Essays

The Paradox of Scientific Progress

Notes on the Foundation of a Historical Theory of Knowledge

Jürgen Renn

Progress Between Coincidence and Necessity[1]

Scientific progress is considered to be a self-evident component of modern industrial societies, without which their survival would be endangered. The only occasion for controversy concerns the necessary or desirable scale of this progress and its effects: Are enough funds devoted to education and science? Is scientific and technological progress proceeding at such a rapid pace that its excesses could threaten nature and man? Despite disagreement about the answers to such questions, supporters and detractors of science share an understanding of scientific progress as a Golem striding incessantly onward, whose steps determine the rhythm of modern industrial societies for good or evil.

The burning of the library in Alexandria by the Romans under Julius Caesar in 47 B. C. Schedelsche Weltchronik (Nürnberg, 1493)

Quinta etas mundi

TRes ſoles poſt mortẽ Julij Ceſaris ſequẽti die
exorti ſunt ⁊ apparuerũt in oriẽte: qui paulatim
in vnũ corpꝰ ſolare redacti ſunt ⁊ in eundẽ orbes
cõuenerũt: ſignificãtes dominiũ Lucij antonij ⁊ M. an
tonij atqꝫ Auguſti in vnã monarchiã reducendũ: vel po
tius qꝫ noticia triniꝰ ⁊ vniꝰ dei toti orbi futura imminebat.
Bos quoqꝫ aranti in ſuburbano rome locutꝰ eſt: dicens
fruſtra ſe vrgeri quia in breui magis homines defuturos
eſſe qꝫ frumenta.
Bibliotheca toto orbe terrarũ nobiliſſima .40.mi-
liũ librorꝫ in egypto cõburit. Ibi accurata diligen
tia in colligẽdis libris p veteres laudat. Nã ariſtoteles
⁊ bibliothecã ⁊ ſcolã Theophraſto reliquit. Et primus
fuit (vt Strabo arbitrat) qui libros cõgregauit: ⁊ egy-
pti reges bibliothece ordinẽ docuit. Deinde Theophra
ſtus neleo tradidit. Neleus ſcepſim detulit ad hoĩes im-
peritos: qui libros incluſos ac negligẽtes cõpoſitos te-
nebant. Ptholomeꝰ quoqꝫ philadelphꝰ maximũ nume
rũ librorꝫ collegit. Et vt Seneca tradidit. xl. milia libro
rũ alexandrie arſerunt pulcherrimũ regie opulentie mo-
nimentũ: ſicut ⁊ Liuiꝰ qui elegantie curęqꝫ regum egre
giũ id opus ait fuiſſe. Non fuit elegantia illud aut cura:
ſed ſtudioſa luxuria: imo nec ſtudioſa quidẽ qñ nõ in ſtu
diũ ſed in ſpectaculũ cõparauerant: ſicut pleriſqꝫ ignaris
etiam ſeruilũ litterarũ libri non ſtudiorum inſtrumenta
ſed cenationũ inſtrumenta ſunt.

---

[1] *This essay is based on work pursued in the context of the research program of Department 1 of the Max Planck Institute for History of Science since its foundation in April 1994. Its owes much to insights by Peter Damerow and Wolfgang Lefèvre on the role of material means of intellectual labour.*

On the other hand, a mere glance at traditional societies, such as those communities still living in the pre-industrial age today, is sufficient to recognize that the development of human societies is not necessarily linked with an accelerating development of technology, and not at all with the emergence and cultivation of science. Obviously, science is only one of many possible forms of expressing human culture, one that emerged under historically specific conditions, and, as history also teaches us, one that can be annihilated as well. Just think of all the literature and culture of Antiquity lost with the fall of the Roman Empire, or the almost complete suppression of the first beginnings of natural science in China after its unification in the third century B.C. This illustrates a central paradox in determining the historical nature of scientific progress: originally the result of more or less contingent historical circumstances, scientific progress has been for more than a quarter of a millennium a seemingly inexorable motor of societal development as a whole

This paradoxical character of scientific progress becomes even more striking upon closer examination. Of course, there are numerous examples from the history of science that show the extent to which scientific breakthroughs depend on economic conditions, technological prerequisites, national customs and styles of thinking, and on an elusive spirit of the time, as well as on personal preferences and moods – just as does every other cultural achievement, be it a symphony or a motion picture. However, there are evident differences between science and other cultural expressions as well. These differences are reflected in scientists' conception of themselves. Nearly every scientist is personally convinced that he can see further than his predecessors, because he can build on their achievements, or, as Newton put it, because he is standing on the shoulders of giants. In other words, in science each step builds on the previous one and in retrospect the long-term development of science looks like steady growth, progress in the literal sense, occasionally interrupted perhaps by external disturbances such as wars or epochs hostile to science.

Constant Growth of Knowledge or Revolutionary Change?

The fact that science also bears revolutionary traits makes this faith in its "cumulative" character and constant progress appear dubious, however. How can radical changes in scientific thought and intervention be reconciled with the protection and gradual expansion of what has been acquired once and for all? Only in recent decades were the revolutionary characteristics of science emphasized by historians and philosophers, but have since given way to other fashions. Examples include revolutionary upheavals in scientific thought like the shift from the geocentric to the heliocentric view of the world, and the change from classic Newtonian physics to the relativistic physics of Albert Einstein. In his famous study on the structure of scientific revolutions, Thomas S. Kuhn proposed the thesis that the history of science is characterized by such radical changes and attempted to capture their typical contours. According to Kuhn, scientific revolutions can be understood as radical upheavals of complete worldviews, as one exemplary "paradigm" takes over from another. While scientific revolutions were recognized as being dependent on cultural, sociological, and psychological factors, their structures became comparable to those of political and cultural revolutions. On this background, the history of science became for the first time a legitimate part of a comprehensive cultural history. However, Kuhn and his successors did not have a satisfactory answer to the question of how such paradigm shifts can be reconciled with the image of the gradual procession of science. Nor could they explain the origin of a new paradigm, for according to Kuhn, the new paradigm does not build on its predecessor but completely replaces it, like a revolutionary government replacing the *ancien régime*.

Although Kuhn's analysis of the structure of scientific revolutions thus deepened the paradox of scientific progress, the multitude of special studies subscribing to, criticizing or simply ignoring Kuhn has since relegated the actual problem back to obscurity. More and more historians of science are fascinated by the possibilities offered by the perspective of a cultural history of science to investigate an almost infinite spectrum of interactions between science and its context, and they are much less interested in dealing with the tedious question of scientific progress. They are opposed by a gradually disappearing group of specialists, many of whom are narrow-mindedly fixated on the technical details of scientific works, and who believe that the issue of progress has long since been decided in favor of traditional faith in progress. These scholars chronicle contemporary natural science just as the court historians of past eras chronicled the deeds of their rulers: essentially free of critique and reservations.

Can Progress be Shaped?

The paradoxical character of scientific progress is not merely a problem for specialists; rather, it raises questions that must be dealt with by anyone who wants to deal with science, its prerequisites and consequences in our age. Specifically, what are the preconditions for scientific innovation? What must we do to find scientific answers to the global challenges besetting us, from global warming to global epidemics, to supplying billions of people with what they need to live? To what extent can scientific progress be steered and shaped at all? In which processes is scientific knowledge transmitted, generated and spread? What role do the media specific to a given historical age play by transporting this knowledge, be it in writing, as images, in print, or today over the Internet? What role do the institutions specific to each historical age play in organizing the division of labor for producing and spreading knowledge – in particular, schools, universities, academies, and research institutions whose program is—roughly since the Age of Enlightenment—essentially shaped by the sub-division of scientific disciplines. Is the social organization of knowledge which they structure still suitable for the new media used to represent knowledge in the age of the information revolution? Is increasingly specialized science still in a position to bundle its results to allow the development of a comprehensive worldview, or at least solutions for those pressing global problems of our age that defy categorization within the boundaries of individual disciplines?

The answers to these questions depend on one's point of view. From the perspective of an individual who participates in the development of science as a contemporary, this development appears as a natural and ultimately uncontrollable process: Knowledge is constantly increased, while the results and methods of generating knowledge are clearly increasing in complexity. According to this view, scientific progress appears to entail a reflection of our reality that is increasingly precise, but ultimately increasingly difficult for the individual to comprehend. From this perspective the individual has little choice other than to pin his hopes on scientific progress as a kind of perpetuum mobile of the modern age, and on the other hand – in the spirit of postmodern fatalism – to resign himself to the incomprehensibility of its mode of operation and results.

From a historical perspective, however, scientific progress appears quite different. Here, too, a given epoch can be regarded from the perspective of a given contemporary, but in this case such a view is enriched by retrospect: It is known which paths turned out to be dead ends, and the scholar has an overview of how science was embedded in social contexts. The image of science yielded by this perspective resembles the history of any other human activity: It is characterized by misguided hopes, errors, conflicts about priorities, coincidental confrontations, but also crazy ideas that ultimately proved to be ingenious. Science is a showplace for human greatness and fallibility just like politics, society or art. But what is the role of societal and institutional structures, the role of media in the representation and dissemination of knowledge, and the role of concepts that allow a more or less comprehensive overview of the knowledge accumulated? From the perspective of a narrative account of history, all of these roles are ultimately reduced to those of backdrops or extras, which grant the drama of science power and color and make the fates and actions of the main actors a bit more understandable. But even long after the smoke has cleared over the research fronts of past science, revealing a view of the conditions and motives of acquiring scientific knowledge, the view of the abandoned battlefields calls into question not only whether progress can be shaped, but whether progress is possible at all.

Progress Despite Revolutions?

Does a perspective exist at all that allows the reconciliation of the progressive character of science with its historical and contingent nature without encountering paradoxes? Such a perspective would have to provide an explanation for the possibility of scientific revolutions as well. For evidently, such revolutions overturn truly fundamental categories of our understanding of the world, the cognitive process of science does not have to start over after each revolution. The scientific revolution associated with Einstein's name offers an excellent example of this, for it affects such fundamental concepts as space, time and matter, which are elements not only of scientific thought, but also of everyday experience. The two great upheavals in the scientific worldview that proceeded from his work at the beginning of the twentieth century, the relativity revolution and the quantum revolution, changed our understanding of such apparently elementary concepts and states of affairs as simultaneity, speed, causality, the enumerability of particles, the possibility of distinguishing between waves and particles, and the relationship between inertia and gravity, in a manner that is difficult for everyday understanding to comprehend. What is more, our conception of the world as a whole was radically transformed from the image of an infinitely large, essentially static cosmos to an image of an explosively expanding universe with a single origin. Despite this caesura in our understanding of the world, the centuries of preliminary work upon which today's scientific image of the world is based were not simply discarded in the aftermath of the Einsteinian revolution. Rather, the discoveries of Copernicus, Galileo and Newton are simultaneously refuted and confirmed in this revolution. How is this possible, how can the possibility of progress be reconciled with the fact of scientific revolutions?

The Platonic Self-Image of Scientists

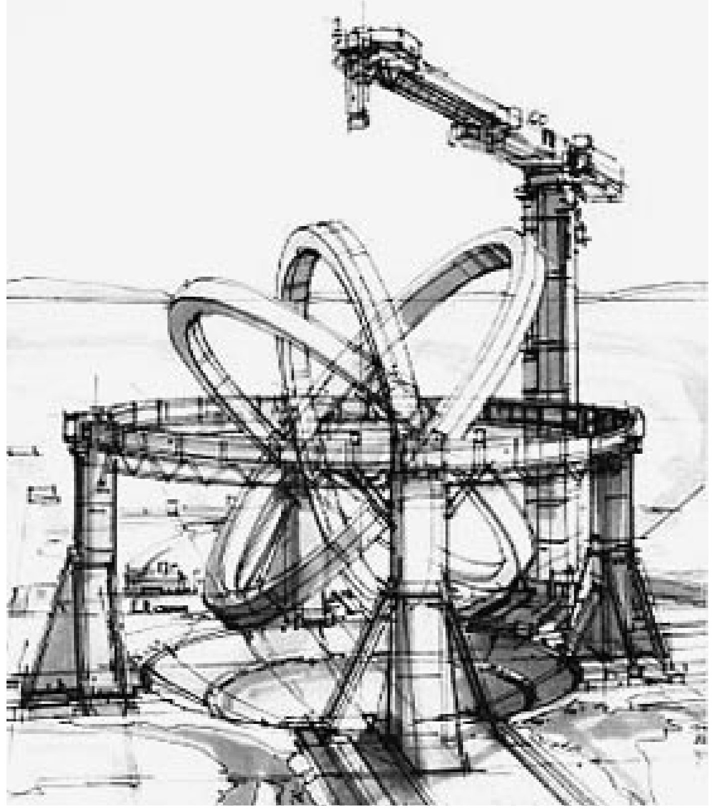
Of course it is conceivable that development presents itself as a gradual approximation of an ideal condition, which, however, can only be reached by way of detours and surmounting obstacles. In essence, this is the way the history of science appears from the retrospective of a contemporary scientist who, from his modern point of view, perceives those scientific efforts of the past that lead to him as progress and the rest as regrettable detours. From this point of view Aristotelian physics appears to take a wrong turn, while Galilean physics is an approach to Newtonian physics; in turn, classical Newtonian physics would be considered a highly useful approximation of Einstein's relativistic physics.

According to this understanding of scientific progress, the individual insights of science not only build on each other but also constantly increase in scope, ultimately approaching a synopsis of nature, which then can possibly be expressed in a global formula or a unified theory. Although many scientists doubt that such a universal theory will ever be formulated, or suspect that such a theory would be in constant need of improvement thereafter, the clearly cumulative character of scientific progress appears to be best captured by the idea of gradually approaching a true image of nature. This conception was discussed in diverse forms in the history of philosophy, but probably found its most concise expression in Plato's theory of ideas. It

Jan Saenredam's engraving after Cornelis Cornelisz. van Haarlem's recreation of Plato's Cave, 1604, © Copyright The British Museum presents what is probably the simplest explanation for the impression that our cognitive processes appear to be controlled by a goal that is not yet known and perhaps unattainable. For according to Plato, the cognition of truth is nothing other than the remembrance of original ideas, of which all of our concrete experiences are mere shadows.

According to this conception it is no wonder that scientific knowledge can be conceived of as enlightenment, as the path from darkness into the light of truth, although this truth can be revealed only gradually and perhaps never in its entirety. At any rate, according to the Platonic reading, because truth is the point of reference shared by all of our scientific efforts, these cannot be conceived of as enterprises separate from one another and entangled in their given local contexts, but eventually lead to a shared destination. Nor does it come as a surprise that most scientists are "spontaneous Platonists" in this sense. For the ideas on which they work, be they mathematical ideas such as triangles or prime numbers, or physical ideas like forces and fields, appear to be of timeless validity. In any case they are held to be more enduring than the concrete experiences upon which each of them are based, although not necessarily in their given current form. In Contact, for instance, the book by Carl Sagan upon which the successful motion picture was based, prime numbers are what make the first communication possible between humanity and extraterrestrials, two civilizations that have so little else in common that any sensory contact between them could only result in repulsion.

Although science may have worldly roots, as far as the Platonists are concerned its success lies in its ability to disengage itself from these roots, to abstract from the concrete and to seek the generally valid essence behind all changeable phenomena, and to formulate these in general principles and laws as mathematically as possible. This understanding of the objective of science, beholden to the search for the "higher" truth, always includes an implicit promise of salvation for the individuals who dedicate themselves to this mission. Science thus can become a substitute for religion – a religion for experts – indeed its promise of salvation is usually directed only to a chosen few. For Einstein and Planck, as for many other outstanding scientists, science offered the possibility of turning away from the exclusively personal and to search instead for comfort and a life orientation in the inquiry of an eternal nature. Of course, such an act of turning away from the human sphere always also means suppressing the suffering and fears residing there.



Preliminary design sketch of a vast alien machine.
Detailed instructions on how to build this machine were sent by aliens in a coded message to earth, announced by a sequence of prime numbers. Taken from the Warner Brothers film "Contact", an adaptation of Carl Sagan's science fiction novel

The Dubious Appeal to a Scientific Method

More important in our context, however, is the question of whether the special character of scientific progress, its apparent imperturbability, even to human suffering and hope, really has anything to do with science's turning away from the murky and haphazard nature of everyday life. Certainly, this is the firm conviction of a long philosophical tradition that regards crystal-clear and unshakable ideas, principles or at least methods to be the guarantee for scientific rationality and by implication scientific progress. After Plato, Kant is the most important patron saint of this tradition. Like Plato, he inferred from the apparently unassailable security associated with fundamental statements in mathematics and the natural sciences that the origin of these statements is not to be found in sensory experience. However, he located this supernatural origin not in an objective world of ideas, but in man's own cognitive faculty.

In this way Kant believed he had explained the secure foundation of scientific progress as well, for all experience that has yet to be acquired must by its very nature fit into the architecture of our system of knowledge, which is already given. It was therefore only natural that Kant also attempted to describe the ground plan of this architecture. According to his expectations, this ground plan would provide orientation for all later developments of scientific research, but would itself remain essentially unaffected by these developments. Kant was convinced that this ground plan could be derived from the laws of reason, whose validity must be presumed prior to all experience. In other words, Kant envisioned a kind of thought machine, for which our experiences constitute nothing more than empirical raw material. With the help of mechanisms that are defined once and for all by the clockwork of the machine, these experiences are then processed into the rational insights of natural science. Among the unchangeable components of this thought machine, Kant counted a certain understanding of space, time and matter, but also the concept of force – precisely those forms of scientific thought which were changed fundamentally through the scientific revolution associated with Einstein. But long before Einstein's revolution, the rapid pace of scientific progress had made clear that its robust character could hardly be explained by the ostensibly universal mode of operation of the thought machine drafted by Kant. For this progress constantly produced new concepts and called old ones into question, such that Kant's ground plan, intended to be universal, in retrospect appears to be nothing more than a philosophically embellished image of the natural science of his day.

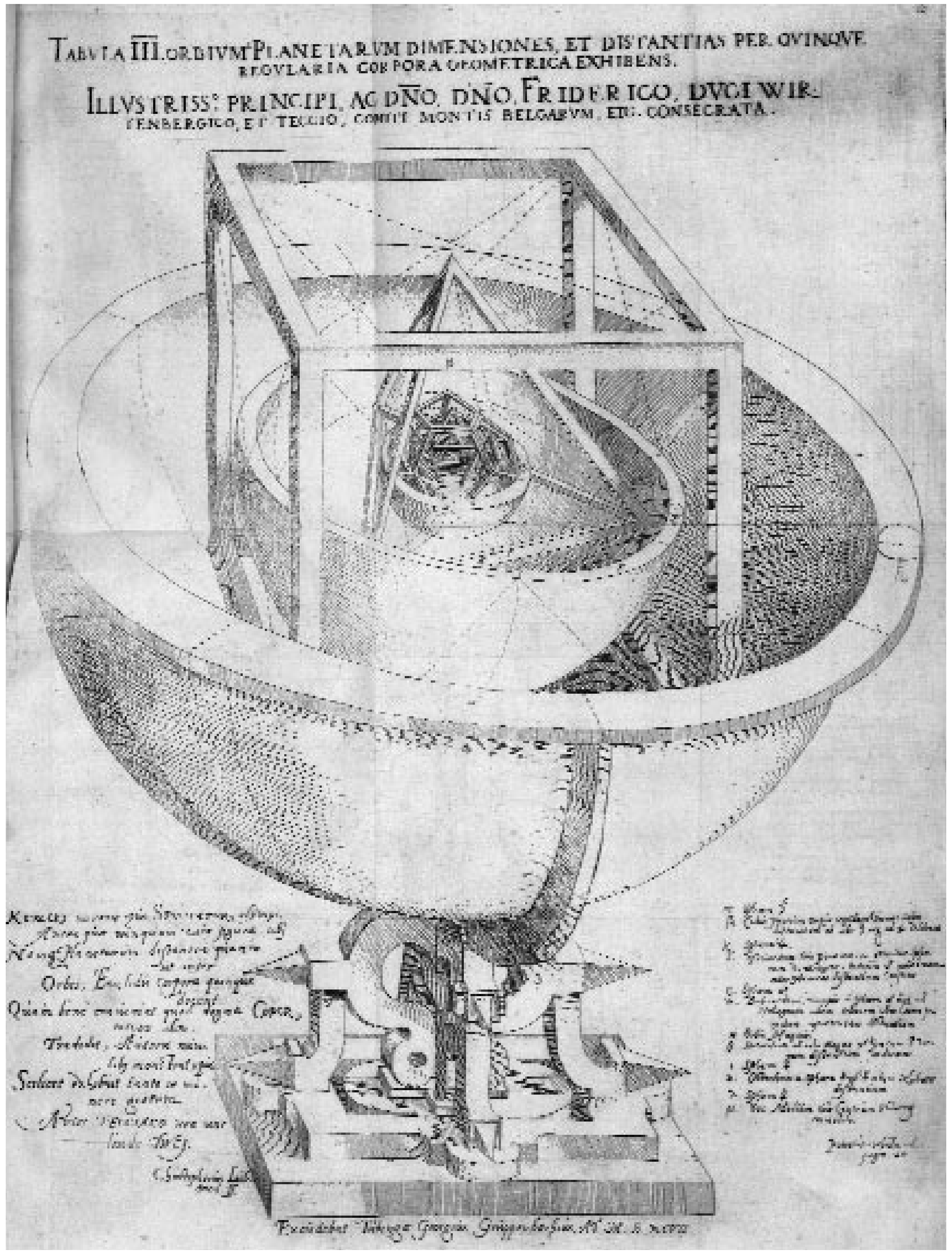
A related possibility for securing and explaining scientific progress consisted and still consists in widespread attempts to identify a scientific method, especially since the early modern period. No matter how diverse the given contents of science, such a method is supposed to guarantee both the cognitive progress of science and the validity of its results. Practically all attempts of this kind share with Kantian rationalism the assumption of an ultimate core of scientific rationality that cannot be affected by experience. They range from Descartes' rules for the direction of the mind to early positivism and all the way to the analytic philosophy of our day. Ultimately, however, such attempts to legitimate scientific rationality independent of any specific contents, despite their truly stupendous success in the history of philosophy of the last centuries, have only managed to diminish faith in any kind of stable foundation for scientific progress. The failure of these attempts also threatens to shake the foundations of any rationality that is to enjoy scientific legitimation.

In any case, despite centuries of efforts the philosophy of science has still not been successful in advancing any convincing, generally valid criteria for the acceptability of scientific theories that could also be implemented in practice. Every straight path that philosophy has attempted to cut through the sheer impenetrable jungle of experienced science has threatened to violate the perception of this reality. It is thus hardly surprising that in practice the evaluation of science, which occasionally concerns not merely differentiating between better and worse science, but also differentiating science from pseudo-science, still lacks any generally utilizable definitions and criteria. In evaluating science it makes little sense to rely primarily on indicators that can be assessed with mathematical precision. More reliable are social processes such as expert evaluation. Excellent science then is defined in practice quite simply as the science which is evaluated as excellent by excellent specialists – at the risk that dogmas or group interests prevail and exert an arbitrary influence on the course of science, more likely hindering its progress than encouraging it.

Perhaps the construction of a cumulative scientific progress, in the course of which one building block of knowledge is simply layered on the others, is actually a mere conceit, which erects from the fragments of historical tradition a building in keeping with the modern understanding and taste of the time, as was the case in earlier epochs of archaeology. The daring concept of “rational reconstruction”, by means of which philosophy of science aspires to make obstinate history more tractable, certainly speaks for this interpretation. Can the discoveries of the past really be interpreted as step-by-step approaches to today’s natural scientific understanding of the world and thus be assigned their place in some seamless whole? Is Copernicus’ assumption of circular planetary orbits really just a step towards Kepler’s ellipses? Is Einstein’s physics of the curved space-time continuum actually just a minor correction to Newton’s classical physics, an adjustment that need only be considered for motion close to the speed of light or when one simply wants to be very precise?

The Alien World of Galileo and Kepler

A closer look at historical studies soon reveals attempts to integrate earlier discoveries within a contemporary worldview to be inadmissible simplifications that distract from the fact that the intellectual worlds of Galileo or Kepler were essentially different from those of the natural sciences of today. For Galileo, for instance, the planetary orbits were certainly not the result of a motion compounded of inertial motion and gravitational attraction towards the sun, as conceived in classical physics. On the contrary, they embodied what he called a “neutral motion,” which was governed neither by the natural tendency of a body toward the centre of the earth, nor by a violent force removing it from this center. Only such neutral motions on circular paths were held to be suited to a harmonic cosmos. In Galileo’s conceptual world, the classical concept of inertia was unknown. This world was rather influenced by Aristotelian natural philosophy, in which the differentiation between natural and violent motion was fundamental, and into which Galileo’s concept of neutral motion could be integrated as a kind of intermediate category. Kepler’s analysis of planetary motion, on the other hand, was shaped both by the precise empirical data he received from Tycho Brahe, and by cosmological conceptions, which were also rooted in ancient philosophical conceptions about a harmoniously ordered cosmos. For instance, Kepler thought of the arrangement of the planetary orbits as being determined by regular Platonic solids, an idea that by its very nature does not fit with the image of modern natural science.



Copper engraving from the first edition of Johannes Kepler, Mysterium cosmographicum. Tübingen (1596)

There is no doubt that, as Kuhn and others have emphasized, self-confirmation is the main purpose of textbook stories that—instead of acknowledging such intellectual diversity—falsely depict a seamless continuity between the present and the history of science. Ultimately these stories embody the kind of founding myths familiar from religious and political contexts and legitimate contemporary science through their authority and paradigmatic status. Anything today's dominant stream of cultural history of science sets against these myths seems pure sacrilege by comparison and is perceived as such by leading natural scientists such as Steven Weinberg.

