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Conference

The Shape of Experiment

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The Shape of Experiment

What is the result of recent studies on the history of experiment? How has our image of science been changed since Ian Hacking's statement, "experimentation has a life of its own," turned into a catch phrase for investigations into the history of science? What is the lesson to be drawn from the studies following Steven Shapin's and Simon Schaffer's *Leviathan and the Air Pump* (1985) and Peter Galison's *How experiments end* (1987)?

In trying to answer these questions, this conference will not aim at contributing to a more developed philosophy of scientific experimentation, nor will it try to return to the grand narratives on the history of science. Rather, the goal of this conference is to identify characteristic configurations within in the history of experimentalization from 1800 to the present. The guiding question is: what are the typical forms of experiment that emerged in the separated and shared history of science, technology, and the arts?

Background

Over the last ten to fifteen years, numerous historical and sociological studies were published focussing on experiment and experimentation. Taking single laboratories or experimental set-ups as exemplary cases, these studies have investigated primarily the "material culture" of experimentation. They have shown that experiments consist mainly of instruments and tools standing or lying on the table of a scientist. At the same time, emphasis was placed on model organisms, technological infrastructures, and laboratory architectures. Furthermore, procedures for registering and computing data as well as interactions with scientific colleagues, engineers, and students were analyzed as components of the material culture of experiment.

Focussing on the materiality of scientific practice has led historians to contextualize experiments in novel ways. In particular, scholars have stressed the connection between single experimental set-ups and larger systems of communication and transport. Thus, single laboratories were situated in the dense context of urban landscapes. Cities' infrastructures facilitated certain kinds of experiments while confronting their conduction with sometimes surprising sources of disturbance (noise, vibrations, etc.). In a similar way, the role of computers and computer net works has been highlighted with respect to the emergence of transdisciplinary areas of research (e.g., cybernetics, bioinformatics).

Recent studies of the history of experiment have also opened new perspectives for the "aesthetics of experimentation." As it turned out, 19th-century experimenters produced and controlled the precision of their scientific work at least in part by the individuality of their gestures. The growing interest in processes of producing written and drawn representations of experimental data has also led to a renewed interest in the history of artistic and literary experimentation. Experiments did not just yield specific genres of scientific publication (the "preliminary report," the "abstract"). At the same time they gave birth to specific iconographies dominated by the attempt to visualize the invisible, the unknown, and the new.

One might say that, in a first step, the reinforced interest in the materiality of experiment fostered the elaboration of case studies in the history and sociology of science. Today, questions emerge as to the more general aspects of these studies. There seems to be a lack of something like *a historical and comparative morphology of experiment* taking into account the important results of case studies and at the same time transcending their limited points of view. We are thus driven to circumscribe characteristic configurations in the history of experimentalization, not least for the purpose of orienting future studies in the field.

The "material logic" of experiment

Terminologically, one often distinguishes between "demonstration experiment," "research experiment" (as a means for testing hypotheses), "self experiment," "thought experiment," "test," etc. Concerning the various types of experimentation, Mirko Grmek has suggested the following scheme with respect to the history of the life sciences: 1) undisturbed experimental trials; 2) analogous and/or elementary qualitative experimentation; 3) quantitative experimentation; 4) scientific empiricism; 5) systematic experimentation. In a different perspective, the "modern" kind of experimentation has been contrasted with "post-modern" forms of experiment. The former, it is argued, relied on clear-cut separations between laboratory and society, facts and values, nature and culture. In contrast, the latter manifests itself as a "socio-technological experiment" (Latour) with no boundaries, "carried out in real time and in the scale of 1:1," thus retrospectively changing our perspective on the seemingly modern form of experiment.

Such distinctions of terminological, systematic, and chronological aspects concerning experiment and experimentation are certainly helpful. But often they keep a marked distance from the materiality of experimental set-ups. How about taking materiality itself as a guide for discerning the shape of experiments? Take an example. On a laboratory bench a chronoscope, a fall apparatus, a telegraph key and a rheoscope gather as an assemblage as it was used to measure reaction times in human beings in the late 19th century. If one covered this assemblage with a blanket, the contour of a three dimensional body became visible. One may assume that such bodies emerged in the history of experimental sciences in great numbers. In fact, every research field – be it physiology, chemistry, or molecular biology – contributed to it. As a consequence, the shape of experiments depended on the skeletal and muscular systems of such bodies (instruments etc.) as well as on their systems of vessels (e.g. cables, tubes).

However, the question is whether or not the visual contour of such bodies suffices to grasp the shape of experiments. One might argue that experiments often include components that do not show up on the laboratory bench (such as energy sources, human observers, etc.), or that they are so flat that their exterior form is rather unspectacular (protocols, notes). In addition one could point out that forms are not just located on the level of visible bodies, but depend on networks of differences preceding visibility as such, as George Spencer Brown has suggested.

Experiments typically isolate, dissect, and disconnect phenomena and processes conceived of as "natural" in order to re-combine, associate, and vary these phenomena and processes. Single components of experimental set-ups become miniaturized and/or comprised, while others are extended and enlarged. Some processes undergo experimental acceleration, whereas others are subjected to deceleration. Is it possible to extract from such conjunctions and disjunctions specific kinds of "experimental syntheses" dominating the history of scientific practice over the past 200 years? Can we derive from historically specific associations of the heterogeneous something like a "material logic" of scientific facts?

