4

The characterization of experiment as preparation and test is admittedly very general. Nevertheless, it provides an insight into the structure of the procedure of experimentation which is otherwise impossible to generalize. Beyond this general characterization, the experimental procedure becomes too varied to submit to any general deductive characteristic. Indeed, it appears that there cannot be an exhaustive survey of all experimental techniques. One can certainly characterize different general schemes of experiment such as the scattering technique which has been dominant in high energy physics throughout the twentieth century, or the technique of subjecting radiation to electric and magnetic field; each technique however has its own idiosyncratic features which may not be suitable to generalization.

Thus, however general, the characterization of the procedure of experiment in terms of preparation and test may be useful for the analysis of experiment. This characterization allows for a clear perception of the connection between the actual procedure of the experiment and its implied argument. The preparation stage is principally about presuppositions, whereas the test stage has to do with a mixture of premises and conclusions—the outcome of the experiment. It may be further observed that each of the two stages has two sub-categories. The preparation stage has theoretical and practical sub-categories while the test stage may be conceived of as the recording of the information and its processing.