Recent studies in the history of science have taken Galileo down from the pedestal on which he stood for centuries and thrust him back in the context of his age, placing him, for instance, in the society of courtiers of the Grand Duke of Tuscany in Florence. The strategies of argumentation with which he attempted to prevail against his scholastic opponents there suddenly appear in a new light. From the perspective of cultural history, these are no longer examples of applying the scientific method supposedly established by Galileo, but subterfuges of a clever parvenu who knew how to operate the mechanisms of power skillfully to further his career. Because many of the statements in his writings, especially regarding the precision of experiments that he supposedly performed, have proven to be exaggerated or even incorrect upon closer examination, he is occasionally even treated as a swindler and the patron of modern scientific frauds.

The Perspective of Cultural History

But what can a cultural history of science contribute to the understanding of scientific progress? To many scientists it appears to be nothing but a disruptive force that further undermines faith in scientific progress? While extending history of science to include cultural history originally was supposed to take the blinders off the short-sighted view of a supposedly context-free rational core of science, this extension has long since contributed to discrediting science itself. And once the genie of skepticism has been liberated, it is very difficult to get it back into the bottle. Every case study in the history of science that takes a closer look at a personality or discovery, with a view to its cultural, technological and social contexts, implicitly fosters doubt not only in traditional myths, but also in the authority of science itself, which still appears to be reliant on these myths. The closer the focus, the more pure chance and the human-"all too human" come to light, and the more difficult it is to separate science from its context.

The more science is interwoven into its context in a certain historical situation, the harder it becomes to compare different episodes in the history of science. Against this background it seems anachronistic, even altogether inconceivable, to combine incomparable historical episodes to obtain a comprehensive panorama of progress. Indeed, what could be the link between such episodes, and how could one insight follow from the last, if each belongs to a different world? What might connect someone like the courtier Galileo with today's particle physicists, whose objects of research must seem to be chiefly artifacts of the complex social organization in which they work, at least to historians of science working in the tradition of cultural history? Indeed, how should even one individual communicate with another across the chasms dividing today's highly diverse scientific cultures?

According to today's historical understanding, an *a priori*-given communality of scientists, let alone a claim to universal validity of their insights, can no longer be taken for granted. It even appears to be paradoxical in view of the fact that every aspect of these insights is embedded in specific local conditions. For a long time, the repeatability of findings, independent of space and time, was considered a feature of science; today, the history of science has turned science into a domain of irreproducible results. Every claim that scientific insights might enjoy validity beyond their local contexts demands a historically comprehensible justification, preferably with reasons located in the external – i. e. primarily the cultural and social – conditions of science, because today these are the only conditions believed to be capable of providing an explanatory foundation independent of science itself.

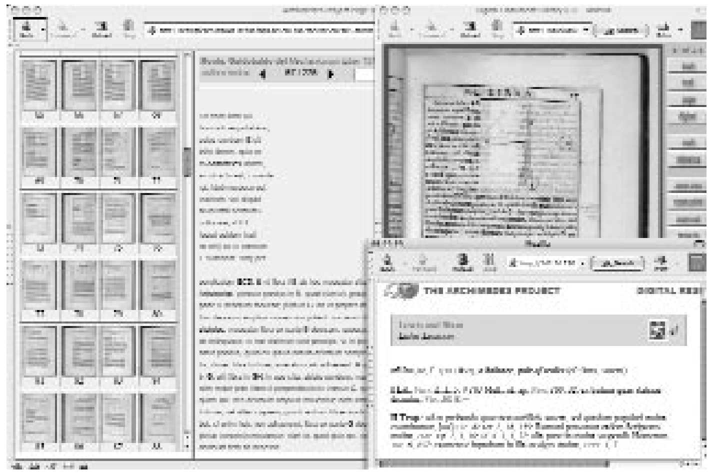
Who would dispute the plausibility of explanations for scientific innovation rooted in changes in the cultural, social, and technical environment? It is Einstein's revolution of our concepts of space and time that shows that in reality there is no absolute boundary between scientific concepts and our everyday thinking. The concepts of space and time, which were changed by the theory of relativity, were not only technical concepts of classical physics in the narrow sense, but rather also covered intuitive concepts such as the meaning of simultaneity, which classical physics had adopted from intuitive thinking without closer scrutiny. Is it thus not conceivable that the change in these concepts also originates in an everyday world that is constantly being changed by technological developments? And why should the synchronization of clocks in train stations, or the delay in transmission of an electrical signal caused by the length of a cable, not suggest a new idea of space and time to a scientist of this age, especially if this idea could be helpful to him in solving problems internal to his science? The heated debate about such attempts to link science back to its context is primarily an indication of the neglect of science's societal dimension in the traditional history of science – ultimately characterized by hero worship.

The Perspective of Traditional History of Science

For the traditional history of science, scientific progress was ultimately a kind of relay race of individual great minds, who passed the baton of their ingenious ideas across the abyss of time. Historians writing a history of science understood as a chronicle of success thus ask the classical who, what, when, and where – questions that are better suited to investigating professional sports than science. At any rate, they cannot take into consideration the problem that the sports involved have been in flux over the entire period. Indeed, how do we evaluate the achievement of Aristotle, the first natural philosopher to lay down rules for the natural motions of heavy and light bodies, when these rules are based on a fundamental differentiation between kinds of bodies that is no longer held today? And what are we to make of Priestley's achievement in being the first to dephlogisticate air, even though according to today's understanding air does not contain such a substance as phlogiston in the first place? And what about Heinrich Hertz's identification of electromagnetic waves in the ether, a medium we no longer believe exists?

Almost inevitably, the traditional history of science must suppress the incompatibility of past conceptions of the world with those of today. This task is made all the easier by the fact that its very approach always focuses on individual personalities, whose occasional errors make them seem all the more worthy of admiration. In contrast to this, more recent history of science has emphasized not only the dependence of these personalities and their achievements on context, but also located and elaborated a number of the social structures through which such dependencies are mediated. This opened the gate to a new understanding of the production of knowledge, which grasps scientific knowledge less as the sum of individual achievements than as shared knowledge which is the result of a collective process. However, the fundamental methodological consequences of this broader perspective are rarely drawn. Rather than comprehending conceptions like the Aristotelian distinction between naturally heavy and naturally light bodies as belonging to a comprehensive system of knowledge, in which individuals take part, these individuals are relativized also as personalities. Such a contextualization thus involuntarily conforms to a widespread public inclination toward iconoclasm and toward leveling anything extraordinary. Science would be so much easier to understand, if, like most other human pursuits, it turned out to be essentially driven only by power, money, sex and fame, and to take its inspiration from objects of our daily life-things that can be touched or easily imagined.

Whatever the public interest may be in re-evaluating the heroes of the history of science, more recent research along the approach of cultural history has contributed more to a realistic picture of science and its development than traditional history of science ever did. Even if such studies have occasionally overestimated the importance of local conditions in structuring scientific contents, the works of the past decades did make a clean sweep of the outdated ideas upon which most traditional studies in the history of science were implicitly based. Among these ideas was, for instance, the tacit assumption that the history of science could be written without paying attention to a complex architecture of knowledge and the equally complex structures of its social organization.



A page from del Monte's Mechanicorum Liber in the Archimedes Project's display environment, with (from left) thumbnail navigation, text with morphological links, page-image in image viewer, and linked dictionary entry for "trutina", "scales". The display environment automatically links together machine-readable text, dictionaries, grammars, high-quality page images and linguistically aware search-engines in a manner hitherto unthinkable in traditional library environments

From the traditional perspective it appeared quite sufficient to treat scientists as stamp collectors who exchange their ideas like rare stamps. Thus one could simply put their writings and letters between the covers of a book, adding a few explanatory remarks, perhaps even including some footnotes commenting on the origin of individual ideas. Legions of editors rummaged through mountains of documents only to leave a trail of such footnotes in their wake. Any reader wishing to bring to life the knowledge buried therein had to trace these editors' footprints through piles of documents scattered widely across archives and

libraries. Taking advantage of modern media, however, one can, as we have done at our institute, make such dispersed historical sources freely available in the Internet, networking them with each other and providing a new point of departure for dealing with systematic questions.

These new forms of presentation can also help us overcome the prejudice, which tends to be reinforced in traditional editing projects, that there is a simple division of labor between theory and experience in science, whereby experience is ultimately the source of all innovation because it provides the raw material, while theories, as interpretations of these materials, embody the quintessence of science. As is well known, these interpretations can shift over the course of time and thus constitute the proper subject of the history of science. And it is the texts in which theories are formulated, be they manuscripts or publications, that make up the primary object of traditional history of science and hence bring home this perspective over and over again.

Progress and Material Culture

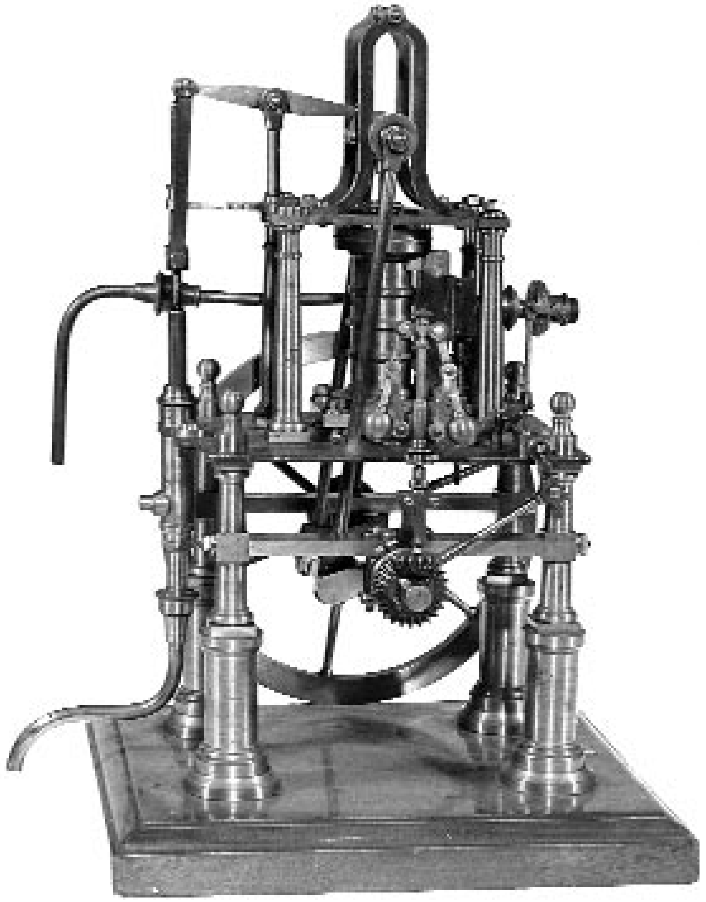
By contrast, only the more recent history of science devoted greater attention not only to the social circumstances under which these texts originated, but also to the material culture as a prerequisite for the empirical base of science. Indeed, what use would it have been to traditional history of ideas to recognize that certain scientific experiments require a facility in dealing with tools that could only be acquired outside of science, e. g., in a skilled apprenticeship? However, the fact that such insights can be obtained only through detailed case studies involving, for instance, repetitions of historical experiments, suggests that including other dimensions of knowledge, such as *tacit knowledge*, may have a price, tempting the historian to narrow his or her perspective to a downright microscopic size. A restriction of this kind becomes

Multiplication Table (ca. 1700 B. C.)
The table uses the sexagesimal positional system of Babylonian mathematics

problematic if it gives rise to conclusions more general than even science itself is trusted with. Insofar as such conclusions deal with the total character of scientific development that interests us here, they are almost purely arbitrary. For, in the end, the individual facets of the microscopic picture can either be combined to a panorama of progress, as they were previously, or to an image of science representing it as a kaleidoscope of unpredictable innovations whose secret appears to be somehow concealed among the very things that count as the material culture of science – be they specialized experimental systems or everyday technology.

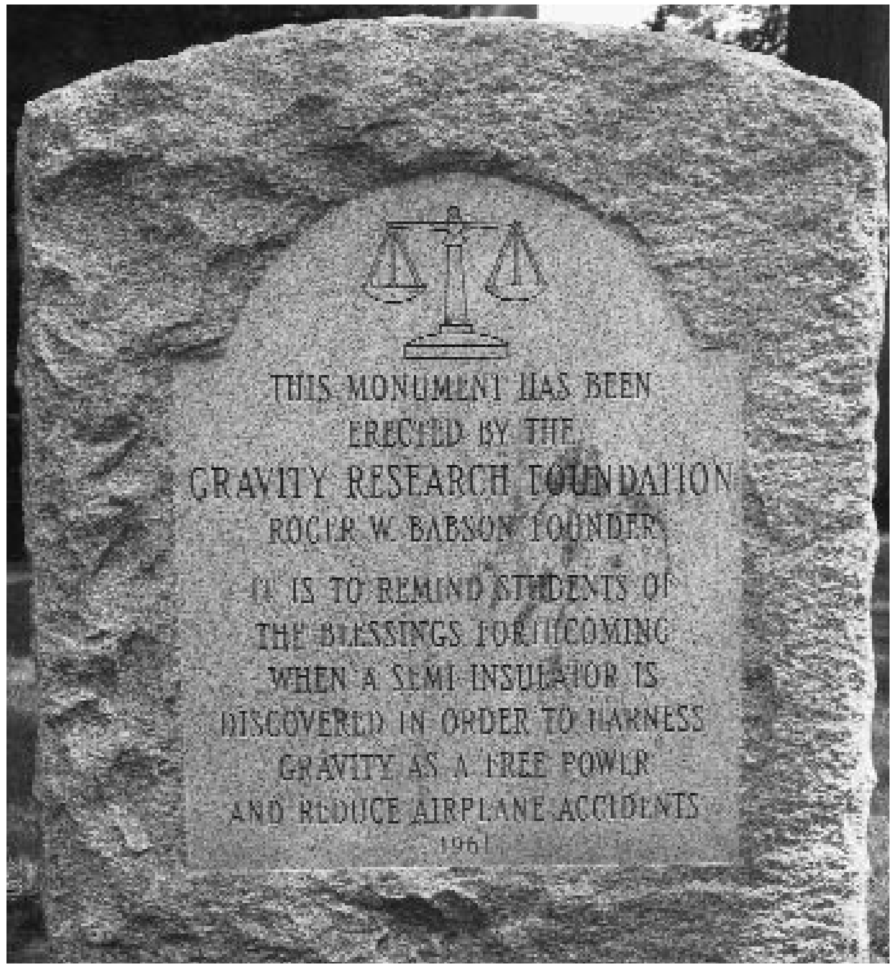
All appearances suggest, on the other hand, that social conditions and material culture leave their mark on the large-scale structures of scientific development as well. Mathematics entered the historical scene for the first time as the investigation of operations with systems of symbols, in those early ancient civilizations that used such systems of symbols as a significant aspect of social or economic control mechanisms. Such systems of symbols played an important role in the complex systems of administration and social rituals of the Babylonian, Egyptian, Chinese and Meso-American empires, which therefore produced a class of specialists who occupied themselves with the rules of these systems even beyond the context of their direct application.

The science of mechanics enters the historical scene in classical antiquity more or less at the same time as the invention of the balance with unequal arms, whose functioning can be explained by the first law of mechanics – the law of the lever. One of the key themes of the science of dynamics, founded by Galileo and others, is ballistics, at the same time a central concern in warfare in the politically fragmented landscape of contemporary Europe. Chemistry also developed as a science in close correlation with practical interests of the modern age in fields such as metallurgy. The relationship between the emergence of thermodynamics and the prevalence of steam engine technology is similarly close, as is that between electrodynamics and electrotechnology in the nineteenth century.



Steam Engine of Maudslay, last quarter of the 19th century, Museum for the History of the University of Pavia (Italy). Museo per la Storia dell'Università di Pavia

Such astonishing parallels suggest a simple causal relationship between scientific developments on the one hand and social developments on the other. Is science perhaps a response to the central economic needs of the time, as historians have surmised since Herodotus, and contemporary politicians hope even today? According to such views, the Egyptians supposedly invented geometry to master the problem of dividing up their fields each time the flood waters of the Nile receded. But if science could really be steered so directly by our human needs, we should in principle be able to foster whatever science promises as solutions for our most urgent problems at any given time, be they diseases like cancer or Aids, or global challenges such as feeding humanity or preventing climate catastrophes. How easy it is to fall short of such self-proclaimed goals, however, was demonstrated in the U.S. after World War II when generous funding was provided for research into "antigravity," but which failed to find a solution to the problem that heavy objects falling from the sky can cost human lives.



Antigravity Research Monument at Tufts University. Photo by Dr. Thomas L. Milbank, Perseus Project

Progress and Zeitgeist

The conception that material culture determines the development of science is in opposition to the conception that science and intellectual culture develop jointly. What certainly speaks in favor of this approach is that it does not postulate a strict causal determination of science by its environment, but only an interaction between a more or less autonomous science and its more or less advantageous external conditions of development.

From the perspective of such an approach it then appears only natural that science, in the sense of a verifiable, rational explanation of nature, experienced its first flourishing in the liberal climate of the Greek city-states and their culture of discourse. Similarly, it appears natural that the intellectual world of the European Middle Ages, ruled by the dogmas of the Catholic Church, remained dominated by Aristotelian philosophy, and that this was not given up until the Renaissance and the early modern age, when the bourgeoisie of the free cities opened European culture to the world. Indeed, it even appears quite conceivable that science and Zeitgeist are linked even more closely. Could the prominent role of atomistic natural philosophy in the early modern age, as represented by such scientists as Galileo, Gassendi and Newton, not be connected with the equally dominant attempts of contemporary social philosophers like Hobbes to explain society from the characteristics of the single individual? Admittedly stretching the idea, one could even argue that it is no coincidence that the splitting of the atom belongs to the same era as the crisis of the classical concept of the autonomous individual, as expressed both in the psychoanalysis of Freud and in contemporary art. After all, one can occasionally still hear earnest discussion about the assertion that the culturally pessimistic doubt in occidental rationality encouraged acceptance of the new perception of causality expressed in quantum theory.

It is, however, often difficult to prove such connections in detail. For this is certainly what distinguishes the Zeitgeist, if such a thing actually exists at all, from the Holy Spirit: The former cannot spread itself without the mediation of processes of reception and communication. But how exactly did the democracy of the Greek polis encourage the science of Antiquity? And how did Christian dogmatics succeed for centuries in convincing not only philosophers, but also uneducated contemporaries of the plausibility of Aristotelian natural philosophy? And how could the individualism of political philosophy in the modern age gain influence on the acceptance of atomism in the theory of nature? And even if, for example, it could be proven that Otto Hahn not only was an enthusiastic pupil of Freud, but also that Freud's work suggested to him the idea of nuclear fission, it would still not be possible in principle to differentiate the influence of Freud's theories from whatever inspiration Hahn might have received (say) from the use of a nutcracker. Success and ideas usually have many fathers, and common terms in the history of ideas, like "influence", "stimulate", "assimilate", hardly do justice to the different roles in the origination of scientific ideas played by more or less coincidental boundary conditions and systematic factors. But without such a differentiation, any attempts to interpret the development of scientific knowledge as part of a comprehensive cultural history must remain just as vague as the antiquated notion of Zeitgeist itself.

Dimensions of Knowledge

What can indications for an interaction between the development of science and its environment actually look like? Up to this point our discussion has addressed a number of dimensions of knowledge that must be taken into consideration if we want to avoid a one-sided abridgement of the history of science and to grasp the possibility of progress. Among these are the cumulative character of science and its dependence on social conditions. These conditions may concern the institutional organization of the production and transmission of knowledge, but also the media in which knowledge is represented as well as the technical and cultural environment of science. Another characteristic of the development of science is the occurrence of scientific revolutions. Against this background, one cannot simply interpret the cumulative growth of scientific knowledge as a continuous enrichment of a theoretical structure given once and for all. Rather, this growth goes hand in hand with ever-new rearrangements of the architecture of knowledge.

This architecture of scientific knowledge, for its part, obviously has a complicated structure. In any case it consists of more than just theories and empirical data. Scientific knowledge is also quite obviously connected with other fields of knowledge, and not only with the knowledge stored in other theoretical traditions such as that of philosophy, but also with the practical knowledge of craftsmen and the intuitive knowledge that each of us must acquire in his or her individual development in order to cope with the material nature of the world. Some aspects of scientific thought appear to be almost immune to empirical scrutiny, at least across medium-term historical distances, a fact which has repeatedly given occasion to speculation about *a priori* structures of knowledge. We have also established that the development of science possesses a large-scale structure that is somehow correlated with long-term social and especially technological developments. The existence of such large-scale structures indicates that scientific knowledge must have a societal dimension and can only be understood as shared knowledge. The phenomenon of more or less simultaneous discoveries made largely independently of each other, which can be repeatedly observed in both technology and science, can hardly be a coincidence; rather, it can only be understood as an effect of this societal dimension.

The question of whether or not there is a perspective from which the progressive character of science can be reconciled with its contingent development shaped by historical boundary conditions can only be answered by taking into account all of these dimensions.

Progress and Development

A clarification of what "development" means here is decisive. Which concept of development allows for a compatibility of the role of chance and arbitrariness in the emergence of knowledge on the one hand, and the notion of a steady accumulation of knowledge on the other? The common concepts of development applied in the history of science generally emphasize one side at the expense of the other. When natural scientists study the history of their fields, they tend to construct this history from the outcome, and to presuppose that this outcome was also the goal of scientific development from the outset. They thus attribute, explicitly or implicitly, a necessary, inevitable character to scientific development which allows other factors at most an accelerating or retarding influence.

When historians, on the other hand, divide the history of science into different cultural and societal contexts, they simultaneously assert – more or less explicitly – its essential dependency on a multiplicity of factors, each of which can have only a contingent character for the development of science itself as, for example, the despair of occidental rationality at the close of World War I. When pursued to its extreme, this view declares the development of science to be the result of its cultural and so cial boundary conditions. According to the understanding of development in science outlined above, it is nature that gives to the development a direction that is fixed from the outset. According to the last interpretation mentioned, it is culture that drives this history, albeit in an aimless manner, and ultimately makes scientific innovation a mere side effect of its circumstances.

Both concepts of development, that of a "teleological" progress determined by the goal, and that of a "contingent" innovation ruled by chance, are also well known from other areas of historical development. In addition, of course, further concepts of development have been tried and tested both in scientific and non-scientific contexts. Examples are the concept of the development determined once and for all by its initial conditions, or the concept of a development that is ultimately determined by a higher power, a concept found in religious histories of salvation, but also in traditional views about education. There have been repeated attempts to interpret philosophical, psychological, societal and economic developments, biological developments and even the history of our solar system or of the entire cosmos using such concepts of development, with varying degrees of success. The names of Hegel, Piaget, Marx, Lamarck, Darwin, Kant and Laplace stand for such attempts.

According to the discussion thus far, a satisfying understanding of scientific progress excludes from the start a number of the simpler concepts of development. Among these is the concept of a development determined by its initial conditions or by its goal. For it is too obvious that the role of more or less accidental, but in any case external boundary conditions on the process of scientific development is not limited to the effect of restraining or accelerating. The interpretation of the history of science as a process essentially shaped by external factors appears just as useless. As plausible as such a pattern of interpretation may be for local parallels between the development of science and the development of culture, it is hardly able to explain the enduring character of scientific achievements, which is independent of the continued effect of their often unique conditions of origination. For instance, it may well be that a number of patterns of argumentation in scientific proofs originated under the specific conditions of the Greek polis. But the fact that they are still viewed as being valid today can hardly be explained in terms of social boundary conditions that have remained unchanged since antiquity.

Requirements for a New Concept of Development

Comparing different concepts of development and their explanatory power in different areas yields, to begin with, a few minimal requirements that must be met by any concept of development for the history of science that is designed to avoid the deficits and paradoxes of the concept of progress described above. The subject of development should not be reduced to the external conditions of its development, but neither should it be allowed to exist completely autonomously. Furthermore, a concept of development compatible with science must include mechanisms that explain the long-term and cumulative character of the increase of knowledge. In order to do justice to the fact that unforeseeable scientific innovations take place, on the other hand, an adequate concept of development must allow for an element of contingency. This element is the reason why the possibilities of further development given with any step are richer than the prerequisites that initially led to this step. Only in this manner does genuine innovation become conceivable without being reduced to a mere unfolding of possibilities given at the point of departure.

A rich concept of development should therefore allow in principle for alternative pathways of development, even if paths diverging from the main strand of development were taken for only shorter historical distances in the actual history of science. The dominance of one main strand of development, as represented by the globalization of Western science, points to a further feature of the development of science, one that is generally characteristic of historical development processes, be they of a societal or biological nature. This feature consists in the transformation of incidental boundary conditions into inevitable prerequisites essential for the further development. The impact of the Yucatan meteorite 65 million years ago, for example, was an event of initially only accidental and external nature for the development of life on Earth. But because the ensuing catastrophe led to the extinction of the dinosaurs, the impact of the meteorite became a necessary precondition of the further development of life, which definitively excluded other paths of development. As in biological evolution it is inconceivable that an alternative development of science could start again today from the point where it was interrupted millennia before, as is the case for Chinese science after the suppression of philosophical schools and the burning of their books in the third century BC. Today, asking what Chinese science would have become without this interruption is just as idle as asking whether the descendants of the dinosaurs would have learned to eat with knife and fork or with chopsticks.

Fig. 9. Cambridge students, who lowered a monkey marionette from the gallery when Darwin received his honorary doctorate in 1877, gave the Punch cartoonist this idea about the 'missing link' between men and apes. Punch, 1 Dec. 1877

As fruitful as the comparison between scientific and biological development may be, it can also easily prove misleading. Too great is the temptation to infer causal connections from superficial parallels. Anyone who fails to consider the many intermediate stages that separate human thinking – stamped as it is by hundreds of millennia of cultural history – from its biological foundations, runs the risk of jumping to speculative conclusions. From this perspective, it may be tempting to assume that the essential structures of this thinking can be derived directly from biological evolution. In any case, without comparative cultural and historical studies, direct inferences from mental achievements observed today to their biological roots are about as convincing as the inference from our remarkable difficulties in understanding statistical relationships to the assumption that if hominids with statistical abilities ever existed, they were certainly not among our ancestors.