Organizing experimentation

Laboratories are often described as factories. Recent studies on the history of experimentalization in the 19th and 20th centuries suggest that during this period a transition took place leading from single, manufacture-like experiments to massive, factory-like experimentation. In the 1840s, Berlin physiologist Emil Du Bois-Reymond conducted his pioneering trials on animal electricity in his own apartment. Du Bois-Reymond's goal was to carry out such experiments in larger scope. After many struggles with the Prussian administration, this led eventually to the construction the Berlin Institute for Physiology. This "factory" realized the much-desired extension of research activities, but at the same time broke with some of Du Bois-Reymond's ideals concerning experimentation. Instead of contributing to the formation of holistically cultivated researchers, the fully developed physiology plant merely produced scientific facts "by the dozen." This, however, did not prevent the Berlin Institute from quickly acquiring the status of a model institution having a huge impact on the construction of other such sites, including, e.g., Pavlov's reflexological laboratory.

Besides the factory, Foucault's "panopticon" has been highlighted as another dominant form of scientific knowledge production, although in a slightly different context. The scientific practices dominating a late 19th-century observatory were marked by a disciplinary regime by means of which the interaction between astronomers, assistants, and instruments could be organized in such a way that mutual exchangeability of human observers was guaranteed. As Schaffer has suggested, this form of organization can be considered as a "panoptical regime:" astronomical observers were directed to follow a strict practice of seeing, resulting in a situation where the observers themselves would no longer have to be observed. One may assume that collective research practices in laboratories are to be regarded in a similar way: as a disciplinary regime that adapted human beings and other organisms to technological contrivances so that even the problems of adaptation became epistemologically relevant.

One wonders, however, about the exact results of inscribing the history of experiment and laboratory into the grand narratives of industrialization and disciplining. On the one hand, connections are established with the more general topics of these developments (economy, power, techniques of the body etc.). But what are the consequences for these topics? On the other hand, the rapid synthesis with the history of industrialization and disciplining tends to threaten the specificity of scientific activities. Is a laboratory really nothing else than a factory, a panoptical regime?

The experimental production of form

Experiments produce specific forms of time. Their components mutually interact according to specific time relations. In fact, laboratories can be seen as arcades traversed by energy sources, human beings, model organisms, data sheets, notes, and protocols combining with one another and then separating in order to gather again in different ways. The question as to what guarantees from within the cohesion of the heterogeneous components of an experiment thus leads to the problem of experimental temporality: the development of experimental set-ups, their series and sequences, the combination of repetition and difference.

In art history in particular, the problem of form has become linked to the problem of time. Understanding form as a dynamical organization resulting from mutual reactions between living bodies and their surroundings, Henri Focillon has highlighted the temporality of form with respect to artistic experiments. According to Focillon, forms in art result from experiments in which certain rules are followed and reasoning is combined with inference. Pointing to the famous example of gothic cathedrals, Focillon explains that their forms imply a specific kind of knowledge. Following Focillon, George Kubler has suggested conceiving of all man-made forms – sculptures, tools, or writing – as *aesthetic* forms and deciphering these forms in view of their specific temporality. To represent them, Kubler looks for and at series and sequences resulting from groupings of things and problems, implying that things are materialized attempts to solve problems. Here, another question emerges. The "shape of time" (Kubler) is not immediately given in the things themselves, but results from the work of the historian. The series and sequences into which he or she groups the forms of things and problems retrospectively alter the arrangements of things that were hitherto accepted. As a consequence, the historian changes even the forms themselves. Against this background, the history of experimentation might be read as a succession of shapes, the production of which sets in motion a cascade of retroactive re-shapings.

This historiographical reflection sheds some light on the history of scientific experimentation. When, in his investigations into physiological acoustics, Hermann von Helmholtz discovers a kind of tacit knowledge about hearing embedded or embodied in musical instruments, he is confronted with the experimental development of shapes. And it is not merely the history of instrument manufacture as an experimental practice that enters into play here. One might also refer to the experimental activities of Ernst Florens Friedrich Chladni, in which acoustical research combined with the construction of instruments. At the same time, forms of visualization – the famous Chladni sound figures – connected with forms of musical instruments, in so far as their construction and investigation was directed and supported by these figures. In other words, when Helmholtz started to explicate the tacit knowledge of musical practice, he re-wrote the history of music as a history of experimentation. But how far did this impact on his own experimental practice as a physiologist? And how did Helmholtz's research retrospectively change the grouping of scientific and artistic things? More generally: what are the consequences resulting from the forms of art as emerging from experimentation with respect to the shape of scientific experiments?

Perhaps the development of information technologies is to be understood in a similar sense: as an in-formation or formatting that retrospectively changed the grouping of scientific *and* artistic things. Is the development of the personal computer as a particular form to be described as a result of experimentation? Or is this form not as stable as it often appears to be? Have the corresponding experiments not yet come to an end? In any case, when a recent journal for "unusual sound sources" chooses "Experimental Music Instruments" as its title, this choice is not so much to be understood as the last chapter in the history of experimentation in instrument construction as its re-opening. Today, the explicit conception of art as experiment inserts itself into the interface between art, science, and technology that is currently known under the name of "Information Arts" (Stephen Wilson).

To sum up, one could say that the history of experimentation is to be written as a history of permanent re-shapings: this concerns the question of experimental set-ups and their development as well as the problem of representational modes (images, texts, etc.) that interact with the shape of experiments. What relation does the history of scientific experimentation entertain with experiments? What are the experiments of representation in history of science, and what are the experimental shapes of this discipline?

Welcome

Hans-Jörg Rheinberger

Dear participants, dear guests,
I would like to welcome you to the conference on "The Shape of Experiment," which does not stand alone and for itself, but is part of a larger enterprise. It is part of a Project on "The Experimentalization of Life" with which we had set ourselves the goal to understand and analyze configurations between science, technology, and art in the field of the life sciences from the nineteenth to the early twentieth century. The project was the first to be funded through the program "Key Themes in the Humanities" (Schlüsselthemen der Geisteswissenschaften) of the VolkswagenStiftung, and it has now been working for four and a half years. Four scientific coworkers – Sven Dierig, Peter Geimer, Julia Kursell, and Henning Schmidgen – and five doctoral students – Björn Brüsch, Philip Felsch, Kathrin Solhdju, Julia Voss, and Margarete Vöhringer – have been involved in the project over that period. Not the least intention of this meeting is to present some of the work they have been doing over these years – explorations of the ways in which different disciplines of the life sciences, the sciences of man, technological developments, urban spaces, and aesthetic movements came to interact in an open horizon that was not only created, but also sustained by experimentation. The project has thus and will continue to contribute to an analysis of the material culture of science. With its Virtual Laboratory <http//:vlp.mpiwg-berlin.mpg.de>, has also opened a space for further research in this area that will carry the idea of the project in the future.

As mentioned, this is not the first of these meetings. It started with a conference in December 2001 on "Experimental Cultures," and a series of annual workshops followed, one centered around issues of time and time measurement, another on the boundary between the living and the non-living, another on the interaction between science and crafts in Berlin, and yet another on the academic spaces of laboratory and seminar. Not to forget the exhibition "Apoll in the Lab" actually to be seen in the Museum for Medical History of the Charité which we plan to visit together tomorrow afternoon after the regular session. In addition, there were many smaller meetings with our project cooperation partners, in particular the Helmholtz Center for Cultural Techniques at the Humboldt University, the Bauhaus University Weimar, the Center for Literary Research in Berlin, and the Program in History of Science of Stanford University. The project is indebted to all of them. We would like to thank for all the input, and we hope to get fresh vistas from this meeting to continue our work on the history of experimentation.

With that, I turn over to Henning for the introduction of the Conference.

Introductory Remarks

Henning Schmidgen

To give you a little introduction to the topic of this conference, I would like to talk about ... a sewing machine. Not some ordinary sewing machine, but a special one, a wrapped sewing machine, hence a machine cut off from its regular function, but still highly productive – productive, for example, for providing some conceptual framework for this meeting.

The photograph reproduced on the conference program and poster was made by Man Ray in the early 1920s. Its title is *The Enigma of Isidore Ducasse*. Unfortunately, the original object that you can see in the photograph doesn't exist any more. But as we know from reproductions that were made under Man Ray's supervision in the early 1970s, the initial assemblage probably consisted of a sewing machine wrapped in a blanket and tied with rope.

The message of Man Ray's piece is more or less explicit. Isidore Ducasse is well known under his pen name of Comte de Lautréamont, and Lautréamont is quite famous for his surrealist definition of beauty: "Beautiful as the chance encounter of a sewing machine and an umbrella on a dissecting table."

Apparently, it is this encounter that Man Ray staged in his photograph, replacing the umbrella, however, with an envelope, a blanket and a rope, which in mysterious ways are evocative of sewing.

Perhaps it is not completely far-fetched to see one of the *leitmotivs* of recent studies in the history and sociology of science as a remote echo of Lautréamont's famous formula. In fact, several authors have dealt with the systematic combinations and chance encounters that happen on the laboratory benches of scientists – I mean combinations of or encounters between model organisms, instruments, inscription surfaces, concepts, and the like. In this connection much has been said about the multiple matter-cultures of science, about experimental systems, heterogeneous collectives and the "mangle of practice."

To be sure, the study of such multiplicities is not just motivated by surrealist aesthetics, but the objects of science itself sometimes seem to be surrealist, or more precisely: superreal. Using a crystal of DNA as an example, a historical epistemologist argued some 30 years ago: "It exists not as an artefact, but as a superreal [the French text says *surréal*] object, as a non-natural object, the project of considerable technical and theoretical labour." Perhaps a similar argument can be made with respect to the world of the Ultra Cold that Ian Hacking is going to introduce us to tonight.

"The material logic of experiment." That's the title of the first thematic focus of our conference. The meaning of this title can be clarified if we assume for a minute that "the technical and theoretical labour," which, according to Georges Canguilhem, makes possible the superreal objects of science, is nothing other than the systematically created encounters between organisms, machines and theories in the laboratory. Hence the following questions for the first part of the meeting: Are there rules or patterns that we can read out of the thickness of these encounters? Can the heterogeneous materiality of experimentation, the conjunction and disjunction of quite diverse components, be taken as a guide for discerning the shapes of experiments? What is at stake here is not simply distinguishing types of experiments, such as the demonstration experiment, the self experiment, the test, etc. What we are looking for is some sort of comparative morphology of experiments, based on close analyses of various machinic assemblages.