Clearly, biology cannot offer an answer to the question of how to conceive of the history of science as the history of progress. The biological theory of evolution can at best serve as a role model for an independent historical theory of the development of knowledge. More precisely, a theoretically grounded history of knowledge can learn something from the biological theory of evolution about the measures and standards for an appropriate concept of development, for such an evolutionary theory of knowledge, if it can be formulated at all, will hardly be any less complex than Darwin's theory. The object of such an independent historical theory can be only human knowledge itself, understood as an irreducible element of the historical development of humanity. This knowledge has developed since the beginning of human history, in a ramified yet coherent process, just as life on earth has, according to the theory of evolution, developed – in spite of its immense diversity – as a cohesive planetary process. From this perspective science is a part of the shared knowledge of humanity – just as the order of the primates to which we belong is a delimitable, historically specific part of life on Earth.

The development of knowledge is a coherent global process in the sense that the knowledge of every human individual in a certain historical situation is part of a societal system of knowledge in which that individual participates. Such different systems of knowledge are interconnected through historical development and global processes of dissemination. In fact, it is possible to identify long-term strands in the tradition of knowledge going back to the beginning of human history, especially concerning the use of tools. Some of these, it is true, developed in isolation from each other for long periods of time, but interactions between them took place repeatedly, such that their history can only be written as the kind of global history of development as is indeed pursued by paleoanthropology. Traditions in the use of tools are necessarily at the same time traditions of the development of knowledge. We have become used to labeling past epochs – such as the stone or bronze age – according to the few relicts still remaining, focusing on the tools rather than on the knowledge that produced them. Yet this designation only makes sense when these tools are regarded as material representatives of a cultural tradition whose essential elements must have included the knowledge necessary to invent, produce, and use these tools.

The Decisive Role of Material Means

The transmission of material means like tools was just one albeit decisive aspect of the transmission of knowledge. And this knowledge could always be handed down only as shared knowledge, which individuals could acquire by participating in their manufacture and use. Despite all of the differentiation visible today among individual celts, classifying them historically is not a matter of telling the stories of great skilled craftsman, and not just because exact knowledge of the personal data of their producers is lacking. The long-term history of knowledge is actually a history of systems of knowledge, which, for all their minor fluctuations, are characterized both by long periods of essential stability and profound transformations, which can be read from the material representations of the handed-down knowledge as in the transition from stone to bronze tools.

Tools are more than just an indication for continuity and thresholds in the historical process. If we return to the analogy between the development of human culture and the development of life, then transmitting material culture corresponds to the process of heredity, without which continuity would be inconceivable. That every generation does not have to start from the beginning in building up a society and civilization is primarily due to the transmission of material culture and the knowledge about how to use it. Of course, such transmission also includes the efforts of appropriating what was handed down, in keeping with Goethe's motto, "What you have inherited from your fathers, appropriate it, to possess it!" These processes of appropriation cover a broad spectrum of social activities, ranging from raising children through participation in work processes all the way to school and advanced education. It is part of the nature of such processes of appropriation that innovations become possible in their course. This potential for innovation is already embodied in the material character of the stock of tools handed down from generation to generation and guarantees the element of contingency in the development of human knowledge which we have demanded above.

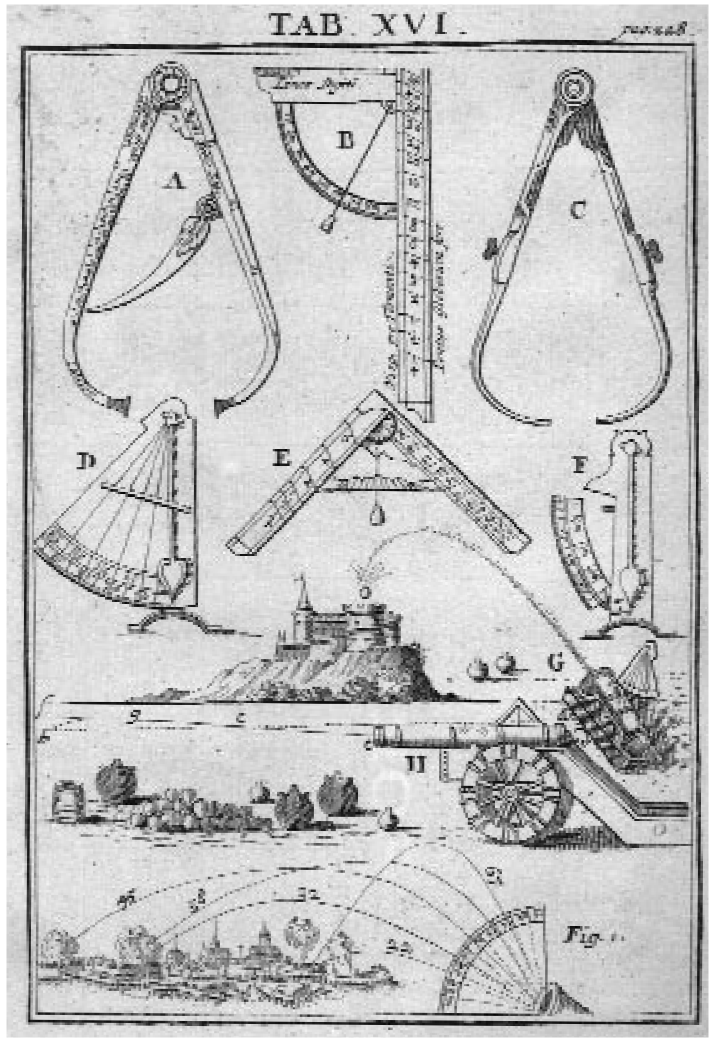
The material character of a tool is the reason why the horizon of its possible applications always extends beyond the specific purpose for which it was originally invented. Exploring this horizon thus can lead to the discovery of surprising new potential applications of a tool, possibilities that never entered the mind of the original inventor. This quality that the means is more general than the purpose is what Hegel called the "cunning of reason"; he declared it to be a central mechanism of human development. Other additional aspects of this cunning of reason are just as important for the possibility of progress in civilization. Just as relevant is the difference between the knowledge necessary to invent a tool and the knowledge that is required to produce it, and also the knowledge that must be applied just to use it. Were there no difference between these different kinds of knowledge, every television viewer would have to understand something about semiconductor technology, and it would be impossible for any repairman to replace an electronic component without first understanding how it works on the basis of quantum theory. One of the things that makes progress possible is that tools so embody the knowledge that went into them that it does not have to be reproduced again every time the tool is used, but in a sense is supplied implicitly along with the tool.

The very fact that tradition and innovation are so closely connected to each other in handing down tools and, more generally, in the material culture of a society, makes long-term progress possible. Progress is thus integrated as a possibility in principle in the development of human culture. The extent to which this possibility is realized, however, depends on external conditions. They can not only dampen or encourage the exploration of the potential for innovation dormant in tools, but they may also interrupt traditions, thus making even the preservation of already acquired knowledge over generations impossible.

A Historical Definition of Science

Science may take on completely different forms in various cultural and historical contexts, but all of these forms of the human acquisition of knowledge share a general nature that lies in their exploration of the potential for innovation embodied in a given material culture. This exploration occurs independent of the specific applications also given with this culture, through its tradition and focusing on certain goals. Against the background of such a historical definition of science, the remarkable dual character it possesses, which gave rise to the contrary interpretations discussed above, becomes more understandable. The staying power of science and its relative stability are based on its roots in technology with which the human race reproduces its social system. By contrast, science's lack of endurance and relative fragility lie in its dependency on the motivations prevailing in any given society. This fragility has been reduced more and more in recent centuries as science has gone from a kind of hobby of small groups of elites to become a decisive element of the technology humanity requires for its survival. However, this has made the remaining element of the fragility of science as a social enterprise particularly significant, because the very survival of the human race is determined to an increasing degree not only by the weal and woe of science, but also by the direction and the conscious shaping of scientific progress.

Geometric instruments for military purposes.
Charles Hutton, A Mathematical and Philosophical Dictionary.
London: Davis (1796)

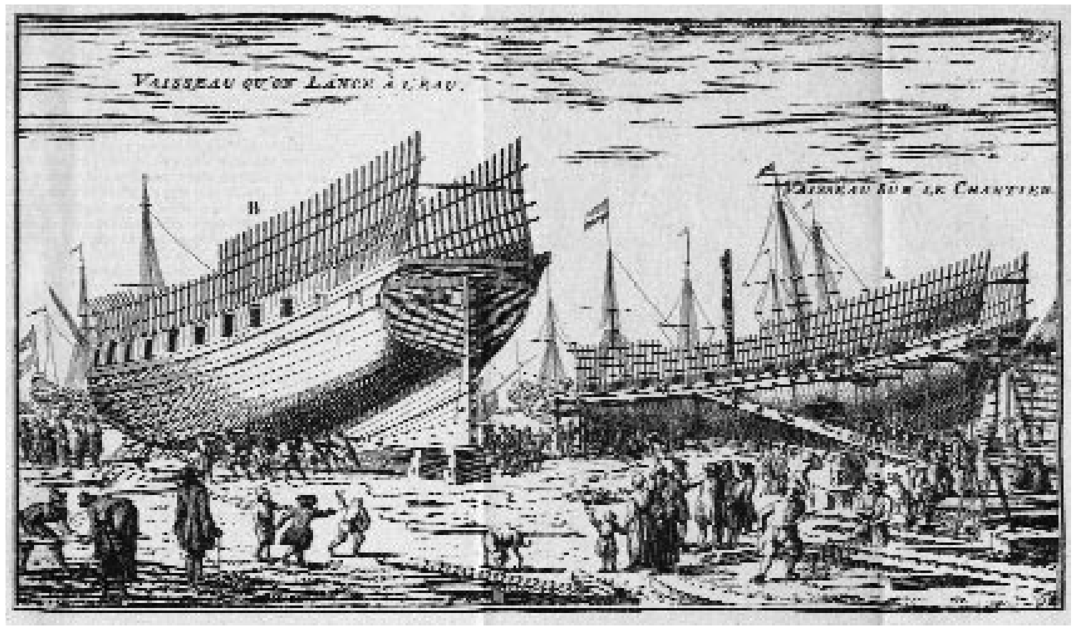


The possibilities of scientific thought always were and are influenced by experiences in dealing with the technology available in a given period. It is obviously no coincidence that instruments as old as measuring rods and clocks were still able to play a key role in the origination of the theory of relativity. Einstein's occupation with the question of whether measuring rods and clocks behave differently in a moving laboratory than in a stationary one may appear at first glance to be a consequence of a predilection for strange thought experiments. However, this occupation would hardly have exerted such profound effects on our understanding of space and time if it had not touched the very foundations of the knowledge history of humanity, in this case the material framework supporting our concepts of space and time. In fact, Euclidean geometry based on constructions with compass and ruler determined our theoretical understanding of spatial relationships for over two millennia to such a degree that philosophers held its concepts for structures of human thinking, given prior to all experience.

Material Culture and the Problem of Necessity and Chance

These considerations also make it easier to understand which significance the material culture of a society has for a developmental theory of scientific knowledge. Over long periods of time it was this material culture that provided science with a base in experience. Viewing the development of science from the perspective of the development of society, this base appears to create preconditions that are altogether imperative, but from the perspective of science itself, it certainly involves an element of chance. Thus it was practically inevitable that the mechanical technology of the early modern age, from artillery to architecture to shipbuilding, became an arsenal of challenging objects for contemporary science. These objects were investigated by applying concepts, most of which were taken over from antiquity, with varying degrees of success, until the theoretical horizon fixed by those concepts could finally be overcome. From the perspective of a long-term history of the development of thinking, it is particularly remarkable to see the great extent to which the direction of scientific progress was influenced, in this case by the specific contemporary material culture together with its social circumstances.

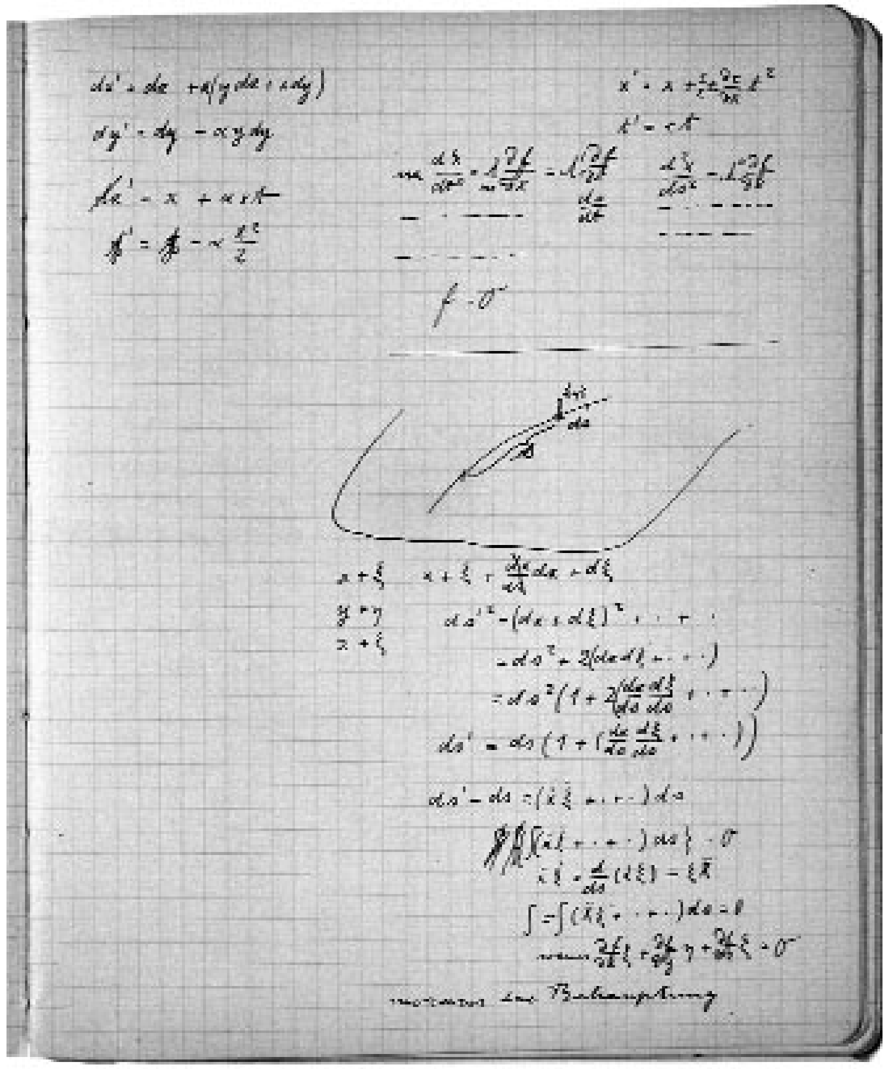
A Plate from L'Art de Batir les Vaisseaux, Witsen, van Eyk, Allard et. al., Mortier: Amsterdam (1719)



Without the intensive study of specific problems suggested by practice, such as the motion of a cannonball, certain theoretical principles like the law of inertia would hardly have played the central role in the development of physics that they did. According to the law of inertia, the motion of a body proceeds in a straight line in uniform motion as long as no external force is exerted upon it. This completely contradicts not only our everyday experience, according to which motions come to a halt on their own unless they are maintained by a continuing force. The law of inertia also appears to contradict the observation that celestial bodies, without any visible influence by external forces, move non-uniformly on paths that are anything but straight lines. On the other hand, the law of inertia is extraordinarily suited to serve as an axiomatic point of departure in developing a science of dynamics to serve as a basis for dealing with idealized terrestrial motions like that of a cannon ball along a parabolic path, or of a billiard ball after being struck by another billiard ball. Thus even central scientific schemes of interpretation, such as the concepts of space and time and the law of inertia of classical mechanics, owe their existence to a specific, and with regard to its material basis, contingent historical development.

However, if one views science as a subsystem of society, this contingent development at the same time possesses a certain necessity. Furthermore, the sheer mass of problems that could be mastered using the scientific concepts that emerged from this development bestowed upon them a long-term validity. Nevertheless, the rather contingent selection of challenging objects, as seen from the perspective of science, opened up the possibility of a fundamental change in this scheme of interpretation. In the case of the law of inertia, this possibility was ultimately realized in the fundamental change brought about by Einstein's theory of relativity. Its conception of motions under the influence of a gravitational field contradicts the Newtonian conception of such motions. While Newton presupposes an absolute space that is everywhere homogenous, in which such motions are interpreted as the result of the action of a force, for Einstein they are paths in a curved space-time continuum distinguished by their naturalness. In some respects the Einsteinian view thus better corresponds with the understanding suggested by our intuitive knowledge, that the world is not everywhere homogenous, and that there are such natural motions as free fall – just as Aristotle asserted. It thus also appears to belong to the paradoxical character of scientific progress that it occasionally moves in circles.

Or at least it could appear this way if the architecture of knowledge and the structures of its long-term development are not taken sufficiently into account. The above example of Aristotelian natural philosophy offers a particularly vivid illustration of how important it is to understand the complex architecture of knowledge when regarding long-term developments in the history of science. It would be superficial to trace the century-long dominance of the Aristotelian world view back exclusively to the influence of the Catholic Church on thought in the Middle Ages (although this certainly should not be underestimated) without doing justice to the fact that this worldview was plausible on the basis of intuitive knowledge. It would be equally rash to conceive of the Einsteinian revolution as a mere product of local circumstances of the time, which may have favored certain scientific theories over others, without taking into consideration how they were anchored in the practical knowledge of measuring length and time. And this is true notwithstanding the element of historical contingency in Einstein's strong operationalists leanings, which perhaps overemphasized the importance of such measuring procedures. In any case scientific knowledge is only the tip of a colossal iceberg, whose substance also includes intuitive and practical knowledge that is only occasionally expressed explicitly in scientific theory, and generally not until it has become a problem. And like the processes affecting icebergs, the processes of changing knowledge take place on widely divergent time scales, ranging from geological slowness to abrupt collapses. But how exactly can the structure of such changes be understood in the case of the development of knowledge?



A page from Einstein's
Zurich Notebook (41 R)
showing motion along a curved surface

The Principle of Actualism

In addressing this question it is worth taking a look at other areas in which developments are investigated. After all, even Darwin's understanding of the evolution of life was able to profit from such comparisons. This is also true for the methodological principle of actualism which he adopted. According to this principle only those processes that can still be observed in the present should be allowed in the explanation of historic changes. This principle introduced for geology by Lyell turned out to be the key for Darwin (even if it has come in for sharp criticism by Stephen Jay Gould and others in more recent times). In explaining the historical changes in life forms, this principle meant that he could only take advantage of those mechanisms that still have an effect on changes in life forms today. He found such mechanisms in the contemporary practice of breeding, which gave him the idea of transmitting the model of human selection for breeding to the principle of natural selection. Despite the many difficulties presenting obstacles to such a transmission of the model of selective breeding by humans to selective breeding by nature, the merging of the historical and systematic knowledge of biology with the practical knowledge of breeders about current processes of change in life forms remained the critical step in the origin of the Darwinian theory of evolution.

For a developmental history of human knowledge, such an integration of different knowledge resources is just as conceivable in principle as it was for biology, but hardly attempted so far. For in the field of the history of knowledge as well, there are, on the one hand, historical documents serving as indications of past forms of knowledge, analogous to the function of fossils in biology. On the other hand, much is known about the way knowledge is transformed in processes of learning that can be observed today – practically, from the learning processes in our society, and on the scientific level from educational research, psychology, and cognitive science. Most importantly, the sciences that deal with cognitive performances that can be observed today offer a theoretical and methodological framework for a historical theory of the development of human knowledge, in which both architectures and processes of the transformation of knowledge can be described more precisely than is possible in a conventional history of ideas. This presents an opportunity to go beyond the terminology of "influencing," "after-effects," and the like. Not only can the structures of knowledge be described more precisely, but these descriptions can also be verified on the basis of both historical sources and thinking processes that can be observed today. At the same time, today's research on cognition is offered the chance to cross over the narrow confines of contemporary forms of thinking and knowledge, which induces them to consider these forms as being universally valid.

Against the background of intuitive knowledge as conceived in psychological studies, it becomes clear why, for instance, Aristotelian natural philosophy maintained its credibility over millennia, despite a number of scholastic ornaments that had been linked to the intuitively plausible core ever since its emergence. Its roots in intuitive knowledge also explain why even today pupils educated in classical physics still respond to questions about the cause or the course of motion with answers closer to the Aristotelian understanding than to Newtonian physics. In fact, as stated above, the Aristotelian principle of the necessity of an external cause of motion is much more familiar to us as a "mental model" from everyday life than is Newton's principle of inertia, according to which a motion continues steadily and in a straight line as long as no force is exerted upon it. For the physics of the heavens, too, the Aristotelian assertion that the planets move themselves without external coercion seems more reasonable than the Newtonian view that they are diverted from their actually straight, homogenous motion by gravitation, a mysterious attractive force of the sun. And finally, the Aristotelian idea of a space in which there are qualitatively different directions (like up and down) and also qualitatively different regions (like the spheres of celestial and earthly motions), is intuitively easier to accept than Newton's infinitely extended, isotropic and homogenous space that incorporates the concept of space in Euclidean geometry. It is interesting to note in this context that this view, i.e., the interpretation of the motions of celestial bodies as being free from external forces and the idea of an inhomogeneous space was reborn in Einstein's general theory of relativity, despite all the other non-intuitive aspects of this theory.

The Dynamics of Scientific Revolutions

Thus the long-term effectiveness of such schemes of interpretation makes apparent that scientific knowledge includes not only a theoretical dimension, but also intuitive and practical knowledge, as deeper layers that can be described in detail using such concepts from cognitive science such as that of mental model. While theoretical and practical knowledge are subject to cultural and historical influences, not least because of their material foundation, it seems clear that intuitive knowledge, if not universal, at least clearly transcends such local contexts, even if this has only been established empirically so far by a handful of comparative cultural studies.

In summary, the continuity of knowledge can be explained essentially by the continuity and transmission of the material culture of a society to which it belongs, and by the material nature of our environment. In order to recognize that such continuity does not amount to infinite recurrence, what is necessary is not only a more exact analysis of the architecture of knowledge, but also an understanding of the dynamics of its development. As we have seen, a key role is played here by the Hegelian cunning of reason, which is based on the potential for innovation inherent in the material culture upon which the development of knowledge is based. In principle, the fact that the means can be applied more generally than the purpose for which they were invented may explain the emergence of novelty in science. However, it does not directly follow from this cunning of reason how and why the nature of novelty is such that the development of science, which over long historical distances proceeds continuously, involves occasional leaps.

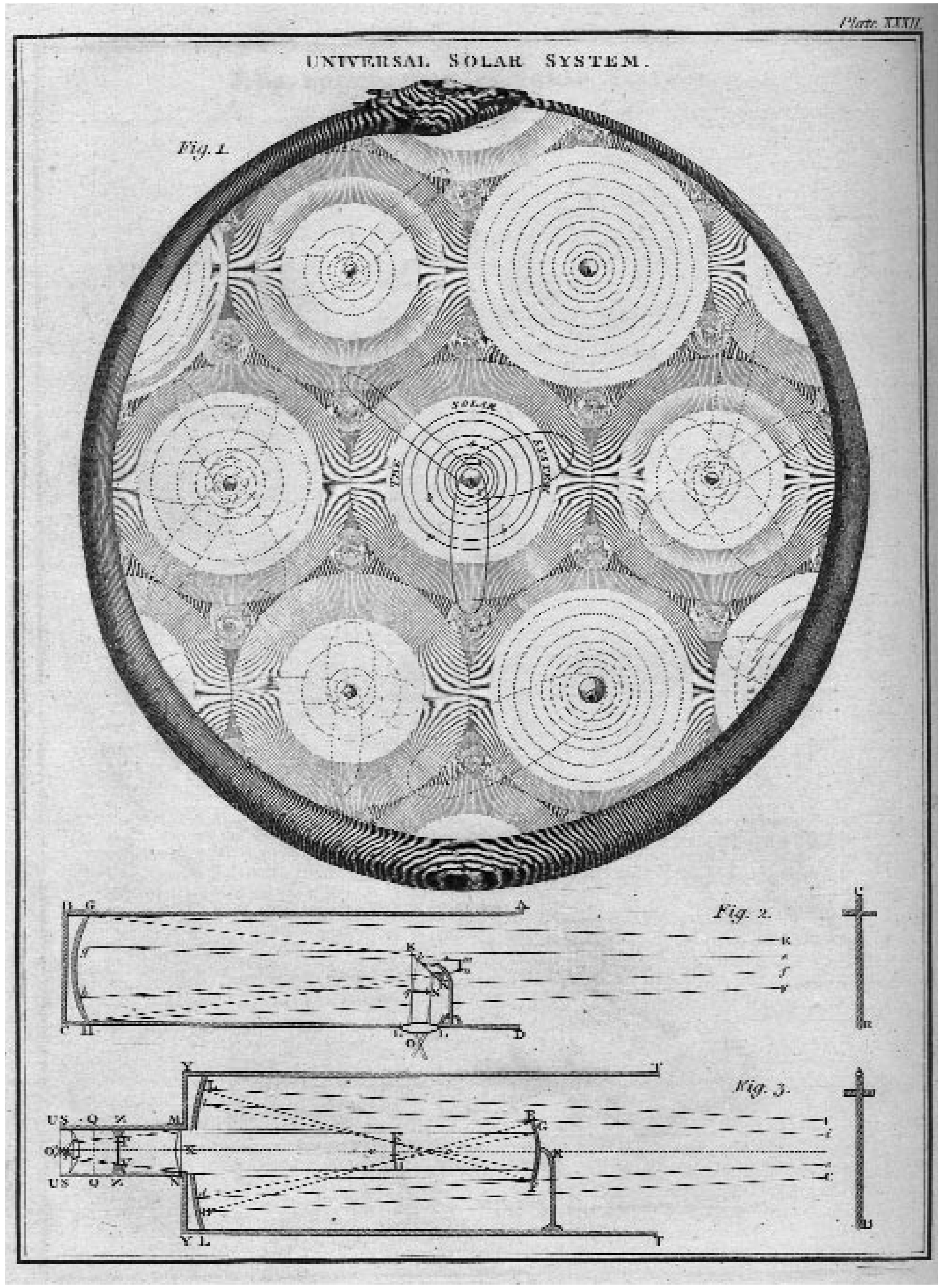
According to the discussion so far, scientific revolutions can be understood as restructuring systems of knowledge. Continuing with the analogy to developmental biology, such a restructuring corresponds to the emergence of a new species. As in biology, the possibility of the emergence of a new structure of knowledge rests upon an inner variability of the system. In the case of a system of knowledge, the inner variability constantly increases by exploring its limits using the available material means. The system's variability typically results from the growth of possibilities to generate from one and the same system competing results – be they experimental outcomes or theoretical conclusions. But just as a new species does not emerge until the conditions are given for the divergence of the new from the old species (often through the geographical separation of a group of individuals), such a divergence is also required before a new system of knowledge can be generated.