In a second step, I would like to ask you to step back a little and to forget about the superreal and the surrealists. After all, what we see is a wrapped sewing machine. As a technological object, this machine has its own history, a history connected to questions about such things as the advent of mechanization, the division of labour and the difference between working in a factory and at home. Moreover, the sewing machine is connected to other machines. Thus we enter a cotton factory, we see spinning machines and weaving machines. We learn something about power supplies, transmission belts, and machine tools. And above all we are confronted with questions of organization, of labour division and time management.

Along these lines, historians and sociologists of science have aimed at drawing comparisons between laboratories and factories. In fact, during the nineteenth century, science became quite a productive force in this sense, and some of the studies done within the framework of our project have convincingly shown the similarities between experimentalization and industrialization. One may wonder, however, about the exact consequences of inscribing the history of experiment into this grand narrative. Is the latter thereby enriched, perhaps even transformed? Or is it just a stable frame of reference? And what about the other, less industrial modes of experimentation, with their peculiar aesthetics and sometimes playful aspects on the one hand, and their relation to warfare and practices of punishment on the other? Questions such as these give you an idea of the second thematic focus of the meeting, “organizing experimentation.”

Now, let’s go back once more to the sewing machine and its remarkable activity: the assembling of parts of textiles, of leather and the like by means of needles and thread. Man Ray veils this activity, and he does so in a double sense: first, he covers the sewing machine with a blanket; second, he is not even presenting the wrapped object, but its photograph. Despite this double disguise, we encounter the act of sewing, one might even say, its history. Is not the pulling together of a blanket by means of ropes some primitive or preliminary form of sewing? Perhaps one could say: the photograph veils in order to make visible, it exhibits technology by covering it with its own archaic past.

A similar gesture seems to be involved in the third thematic focus of the meeting: “the experimental production of form.” Our starting point here is the rather blunt observation that experiments create forms: from specific optical and acoustic patterns, as they are used in physical and physiological research, to graphic representations of all kinds and new genres of writing and publication. More generally speaking, a whole branch of recent science and technology, the information sciences, is actively involved in the production and investigation of forms. The idea of the third focus of our meeting is to explore the ‘life of forms’ created by experiments and to feed the results of our exploration back into the analysis of the shape of experiment: from the outside to the inside, from representation back to production.

In 1924, Man Ray’s photograph was published in a remarkable context. It figured on the first page of the preface to the first issue of the famous magazine *La Révolution surréaliste*. The authors of the preface were Jacques André Boiffard, Paul Éluard and Roger Vitrac. After praising the power and poetics of dreams, they wrote: “Every discovery that changes nature, the destination of an object or a phenomena constitutes a surrealist fact.” If we are able, in the coming days, to make some “discoveries” concerning the shape of experiment, I have nothing against calling the outcome of this conference a surreal or perferably superreal fact. In other words, I wish us all a stimulating and productive scientific meeting.

Another New World Is Being Constructed Right Now: The Ultracold

Ian Hacking

INTRODUCTION

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INTRODUCTION

Last year, 2005, we commemorated Einstein for all the things that he invented a century ago. Twenty years after the annus mirabilis, he published a paper in the Proceedings of the Mathematics and Physics section of the Prussian Academy of Sciences.[1] It began to yield an incredible cornucopia of experimental results only in 1995. Even so, when I say in my title, 'right now', I mean exactly that. Two groups, one in Boulder, Colorado, and one at MIT, one in June and one in September, 1995, produced their first *Bose-Einstein Condensates* about as close to absolute zero as one can meaningfully get. I am less concerned with such recent history than the present. About 30 teams had made BEC by 2002, for instance in Hamburg in September of that year. By now the number will exceed 150. Two different laboratories at the University of Toronto succeeded on 16 March and 27 April 2005. It is tricky, it takes two or more years to set up the laboratory and get everything working right, but the process is becoming routine.

Why all the excitement? Leave aside the delight in achieving a technical triumph, and the vindication of a seventy-year-old research programme. Bose-Einstein condensation occurs at very cold temperatures, that is, within a nanokelvin of absolute zero. At 10^{-9} degrees above zero, atoms are very unenergetic. They do not move much. Hence it is possible to observe them and to manipulate them quite easily. We are able to have a more intimate interaction with the microworld than ever before. This break-through in experimental physics enables us not only to plan a whole new range of investigations, but also to recommence a lot of theoretical modelling which had become, if I may say so, rather moribund.

I intend to use the ultracold as a new source of illustrations for thinking about experimentation, and more general questions in the philosophy of the sciences. The questions are stated in the contexts of physics; many, although not all, have wider application. My presentation will be a little lop-sided, because I shall try to explain, in elementary terms, a few aspects of recent physics, and this will occupy as much space, I fear, as the philosophical inquiry. The paper is divided into three parts. I begin by over-stating some rather grand themes that are properly called 'philosophical' in a popular sense of the word. Then I shall sketch out a little of the physics, implicitly mentioning some matters of importance in the more technical philosophy of the experimental sciences. I shall conclude rather briefly by pointing explicitly to philosophical matters arising.