In this case the necessary divergence results first of all from the emergence of internal contradictions in the existing system of knowledge – also a typical result of the continuing exploration and internal networking of a system of knowledge –, which ultimately forces decisions about the alternatives presented. Secondly, the divergence necessary to shape a new system of knowledge arises from a reorganization accomplished by means of reflective thinking, which takes certain – often problematic or marginal – consequences of the existing system as the points of departure for the new system. This could be called a "Copernicus process," in analogy to what is probably the most famous example in the history of science for this kind of restructuring of an existing system of knowledge. Indeed those reflective breaks in systems of knowledge known as scientific revolutions take place in a similar way to the revolution of Copernicus. He created a new world system by shifting a formally peripheral celestial body, the sun, to the center. In doing so he essentially took over the complex mechanism of planetary astronomy that had been worked out previously, rather than starting with a *tabula rasa.*

The transformation of preclassical mechanics into classical mechanics took place in quite a similar manner. Thus, for instance, the assumption that bodies not subject to any force move uniformly and in a straight line was a conceivable but highly doubtful statement located at the margins of Galileo's preclassical theory of motion, which had its roots in Aristotelian physics. It was certainly plausible in that it was the simplest presumption that provided an explanation of the parabolic shape of the trajectory of a projectile, correctly recognized by Galileo. However, at the same time it was problematic because it directly contradicted the basic Aristotelian assumption that every motion can be traced back to an immediate cause. Galileo's disciples were the first to explicitly formulate the principle of inertia – in a process of reflection on Galileo's results – and to use it as the point of departure for a theory of motion that could no longer be reconciled with Aristotelianism. Similarly to Galileo's disciples, in his revolution Einstein took the problematic consequences of the theories of his predecessors, the theories developed by the masters of classical physics, Lorentz, Planck and Boltzmann, as the point of departure for a new system of knowledge. This new system preserved the heritage of classical physics, but, through the introduction of new concepts of space and time, could no longer be reconciled with its superseded concepts. It is not a paradox, but merely an illustration of the complex nature of progress, that this revolution preserved the ancient mental model of natural motion.

In the framework of a historical theory of knowledge, as it has been outlined here with reference to the major research projects of Department 1, progress not only loses its paradoxical character but Department re-enters the sphere of human intervention. After all, the source of scientific innovation and the precondition for dealing responsibly with the possibilities of science, both being given by reflection, have turned out to be, in essence, the same.

right page:
Universal Solar System.
Charles Hutton, A Mathematical and
Philosophical Dictionary.
London: Davis (1796)



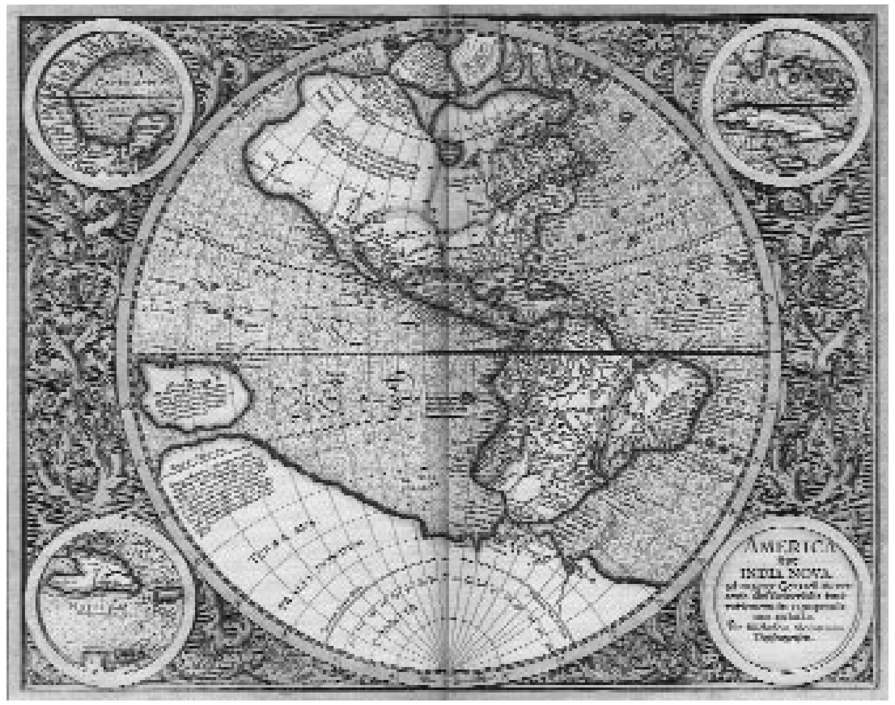
Scientific Error and the Ethos of Belief

Lorraine Daston

Introduction: Knowledge and Belief

The research project on “Knowledge and Belief” (2005–7) has singled out three foci for special attention, with working groups organized on each: theology, science, and philosophy in Christian, Judaic, and Islamic contexts in the fifteenth century; traditions of natural theology from the twelfth through twentieth centuries; and the epistemology of belief. Of these three topics, it is the last that concentrates on the problem of knowledge and belief largely within the sciences, rather than on the interactions of science with religion in diverse intellectual and cultural milieux. For the modern sciences, the boundary between what counts as established and reliable knowledge and what as hypothesis, conjecture, and tentative belief shifts constantly, according to the dynamic of research and debate. Today’s reigning theory may be toppled by tomorrow’s finding; within the span of a single scientific career the received wisdom of a discipline may be fundamentally revised not once but several times. What was once judged to be an audacious speculation may be confirmed by ingenious empirical tests; conversely, the very axioms of mathematics may be confronted with alternatives. On the basis of the latest research, knowledge is demoted to the status of mere belief, and belief promoted to that of knowledge; hence the instability of the boundary between them – and the dynamism of the modern sciences. The price of scientific progress is the obsolescence of scientific knowledge.

The problem of knowledge and belief was born with the modern sciences themselves in the sixteenth and seventeenth centuries. During this period, a whole range of explanatory system and empirical claims that had been accepted as eternal truths for centuries were overturned. The cross-fertilization of natural history, natural philosophy, craft knowledge, and mathematics created new forms of inquiry, test, and proof – a whole “new science”. The origins of modern philosophy, one might argue the origins of modern Western thought *tout court*, lie in a seventeenth-century diagnosis of pathological belief. The beliefs in question ranged from the theological to the astronomical to the geographical, from the anatomical to the natural philosophical: the voyages of discovery, the Reformation, the triumph of Copernican astronomy and Newtonian natural philosophy, the demonstration of the circulation of the blood – all confronted early modern thinkers with dramatic and disturbing examples of errors that had persisted for centuries on the authority of the very best minds.

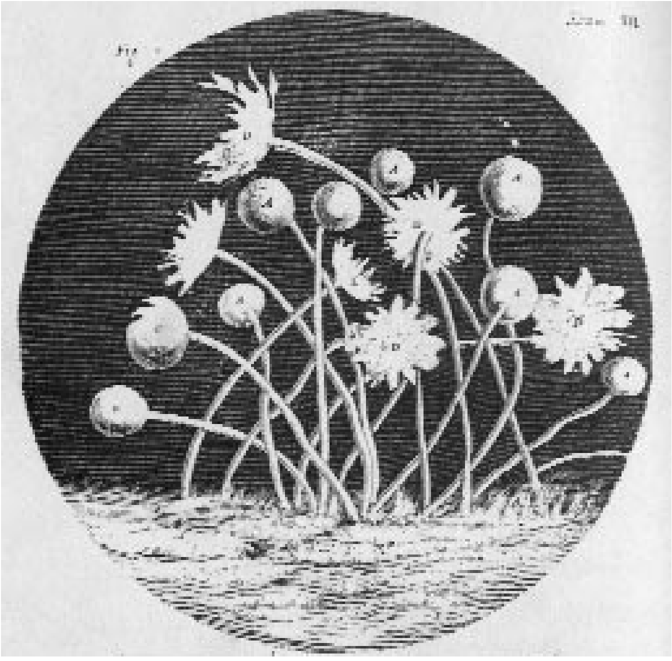


Map of America or "New India". Gerhard Mercator:, Atlas sive cosmographicae meditationes de fabrica mundi et fabricati figura (Duisberg, 1595)

It is difficult to capture the enormity of this revelation of pervasive and enduring error for those who had been educated largely in the old systems of thought – the sickening realization that so many respected authorities could have been so wrong for so long. Some of the most famous projects of the Enlightenment, such the *Encyclopédie* of Denis Diderot and Jean d'Alembert, germinated in this overwhelming awareness of having only recently emerged from over a millennium of collective intellectual error: one of the avowed aims of the *Encyclopédie* was to serve as a kind of time capsule to preserve the new discoveries, should war and pestilence plunge Europe once again into darkness.

The search for an explanation and thereby an antidote to future intellectual disasters centered on the problem of excessive belief. This was regarded as an emotional, ethical, and even medical, as well as an intellectual malady, and one with potentially devastating consequences. Much blood as well as ink had been spilt in early modern religious controversies, and throughout the late seventeenth and eighteenth centuries "enthusiasm" and "superstition" were reviled as sources of ecclesiastical and civil unrest. Excessive belief stemmed from psychological and corporeal causes, both of which had to be strictly managed in the susceptible: too great an appetite for the wondrous (asserted to afflict the vulgar and unlettered), a too soft and therefore impressionable brain (as allegedly found in women and children), or too much black bile (the temperament of melancholics) might all cause credulity. The fact that excessive belief was understood at least partly in medical terms by no means exonerated sufferers from the moral responsibility of restraint; spiritual and bodily regimens must be rigorously followed in order to rein in such dangerous inclinations. Among philosophers, the responsibility was intellectual as well as ethical, e.g. Descartes' instructions to take inventory of all one's stock of beliefs and discard those with the least blemish on uncertainty, or Locke's insistence that belief be apportioned to evidence. These religious, philosophical, and theological programs for disciplining belief not only raised the threshold of the credible; they also changed the nature of belief itself. Whereas belief had previously been conceived as an involuntary state and, in religious contexts, as a divine gift, by the late seventeenth century it had become a matter of voluntary assent – the "will to believe" – or to disbelieve – had become possible.

The shock of the seventeenth-century encounter with past error left a lasting mark on philosophy, and, in different ways, on science. Until the mid-seventeenth-century, intellectuals in Latin Europe had generally worried about incredulity rather than credulity, about believing too little rather than too much. The avalanche of novelties – from the discovery of the New World to the invention of the telescope and microscope – encountered by early modern Europeans had initially worked to reinforce the prejudice against incredulity; it was a mark of provincialism and little learning to doubt reports of armadillos, Chinese paper money, or microscopic animals in a drop of water. But by the early eighteenth century, the pendulum had swung to the opposite extreme – to the point that scientific academies refused to credit reports of meteor showers as smacking of the prodigious – and stayed there. The insistence that belief be "warranted" became and remains a philosophical dogma; according to the doctrine of warranted belief, the fact that a belief is true is by itself insufficient grounds for holding it without further explicit, reasoned justification. The emphasis upon warranted belief led to the spectacular rise of epistemology and the equally-spectacular decline of metaphysics since the late seventeenth century.



Mold viewed under the microscope.
Hooke, Robert: Micrographia,
or Some Physiological Descriptions of
Minute Bodies Made by Magnifying
Glasses with Observations and
Inquiries thereupon
(London, 1665), p. 124, pl. 12

Epistemology is the study of the justification of belief, the vigilant monitoring of the match between belief and evidence and the relentless rejection of beliefs that exceed their empirical and logical warrant – as Hume rejected the idea of necessary connection and Kant rejected any knowledge of the noumena. Epistemology ceases to be an exclusively philosophical worry and enters the practice of the sciences with the diagnosis of error: what kinds of error are most likely and most dangerous to the growth of scientific knowledge and what precautions must be taken in order to avoid them?

Epistemology and Its Discontents

All epistemology is born in fear: fear of the several sorts of errors that can corrupt, undermine, or impede knowledge. Epistemology is the diagnosis and therapy for intellectual error, reason's physick. Truth may be one, but the sources of error are many, and each kind requires its own remedy. Depending on which errors are most feared, epistemology takes on different forms; intrepid inquirers untroubled by such fears may dispense with almost all epistemology. Philosophy, including natural philosophy, has on some occasions been supported for centuries by only the most lightweight epistemological apparatus. Aristotle's treatises pertaining to animals, the soul, motion and change, and the phenomena above and below the orbit of the moon are rich in metaphysics (and empirical observations) but scant in epistemology; a famously optimistic passage in the *Posterior Analytics* (II.19) suggests that human cognition is happily so constituted as to be able to forge valid universals from the particulars of experience, "as in a battle when a rout occurs, if one man makes a stand another does and then another, until a position of strength is reached ... perception too instills the universal in this way." To extend Aristotle's military metaphor to cover his own example, ambitious research programs in natural knowledge need not don heavy epistemological armor to make headway.

Yet since the early seventeenth century, scientific inquiry has been inseparable from reflections on scientific error, some abstract and philosophical, others concrete and tightly meshed with specific scientific practices. The early modern revival of academic skepticism as well as the invention of new instruments such as the telescope and microscope certainly challenged insouciant Aristotelian accounts of how we know what we know. A general skeptical distrust of the senses was fortified by more specific doubts about chromatic distortion in refracting telescopes or the resolving power of microscope lenses. (Nor were such concerns restricted to the sciences of nature: during the same period, historians, philologists, and antiquarians developed critical methods for evaluating the authenticity and credibility of their sources.) But analyses of the sources of scientific error cut deeper and lasted far longer than these seventeenth-century episodes of uncertainty. Worries about the possibility and reliability of scientific knowledge not only inspired the philosophical tradition in epistemology from Descartes to Kant to Husserl and beyond; science itself also became infused with epistemology. Prophylactic deliberations about the errors most to be feared and how to counteract them, as well as about the limits of knowledge, became part and parcel of doing science. The fact that the philosophical and scientific traditions overlap to a striking degree in their lineages of key figures (Galileo, Descartes, Newton, Leibniz, but also Helmholtz, Poincaré, Planck, Einstein, Schrödinger, Bohr) is no accident. A preoccupation with error is a – perhaps *the* – hallmark of modern accounts of what knowledge is and can be.

This acute awareness of error was central to the original definition of what it meant to be modern in seventeenth-century Europe and thereafter. To take the modern side in the quarrel between the Ancients and the Moderns was to inventory the errors of antiquity as well as the inventions and discoveries of recent times. In addition to the cascade of novelties that deluged early modern Europeans – new inventions, new lands, new religions, new stars seen with new instruments, new flora and fauna, new philosophies –, the recognition of old errors heightened the self-conscious sense of living in a "new time" (*aetas nova*). One might without exaggeration speak of a collective and prolonged epistemological shock that still reverberates in philosophical and scientific attempts to determine the ideal relationship between knowledge and belief.

Hence epistemology since the seventeenth century has consisted largely in an elaborate nosology of errors: what their species and varieties are, and how they may best be avoided or cured. Which errors are singled out as the most dangerous vary considerably and consequentially, according to historical context, as do the recommended countermeasures. In all cases, error since the seventeenth century has been understood as a case of pathological belief, of credit extended recklessly or lazily or slavishly. The knowledge thereby attained eventually reveals itself to be an imposter, an illusion falsely taken for real. Thus on this scheme, the analysis of error is integral to demarcating genuine knowledge from mere belief and, among beliefs, the justified from the unjustified. Vigilance on this score is conceived to be moral as well as intellectual: the will as well as reason must be mobilized in order to grant assent only to those claims that, after thorough epistemological vetting, deserve to be credited. Just what kind of error is at stake has weighty implications for the ethos of belief; just as each of the various cardinal sins requires its own inner defense, so different kinds of scientific error call forth different precautions – and thereby redraw the boundaries between knowledge and belief.

From the seventeenth century on, at least three models of scientific error have been articulated, here presented in ideal type form: idolatry, seduction, and projection. Each model first emerges in distinctive historical circumstances – idolatry in the early seventeenth, seduction in the mid-eighteenth, and projection in the mid-nineteenth centuries – but they do not replace one another in a sequence. Rather, they accrete, slowly constituting a repertoire of epistemological diagnoses still more or less available to scientists, depending on the specificities of their discipline and circumstances. These models do not, however, always harmonize or even peacefully coexist with one another; conflicts can and do arise among the diverse ways of identifying and eradicating error. These conflicts have also riven our understanding of scientific knowledge and belief.

Idolatry

"When the people saw that Moses delayed to come down from the mountain, the people gathered themselves together to Aaron, and said to him, 'Up, make us gods, who shall go before us; as for this Moses, the man who brought us up out of the land of Egypt, we do not know what has become of him.' And Aaron said to them, 'Take off the rings of gold which are in the ears of your wives, your sons, and your daughters, and bring them to me.' So all the people took off the rings of gold which were in their ears, and brought them to Aaron. And he received the gold at their hand, and fashioned it with a graving tool, and made a molten calf; and they said, 'These are your gods, O Israel, who brought you up out of the land of Egypt!'"
– *Exodus* 32: 1 – 4.

“The idols and false notions which are now in possession of the human understanding, and have taken deep root therein, not only so beset men’s minds that truth can hardly find entrance, but even after entrance is obtained, they will again in the very instauration of the sciences meet and trouble us, unless men being forewarned of the danger fortify themselves as far as they may be against their assaults.”
– Francis Bacon, *Novum organum* (1620), I.xxxviii.

Bacon’s critique of the Idols of the Tribe, Cave, Marketplace, and Theater in his blueprint for a reformed natural philosophy, the *Novum organum* (1620), is one of the most celebrated accounts of scientific error ever written, one so evocative that it is still cited in the context of the latest scientific felony or misdemeanor. Yet it was framed in categories tailored to the circumstances of early seventeenth-century natural knowledge, and even to Bacon’s own. Raised by a Puritan mother who abhorred papist images and ceremonies as reverence paid to the golden calf, Bacon did not choose the metaphor of idolatry lightly. The “idols” are not synonyms for scientific error in general, but for a quite specific and particularly pernicious sort of error.

It is one thing not to know the true god, but quite another to worship false gods; to be ignorant of true knowledge about nature is a wholly different matter than to be in the grip of false theories. Scientific error conceived on the analogy of idolatry implies that it is better to be tabula rasa than to embrace falsehoods, better to have no beliefs than to have the wrong ones. To worship idols is not only to make a mistake; idols usurp the place of the truth and block its way. By providing a substitute for the real article, idols induce their worshipers to cease their quest for enlightenment, be it religious or scientific. Whatever yearning may have goaded the seekers into further inquiry is quelled by the counterfeit. Idolatry is the error that cuts off the means of correcting error. This is why Bacon (rather exceptionally among his anti-scholastic contemporaries) judged Aristotle, whom he condemned for bending experience to foregone conclusions, as “more guilty than his modern followers, the schoolmen, who have abandoned experience altogether.” Some of Bacon’s idols were fashioned by the mind itself; others were imposed from the outside. The Idols of the Tribe were those common to all human intellects (e. g. a tendency to find “more order and regularity in the world” than actually exists); the Idols of the Cave arose from “the peculiar constitution, of each individual; and also in education, habit, and accident.” These internal idols join forces with external ones, the Idols of the Marketplace (errors spawned by the mismatch between words and things) and Theater (philosophical systems, such as the Sophistical, Empirical, and Rational schools of the ancients). Again, some idols creep into the mind “secretly”, as in the case of the Idols of the Tribe and Cave, but also those of the Marketplace; others are explicitly taught, as are the “playbooks” of the Idols of the Theater. None of the distinctions that would matter crucially to later epistemologists – whether the errors stem from nature or from nurture, from sources internal or external to the mind, from unconscious tendencies or conscious tenets – counted as fundamental to Bacon. Each of the idols had its own specific remedy, and all “must be renounced and put away with a fixed and solemn determination, and the understanding thoroughly freed and cleansed; the entrance into the kingdom of man, founded on the sciences, being not much other than the entrance into the kingdom of heaven, whereinto none may enter except as a little child.”

Detail, Lucas van Leyden:
De dans om het gouden kalf
(ca. 1530), Rijksmuseum Amsterdam

The notion of an evacuation, a purgation, even a mortification of the mind is a recurring motif of seventeenth-century epistemology, even among authors of otherwise opposed viewpoints on the origins and obstacles to knowledge. From this perspective, Descartes’ radical doubt and Locke’s mental blank slate can be seen as kindred fantasies about an entirely fresh start for knowledge. Descartes explained in the *Discours de la méthode* (1637) that he had lived in a “world of books” since childhood, but discovered after completing his studies that he had been thereby “saddled with so many doubts and errors that I seemed to have gained nothing in trying to educate myself unless it was to discover more and more fully how ignorant I was.” Although he considered it prudent to follow accepted laws and customs rather than tearing them down to be later rebuilt, he nonetheless permitted himself to reject all the opinions he had been taught “completely for once in my lifetime, and to resume them afterwards, or perhaps accept better ones in their place, when I had determined how they fitted into a rational scheme.” Bacon was a sworn enemy of the *Acatalepsia* of the sceptics, and Descartes adopted none of Bacon’s Puritan talk of idolatry. But for both (and for many other seventeenth-century reformers of natural knowledge) the principal obstacle was not ignorance but false learning, and the first corrective “a well-purged mind”.

The metaphors of cleansing, purification, and purgation correspond to a more literal view of the body as supercharged with humors within and besieged by miasmas from without—the one necessitating bleeding and purging to stave off corruption; the other, protections such as ointments, pomanders, and as few baths as possible, lest the pores be opened to baleful effluvia. Bacon himself not only practiced and recommended regimens based on the regular purgation of the body and the therapeutics of smell (e. g. breathing freshly dug up clods of earth to concentrate the vital spirits); he further evolved an elaborate ontology of subtle effluvia responsible for everything from magnetism to the plague to the material powers of the imagination. The imagined self, both body and soul (with vital and animal spirits bridging the two), was permeable, dependent on a hydraulic economy of fluids, both subtle and coarse, to maintain health and equilibrium. Perception was conceived as a process of impression and transmission of sensations upon the animal spirits, as a seal imprints wax; the highly impressionable might even be susceptible to the emanations of another person’s imagination, at least at short distances. In such cases, permeability verged on vulnerability: Bacon warned that just as the stench of prisons had been known to make judges sicken and die, so might emanations from many envious eyes cause "persons in glory, triumph, and joy" to be "ill-disposed for some days following."

It is this image of corporeal and spiritual vulnerability, combined with the realization that received learning was riddled with errors, that informs Bacon's curious choice of verbs to describe the action of the idols on the mind, which they not only occupy but invade. Since it is ordinarily thought to be of the very nature of idols to lack the agency of genuine divinities, to be inert forgeries of the real thing, it is odd to envision them laying siege to their worshipers: did the golden calf "assault" the Israelites? But for Bacon and other seventeenth-century critics of the false learning of the schools, some of the most insidious idols were indeed imposed from without, through upbringing and education.

Writers of the most diverse philosophical leanings – Baconian, Cartesian, Spinozist, Lockean – in the seventeenth and eighteenth centuries indicted not only the content of traditional learning but also its authoritarian transmission, impressed upon the (literally) tender minds of the young. Custom, Locke contended in the *Essay Concerning Human Understanding* (1690), forged artificial associations between objects not naturally related, especially if instilled in early childhood, "the time most susceptible of lasting impressions". Just as children may be raised to loathe honey or to dread the dark, so they may also be indoctrinated with bizarre philosophical and religious beliefs that habit further entrenches. "That which thus captives their reasons, and leads men of sincerity blindfold from common sense, will, when examined, be found to be what we are speaking of: some independent ideas, of no alliance to one another, are, by education, custom, and the constant din of their party, so coupled in their minds, that they always appear there together; and they can no more separate them in their thoughts than if they were but one idea, and they operate as if they were so." This mechanism operated in science as well as in religion. The French mathematician and *philosophe* Condorcet observed that new ideas, however well-confirmed, often made very little headway among "even the best minds, accustomed to certain abstract ideas acquired in their youth". Hence a genius *[homme de génie]* who advanced bold new truths found a hearing only among "his peers, and a few young people raised far from the prejudices of the public schools".

Throughout the seventeenth and eighteenth centuries acquisition of false natural knowledge was regularly linked to the acquisition of false religion: both were instances of how false beliefs could be embraced at the expense of true, of substituting idols for the true god. Only the most drastic measures could uproot beliefs planted so early in young minds: Bacon's "true and legitimate humiliation of the human spirit," Descartes' radical doubt, Locke's relentless review of the evidence for and against each and every belief he held. The seventeenth-century movement for the reform of natural knowledge branched into divergent programs of research, which tracked different objects of inquiry by contrasting methods. Given these differences of means and ends, the consensus concerning which errors were most dangerous and why is all the more striking. Neither ignorance nor the labyrinthine complexity of nature nor the infirmity of the senses but rather idols – and idols in most cases pressed upon the knower in the form of custom, language, and school learning – were the chief enemy.