On a more personal note, Hans-Jörg Rheinberger, in his announcement for this meeting, kindly mentioned my *Representing and Intervening* of 1983 as a starting point for philosophical, historical and sociological work on experimentation. In fact many voices came to the fore in the 1980s, urging that we think about experiment as intensely as we had been thinking about theorizing. Mine only happened to be an early contribution. The second part of my book was, quite simply, 'a plea for experiment'. That is, a plea for considering experiments in their own right, and not merely as auxiliaries to theories. Since that work I turned my philosophical attention to other topics. So the present paper is, for me, a return to experiment. It was encouraged by an invitation to give the Carl Friedrich von Weisäcker lectures in the philosophy of physics later in

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[1] A. Einstein, 'Quantentheorie des einatomigen idealen Gases', *Sitzungsberichte, Preussische Akademie der Wissenschaften, Physikalisch-Mathematische Klasse* 22 (1924): 261-267, and 23 (1925): 3-14.

2005, but it began in February when I began attending lectures on Bose-Einstein condensation by Sandro Stringari of the University of Trento. He is perhaps the leading phenomenologist of the subject, that is, a theorist and modeller who gives close attention to the ways in which the models hook up with experimental possibilities. Last year he held the European Chair at the Collège de France, so I was able to follow his lectures. Before coming to Berlin for this presentation I received great hospitality from the laboratories directed by Rudolf Grimm at Innsbruck, one of the central places in Europe for this research, and in August I went to Eric Cornell's lab in Boulder. Everywhere physicists have received me with great generosity, a curious philosopher who asked stupid questions. At home in Toronto two new labs, run respectively by Aephraim Steinberg and Joseph Thywissen, have been enormously helpful, as has Alan Griffin, who did the theory of the subject in those long years when there were no experimental results. It has been a wonderful lesson in the possibilities of scientific and intellectual communication.

A LARGER PHILOSOPHICAL THEMES

A1 The last dichotomy

If you are thinking about how to distribute a number of things into a number of classifications, there is exactly one intuitive way to think about it: you imagine all the distinct arrangements of the individual things into a number of distinct boxes. You can imagine variations on the theme: distribute balls into boxes without making distinctions among the balls. Or distribute balls into boxes allowing only one ball in any box. And so on, many games can be played, of which these three may seem the simplest. The statistics of the first case is the way in which Maxwell and Boltzmann thought about ideal gases, and it is called Maxwell-Boltzmann statistics. The second case arose, as will be described later, when an obscure Indian mathematician was thinking about photons, and he wrote to Einstein, who picked up on the idea. We speak of Bose-Einstein statistics. That was 1924/5. Soon after it was realized that a third model was needed, and so we have Fermi-Dirac statistics, named after the two fundamental thinkers who saw their physical significance.

The first great textbook of probability that I read was Feller.[2] (Published over fifty years ago, it is still an excellent place to start taking probability seriously.) On page 5, volume 1, I read (in 1962):

> The appropriate, or 'natural' probability distribution seemed perfectly clear to everyone and had been accepted without hesitation by physicists. It turned out, however, that physical particles are not trained in human common sense, and the 'natural' (or Boltzmann) distribution had to be given up for the Einstein-Bose distribution in some cases, for the Fermi-Dirac distribution in others.

I commend the rest of the paragraph too, about the way in which what we were later to call 'intuition' is taught by the real world.

There are two basic kinds of things in the world, and they satisfy the two post-Boltzmannian statistics, after which we call them *bosons* and *fermions.* By now we know they are constituted by

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[2] William Feller, *An Introduction to Probability Theory and its Applications*, 2 vols., New York: Wiley, 1950.

aspects that seem more fundamental than their probability distributions, namely their spin, a concept invented after Bose statistics were devised. Nevertheless, just from the statistics, one sees that fermions are solitary – no two of them can ever go into the same energy state. That is a consequence of the Pauli Exclusion Principle, also a contribution later than Bose's. Bosons are gregarious, and indeed when they are cold enough – have low enough energy – lots of them can all go into the same lowest energy state.

It is just a historical accident, perhaps, that it was the statistics that started everything off. It just happens that the concept of a boson, and of the spin that distinguishes them, came after the statistics. But even today it has seemed natural for experimenters whose lives have been forged by bosons to say that they find the statistical difference utterly mysterious.[3]

In the last accounting, the world has exactly two kinds of things in it, bosons and fermions.

A2 Recollect Eddington's two tables: and then turn to macroscopic quantum phenomena

Perhaps the only two things that we recall about Arthur Eddington is that he supervised the international observations in 1919 that were the public confirmation of Einstein's general theory of relativity, and that he said there were two tables in front of him, the table of common sense, and the table of physics. Philosophers have enjoyed making fun of the second statement ever since. By now they are more likely to have heard it mentioned than to have read it. So let us recall what he said. The Gifford Lectures at the Scottish Universities have had remarkable speakers for well over a century. Eddington, who gave them 1926-7, understood that a Gifford Lecturer should address 'the problem of relating these purely physical discoveries to the wider aspects and interests of our human nature.' He began his introduction,

> I have settled down to the task of writing these lectures and have drawn up my chairs to the two tables.Two tables! Yes, there are duplicates of every object about me – two tables, two chairs, two pens.
> [...]
> One of them has been familiar to me from earliest years. It is the commonplace object of that environment I call the world. How shall I describe it? It has extension; it is comparatively permanent; it is coloured; above all it is substantial. [...]
> Table No. 2 is my scientific table. [...] My scientific table is mostly emptiness. Sparsely scattered in that emptiness are numerous electric charges rushing about with great speed; but their combined bulk amounts to less than a billionth of the bulk of the table itself.[4]