Seduction

"C'est une coquette, qui, uniquement occupée du désir de plaire, consulte plus son caprice que la raison. Toujours également complaisante, elle se prête à notre goût, à nos passions, à nos foiblesses; … L'imagination a sur-tout les agrémens en vue, mais elle n'est pas opposée à la vérité. Toutes ses fictions sont bonnes lorsqu'elles sont dans l'analogie de la nature, de nos connoissances ou de nos préjugés; mais dès qu'elle s'en écarte, elle n'enfante plus que des idées monstrueuses et extravagantes."
– Etienne Bonnot de Condillac, *Essai sur l'origine des connaissances humaines* (1746)

Idols, especially the Idols of the Theater, continued to exercise Enlightenment epistemologists, especially those worried about the pernicious effects of systems in natural philosophy. But by the middle decades of the eighteenth century, anxiety had shifted from errors imbibed in early youth and at school to those fabricated by the mind itself, more specifically by the faculty of the imagination. Imagination was the good-time girl of the mind (even in languages without grammatical gender, Enlightenment personifications of the imagination were invariably feminine), not necessarily corrupt in herself, but always willing to sacrifice principles to pleasure. The mind given over to the delights of the imagination was alarmingly sealed off, impervious to reason, indifferent to sensation, and incommunicado to society. So strong were the attractions of fantasy that the world it invented could, in extreme cases, supplant the real world of nature and society. For savants, the dangers of the imagination were threefold, moral, medical, and intellectual: social isolation and consequent neglect of familial and civic duties; nervous and digestive maladies brought on by long hours of solitary contemplation; and belief in glittering castles in the air, with no foundation in reality. This sketch concentrates on the last, but Enlightenment epistemological critiques of the imagination were tightly intertwined with moral admonitions and medical warnings

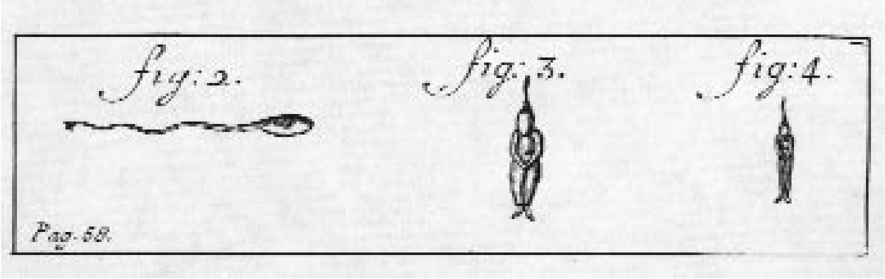
Emblem of Imagination.
Jean-Baptiste Boudard, Iconologie (Vienna, 1766), II, pl. 103

Idolatry and seduction both foment errors by substituting false beliefs for true, but do not operate by the same mechanisms. Idolatry means to worship the wrong gods in the wrong way, and in its earliest expressions in the Hebrew bible seems to have been associated, at least metaphorically, with betrayal and marital infidelity. However, the early modern associations of idolatry were more with benighted superstition than with lusting after the wife or goods of one's neighbors. And although the Israelites may well have enjoyed worshiping the golden calf more than the rather dour good of Moses, their motives seems to have been first and foremost pragmatic, to find a new protector: "Up, make us gods, who shall go before us; as for this Moses, the man who brought us up out of the land of Egypt, we do not know what has become of him." Idolatry (and superstition) were understood to feed upon fear – whether of supernatural caprice or human coercion. In contrast, seduction worked by the promise of pleasure. In the context of Enlightenment natural philosophy, tidy, symmetric, harmonious systems beckoned savants to desert the strait and narrow path of slow, stumble-blunder, piecemeal empiricism. Facts were notoriously stubborn and unaccommodating, but imaginary systems indulged every taste and temperament: the philosopher who is pleased by “causes interlocked to infinity” chooses a universe arranged according to the principles of “order and wisdom”; another, more melancholic and misanthropic, prefer “destiny, fatality, chance, necessity; there is his system.”

The self of the Enlightenment was at once a pastiche and a conglomerate: a pastiche of sensations and the traces they left in memory, combined by the principles of association and held together by the continuous thread of consciousness; a conglomerate of faculties, the chief of which were reason, memory, and imagination, which operated upon raw sensations to produce complex ideas. This was a self constantly menaced by fragmentation, so much so that some eighteenth-century philosophers, most notably David Hume in *Treatise on Human Nature* (1739), wondered whether the sense of having a coherent self might not be illusory, and reduced personal identity to “nothing but a bundle or collection of different perceptions, which succeed each other with an inconceivable rapidity, and are in a perpetual flux and movement.” On the one hand, gaps in memory or interruptions of consciousness could fission the self into separate selves. Locke and his eighteenth-century readers toyed with the idea that not only amnesia, but also drunkenness and even sleep might split the self. On the other, the inferior faculties, most particularly the imagination, might revolt against the rule of the superior faculty of reason, causing “alienation” of the self from itself and, in extreme cases, madness. It was axiomatic among eighteenth-century alienists that a deranged imagination lay at the root of the mental ailments of melancholy and hypochondria, both far more severe psychic and somatic disorders than the milder modern meanings of the words might suggest.

From the standpoint of scientific virtues and vice, the Enlightenment self was susceptible to several kinds of temptation. Insufficient experience, compounded by inattention, impatience, and inexactitude, could spoil observations. Just as moral responsibility for one’s past actions depended on remembering them, on connecting past and present selves, so scientific responsibility for one’s observations depended on recording and synthesizing them. A different sort of temptation waylaid the savant from within, replacing real impressions derived from memory and sensation with fanciful but alluring systems. Within the mind, reason might succumb to the blandishments of the imagination, that “coquette” who aimed primarily at pleasure rather than at truth. Vanity as well as beauty seduced natural philosophers into abandoning reality for the systems wrought by their own imaginations. Goethe warned against the temptation to connect isolated experiments into “theories and systems, that honor the perspicacity of their author”, but ultimately impede intellectual progress.

The healthy imagination was regarded as essential to an integrated self, for it was the faculty responsible for fusing sensations from the various sense organs into a unified sense impression, for presenting past sensations to memory and thereby assuring the continuity of consciousness, and for combining and recombining ideas to create novelty in the arts and sciences. Yet unless due precautions were taken, the imagination might usurp the prerogatives of reason and judgment, confusing genuine sensations with hallucinations and fabricating chimerical combinations. Because the self was conceived as a polity of faculties, a “bundle” rather than an organic whole, one or another faculty might rebel against monarchial reason, creating a kind of civil war in the mind and body. The imagination was considered to be particularly prone to insurrection, as much a force of mental disintegration as integration.

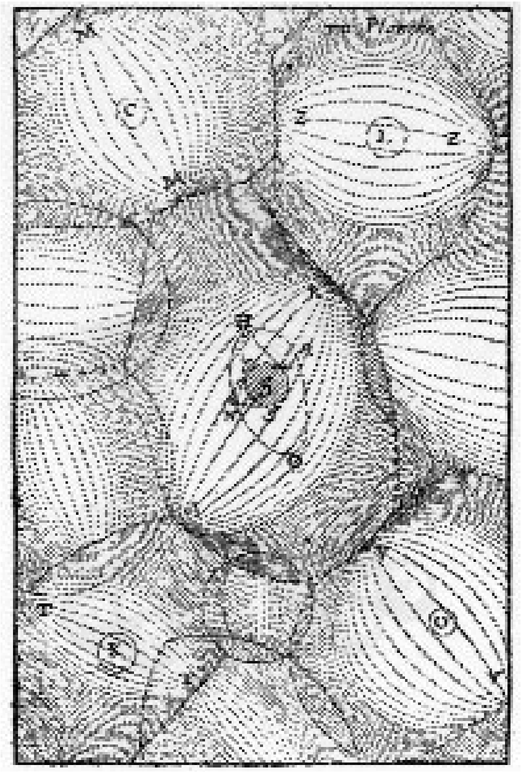


Homunculus in sperm, observed under microscope.
Leeuwenhoek, Antoni van, Epistolae ad Societatum Regiam Angliam, et alios illustres viros (Leiden, 1719), p. 68

Given this delicate balance between the healthy and pathological imagination, it is not surprising that Enlightenment (and later) accounts of the imagination abounded with distinctions between Dr. Jekyl and Mr. Hyde versions of this ambiguous faculty. Hume elevated the imagination to a position of unprecedented importance among the faculties, but nonetheless felt obliged to condemn the illusions of overwrought fancy: “Nothing is more dangerous to reason than flights of the imagination, and nothing has been the occasion of more mistakes among philosophers.” Voltaire drew a line between the *imagination passive*, which retains impressions of sensations in the mind, and the *imagination active*, which combines these impressions in myriad ways. Both were subject to pathologies. The passive imagination, common to humans and animals, spawned the superstitions of the vulgar and the deformations imprinted on the fetus by the pregnant mother’s agitated brain; the active imagination, cultivated only by superior minds, could degenerate into religious enthusiasm and artistic grotesquerie if not corrected by sound judgment.

Not only poets, but also savants were at risk from maladies of the imagination. The image of castles in the air, shimmering but insubstantial, recurs in scientific censures of deluded systematists. The naturalist Georges Cuvier excoriated his colleague Jean-Baptiste Lamarck for his transformationist theory, one of those "vast edifices [constructed] upon imaginary foundations, resembling those enchanted palaces of our old novels that can be made to vanish by breaking the talisman upon which their existence depends." The antidote to an overweening imagination in science was to cultivate habits of patient and exact observation, thereby reconnecting the mind to the world, and to strengthen the rule of reason over the other mental faculties, thereby restoring hierarchical order within the mind. The seductive pleasures of the imagination were admittedly of a rather special kind: the completeness, coherence, and certainty of systems, as opposed to the fragments, contradictions, and surprises of observation and experiment. No Enlightenment savant ever seems to have been tempted by an imaginary fact, only by the rounded wholes of systems.

In addition to the glory of devising a Grand Theory of Everything, imaginary systems offered a refuge from the hard work of empiricism. Late eighteenth-century descriptions of scientific observation emphasized its arduous, painstaking, and even risky character—it was not unknown for virtuoso observers, like the naturalists Jan Swammerdam and Charles Bonnet, to go blind. Retreat into the crenellated castles of the imagination was also a retreat from the seemingly endless and futile labor of collecting facts, which often added up to no conclusion or even contradicted one anot-



Vortices from the Cartesian system of the world. Descartes, René, Principes de la philosophie (Paris, 1647)

her. To succumb to the seductions of the imagination was the intellectual equivalent of quietism, a withdrawal into the tranquil solitude of the mind, where one was neither distracted by company nor frustrated by facts. Savants like Descartes who surrendered to the error of seduction had forged (in every sense of the word) a counter-world; they created not false gods but a false creation.

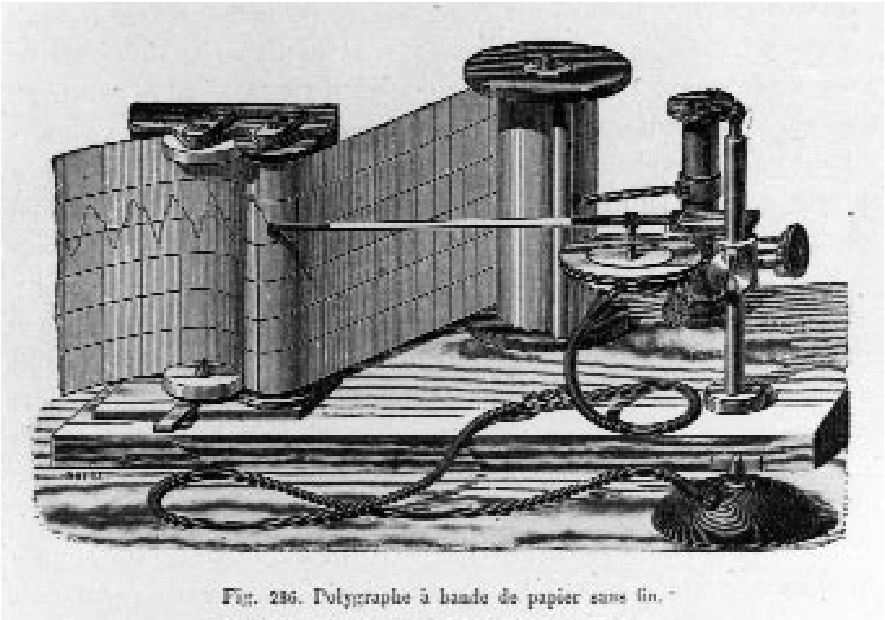
Projection

"Oui, sans doute, l'expérimenteur doit forcer la nature à se dévoiler, en l'attaquant et en lui posant des questions dans tous les sens; mais il ne doit jamais répondre pour elle ni écouter incomplètement ses réponses en ne prenant dans l'expérience que la partie des résultats qui favorisent ou confirme l'hypothèse."
– Claude Bernard, *Introduction à l'étude de la médicine expérimentale* (1865)

"Und sollte nicht selbst bei der höchsten Ausdeutung des Wortes Objektivität eine Illusion mit unterlaufen?...Oder sollten sich in jenen Momenten die Dinge gleichsam durch ihre eigene Tätigkeit auf einem reinen Passivum abzeichnen, abkonterfeien, abphotographieren?"
– Friedrich Nietzsche, *Vom Nutzen und Nachteil der Historie für das Leben* (1874)

An object throws itself against us; a projection throws us against the object. Put less etymologically, projection casts some aspect of the subjective self – its hopes, fears, preconceptions, conjectures – onto the objective world. In its most extreme form, projection becomes the curse of Narcissus, doomed to see nothing but his own reflection and to mistake it for another person. As a category of error, projection began to frighten scientists in the middle decades of the nineteenth century. Its putative victims were not acolytes of false learning or dwellers in castles of the imagination, but laboratory and field researchers who conducted investigations on an unprecedented scale, mimicking the machinery and division of labor of factories. No one ever accused French physiologist Claude Bernard, practitioner and publicist of dramatic vivisections on animals, of shirking empiricism or kowtowing to conventional wisdom. In his *Introduction à l'étude de la médicine expérimentale* (1865), his metaphors of experimental inquiry were audacious, invasive, even violent. Yet after having "unveiled" and "attacked" nature with his probing questions, he fretted over the possibility that he might put words in her mouth, when he ought to have been silently taking dictation. The active experimenter must abruptly become the passive listener, ideally the receptive, objective photographic plate of a scientist mocked by Nietzsche. So alarmed was Bernard by the risk of projection, that he split the persona of the scientist in two: an experimenter who plans research in light of theories and hypotheses to test; and an observer (preferably an uneducated assistant or a self-registering instrument) who registers the results with "passive senses". This is a division of labor that would have deeply shocked Enlightenment savants, who regarded observation as the supreme act of the informed, active, scientific intelligence.

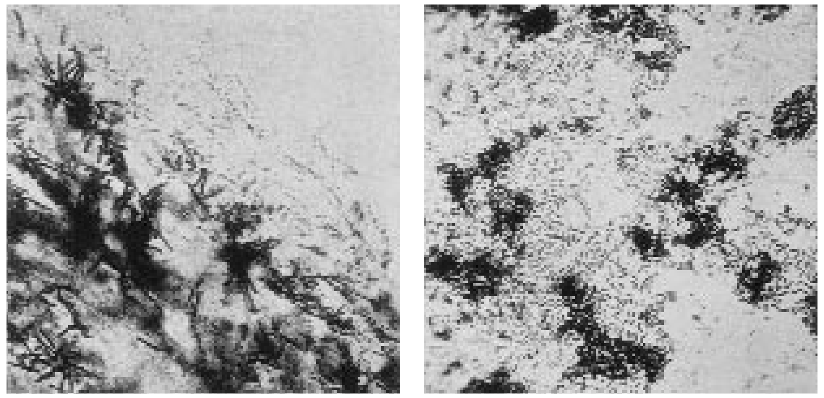
Although Bernard admitted that the splitting of the scientist into active experimenter and passive observer was usually impracticable, the notion that the scientist must divide the self in order to control the urge to project preconceived ideas onto nature was echoed by many of his contemporaries. Only a heroic act of self-discipline and self-denial can rein in these projections. The British physicist Michael Faraday gave voice to this ethos of iron will and clenched teeth: "The world little knows how many of the thoughts and theories which have passed through the mind of a scientific investigator have been crushed in silence and secrecy by his own severe criticism and adverse examination." In words deliberately resonant of Christian asceticism, nineteenth-century French modernist Ernest Renan praised the "oeuvre pénible, humble,



Self-registering polygraph. Marey, Etienne-Jules, La Méthode graphique dans les sciences expérimentales (Paris, 1878), p. 457

laborieuse" of writing narrow scientific monographs, and the "vertu scientifique profonde" required "pour s'arrêter sur cette pente fatale et s'interdire la précipitation, quand la nature humaine toute entière réclame la solution définitive."

Techniques such as photography and self-registering instruments were touted as further, mechanical precautions against projection of pet hypotheses on the data. Even if a blurred, black-and-white photograph was less accurate than a carefully executed color drawing, at least it was free of all traces of subjectivity. The German bacteriologist Robert Koch admitted in an 1884 article that photographs of microorganisms had several scientific disadvantages, including less vivid stains and an enforced two-dimensional cross-section of the object (which could produce visual artifacts). But he insisted that the price must be paid, in order to eliminate "zahlreiche subjectiv gefärbte Anschauungen und infolgedessen mehr Meinungsverschiedenheiten ..."



Photographs of bacilli in the blood of a dead mouse.
Robert Koch, "Zur Untersuchung von pathogenen Organismen," Mittheilungen aus dem Kaiserlichen Gesundheitsamte, 1(1881): 1–48, pl. 7, fig. 41, 42

Wishful thinking, anthropomorphism, the pathetic fallacy, and other ways of humanizing the non-human world are no doubt as old as time. What is historically specific about the scientific error of projection was first, the timing – of all the possible errors a scientist might commit, why thrust this ancient human foible onto center stage in the mid-nineteenth century? – and second, the mechanisms – why choose the metaphor of the projection of an image onto a blank screen to describe the workings of this sort of intellectual error? The answers to both questions are bound up with a new vision, first articulated by Kant and subsequently reworked by a generation of philosophers in light of early nineteenth century political, economic, and intellectual transformations, of self and the world divided along the lines of the objective and subjective. Whereas the Enlightenment self was constructed by reason out of the raw materials of sensation and memory impressions, the subjective self was made out of will and representations. The will was one faculty among several in Enlightenment theories of the impressionable self, and of minor cognitive consequence: it was charged with controlling the passions, but not sensation and imagination, which were properly subordinated to reason and judgment. In contrast, the will dominated all aspects of the subjective self, including those related to the acquisition and evaluation of knowledge. Scientific objectivity was a response to the subjective self, conceived as constituted by and tightly organized around an autonomous will. It was a unity, not a bundle of more or less coordinated faculties. The excesses of this kind of self were those of the unbridled will, which assumed an epistemological dimension. The unbridled will could impose itself upon nature, distorting, fabricating, and perfecting the facts.

The peculiar form this imposition took was projection, a procedure well known since the sixteenth century in the context of the camera obscura and magic lantern. In the context of the permeable and impressionable selves of seventeenth and eighteenth-century psychology, it would have made little sense to talk about the mind's projections onto the world; the arrows of action pointed in the other direction, from the world onto the mind. On this account, the only way for the mind to shelter itself from the world was a pathological withdrawal into the interior chambers of the imagination; the healthy mind was open to and therefore molded by experience as conveyed by the "impressions" of sense and memory. Only in the context of an overweening will that reached outward to impose itself upon the world did the metaphor of projection on a receptive screen make sense, reversing the tabula rasa metaphor of sensationalist psychology.

However dynamic and outreaching the will-centered self was conceived to be, it is still surprising that its scientific manifestation was thought to be so powerful as to overwhelm objective reality. Why would scientists convinced that an ugly fact could murder a beautiful theory (as British zoologist Thomas Henry Huxley put it) nonetheless take heroic precautions to protect those burly facts from flimsy subjectivity? Why must the will be enlisted to contain the will by means of resolute acts of self-restraint of the sort praised by Faraday and Renan? It seems paradoxical that the more nineteenth-century scientists insisted on the obduracy of hard facts, the more they feared the power of subjectivity to melt those facts.

The key to the paradox lies in another fear that began to haunt scientists in the mid-nineteenth century: the fear of vertiginous, open-ended progress. In the eighteenth century, the sciences had seemed destined for smooth, steady, expansive progress; between 1750 and 1840, histories of science documented the existence and extent of progress in various disciplines. But the progress envisioned in these histories was of change without transformation. Once the foundations of the new science had been laid in the seventeenth century, so went the story, the edifice could be expanded but need never be remodeled. Starting in the 1830s, this cumulative view of progress received a rude shock, as venerable scientific theories (e.g. Newton's corpuscular theory of light) were summarily dethroned. Was scientific progress so inexorable, so durable after all? The response of the scientists was to take refuge in a description of facts, in order to salvage a stable core of knowledge from the ever-accelerating succession of theories. Both the first and second waves of positivism in the 1830s and 1880s, respectively, were explicit about this motivation. Never before had science bustled and flourished as it did the latter half of nineteenth century, but science not only grew; it also changed, and at a breakneck pace that reduced the lifetime of theories from centuries to mere months. Only facts seemed to hold out the hope of permanence in science; hence the fervor of scientists to sever the objective from the subjective, and to safeguard data from projections.

Conclusion: The Ethos of Belief

Idolatry, seduction, and projection are all errors of substitution: false beliefs usurp the place of true knowledge. Although it is possible to entertain true as well as false beliefs, the category of belief as a whole is in contradistinction, if not opposition, to that of knowledge. It is possible that some beliefs may graduate to the status of knowledge, having withstood rigorous evidentiary tests. But epistemology since the seventeenth century has been overwhelmingly concerned with false beliefs – idols of fallacious learning, seductions of imaginary systems, projections of subjective expectations – that compete with knowledge, just as heresies once vied with the one true faith. And just as heretics were deemed to be damnably complicit in their errors, so inquirers who surrender to cognitive errors have been judged morally responsible. They are culprits in as well as victims of deception.

The moralization of belief is surprisingly constant across all three models of error. Even the medicalized accounts of the seventeenth and eighteenth centuries, which attributed the excesses of religious and philosophical enthusiasm to a melancholic temperament caused by an excess of black bile, or susceptibility to the wiles of the imagination to soft brain fibers and irritable nerves, did not thereby exonerate sufferers. Seventeenth-century moralists tartly admonished melancholics to watch their diets and be bled regularly; their eighteenth-century successors advised savants suffering from maladies of the imagination to take regular exercise and go out in society. These were the counsels of an Aristotelian ethics of habit and regimen, rather of a Kantian ethics of will, but they were none the less moralized for that. Once epistemology was construed as a matter of the will reining in the will, as in the case of nineteenth-century scientific exhortations to suppress subjectivity, the high moral tone becomes more audible to modern ears. But for all three models, there is no such thing as a purely innocent error. It is a matter of rectitude as well as prudence to withhold credence from suspect propositions.

This state of withheld or suspended belief is known as skepticism, and it comes in varying strengths, from mild demur to radical doubt. It is so reflexive an intellectual stance for moderns that some effort is required to appreciate its strangeness. Perform the following analogical thought experiment: imagine a person who, on principle, withheld trust from others until their reliability had been proven, rather than the other way around. The local shopkeepers would be assumed to be swindlers, friends warily eyed for the slightest signs of disloyalty, family members suspected of calculating legacies and life insurance premiums. We have met such people in plays by Molière; they are called misanthropes and are to be chastised or pitied, but not admired. Yet they are the moral equivalent of skeptics, who refuse to trust – their senses, received wisdom, testimony, scientific hypotheses – until shown the evidence, bushels of it. Even if the prototype of the skeptic is taken to be not Descartes, with his unsettling fantasies about malevolent demons, but rather the debonair and self-ironic Hume, there is more than a whiff of paranoia in the mental exercises of suspending belief about everything, including whether the sun will rise tomorrow. As philosopher Stanley Cavell remarks in his study *In Quest of the Ordinary* (1988) apropos of romanticism as a promise of self-recovery: “But in all philosophical seriousness, a recovery from what? Philosophy cannot say sin. Let us speak of a recovery from skepticism. This is means, as said, from a drive to the inhuman.”

But if there is something inhuman about the skeptical withholding of belief, lest an error be committed, it is an inhumanity bred of fear. The fear is historical, memorialized in the very way European history is divided up: Antiquity, Middle Ages, Renaissance, Early Modern, Modern, terms freighted with a telos that is awaited, anticipated, and finally achieved. The first expression of modernity was to recognize that all that preceded it had been a huge mistake; to realize the magnitude of the mistake was to fear ever erring again and to vow vigilance, constant and even inhuman. Skepticism is the continuing after-shock of that earthquake. Yet fear alone cannot explain the profoundly moralized character of withheld belief: fear can breed extreme caution and circumspection, but not a sense of duty. Why is it a duty, not just a maxim, to withhold belief? The template of all belief is religious, and the close analogies between how epistemology analyzes error and theology analyzes heresy reveal that this template has not been wholly discarded. In religion, belief granted lightly can indeed be culpable. But belief withheld can be still more so. Moreover, religious faith, as an internal state, differs crucially from intellectual belief as William James pointed out in *The Varieties of Religious Experience* (1902): “The faith-state may hold a very minimum of intellectual content … It may be a mere vague enthusiasm, half spiritual, half vital, a courage, and a feeling that great and wondrous things are in the air.” Hence it would be overhasty to conclude that the moralized character of scientific belief is just so much displaced religion.

The ethos of belief preached by epistemology may occasionally borrow the vocabulary and timbre of religion, but it springs from fundamentally different impulses. To grant belief to claims, theories, and propositions does not resemble a state of religious conviction, even though both may command the full investment of the self. The one seeks at all costs to avoid credulity, the other incredulity, but even this opposition does not fully capture the distinction. Epistemological belief – and still more principled disbelief – is willed and cultivated; on this account, assent is freely granted by an autonomous cognitive agent who bears responsibility for this decision. In contrast, religious faith is a gift, freely endowed but not willed. Even Pascal’s advice to “allez en avant, la foi vous viendra” assumed that faith would follow from voluntary observance, not that observance alone would suffice. It is quite conceivable that the modus of epistemological belief might be turned to the ends of religious faith, as in the case of rational sects like deism, but then religion derives its moral aura from epistemology, not vice versa. The moral aura that surrounds epistemological belief is itself grounded on an ur-belief: that it is both possible and desirable to believe only what one wills to believe, and that the will to believe can be compelled by reason. The only remaining givens, the only gifts received and not chosen, are the data themselves – the elusive data masked by idols, seductions, and projections.

Intersections

Some Thoughts on Instruments and Objects in the Experimental Context of the Life Sciences

Hans-Jörg Rheinberger

This Essay is an English version of a paper to appear in Instrumente in Kunst und Wissenschaft. Helmar Schramm, Ludger Schwarte and Jan Lazardzig (eds.). In preparation. I thank Sven Dierig, Peter Geimer, Henning Schmidgen, and Laura Otis for a discussion of the text and for helpful and valuable suggestions.

Since the seventeenth century, scientific instruments have been seen as the emblems and signposts of experimental science. Over and over again, they have been described and hailed as ideally transparent media that either help to prolong, reinforce, and super-elevate the senses or to isolate, purify, and quantify experimental perceptions. Historical literature on instruments has been proliferating over the past decades; it shall not be reviewed here in detail. Importantly, historians of science have thoroughly challenged and questioned the assumption of instrumental transparency. In particular, many case studies have shown that as a rule, instruments do not work by themselves and do not generate evidence by themselves. Rather, they are embedded in historical and local contexts of skill and application, without which their production and their efficacy cannot be understood; outside of which their functioning cannot be granted; and which also determine the range of their circulation.

1. Epistemological Preliminaries

I would like to use this essay in order to raise the basic question of the relation between instrument and experimental object. What configurations can it assume? Where does the experiment take place? Is it at the instrument, with, before, within the instrument? How, accordingly, can the particular epistemic value of an instrument be determined? In what follows, I will not present another case study and not reconstruct another local historical context. Rather, I would like to discuss a confined but fundamental epistemological problem. I would like to show that the use of instruments in the biological sciences brings with it the necessity to create very differently shaped, generally opaque *intersections* between the possible objects of inquiry and the instruments that become involved in the investigation. Such intersections mark the contact surface between the apparatus and the object. In examining a series of instruments that came to be employed in the life sciences of the nineteenth and twentieth century in particular, I will show how these points and planes of intersection between the living and the non-living, between the organism and the technical apparatus were configured. The investigative value of an instrument depends on the shape of such intersections; they decide about whether a particular instrument and a particular object can be brought together at all and bound into a fruitful analytical constellation. Consequently, such constellations have been the *locus* of particular artisanship and attention. The intersections are the places where the hand of the instrument maker shades and grades into the hand of the experimenter, and even in the age of the industrial production of research technologies, they remain the *loci* of handicraft. In the context of the development of new research technologies, the exploration of interceptions between object and instrument is often at least as important as the technical scientific implementation of the principle embodied by the instrument, although both need not necessarily have an intrinsic, theoretically motivated relation with each other. Work on the intersections may even at times develop into a separate industry, as was the case, for instance, with electron microscopic specimen preparation. Unfortunately, the work at the intersections between the instrument and the object of investigation has not always received the historiographical attention it deserves. It is this particular, rather narrowly circumscribed question that I want to address here. It stands, however, in the context of a wider epistemological problematique which I therefore want briefly to explicate first.

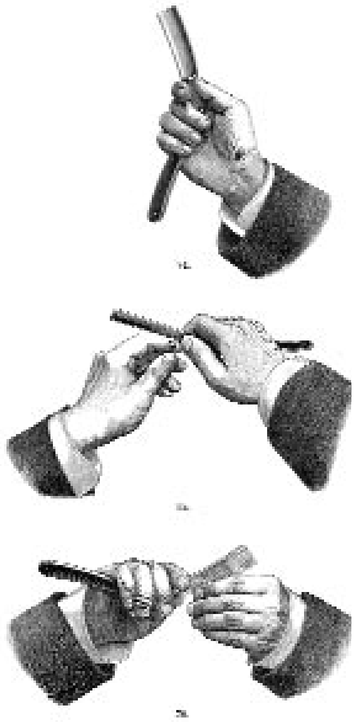
This wider question concerns the relation between epistemic objects and the technical conditions of their manipulation in the framework of experimental systems. In describing experimental systems I have, on several occasions, pointed to the fact that the productivity of such systems essentially depends on a well-balanced dialectics between epistemic and technical things. The technical things bound and confine experimental systems. They constitute a more or less rigid frame of conditions, and at the same time, they determine a scope of action in which an epistemic object can unfold. My general claim is that instruments receive their epistemic meaning only in relation with and framing through experimental systems. Taken by themselves, they are epistemically indeterminat. Although they can be viewed as embodiments of certain theories or concepts, that is to say, with Gaston Bachelard, as “reified theorems,” they are not knowledge generating instruments by themselves. As a rule, instruments enter as technical things into an experimental arrangement, as the identity conditions of an experiment, but they can also turn into epistemic things, if in the course of their use they generate unexpected questions. In the development of research technologies we often observe that the crafting of an instrument goes hand in hand, and is inextricably linked with, the process in which an epistemic object takes shape. The intersections between the instrument and the object thereby form a particular problem zone of the articulation between epistemic and technical things.

As far as our narrower question is concerned, we have to consider the fact that in biological experimentation, the intersection between the object of investigation and the technique of its representation or measurement is always also, very concretely, a boundary between an organic body and an inorganic entity. It is a place where life and technique confront each other, and since, as a rule, the living part is wet and the technical part is dry, their encounter requires particular precautions. The success of a biological experiment depends on mastering this transition, that is, on shaping the joints intended to make compatible the wet and the dry, the soft and the hard, the fluid and the solid. In the long run, the productivity and the sustainability of biological experimental systems is determined by the handling of this boundary.

Such boundaries, such intersections shall be characterized in this essay. I would like to demonstrate their multiform shapes by describing a few examples more precisely, and thereby touch upon a few epistemological questions of a more basic nature, all of which are concerned with instruments and experimentation. They all refer to the particular materiality that characterizes epistemic objects in the life sciences. Therefore it is not the architecture of the great boundaries, such as the articulation between the sciences and the arts, the demarcation lines between disciplines, or the relation between the sciences and other formations of culture that is in the foreground of attention. What is at stake is rather the small boundaries: those soft lines of demarcation and partition between what is to be taken as biological nature and what is to be rejected as artifact. As soon as scientists were ready to engage in the endeavor of turning outward the inward constitution of organisms, they were confronted with the problem to confine it accordingly. The sciences of life are haunted and persecuted by the question of the legitimacy of reconfiguring these boundaries since the time they took up the adventure of exploring organic nature in its inner structure, that is, of putting hands on its materiality. We will see that the drama of drawing these small boundaries is not only pervading all of the sciences of life, but also decisive for an understanding of the big divides between disciplines and scientific cultures.

2. The Microscope

I will start with microscopy. With the emergence and diffusion of the microscope since the second part of the seventeenth century, the question of the object of observation poses itself in a new manner in natural history. A new form of specimens of small dimensions emerges that corresponds to the new technology of magnification. Microscopy is a good example of the general need to correlate forms of observation tied to new instruments with the state into which things have to be brought in order to become visualized by these means. On the one hand, the things prepared and set in order for the lens can themselves not be seen in the process of preparation, at the very least not in those details on which the future success of the observation depends. Their preparation escapes the capturing eye; it is only the gaze through the microscope after the fact that will decide whether the preparation was successful. This forces the preparator to direct the attention on the regulation of the process of preparation. Since the object of inquiry remains withdrawn from the immediate control by the eye, the procedure must, in one way or the other, function blindly. It is therefore not by chance that the scientific literature pays ever more attention to the techniques of preparation and describes them with great circumstantiality. It is in the nature of fresh specimens that they must be newly supplied for each observation. But how to ensure the orderly repetition of the procedure? In view of this question it does not come as a surprise if, to take just one example, Matthias Jacob Schleiden, in his debate on the fertilization of plants with Franz Ferdinand Meyen, describes his proceedings in painstaking detail. In the second part of his *Botany as Inductive Science* (1846), we read: “Here I would like to add a few words on the manufacture – ‘Darstellung’ he says – of such preparations. If the buds of the seeds do not lie very tightly enclosed and immobile in the ovary, I prepare them free, then take them in such a way between



Production of botanical cuts with a straight-razor.
Behrens, W. J.: Hilfsbuch zur Ausführung mikroskopischer Untersuchungen im Botanischen Laboratorium (Braunschweig, 1883), pp. 150–152, figs. 74–76.

forefinger and thumb that I can sever them with a sharp razor blade exactly in two halves. […] The two halves thus won I lay, one after another, with their cut surface pointing toward the thumb, again between the two named fingers, and then I cut off with the razor blade from the sectional plane a section as tender as it is possible. – Then I bring these two disks under the simple microscope and, with the help of fine needles and knifelets, lay bare the respective parts, if they are not, this case always being the best, already exposed through the cut itself" (pp. 370–371).

In the "exposure through the cut itself," in the exertion of a minimum of additional manipulation, the freshly prepared, wet botanical specimen finds its master. But soon attempts to render it durable follow, for, in the last instance, only the dry preparation grants the permanence of the viewed object and with it, the possibility of rehearsal and comparison with other preparations. With that, differential reproduction of specimens becomes possible. But since making them durable requires additional interventions, the question of what is nature and what artifact in view of the preparation acquires the status of special epistemological urgency. For here, in contrast to macroscopical observation, visual control against the 'living' counterpart is impossible. Microscopic preparations are therefore epistemically highly laden things of knowledge. It is only consequent that the methodical critique of the knowledge practices of the life sciences in the latter part of the nineteenth century life sciences has crystalized to a considerable part around these preparations.

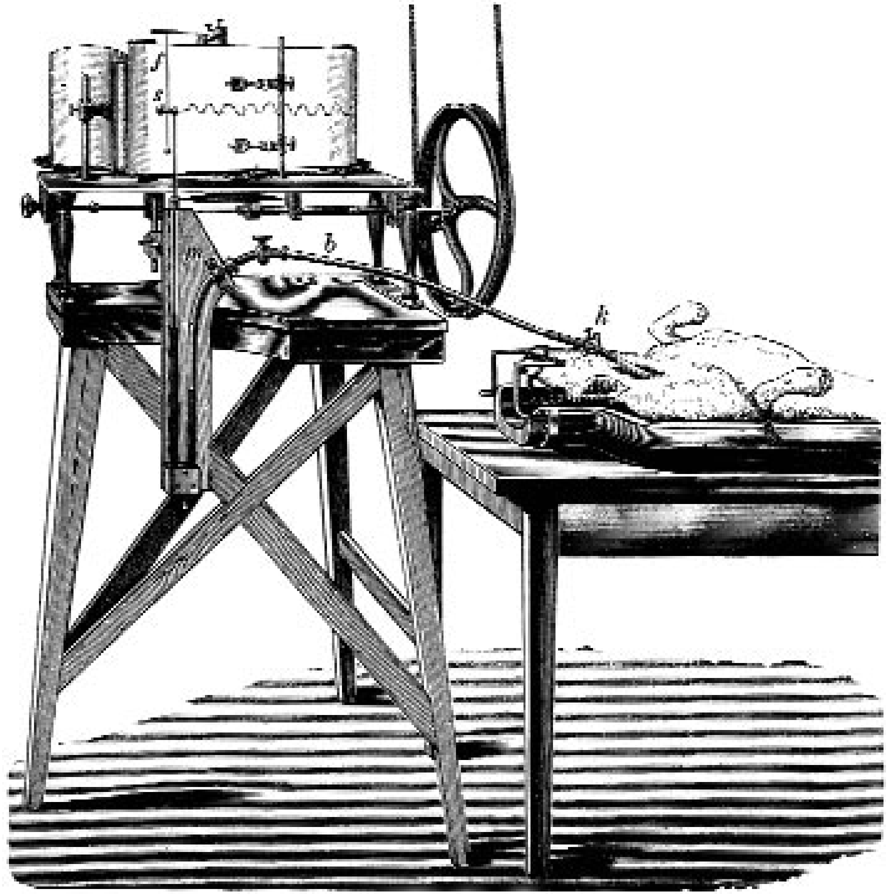
In addition, it is a defining feature of microscopic preparations that they reduce the objects fixed in them to two dimensions. They flatten them out. This is a necessity grounded in the functioning of the apparatus whose focus is precisely on a plane. The microscope with its imaging capacity does not only remain on the surface, it remains bound to a flat plane, and the flattened object is realized in the "cut." In tight connection with the establishment of the cell theory, the tissue section becomes an emblem of animal and plant microscopic morphology in the nineteenth century. The zoologists, botanists, anatomists, physiologists, and microbiologists of the second half of the nineteenth century have revolutionized the craft of the microscopic cut by mobilizing the newly acquired powers of inorganic and organic chemistry, of acids and of dyes. Various procedures of fixing, dying, and hardening have rendered the preparations lasting, made new contours visible, soft things cuttable. These procedures do not only mark the severing point between the organic body and the optical apparatus, they also form and shape it. This breaking line is the precarious point where the epistemic object and the instrument become intertwined. Around it, the experimental systems of microscopy accrete. It is here that the organic and the technical engage in mutual action. Here, at the very point of magnification, the decision is made about the way in which the specimen will enter into the picture.

Without the procedures of trimming specimens, it would not have been possible to exhaust the potentials of the new lens technologies developed in the course of the nineteenth century. With them, the *locus* of magnification has been shifted from the wet side to the dry side. The fresh cut of the razor blade has been subjected to a changing bath of chemical reactions. The microscopic things themselves, treated with acids, dyes, and crosslinkers, embedded and welded between object carrier and covering glass, have started to fill the cases and boxes of microscopic archives with a new form of epistemic objects rendered durable. What was especially difficult to cut, what could not be taken between thumb and forefinger, became embedded into resins. Microtomes were developed that were able to cut slices of a hitherto unknown thinness. Using this whole arsenal of new techniques, the microscopists transformed the work of preparation into a space in whose coordinates they came to play out a continued dialectics of fact and artifact – to pick up again an expression of Bachelard in this context. The clearer and sharper they tried to make something visible, the more they brought it near to that boundary at which one can no longer decide what one has conserved: the object or the means of its objectification. In the borderline case and as Peter Geimer has argued on the example of scientific photography, the preparation comes to represent the preparation technology itself. All representation in research revolves around such cusps. Instead of problematizing them as traps of knowledge, we should see and understand them in their positivity: as driving forces, as engines that maintain an epistemic dynamics which brings the object of interest into a form that can, if necessary, be left behind and surpassed. It is in the nature of epistemic objects in general that they can become outstripped. They are obtained in a recursive fashion, and they remain relevant for research just as long as the work of deconstruction can go on with and around them.

It is therefore not by chance that microscopy occupies a decisive place in a developing epistemology of error, as Jutta Schickore has shown in her investigations on the discourse of error in the microscopic life sciences of the nineteenth century. Around the work of microscopy the methodical consciousness of a science crystallizes that constantly moves along the boundary between the visible and the invisible – which is at the same time a boundary between the living and the dead – and that, in order to displace this boundary, has to subject its potential objects to ever new interventions.

2. Physiological Apparatuses

Apparative physiology of the nineteenth century exhibits another picture of the intersection between the organic body and the technical gadget. There exist plenty of investigations on the experimentalization of nineteenth century physiology that cannot be considered here in detail. What is alone of interest in the present context is the configuration of that point at which the organism – or parts of it – and the instrument come in touch with each other. We can take the kymographion of Carl Ludwig as an example. In the context of this registration device the point of intersection takes on the genuine form of a lesion, if not mutilation. The apparatus developed by the Leipzig physiologist Ludwig allowed the measurement of blood pressure on a living animal. In the process, a "communicator" short-circuited the open wound



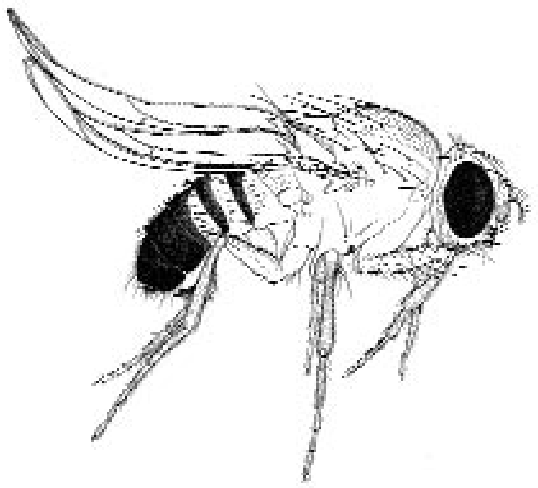
Blood pressure experiment with a kymographion. Langendorff, O.: Physiologische Graphik: Ein Leitfaden der in der Physiologie gebräuchlichen Registriermethoden (Leipzig, Wien, 1891), p. 206, fig. 169.

of the animal with the curve registration part of the machine. In his work on nineteenth century laboratory physiology, Sven Dierig has argued extensively that the success of the instrument, that is, of obtaining reliable blood pressure curves, depended critically on the form and the properties of this interstitial piece. All possible forms and material means of connection were tried out in collaborations between physiologists and instrument makers, until finally the mercury pressure gauge succeeded. Here, the liquid metal took up the pressure of the arterial blood through a glass cannula inserted into the artery; at the other end of the U-turn, the ups and downs of the meniscus of the metal were transmitted to a pencil that transformed them into inscriptions on paper mounted on a rotating cylinder. The medium that transformed the organic movement into a technical movement had to be in resonance with the investigated manifestation of life. In this case, the conducting medium was a fluid that reacted on pressure. In experiments with nerves and muscles, however, electrical circuits formed the mediating connections. In Etienne-Jules Marey's apparatuses for measuring the gate, investigated in detail by Andreas Mayer, it was a compressible gum bladder that formed the contact sur-face between the foot and the soil. In such experiments of a technologically highly equipped physiology characteristic of the second half of the nineteenth century, the organism becomes an element in a technical construction in which everything depends on the seamlessness of the joints. Such frameworks not only serve to measure certain manifestations of the life of an organism; they also sustain these manifestations at the limit. Peter Geimer has pointed to the paradox that with these experimental hybrids, these cyborgs of the nineteenth century, the question of life or non-life, the question of where nature ceases and technology begins, is no longer answerable in an unequivocal manner. For, at least in extreme cases, the organism whose life manifestations are analyzed and measured is no longer able to live outside the apparatus. It lives exactly as long as the machinery turns. During the nineteenth century, the big organic circuits such as respiration, blood circulation and nervous conduction were transformed into objects of analysis in such a way that the organism was transformed into an organic element, even an organic switch at times, in a technically determined circuit. According to each particular organic function, the junction was made by a corresponding mechanical equivalent of that function.

3. Model Organisms

At this point, I would like to move to the other extreme and talk briefly about a different, counter-intuitive 'instrument' that has become characteristic for the life sciences of the twentieth century. It is the organism itself as a model. As Robert Kohler has forcefully argued for the pet of classical genetics, the fruitfly *Drosophila melanogaster*, model organisms function not only as exemplars, but also as 'instruments' of research. What does that mean epistemically, however, and is this manner of speaking more than a metaphor? In order to be able to answer this question, it is useful to come back once more to the concept of the experimental system. As an instrument, the model organism belongs to the technical conditions under which an epistemic object assumes its contours. Staying with the example of classical genetics, we can state that in the context of working out gene maps, *Drosophila* mutants do function less as epistemic objects than as tools with which genes – the epistemic entities of concern – can be localized and their places fixed on chromosomes. And indeed, many of the *Drosophila* mutants identified in Thomas Hunt Morgan's laboratory were not interesting in themselves, but only as mapping markers. Taken by themselves, mutants of eye pigmentation and morphological mutants that were used already early on as instruments in this sense are monsters, but as tools they are interesting in just this form: not because of the specificity of the defect, but because of the chromosomal location of the genes presumed to cause it. They took on the character of epistemic objects only decades later in the context of biochemical and developmental genetics, when the processes that underlie these features turned themselves into objects of investigation. Model organisms as instruments are peculiar in that they dispense with the problem of the intersection between organism and instrument. They are organisms turned into instruments. Exactly this makes them so powerful as tools. Here, the instrument is made principally of the same organic stuff as the object of investigation.

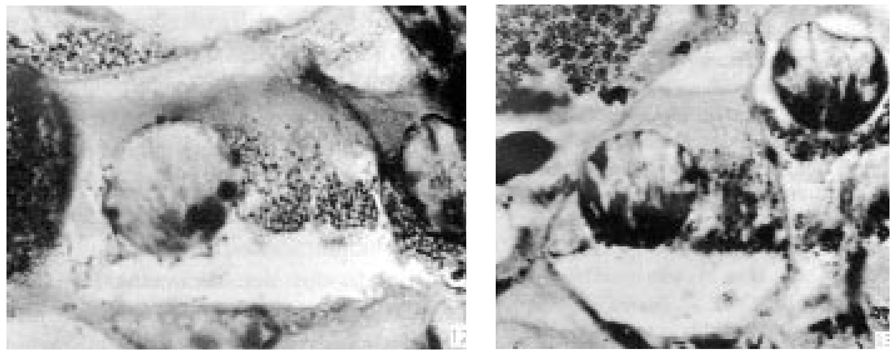
Fruit fly mutant with "ski" wings. Morgan, T. H., C. B. Bridges and A. H. Sturtevant: The Genetics of Drosophila, Chapter VI "Modifying factors and selection". In: Lotsy, J. P. and H. N. Kooiman (eds), Biobliographia Genetica, vol. II ('S-Gravenhage, 1925), p. 42, fig. 16.



4. Test Tube Experiments and the Ultracentrifuge

The question poses itself in a completely different and radicalized manner with the biochemical experiment of the twentieth century. It was instituted by Eduard Buchner's efforts to obtain a cell-free alcoholic fermentation enzyme shortly before the turn of the century. What happens in the biological test tube experiment? Here we observe a new displacement of boundaries. It came to express the problem of what is biological nature and what is artifact in the process of investigation in the form of a special linguistic dichotomy. From now on, biologists distinguished between *in vitro* and *in vivo* experiments. As the expression betrays, with the *in vitro* experiment a vitreous envelope is created which replaces the walls of the cell, the wraps of the organism. What Claude Bernard termed the "milieu intérieur," the inner environment of the organic processes, is replaced by a chemo-technical milieu that at the same time opens a new analytical space; a space turned inside out, tipped over, and trimmed for new connections. The *in vitro* experiment, which for biologists was counter-indicated for a long time in view of the specificity of biological organization, develops its dynamics by allowing scientists to isolate particular organic reactions and their carriers and to represent them separately. One might be inclined to say that the spaces of intersection are now inserted between the parts of the organism itself, and become vitrified. The price that has to be paid is yet another radicalization of the question of what is being measured. Is it still a biological function, or has it shrunk to a chemical process? Is it something going on within an organ, or something created in the test tube? The question of how the results of an *in vitro* experiment can be reconfined to the space of the organism, of how they can be localized in the living, becomes the decisive question for a biochemistry that still aspires to understand itself as biological chemistry.

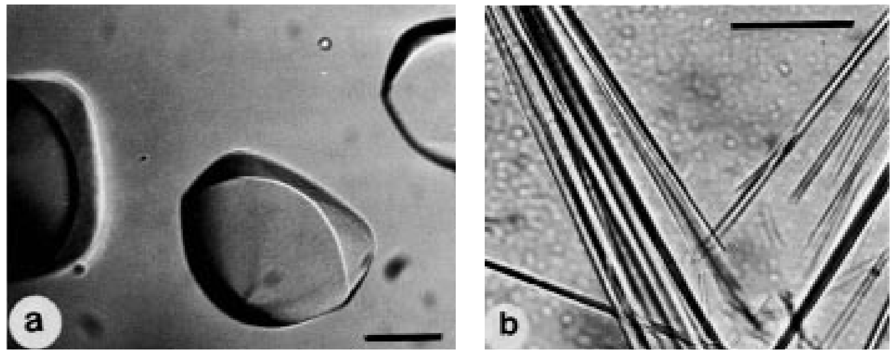
The ultracentrifuge is an instrument that played a particular role in this context. Developed in the 1920s by Theodor Svedberg, it came to be used in the 1930s for the separation of cellular components and, as Angela Creager has described in detail, for qualitatively characterizing and quantitatively isolating viruses. The tube inserted in the rotor of the centrifuge and oriented in the gravitational field becomes a container in which the contents of homogenized cells reorganize themselves according to a single parameter: the molecular weight of their constituents. Centrifugation allows investigators to decompose the cell sap in the rotor tube into sections separated from each other by sharp bounderies. If the tissue is homogenized, that is, if the cells are broken up, the morphological and functional relevance of the centrifugal bands must subsequently be reconnected to *in vivo* conditions through additional test systems involving whole cells. These checks and balances may be of a histological nature. A particularly elegant example of such a feedback is the centrifugation of intact cells. But the fractions can also be subjected to microscopic inspection under conditions similar to these described in section one. The tests may as well be *in vitro* experiments, such as, for example, an enzyme assessment. In any case, however, the centrifugal partitions participate in the dilemma which haunts – and drives – all modern experimental biology, namely to assure itself of the boundary between the still organic and the no longer organic after that boundary has already been transgressed. These acts of transgression therefore always happen in the anticipation of a possible recursive assurance whose success no one can predict in advance.