Most philosophers have thought that Eddington's worry is to be met by philosophical analysis. But it is also the case that there is a lot more physics now than there was in 1927, and that we have a lot more understanding of the layers of physics that lie between the microscopic table (as I shall call it) and the macroscopic one. Here is how these two terms are defined in Messiah's old but classic exposition of quantum mechanics:

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[3] Eric A. Cornell and Carl E. Wiemann, 'Bose-Einstein Condensation in a Dilute Gas: The First 70 Years and Some Recent Experiments', Nobel Lecture, 8 December 2001, <http://nobelprize.org/nobel_prizes/physics/laureates/2001/cornell-lecture.html>, p. 78.

[4] Arthur S. Eddington, *The Nature of the Physical World*, London: Macmillan, 1928, pp. xii-xiii.

> We define the 'microscopic' scale as the one of atomic or subatomic phenomena, where the lengths which enter into consideration are at most of the order of several angstroms (1Å = 10^{-8} cm). The 'macroscopic' scale is the one of phenomena observable with the naked eye or with the ordinary microscope, i.e. a resolution of the order of one micron (10^{-4} cm) at best.[5]

Eddington's lectures of 1926-7 are well aware of what was going on – remember, he gave them at exactly the time that the second quantum theory came into being. At that time and for a long time to come, the macroscopic and the microscopic were worlds apart, or at any rate, two worlds. Eddington's two tables were not some confusion caused by bad philosophical grammar. They were the state of physics.

No longer. In recent years there has evolved an entire field called *mesoscopic* physics, which is, of course, the physics of what goes on between these two edges, between 10^{-8} cm and 10^{-4} cm. Not much exists in an ordinary 'material' way in that gap between 10^{-8} and 10^{-4}. Mesophysics builds objects that fill it. Hence we might call it synthetic physics, by analogy with synthetic chemistry. It is an unusual and highly imaginative way to engage in nanoengineering, where that tends to refer to the scale above 10^{-9} metres, or 10^{-7} cm. Unlike nanoengineers, mesophysicists are less in the business of moving atoms around than of making new ones.

Eddington quite rightly felt a gap between the microscopic world, which we could only infer, and the macroscopic world, which we touch and press, as when we lean on Table 1. But there is now an amazing domain of human interaction and construction that lies between the two.

We nevertheless have a sense that quantum phenomena are essentially in the world of Table 2, both because they are microscopic, and because they need two descriptions, one particulate, and one in terms of waves. This is where Bose condensates come in. One of the more common phrases I encounter in conversation in the lab is, *macroscopic quantum phenomena*. This might seem to be a contradiction in terms, but it arises in the following way. Cold condensed bosons are ones that can all go into the same ground state. Normally a cloud of atoms consists of atoms in a great many states, each of which is itself a superposition. But in a Bose condensate, all the atoms are in the same state, so an observation on their average wave function is a macroscopic look at the quantum wave function, because it is the average of a lot of identical functions. When I was in Innsbruck I asked: What do you *mean* by a macroscopic quantum phenomenon? – A long pause, then – It is when I can *see* it.

In *Representing and Intervening* I devoted a chapter to the question, Do we see through a microscope? The most sensible answer, I argued, is 'Yes', although that requires cautious explanation. What we see through a microscope is, however, macroscopic in Messiah's definition of the term. So here we have a new question for philosophical analysis: Do we now, in certain circumstances, see quantum phenomena? I do not mean, do we see phenomena that can be explained only by quantum mechanics, for which the answer is plainly yes. It is rather the sense that we are seeing a quantum wave function, somehow in itself. Eddington's lectures, recall, were called *The Nature of the Physical World*. I suggest that the work I am describing leads to a new sense of the texture of the physical world and our experience of it. I suggest that the texture of the world,

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[5] Albert Messiah, *Quantum Mechanics*, New York: John Wiley & Sons, 1958, vol. I, p. 3.

which for Eddington was a duality of the physical world and the commonsense world, is increasingly becoming one world. *That* dichotomy is fading fast.

A3 Tabletop experiments

Let us turn to ordinary tables, companions to the wooden Table 1 on which Eddington placed his paper to write his lecture. (A table that, he said, was also a Table 2, for there were two tables ...)

When I wrote *Representing and Intervening* around 1980, the most visible physics was still Big Science, the label coined by Derek de Solla Price, and whose epitome was the Manhattan project. I was in Stanford at the time, and some of my friends worked at the Linear Accelerator. Big Science. I have in addition long followed another Stanford project, Gravity Probe B, the forty year programme for building a little laboratory in space to test the general theory of relativity. Its director is Francis Everitt, who first aroused my interest in experimental science; bits of a joint paper we wrote are labelled (E) in *Representing and Intervening*. Gravity Probe B may be laboratory science but it is big science. The gyroscope in space began taking the critical observations since April 2004, and concluded in August 2005. The announcement of results is planned for April 2007. In the publicity for this very expensive project, we read, '400+ physicists, 2100+ engineers, thousands of students ...' In short, Big Science.