Intracellular segregation of the components of Amphiuma liver cells by high speed centrifugation. Claude, A.: Studies on cells: morphology, chemical constitution, and distribution of biochemical function. The Harvey Lectures 1947–48, vol. 43 (1950), pp. 121–164, figs. 12 and 13.

5. X-Ray Crystallography

X-ray structure analysis is another molecular technique that was first applied to polymers in the context of organic fiber research in the 1930s. Here the intersection with the biological object of investigation takes the form of a particular physical object, a crystal. What cannot be crystallized does not exist as an epistemic object for this technology. It contributed decisively to a biophysical view of the basic structures of life. Already in the eighteenth century, the crystal analogy was a favorite metaphor, and it found multiple uses in the nineteenth century. But only in the twentieth century did it materialize in the form of macromolecular biocrystals. During the twentieth century, the crystallization of biomolecules such as nucleic acids and proteins has decisively contributed to the breakthrough of a new view of biological order. On the one hand, X-ray crystallography led to the image of the iterative, double helical structure of DNA, in whose nucleotide sequence the hereditary information is stored. On the other hand, it helped to visualize the three-dimensional structure of proteins as the translation products and functional correlates of nucleic acids. Soraya de Chadarevian has pointed to the fact that in order to translate back the mathematical Fourier world that is engendered at the intersection between the organic molecule turned crystal and the X-ray, the crystallographers had to create a parallel world of macroscopic models that helped to project the molecular, crystallized point of intersection back into the world of three dimensions.



Crystals of ribosomes (70S) from the bacteria Thermus thermophilus (a) and Bacillus stearothermophilus (b). Yonath, A., W. Bennett, S. Weinstein, and H. G. Wittmann: Crystallography and Image Reconstructions of Ribosomes. In: Hill, W. E., A. Dahlberg, R. A. Garrett, P. B. Moore, D. Schlessinger, J. R. Warner (eds), The Ribosome, Structure, Function & Evolution (Washington D.C., 1990), p. 136, fig. 2 (a, b).

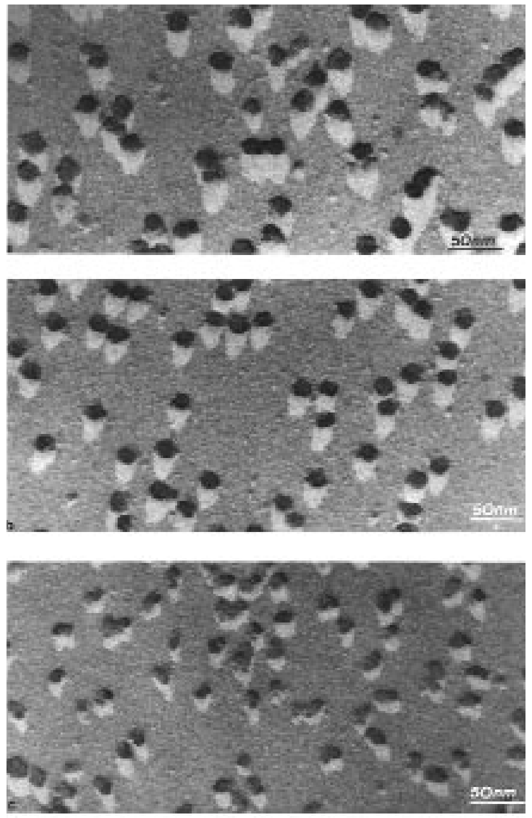
6. The Electron Microscope

Twentieth-century electron microscopy has once more radicalized the process of microscopic specimen preparation already described. Nicolas Rasmussen – for the case of the United States – and Bruno Strasser – for the case of Switzerland – have characterized in detail the precautions and frictions under which a technology that had been developed in the context of the sciences of matter, and that had not initially been intended for biological application, was made suitable for biological objects.

On the one hand, electron microscopy has forced to preparing 'ultra-thin sections.' Penetration by the electron beam could only be granted if the thickness of the specimens was dramatically reduced. New embedding procedures, microtomes with minimalized feed, and electron dense 'dyes' for contrast enhancement started to form a new field of research undertakings that developed in parallel with the instrument itself, the electron cannon. The biological material itself had to be treated and trimmed in a such a manner that it resisted the harsh conditions to which it was exposed in the microscope, at least as long as an electron shade was generated and recorded. The durability of an electron microscopic biological preparation is almost inevitably restricted. The specimen has to be exposed to a high vacuum and is consumed by the bombardment of the electron beam. In contrast to the procedures of light microscopy, the interaction with the instrument during the screening process and at the moment of picture formation, is so strong that the preparation itself tends to be destroyed. Strangely enough, here, at a new peak of specimen preparation technology, a point is reached where the object of investigation again becomes transitory. It is lost in the act of making it visible.

As a result, the electron density and the electron resistance of the material brought under the beam become the decisive parameters for modulating the surface of intersection between the instrument and the epistemic object. This is also the point where contrast enhancement comes in. One 'dyes,' for example, with electron dense salts containing heavy metals. One of the most remarkable modulations of the intersection plane between the electron cannon and the biological material consists in converting the organic cut into a metallic replica. The specimen is covered with a coat of metal evaporated from a metal source at a certain angle. Its 'shade' visualizes the contours of the objects, whose organic remainders themselves have to be carefully macerated away from the metal copy. As a condition of its representation thus, the original probe has to be eliminated altogether. The plane where instrument and object come in touch, the intersection point itself, is transformed into a new object endowed with resistance and permanence.

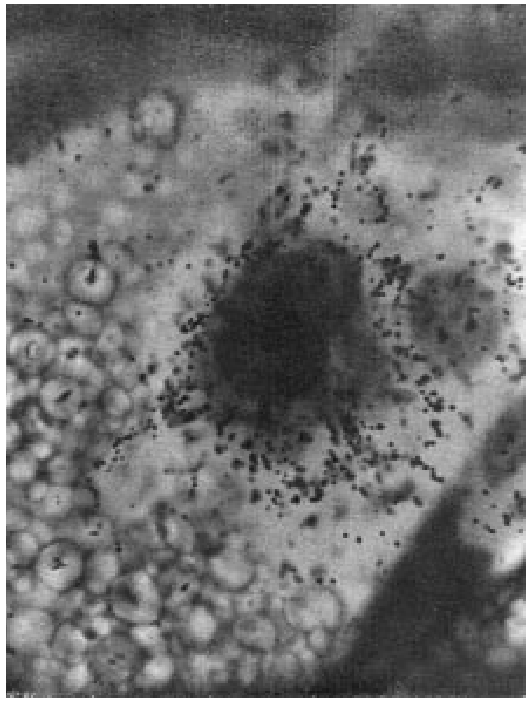
Freeze-dried and tungsten-shadowed ribosomes from Escherichia coli. Stöffler, G., R. Bald, B. Kastner, R. Lührmann, M. Stöffler-Meilicke and G. Tischendorf: Structural Organization of the Escherichia coli Ribosome and Localization of Functional Domains. In: Chambliss, G., G. R. Craven, J. Davies, K. Davis, L. Kahan and M. Nomura (eds), Ribosomes. Structure, Function, and Genetics (Baltimore, 1980), pp. 171 – 205, fig. 3



7. Radioactive Isotopes and the Scintillation Machine

In a review of the arsenal of instruments that became crucial for twentieth-century molecular biology, the technology of radioactive labeling occupies an important place. It represents an instrument of a particular character, and it has revolutionized the analysis of metabolic processes since World War II. Only now, however, have historians of the life sciences started to engage with this technology in detail. Radioactive tracing was introduced on a grand scale when radioactive variants of the most important atomic constituents of biomolecules became available in high yield as byproducts of the nuclear reactors of the Mannhattan project. In particular, these were isotopes of hydrogen, carbon, sulphur, and phosphorous. Immediately after the war, the American National Laboratory of Oak Ridge became the center for their production and for a distribution campaign known under the slogan of "atoms for peace." Quickly distributed throughout the laboratory world of biological-chemical and biomedical research, the practice of radioactive tracing consisted of replacing individual atoms in biomolecules with their radioactive isotopes and subsequently registering the decay events they produced. In principle, they behaved like probes that triggered tracer fires in particular metabolic reactions. This technology had a lasting influence on the molecular sciences. It allowed quantification in a concentration range which was not accessible to classical chemical measurement. With the penetrating power of this technology, the registration range of signals became enhanced by about six orders of magnitude, from the micromolar to the picomolar. The scope of this dramatic turning point in the history of molecular biology may have escaped historical assessment because the new tool was of a distributive order and unfolded its action not as big, self-imposing machinery but rather in a capillary fashion. Within two decades, between 1945 and 1965, it filtered through the biomolecular and biomedical sciences like a plexus of anastomoses.

Radioactive markers can be seen as molecular instruments. They become deeply immersed in specific pathways of the metabolism, and there they sparkle and leave traces. They make possible a completely new form of biological chemistry. A reaction or a molecular component that one wishes to analyze no longer has to be isolated or purified before it can be measured. It can be visualized in vanishingly small concentrations and in a whole mixture of compounds. Individual reactions can be represented with high selectivity before a background full of noise. The autoradiogram makes visible the specific place of reaction right in the tissue or cell itself, that is, *in situ.* The markers allow one to follow molecular reactions in solution in the test tube. They are tags incorporated into the molecules whose movement one observes, and they do not alter their chemical constitution. Here, the intersection of the instrument



Autoradiograph showing very strong incorporation of amino acids (14 C-adenin) in the nucleolus of Acetabularia.
Brachet, J.: The Biological Role of Ribonucleic Acids (Amsterdam, 1960), Sixth Weizmann Memorial Lecture Series, April 1959, p. 109, fig. 32.

with the epistemic object coincides with the epistemic thing whose traces, in contrast to the case of electron microscopy, can be followed on the wet side, without having to be transferred to the banks of the dry.

In an exemplary fashion, radiolabeling exposes the deep paradox of the generation of traces. The creation of the trace goes hand in hand with the destruction of the isotope. At the very moment of the creation of the trace – and this irrevocably – its source decays. Consequently, the radiogram makes visible something that no longer exists at the place where the trace testifies to its presence, and where it now stands in for its past. The radiogram therefore is an instantiation *par excellence* of what makes a trace a trace: the absence of a reference.

However, in order to get such records, the radioactivity of the probes has to be measured. In the case of the autoradiogram, a sensitive photo plate will do. Samples of another aggregation are more difficult to register. In parallel to the massive use of radioactive carbon, hydrogen, and sulfur, a new counter was developed that served as an alternative to traditional Geiger counter tubes and worked particularly well in the range of the weak ß-rays of these isotopes. The procedure rested on the transformation of the radioactive decay events into flashes of light in a liquid medium. The flashes then were sent through photomultipliers. Within a decade, the liquid scintillation counter conquered the laboratories of molecular biology, and together with the ultracentrifuge, became an emblem of cutting edge laboratory technology. The development of an automated counting device with a capacity of hundreds of samples had much more than a quantitative influence on molecular biological experimentation. Besides opening the possibility of serial testing, it allowed for the development of qualitatively new experimental designs. The liquid scintillation counter offers a good example to study the effects of the introduction of a new instrument into an experimental system. Such instruments can lend new qualities to the system, although in this case we are dealing with a comparatively straightforward procedure: the simple introduction of a new counting device. The change in measurement plays out the potentials of a modified form of intersection between device and sample. On the long passage of the radioactive specimen out of the test tube and into the apparatus, the fluid intersection between the device and the sample reflects and complies with the inherent disposition to 'wet' experimentation in biochemistry and molecular biology. The liquid scintillation counter reconfigured this boundary in an extraordinarily flexible and versatile fashion. Despite its massive lead chamber and the electronic environment of the sample detector, the probe to be measured remained in the liquid environment of a glass or plastic vial.

First automated Liquid Scintillation Detector, steel shielding, dual elevators, 100 vial samples in four circular rows (1957).

8. Concluding Remark

By pushing the frontiers of analysis into the space of physics and chemistry, molecular biologists created a science that rests on a consistent 'extracellular' project. It relied on a battery of research technologies that brought forward a multiplicity of diverse intersections between the central epistemic objects of molecular biology, the biological macromolecules, and these new technologies. It made them measurable. A few instruments crucial for this endeavor have been described in this essay. Ironically, the consequent pursuit of this program, as indicated in the last section, led to a situation in which the former objects of epistemic interest themselves, the macromolecules, became transformed into an arsenal of molecular tools. Within the past 30 years, they have swallowed the extracellular technology and transformed it, within the space of the cell itself, into the project of gene technology. The genetic engineers of today no longer construct the technology around the organism and adapt it to its surfaces, but insert their molecular instruments into the depth of the cell and let them act from within. They no longer analyze the organism; they recompose and reshape it. They are thoroughly constructive and synthetic. Under this kind of analysis, the organism itself is being transformed into an instrument, not only of research, as in the case of model organisms, but of a cultural project at large. The intersection between nature and culture appears thus to have been reversed. Culture is now at work within the innermost core of nature.

Eighteenth-Century German chemists (61)

education	
academic degree	Med. Doct.: 37, PhD: 6
no acad. degree	appr. apothecary: 13, other: 5
members of scientific societies	54
teaching	
university prof.	31
schools (only)	9
private (only)	8
no teaching	13
relation to the arts	
pharmacy	24
mining and metallurgy	31 (11 travelling only)
porcelaine	7
sugar from beets	7
mineral water	4
tobacco, coffee, brandy	4
brewery, distillery	2
salts	5
dyeing	5
chlorine bleaching	2
physicians	26

Chemical techno-science in eighteenth-century Europe

Ursula Klein

The art and *scientia* of chemistry (or chymistry, alchemy), which had been practised in the Renaissance by independent individuals,[1] became institutionalized as a learned discipline during the seventeenth-century, when medical faculties, national academies, local scientific societies, botanical gardens, museums (such as the Ashmolean Museum at Oxford), and mining boards invited chemists to teach chemistry and to perform chemical experiments. In the eighteenth-century chemistry was already a well-established part of the European intellectual world. Chemists were teachers and professors, authors of learned books and experimental essays, members of academies and scholarly societies, and frequent visitors of coffee shops and salons. Yet, eighteenth-century chemists differed markedly from other savants of the time, not only since they were passionate experimenters, who spent hours a day in their laboratories, but also because of their various artisanal occupations. Eighteenth-century chemists were both savants and learned practitioners, such as apothecaries, metallurgical officials, consultants, inspectors of manufactures, entrepreneurs, and members of state committees and technological boards. Apart from France, eighteenth-century German lands provided ample support to a flourishing chemical community. A few data may illuminate the hybrid technological-scientific careers of German chemists, whose activities extended from the writing bench and teaching laboratory to the pharmaceutical officine, mining board and manufacture (see table 1).[2] Among the 61 Germans acknowledged in the eighteenth century as “chemists,” 43 had earned an academic degree, most of them a medical doctorate, and 54 were members of academies and other learned societies. 48 of 61 chemists held teaching positions at universities and professional schools, or gave private courses of chemistry. As can also be seen in the table, however, the great majority of these chemists was also engaged in the arts and crafts, in particular in pharmacy as well as in mining and metallurgy.

The connection between chemistry and pharmacy had a long tradition that went back to medieval times. The distillation vessels used in eighteenth-century apothecary shops originated in the late medieval alchemical tradition. The same is the case for chemical operations, such as distillations and extractions with solvents, which were not invented in apothecary guilds but learned during the fifteenth century from alchemical practitioners. Eighteenth-century pharmacopoeias and other apothecary

---

[1] *The term “independent” here means not belonging to a guild, corporation, or other forms of professional organization. Of course, these “independent” chemists depended on patrons.*

[2] *The table and my following analysis are based on Hufbauer, Karl. 1982. The Formation of the German Chemical Community (1720–1795). Berkeley: University of California Press. For an overview on similar connections in eighteenth-century France see Gillispie, Charles C. 1980. Science and polity in France at the end of the old regime. Princeton: Princeton University Press.*

books included recipes for hundreds of chemical medicines originally introduced by the Paracelsian iatrochemical movement. Inversely, almost all of the eighteenth-century chemical textbooks presented numerous recipes for the fabrication of medicines, and described the properties and medical virtues of the chemical medicines. Many eighteenth-century chemists were also physicians who produced and sold their own chemical remedies. For example, Georg Ernst Stahl (1659 – 1734) and Friedrich Hoffmann (1660 – 1742), both of them professors of chemistry at the University of Halle, belonged to the most famous eighteenth-century chemists who earned a fortune by selling their own, mostly secret, remedies.

But most chemists involved in the pharmaceutical art had completed a traditional apprenticeship. The chemist-apothecary was a widely respected persona in all eighteenth-century Europe. Andreas Sigismund Marggraf (1709 – 1782), for example, had completed an apothecary apprenticeship, and between 1735 and 1753 administered his father's apothecary in Berlin. When he was a journeyman between 1733 and 1735, he also took a few courses in medicine at the University of Halle and learned assaying with Johann Friedrich Henckel (1678 – 1744) in Freiberg. In this time he began to collect minerals and to organize a mineral collection. Furthermore, Marggraf was the first German chemist who performed experiments for extracting sugar from beets, which eventually led to the industrial production of beet sugar in the 1790s by his pupil Franz Carl Achard (1753 – 1821). Despite the fact that Marggraf had never earned an academic degree, in 1738 he became a member of the Berlin Society of Sciences (renamed in 1744 as Berlin Academy). Beginning in the 1740s, he also gave private courses in chemistry, which contributed to his growing reputation as an excellent chemist. In 1754 Frederick the Great made him the director of the Academy's new chemical laboratory. Four years later, he even became the director of the Physical Class of the Academy.

Although the connections between eighteenth-century chemistry and the apothecary trade are well known in principle,[3] the fine-grained historical details of that connection have largely remained in the dark. Even less known is the intersection of eighteenth-century chemistry with mining and metallurgy, which was particularly strong in the German lands and in Sweden. Travels to mining districts and visits to mines and adjacent salt-works and foundries were a highly appreciated part of eighteenth-century chemists' technical education. In this way, chemists gathered information about the labour processes of mining, smelting and assaying, the extraction of salts, and the properties and uses of machines and materials. They brought back from their travels samples of minerals, as well as improved natural historical knowledge about minerals, mountains, and strata of rocks. But not only occasional travelling created bonds between academic chemistry and the world of mines and foundries. The Swedish Board of Mines maintained a chemical laboratory from 1683 onward, where chemists-mining officials analysed minerals and mapped the Swedish mineral resources.[4] In the middle of the eighteenth century, this laboratory became a pioneering place for the use of the blowpipe in mineral analysis. Many German chemists held positions as mining and metallurgical councillors in mining towns, such as Freiberg, Brunswick, and Schemnitz, being charged with the control and improvement of the technology, economy and organisation of labour in mines and foundries, and with the analysis of minerals.

---

[3] *See, in particular, Schneider, Wolfgang. 1972. Geschichte der pharmazeutischen Chemie. Weinheim: Verlag Chemie.*

[4] *See Porter, Theodore M. 1981. The promotion of mining and the advancement of Science: the chemical revolution of mineralogy. Annals of Science 38:543 – 570.*

Among this group, in the first half of the eighteenth century the following seven chemists held salaried long-term positions: Christoph Andreas Schlüter (1673 – 1744), who served as a smelting comptroller in the lower Harz from 1698 onward; Johann Friedrich Henckel, a student of Georg Ernst Stahl and medical doctor, who became a famous Saxon mining Councillor first in Dresden and then in Freiberg, where he gave courses of assaying in a public laboratory; Johann Andreas Cramer (1710 – 1777), a councillor in Brunswick responsible for the smelting works in the Weser district and a teacher of assaying to the famous Dutch chemist Herman Boerhaave; Christlieb Ehregott Gellert (1713 – 1795), who from 1753 onward was a mining councillor in the Saxonian town of Freiberg and in 1765 became the first professor for metallurgical chemistry in the newly founded Mining Academy of Freiberg; Johann Gottlob Lehmann (1719 – 1767), a Prussian Mine Director in Hasserode (Harz) and later a Mining Councillor in Silesia and Berlin; Giovanni Antonio Scopoli (1723 – 1788) and Nicolas Joseph Jacquin (1727 – 1817), both of them mining councillors and chemistry professors in the Schemnitz Mining Academy. Most of these men were also teachers (apart from Schlüter), authors of chemical and metallurgical treatises, and members of academies and scientific societies. This dual carrier as a chemist and salaried mining and metallurgical councillor continued well into the later eighteenth century. To be mentioned are in particular Carl Wilhelm Poerner (1732–1796), Carl Abraham Gerhard (1738 – 1821), Ignaz Born (1742 – 1791), Carl Friedrich Wenzel (1747–1793), Johann Friedrich Westrumb (1751 – 1819), Jeremias Benjamin Richter (1762 – 1807), Alexander Nicolaus Scherer (1771 – 1824), and Wilhelm August Lampadius (1772 – 1842).

Apart from mining and metallurgy, and pharmacy, there were other arts and crafts eighteenth-century chemists were actively involved in. For example, in the 1740s, the Prussian King Frederick II commissioned Johann Theodor Eller (1689 – 1760), Johann Heinrich Pott (1692 – 1777), and Johann Andreas Cramer to study the manufacture of porcelain; in 1745 – 1746 Pott established a porcelain works in Freienwalde funded by the Prussian King. In France, Pierre Joseph Macquer (1718 – 1784) made similar investigations, which in 1769 culminated in the production of the first French porcelain at Sèvres. Mercantilist policies supported the search for all kinds of surrogates for precious imported commodities, in particular sugar, tobacco, coffee, brandy and liqueurs. The extraction of sugar from beets, for example, which had been initiated by Marggraf in the 1840s, was pursued by seven German chemists. Most successful was Franz Carl Achard (1753 – 1821), who in the 1790s received a salary and an estate from the Prussian King Frederick Wilhelm II to establish a sugar manufacture. In the royal manufactures of France many chemists held leading positions as inspectors. For example Jean Hellot (1685 – 1766) was inspector general of dyeing in the 1740s, and Pierre Joseph Macquer and Claude Louis Berthollet (1748 – 1822) were inspectors of dyeing at the manufacture of the *Gobelins*, where they performed quality control experiments in the manufacture's laboratory.[5] Berthollet's experiments in the 1780s on chlorine bleaching were almost immediately implemented in French and British manufacturies. Chemists had also the lead in the technological improvement of gunpowder, as can be observed, for example, in Antoine-Laurent Lavoisier's work from the 1775 onward at the *Règie des poudre et saltpêtre*.

---

[5] *See Nieto-Galan, Agusti. 2001. Colouring Textiles. A History of Natural Dyestuffs in Industrial Europe. Vol. 217. Boston Studies in the Philosophy of Science. Dordrecht, Boston, London: Kluwer.*

It was less theoretical knowledge than experimental and natural historical expertise on a broad range of materials, connoisseurship required for the identification and classification of materials, experimental skills, and familiarity with chemical analysis that equipped chemists for their various technological occupations as hybrid technologist-savants. In this context it is particularly significant that until the middle of the eighteenth century the instruments, tools, and materials used in chemical laboratories did not differ substantially from the inventories of pharmaceutical laboratories and workshops of assayers, smelters, and distillers. We know from drawings of chemical laboratories and instruments, as well as from their verbal descriptions, that eighteenth-century chemists relied to a high degree on the instruments and materials provided by ordinary craftsmen and merchants. Their smelting and testing furnaces, bellows, crucibles, calcination dishes, and balances were largely the same used in the workshops of assayers and smelters. Evaporation vessels, crystallising dishes, phials, retorts, alembics, pelicans, receivers, and transmission vessels were common instruments both in the chemical and the pharmaceutical laboratory; simple retorts and receivers were further shared with distillers for fabricating mineral acids, alcoholic spirits and fragrant oils (see figures).

Chemical instruments in seventeenth- and eighteenth-century apothecary's laboratories (courtesy of Pharmazie-Historisches Museum der Universität Basel)

Studies on the intertwinement of eighteenth-century chemical science and technology shed new light on our historical and philosophical understanding of the emergence and historical development of the experimental sciences and of techno-science. Chemistry was not only the first experimental science that entrenched comparatively large communities of experimenters, in particular in eighteenth-century France and Germany. It was also the first historical form of an experimental science situated in laboratories. The term “laboratory” first referred exclusively to the space of chemical experimentation. As the term “laboratory” also reveals, this was a specific site for doing “work,” for performing daily operations, which often pursued hybrid epistemic as well as technological and commercial goals. Compared to the chemical laboratory, the eighteenth-century “physical cabinet” and the “*theatrum physicum*” were much more sites of collecting and exhibiting curious philosophical instruments and of demonstrating spectacular experimental effects than places for mundane, daily work. As eighteenth-century chemists’ experiments continued a long artisanal tradition, they included many repetitive manipulations and familiar effects that were boring routine for the polite public.

Moreover, whereas those practices of experimental philosophy that eventually became transformed into “experimental physics” may be circumscribed as a new “experimental *method*” – that is, a new way of doing and knowing within the extant tradition of philosophical schools – this definition does not fit eighteenth-century chemical experimentation, even not if it were artifactually limited to experiments performed only in academic spaces. Eighteenth-century academic chemical experimentation is much better characterized as an artisanal tradition transposed to a new social institution and thereby invested with new meanings and epistemic goals – as a hybrid artisanal-epistemic or techno-scientific practice – than as an unequivocal “method” for the acquisition of knowledge.

Practical physics class of Henry A. Rowland at Johns Hopkins University in Baltimore (circa 1880). Johns Hopkins University Archive

Experience – Experiment

The Changing Experiential Basis of Physics

H. Otto Sibum

“Es ist ein entscheidender Charakterzug der Physik, dass in ihr das Experiment die Beobachtung fast völlig verdrängt hat.”(Auerbach, 1925, 3)

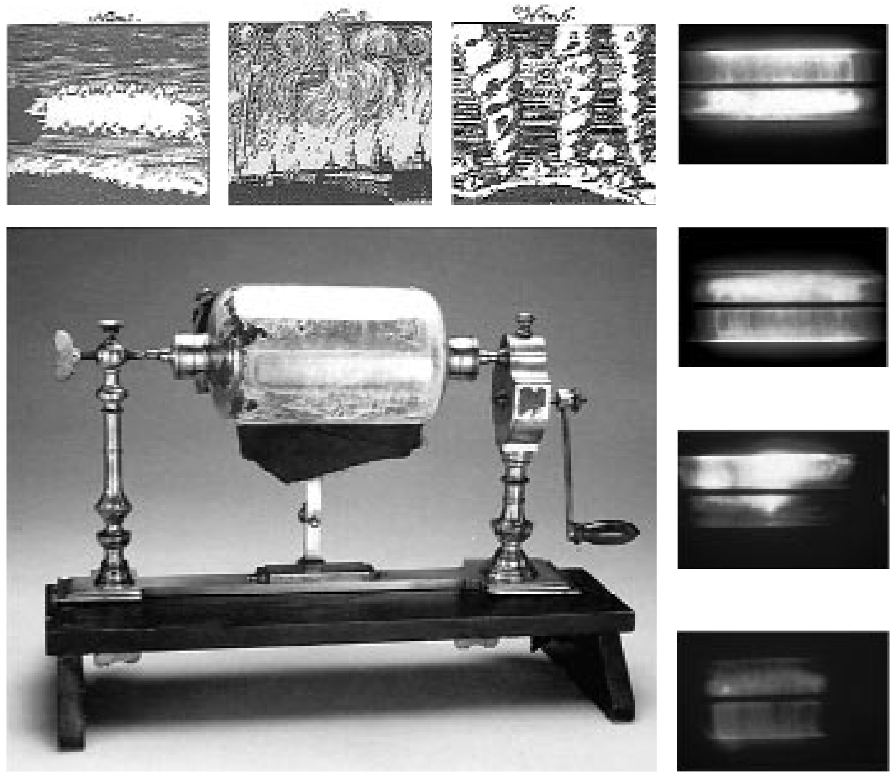
In 1923 the German theoretical physicist Felix Auerbach told his readers that experimental physicists unlike botanists or geologists, do not observe nature but rather artificially create physical phenomena in their laboratories. He made what we would now regard as a contentious claim: that X-rays for example were not discovered by Röntgen but were invented by him. “X-rays are not a ‘natural phenomenon’, until Röntgen there weren’t such, they have been invented by him (this expression is more appropriate then the conventional ‘discovered’); and in case it turns out that there will be such rays in nature, this does not change the issue essentially.”
With this essay I would like to draw your attention to this artificial technological character of experiment, or more precisely to the kind of scientific experience gained through the use of human made devices. As I will show, Auerbach’s reflections on the experiential basis of physics are not just important expressions of his time but an integral part of a long historical process of settling the controversial positions about the epistemological status of experiment and experience. This debate goes back at least until the 17th century but I would like to concentrate here on a time period in which the scientific persona of the experimentalist became fully established – the mid 18th until the late 19th century. It was in this time span that the engineer came to be regarded as a “third man” – a novel actor capable of bridging the divide between theorists and practitioners, science and the arts. This coming-into-being of a new persona was contemporaneous with and coupled to an institutional and social process: the establishment of experimental physics as an academic discipline. I will briefly describe the changing experiential basis of physics to be observed in this period which frames the questions explored through the research project “science and the changing sense of reality circa 1900” undertaken at the MPI.

Scholars and the Art of Experiment

Since the early modern period, scholarly opinions on ‘the art of experiment’ have ranged from denying it had any epistemological value to the nineteenth century conviction that this form of inquiry was the only way to make sense of natural causes. One of the underlying issues in these controversies about the meaning of experiment was that the physical manipulation of objects was seen as not belonging to the scholarly tradition, in which a clear distinction between doing and knowing still predominated. Even enlightened philosophers like Denis Diderot, who described the arts as a form of knowing, conceded that this knowledge operated outside the enlightened discourse. His encyclopaedic project was one answer to the dilemma of how to give the practitioners’ knowledge a language which could be understood by anyone. But together with many other literary approaches, the rising bourgeois culture reduced these complex forms of knowing to visual representations or descriptions of manual techniques, which finally maintained the boundary between epistemology and practice. In the mid eighteenth century the engineer had been seen as the ideal candidate – *the third man* who could bridge theory and practice. However, from the engineers’ perspective, to be this *third man* was an important but still unsatisfactorily position: “In such circumstances, a third man would be needed, who could in himself unite science and art, in order to correct the *theorists’* infirmities and to combat the prejudice of the lovers of the arts, as if they could be therein complete without the *theory*, and leave it [theory] to the idle heads good-for-nothing in the world … Hence … he [Leupold] compared himself to a bat, tolerated among neither birds nor quadrupeds, and he complained that he was hated by the practitioners of art as well as despised by the *theorists*, for he wanted by his nature to be celebrated as a remarkable man by both, and to share fame in the learned world with the latter and happiness at court with the former” (Chr. Wolff, 1764, 2).

In establishing *“physica experimentalis”* within the *Gelehrtenrepublik* (Republic of Letters) experimentalists were experiencing the advantages and disadvantages of the third man’s position. Like bats, experimentalists were difficult to classify. Did their studies of nature, practiced with head and hand – i. e. the art of experiment – lead to a specific form of *knowledge*, did it qualify as *science*? Answers to this question depended on the actors’ stance towards the implicit distinction made in those days between experimental knowledge and science, or knowledge in general and scientific knowledge in particular. This distinction has a largely unwritten history of its own and is intimately linked with the social history of those who work with their hands and those who work with their heads. Furthermore, the dominant understanding of scientific knowledge as universal, autonomous, and permanent was intimately linked with the hegemony of the written text in the scholars’ form of life. Hence even from the mid-eighteenth century onwards, several generations of experimental natural philosophers were required to free the art of experiment from its epistemological stigma and to position their knowledge within the Republic of Letters. The rising experimental research on electricity and magnetism played a key role in changing scholarly opinion about the epistemological status of the art of experiment. Hitherto unknown effects, created daily in these experiments, challenged the traditional and widely accepted scholarly position about the twofold meaning of experience in physics: “Experience gained in physics through the senses is of a twofold kind: one sort we take from God’s creatures, from fire, air, water, earth, from the stars, flowers etc. the other we gain from artificial things, which are made by human hands … But we have no cause to make a great show of it, as if one could discover new and hitherto unknown physical truths through them [artificial things]”

In the course of development this latter position became obsolet, “new and hitherto unknown physical truths” were in fact established by experiment and helped emancipate experimental physics from natural history. In 1755 the translation of an important contribution from the French experimentalist Abbé Nollet entered the German scene. In this inaugural lecture Nollet had attempted to position “*physique expérimentale*” within the Republic of Letters (J. A. NOLLET 1753, German translation 1755). He insisted that this mode of investigation should be differentiated from the practices of natural history. The latter was a form of inquiry which completed the “inventory of our wealth” but did not investigate the “causes of what happens in the natural world.” However, both were intimately linked with each other: “For indeed, he who endeavours to investigate nature without understanding its history speaks at random and about things that he does not know in the least; but he who knows nothing else of nature than its history justly deserves a place among those natural philosophers who exercise their memory only. Accordingly, to practice experimental physics is nothing else than to investigate nature, not only with regard to its effects, but equally with the intention [of studying] the tools by which [nature’s] effects are produced; in short it means to study what [nature] does, in order to be in a position to say how she does it.”



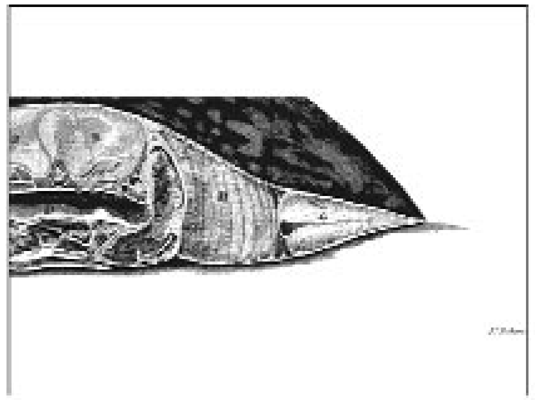
Experiments to model the „Aurora Borealis“
Top: Engravings of the Northern Lights as seen in Danzig in 1716. Deutsches Museum (Bildarchiv)
Below centre: 18th century electrical machine, A. Q. Morton: Science in the 18th Century (London, 1993), p. 11
Right: Illuminations generated in an evacuated glass tube mounted on a replica of an electrical machine.

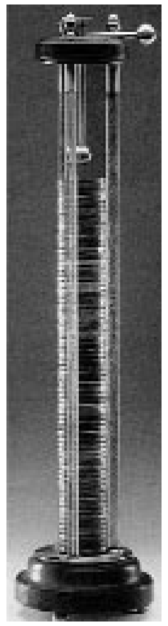
However, until the early nineteenth century, being a scholar at German universities first of all meant to be a writer who devoted much of his publishing activity to translations, textbooks, and compendia. And it is not a coincidence that the prime mover of experimental physics at Göttingen University, Georg Christoph Lichtenberg, was both, man of letters and experimentalist. His editing of Johann Christian Polykarp Erxleben's *Anfangsgründe der Naturlehre* (Erxleben, 1794) for example shows quite clearly how the "new and hitherto unknown physical truths" established by means of experiment could be used constructively to change traditional practices of the scholarly exegesis of texts. The many footnotes to be found in the sixth edition demonstrate how rapidly the state of art of *physica experimentalis* was changing and how it affected the truth claims made in the script of nature. Locally produced experimental knowledge changed the practices of publishing and research of the traditional University scholar. As we will see in the following sections 'scientific invention' ("das Erfinden im Scientifischen") gradually replaced older encyclopedic mentalities.

The main challenge to traditional text-based scholarship was that experimentalists' investigation of nature's effects meant to develop and study instruments. From the engineers' point of view instrumental intervention was unproblematic. According to historians of engineering, the "engineer was in fact natural, as was the countryside which he confronted. His 'genius' wrote under the dictation of nature, and it was this same nature which he had to try at all times to transform." But that was not the commonly accepted position amongst academicians. Especially the new field of inquiry, electricity and magnetism, was challenging because nearly every phenomena became observable only with the assistance of instruments or apparatus. A key element was the experimentalists' practice of small-scale modelling. In Germany for example the principle of the University of Leipzig Johann Heinrich Winckler inferred from his artificially created illuminations in a vacuum tube that the Aurora Borealis was electrical. But not only macro phenomena were modelled, in the late eighteenth century the Italian physicist Alessandro Volta even succeeded in modelling micro-physical phenomena as well. He constructed a model of the electric fish, today known as the first electric battery, which for the first time demonstrated the existence of an electric current. At the end of the eighteenth century a wide range of knowledge traditions including those of artisans, instrument makers, natural philosophers and engineers were in conflict about the true meaning and scope of their models.

Handwerksgelehrte and the Collective Refinement of Experience

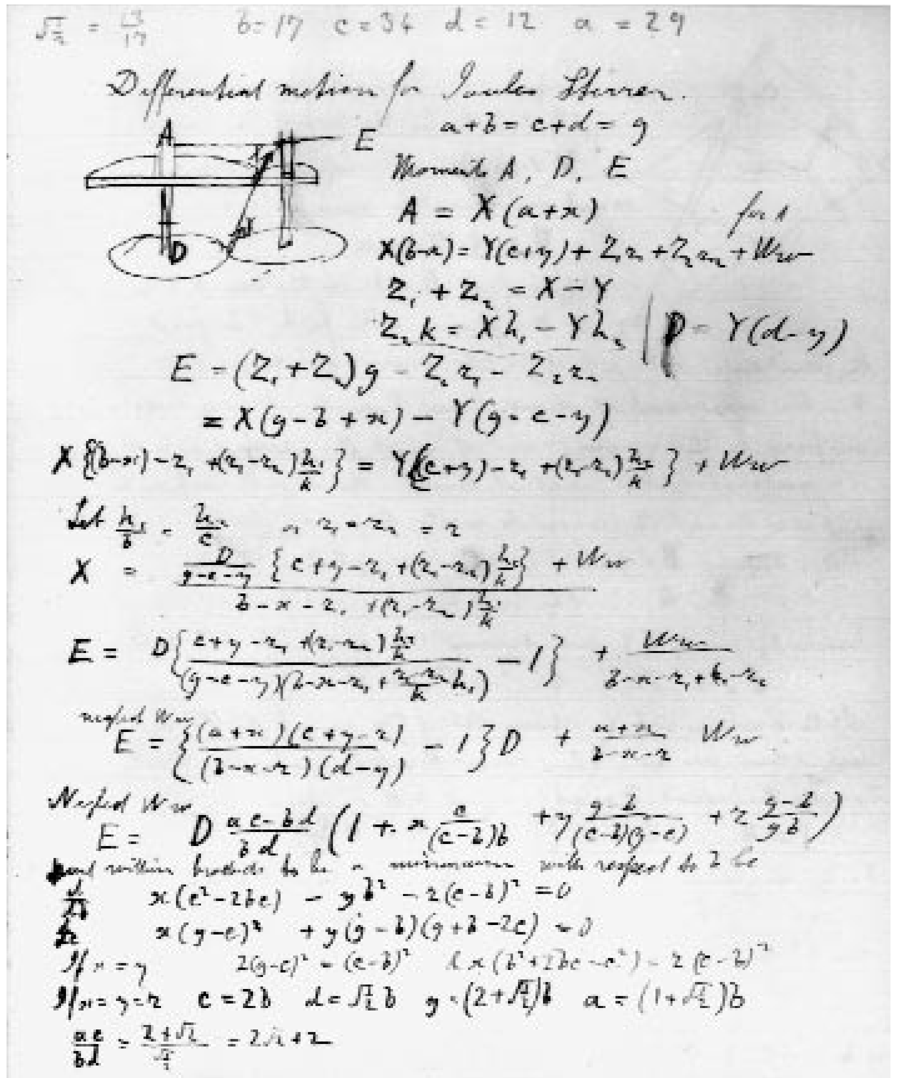
Despite the immense practical achievements in creating "new physical truth" the status of experimental knowledge in 19th century scholarly form of life was still rather controversial. Artisans, merchants, engineers, instrument makers as well as scholars participated in a complex historical process of moulding the physical sciences based on experimentation. However, each country integrated the experimentalist tradition into the elite academic culture in very different ways. As Thomas Kuhn has pointed out, in France the Abbé Nollet had been "a member of the somewhat motley section officially reserved for practitioners of *arts méchaniques*. There, but only after his election to the Royal Society of London, Nollet rose through the ranks, succeeding among others, both the Comte de Buffon and Ferchauld de Réaumur." In late eighteenth century Britain, a few outstanding instrument makers had become members of the Royal Society. In Germany, a few researchers succeeded quite early in establishing "Experimentalvorlesungen" at the universities: Georg Christoph Lichtenberg and in the 19th century Wilhelm Weber in Göttingen, Justus Liebig in Giessen, Robert Bunsen at Marburg, Gustav Magnus in Berlin etc. And as the cases of the Bavarian optician Joseph Fraunhofer, the Manchester brewer James Joule or the Prussian civil engineer Hermann Moritz Jacobi show, artisanal knowledge became constitutive of the experimental sciences. The founding of the Berlin Physical Society in 1845, for example, which included a number of instrument makers was a major achievement in shaping the identity of *physicists* – a term to designate German students of nature.





Above: Anatomical drawing by John Hunter of a dissected torpedo fish which displays the electric organ. J. Hunter: Anatomical Observations on the Torpedo. In: Phil. Transactions, LXIII (1773/74), p. 488 f.
Below: Replica of the first electrical battery constructed by A. Volta, which he described as the „artifical electrical organ". C. Blondel/P. Chairpopoulos: La Pile ou l'autre face de l'électricité. In: Les cahiers de science et vie 26 (1995), p. 89

In the second half of the century even a new term *Handwerksgelehrte* was coined which captures the amalgamation of the experimentalists movement with the traditional academic elite. What had previously been regarded as quite distinct knowledge traditions, i. e. the experimentalists and the bookish scholars, now merged into a distinct community of experimental scientists in which ways of acting and ways of knowing were to become of equal epistemological status. This process of amalgamation lasted the entire second half of the nineteenth century. Laboratories within most of the Universities in Europe and North America were established. Practical physics as a new way of teaching ran parallel to this process. Chairs for experimental physics were set up and even methodology reflected the emancipatory process of the experimentalists. For example, Hermann von Helmholtz as well as James Clerk Maxwell, both chair holders of experimental physics, promoted an understanding of induction which stressed the similarities between the intellectual work of the experimental physicist and that of the artist. Moreover, Maxwell even tried to investigate the commonalities between intellectual work of theoreticians and that of the experimenters. The form of life experienced by Cambridge Wrangler's – with their extensive focus on mathematical practices – did not necessarily qualify them for doing experiments. Speed and accuracy in performing knowledge – the emblem of the Wrangler's world of mathematics – was regarded as the counterpart to the experimenters accuracy of knowledge in manipulating objects. Furthermore, working on experiments resulted in a loss of status within their colleges. Therefore, for Maxwell, as the first professor of experimental physics in Cambridge University, the new intellectual work of the experimenter, involving head and hand, was hard to harmonise with the mathematicians 'tact'. In the beginning it was even difficult for Maxwell to attract a reasonable number of students for his classes. And later on in experiments of research Maxwell tried very hard to make sense of the experimenters knowledge in the traditional context of Cambridge Wrangler culture. Tellingly, he even tried to translate the often unarticulated practices or phenomenological descriptions of the experimenters into formalised techniques of Wrangler mathematics.



Notebook page: James Clerk Maxwell's attempt to translate the technical problems of an experiment into Wrangler mathematics. Courtesy of the Syndics of Cambridge University Library

Finally, teaching practical physics became the strategy to change well founded attitudes and to prepare the grounds for this new kind of research in Cambridge. Visitors to the laboratory were often astonished to see Maxwell and students engaged in historical replications of experiments. But from his point of view that was the secure way of leading students and himself into the experimenters' world. Following his general conviction "that the facts are things which must be felt they cannot be learned from any description of them", he regarded it as an educational value to bring to consciousness the scholars own tacts. Furthermore, reflections about the troubles in getting experiments to work made explicit the fact that it is only by the aid of their own senses that knowledge may be acquired. Or as Maxwell put it by quoting Harvey: "All this has been said more than two hundred years ago by one of our own prophets – William Harvey, of Gonville and Caius College. 'For whosoever they be that read authors, and do not by the aid of their own senses, abstract true representations of the things themselves (comprehended in the author's expressions) they do not represent true ideas, but deceitful idols and phantasms, by which they frame to themselves certain shadows and chimaeras, and all their theory and contemplation (which they call science) represents nothing but waking men's dreams and sick men's phrensies.'"

From the educational perspective Maxwell sought to avoid drawing a line between the practices of Wrangler mathematicans on the one hand and experimental physicists on the other. He regarded exercises in the practical physics courses as being based on the same principle of tuition as those used in the coaching of Cambridge's mathematical elite.

"I shall go on to consider what may be called the Principle of Tuition or the conditions of action with respect to the agent. This principle may be stated thus. 1. Every act has an appropriate sensation of action and this sensation may be willed, if previously known. 2. When a complex operation has been performed its sensation obtains a fictitious simplicity and thus it may be repeated by a single act of volition. 3. When a new operation is to be performed, it must be analysed into operations of which the sensations are already known, and these operations must be willed together in order to perform the new operation… Education as it relates to the person educated is the acquisition of a new power. Now since by the principle of Tuition the power is obtained only by the performance of the act and since new acts are performed by the composition of their known component acts Education is the same as building up Edification or Instruction."

Similarly, Helmholtz explained to his audience at the *Naturforscherversammlung* in Innsbruck in 1869, the peculiar kind of work experimental scientists are performing: "Besides the kind of knowledge that books and lectures provide, the researcher in the natural sciences needs the kind of personal acquaintance that only rich, attentive sensory experience can give him. His senses must be sharpened… His hand must be excercised that it can easily perform the work of a blacksmith, locksmith, joiner, draftsman, or violinist." The new physical scientists had to be collectively trained in the refinement of their sensuous experience. Only then they will succeed in discovering natural laws through experiment.

"A law of nature, however, is not a mere logical conception that we have adopted as a kind of '*memoria technica*' to enable us to more readily remember facts. We of the present day have already sufficient insight to know that the laws of nature are not things which we can evolve by any speculative method. On the contrary, we have to *discover* them in the facts; we have to test them by repeated observation or experiment, in constantly new cases, under ever-varying circumstances; and in proportion only as they hold good under a constantly increasing change of conditions, in a constantly increasing number of cases and with greater delicacy in the means of observation, does our confidence in their trustworthiness rise. Thus the laws of nature occupy the position of a power with which we are not familiar, not to be arbitrarily selected and determined in our minds, as one might devise various systems of animals and plants one after another, so long as the object is only one of classification."

Helmholtz' plea for the collective refinement of experience marks an important change in the epistemic status of sensuous experience in science. Together with Maxwell and others he set sensuous experience center stage in the process of generating scientific knowledge and of bridging the divide between theorists and practitioners.

Experimenting Theory

And yet despite these efforts and the practical achievements of the third man in the age of natural science, reflections about the epistemological status of experimental physics in general and sensuous experience in particular continued. Not only the new *Handwerksgelehrte* but even laypersons forcefully argued for a mediation between knowing and doing, theory and experience. The German tanner J. Dietzgen, for example, while engaged with philosophical problems in generating scientific knowledge, announced in 1869 the *third man's* problem as having been resolved practically only. "The Christian opposition of spirit and flesh is in the age of natural science *practically* resolved. What's missing, in order to free the material interests from their evil reputation, is the theoretical solution, the mediation, the evidence that the spiritual is sensuous and the sensuous is spiritual." To him the tension resulted from a conflict between two philosophical traditions about the sources of knowledge. The idealist regards the source of knowledge in reason only, the materialist in the sensually perceived world. But he saw a way out of this contradiction: "The mediation of this contradiction requires the insight that both sources of knowledge are intimately connected with each other … Therefore even the lowest art of experiment which acts on the basis of experienced rules, is only gradually different from that scientific practice which is based on mere theoretical principles.“

As the various historical studies of the project "science and the changing sense of reality circa 1900" already show the changing experiential basis of physics around 1900 evoked various reflections about these sources of knowledge. However, one important and widely shared understanding of experimental physics emphasized its artificial technological character. And it was Auerbach amongst others who spelled this point out most clearly: "experimental physics does not – as the term already suggests – practice observation of nature like other natural sciences, it deploys artificial experiments which are performed just for a specific purpose. Strictly speaking, physics, with regard to it's method, is not a natural science like astronomy, geology, botany etc; it does not deal with natural but with artificial phenomena produced by intentional acts of the researcher; in this sense we can speak of physics as a technical science." (Auerbach, 1923, 4). By 1900 in Germany more than 90 % of the physicists practiced precisely this technical science. But the physics community was not speaking with one voice and we could list here several different stances about the epistemological status of experiment and sensuous experience in generating knowledge. For example the experimental physicist and director of the Leipzig physics institute O. Wiener did not speak of invention but suggested that the instrument based physical research should be regarded as an evolutionary process of the *extension of the human senses.* Despite their slightly different positions experimentalists as well as theoretical physicists were equally concerned about the question what was the source of physical knowledge. What role did sensuous experience play? Especially the increasing number of techniques to investigate microphysical objects like x-rays, electrons etc. around 1900 opened novel experiential spaces of the physicists and induced this increasing self-reflexivity about their tools and methods. They even were putting new demands on the quest for unity of nature. In the project "Science and the changing sense of reality circa 1900" currently being undertaken at the MPI we are investigating various of these techniques and their interrelations with each other in order to understand better how they changed the scientists' practice and their sense of reality. Furthermore we investigate what role this change of experiential space played in the process of the differentiation of scientific work like experimental and theoretical physics as well as in the strikingly self-reflexive turn in the sciences as formulated so clearly in the early writings of Ludwik Fleck, Michael Polanyi, and Gaston Bachelard.

The work of the theoretical physicist Auerbach indicates that the *third man's* approach of bridging theory and practice even affected the culture of theoretical physics: for him the source of scientific knowledge was always experience. It should be noted that the latter ought not be regarded as the test of a theory, but as the material for building up theory. The implied claim – he argued – that theoretical physics takes the material for constructing its general fundament from experience, might make it appear as if physicists are arguing in a circle. How could one derive the facts of experience from a general schema and at the same time gain this schema by orientating one's self towards experience? In order to persuade his audience he refers to the most striking invention of 19th century electrical engineering: the dynamo – an invention about which he had done extensive research himself. To many in the 19th century it might have seemed impossible to build a machine which could produce electrical energy out of mechanical work and at the same time feed the magnet of this same machine with electric currents. But such a machine had been built, it was the Siemens dynamo electrical machine. In a way such a machine starts producing current immediately when turned because of a trace of magnetism inherent in every piece of iron, a trace which suffices to produce weak electric currents which take care of all the rest. In a similar way we as theoretical physicists just need a minimum of experience. We do not want to address every question directly to nature, but would rather like to gain as much knowledge as possible about nature from a minimum of facts of experience (Erfahrungstatsachen). Of course with some practice we could then build theoretical physics directly out of our heads with the foresight that a retrospective check against experience does not contradict the theoretical claim; but if this happens we would have to restructure our building or eventually replace it through another one.

But it was important to note that according to Auerbach this practice of theorising had to be distinguished from another kind of theoretical physics whose promoters believed the general comes from a mere speculative inside of the researcher. "They construct an ideal world, declare their satisfaction, if the real world matches the ideal. But in case of contradictions these theorists would go that far and declare the real world as false because it does not match with the ideal."