In 1990 I organized an international workshop in Toronto called 'Tabletop Experiments'.[6] Little did I realize then, that fifteen years later I would be spending my time in laboratories where experiments are done on the tops of tables, very much Eddington's tables type 1, *very* solid tables. These are optical tables, poised on cushioned legs to guard against vibration. They are inscribed with high precision threaded holes at precisely regular intervals for screwing in lasers, lenses, split mirrors, and other bits of equipment. Quite expensive tables, I must admit; a high quality set of tabletop and legs costs about 10,000 euros. Many labs often have two, one with the lasers and lenses, a work of art that could well stand in any gallery of contemporary art – exquisite colours, too. On the second table are the atom trap, and various tools for inducing electrical and magnetic fields, more lasers, cameras, and so forth. The two will be connected by fibre optics that carries the coherent light made on the first table to the actual experiment on the second table. There will be other incomparably more messy tables, for the computers, coffee cups, note pads, pencils and doodles. It is of course one of the themes of the 'The Shape of Experiment' conference, to reflect on the material structures in, on, and with which experiments are conducted. More generally, previous work at the MPI has examined the architecture of laboratories. My architecture is, in a word, the table.

I am not the first to draw attention to tables in connection with Bose-Einstein condensation. The two men in Boulder who shared the 2001 Nobel Prize for making BEC were Carl Wiemann and Eric Cornell. Describing one of his early experiences when he moved to Boulder, Cornell

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[6] Pedantically subtitled 'Philosophical and Historiographic Questions about Small-Scale Experimentation in the Physical Sciences'. My 'Introduction' to *Scientific Practice: Theories and Stories of Doing Physics*, ed. by Jed Buchwald, Chicago: University of Chicago Press, 1995, pp. 1-9 – the book of the conference – explains my motivation. I had nothing to do with the book's title. It will be recalled that 'practice' was the buzzword for philosophy of science in those days, and doubtless the editor and publisher thought it would sell, while tabletops would not. But the conference was primarily about experiments done on the tops of tables.

writes, 'In contrast to the other laser cooling experiments I had seen, which took up the better part of a room, Carl [Wiemann]'s experiment could have fit on a card table.'[7] A typical BEC lab will have 6 people working in it – a director, one or two postdocs, one or two graduate students, one or two undergraduates, and the ever essential technical person who may share his skills with several labs in a larger unit. I shall mention labs that have a few more, while some of the best have only three people on deck.

The new world of the ultracold that is being made right now is being made by this or that handful of people. Of course they are, as our sociological friends will rightly insist, part of much larger institutional set-ups. They increasingly buy their equipment from instrument makers around the globe, starting with those optical tables. In the beginning they made much of their equipment, including some of their lasers but not their lenses and split mirrors, which come from optical supply companies. It is nevertheless true that that the node of each laboratory is 6 and often fewer people.

The exciting physics of today has, then, returned to the tabletop, where small is amazing. And 'tabletop' is 'in'. In a different but related field, Gérard Mourou has devised techniques to produce very high intensity lasers which, for a very short time, can produce energies at the level of CERN, on top of a table. He published a semi-popular paper a few years ago subtitled 'Physics of the Extreme on the Tabletop'.[8] He returned from the United States to lead a laboratory near Paris. *Le Monde* recently ran a story headlined 'The intensity of the laser will make matter gush out of the vacuum'.[9] Well, yes and no. He will get matter by having two beams of coherent light hit each other at enormous energy in a vacuum, and in that sense he will produce matter out of nothing (a quantum rabbit out of an empty hat, as it were). Until recently one needed all the majesty of CERN or of SLAC to produce those energies. Now it is done on the top of a table.

B A BRIEF HISTORY OF BOSE-EINSTEIN CONDENSATION

The following two tables may be useful for reference.

Absolute zero	0 Kelvin	- 273.15° Celsius
A millikelvin	1/1000 of a degree Celsius above 0 K	
A microkelvin	1/1,000,000 of a degree Celsius above 0 K	1μK
A nanokelvin	1/1,000,000,000 K (10^{-9} K)	1nK

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[7] Eric A. Cornell, 'Autobiography', < http://nobelprize.org/nobel_prizes/physics/laureates/2001/cornell-autobio.html>.

[8] Gérard Mourou, 'Ultrahigh-intensity lasers: Physics of the Extreme on the Tabletop', *Physics Today* 51/1 (1998): 22-28.

[9] Michel Alberganti, 'L'intensité du laser fera jaillir la matière du vide', *Le Monde,* 20 October 2005.

Here is a short historical summary of low temperature research:

1. Low temperature: Below 4 K.
   A century ago the Dutch physicist Kamerlingh Onnes (1853-1926) was able to produce such temperatures during his manufacture of liquid helium. In 1911, he showed that at these temperatures mercury has no electric resistance: It becomes a superconductor.
   (In 1986 Bednorz and Müller created ceramic near-crystals which were superconducting at 150 K, or -123°C.)

2. Very low temperature: Below 2.174 K.
   The Russian physicist Pyotr Kapitsa (1894-1984) showed in 1937 that Helium-4 is superfluid at this temperature.
   NB : The He-4 atom is a *boson.*
3. Extreme cold: Below 0.005 K.
   At this temperature Helium-3, a *fermion*, becomes superfluid. This superfluidity is a phenomenon very different from the superfluidity of He-4.
4. Ultracold: around 1 nanokelvin. Bose-Einstein condensation occurred in the ultracold – in 1995.

BEC = *Bose-Einstein Condensation* or *Bose-Einstein Condensate*
One often says simply, *Bose condensate.*

B1 Bose-Einstein condensate in the mind: Bose and Einstein 1924-5

Satyendra Nath Bose (1894-1974) taught physics at the newly founded and poorly funded University of Dacca. In 1924 he was trying to explain the concept of the photon to his students – the idea of the quantized little 'grains of matter' that Einstein, in 1905, had proposed to explain the photoelectric effect. By 1924 every active physicist had some conception of a photon, even if many, including Einstein himself, were sceptical of their very existence. That is, they had a merely instrumental view of the photon concept, as one which organized the data, without any commitment to the reality of photons. However useful the idea was, there were outstanding problems. No one could explain the precise observed values for the black-body radiation of photons. Bose's novel idea had a number of aspects, elegantly summarized by Abraham Pais.[10] I shall recall only one element that connects with my fascination with probabilities mentioned in § A1.

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[10] Abraham Pais, *'Subtle is the Lord ...': The Science and the Life of Albert Einstein*, Oxford: Clarendon, 1982, pp. 425-433.

Fifty years earlier, Boltzmann had described the probability with which a molecule of an ideal gas assumes a given state of energy. He took for granted that the molecules of the gas are always distinguishable. In particular, when two particles hit each other, it is always possible to say which one is which after the collision. That is the Maxwell-Boltzmann statistics. Bose proposed that photons do not satisfy this law. They are numerically distinct but indiscernible, violating Leibniz's principle of the Identity of Indiscernibles .The model they satisfy is that of distributing identical balls into boxes, without distinguishing between the balls. Using this model he could deduce the black-body spectrum for photons.

Bose wrote up his idea in English and apparently sent it to several physics journals, which rejected this work of an unknown Indian. He then sent it to Einstein. The great man was inundated with mail at this point in his career, but he read the article, saw what it meant, and had it translated and published in German.[11] He went on to ask what would happen if the atoms of an ideal gas obeyed the same statistics. The result was the paper cited in note 1 above. Bose's original article in English has disappeared, but the manuscript of Einstein's paper turned up in Leiden in August, 2005, and is now on view on the Internet.[12]

Einstein saw that something very weird would happen to such a gas at very low temperatures. When cooled, a gas becomes liquid and then solid: steam, water, ice, in our most familiar experience. But we get a different phase transition with a gas of cold bosons. A great many of the atoms will go into the same state of lowest energy, while the rest behave like an ideal gas whose distribution of energies is Gaussian.

Many of the concepts of modern physics did not exist yet in 1925. Two years later, George Uhlenbeck (1900-1988) introduced the essential concept of *spin*. In the beginning he really did think of the electron as spinning in one direction or another, and hence as having an additional degree of freedom. We have come to see the concept as far more rich and far more abstract. Spin is a quantum number that determines the kinetic energy of a particle. It can have only integral values (0, 1, or e.g. -3) or half integral values ($^1/_2$ or e.g. $2^1/_2$). Bosons, the gregarious entities that satisfy Bose-Einstein statistics, have integral spin, while fermions, the solitary ones, have half-integral spin. Very light particles like electrons tend to be fermions, while heavier ones such as protons are mostly bosons. But photons are also bosons. Atoms in a gas also have spin which varies according to isotopes. Except Beryllium, every element has an isotope with integral spin, atoms of which are therefore bosons. The other isotopes have half-integral spin and are therefore fermions. Hence my conceit that everything in the world is either a boson or a fermion. Or: half the things in the world disobey a weak form of Leibniz's principle of the Identity of Indiscernibles, and the other half obey a very strong form, the Pauli exclusion principle.

B2 Theory and Experiment: BEC and superfluidity

Sometimes theory precedes experiment, and sometimes experiments come before theory. In this respect two kinds of low temperature phenomena present a remarkable contrast. One was Bose-

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[11] Satyendra N. Bose, 'Plancks Gesetz und Lichtenquantenhypothese', *Zeitschrift für Physik* 26 (1924): 178-181.

[12] See <http://www.lorentz.leidenuniv.nl/history/Einstein_archive/Einstein_1925_manuscript/>.

Einstein condensation, where theory preceded its experimental confirmation for 70 years. The other was superconductivity, demonstrated by Kammerlingh Onnes in 1911, with no theory to predict or explain it. When Pyotr Kapitsa established the superfluidity of Helium-4 in 1937, there was no theory to predict that either. I do not want to say that theory was entirely absent throughout this history, but what was lacking was a microtheory to explain the phenomena.

Fritz London (1900-1954) did have a profound phenomenology of superconduction and superfluiditity. He was a rare physicist, a philosopher before he took up the serious study of physics. His doctoral dissertation was on Husserlian phenomenology. He was always anti-reductionist, but he did have a remarkable comprehension of superfluidity as a macroscopic effect produced by the quantum theory. London has become one of the heroes for students of BEC, because he saw from the beginning that Helium-4 was a boson, and that its superfluidity was connected with a Bose condensation phenomenon. But until 1957 no one had a microtheory of superfluidity. Since then we have had the BCS theory, named after John Bardeen, Leon Cooper, and John Schrieffer. Bardeen was the first physicist to receive 2 Nobel prizes, one for his contribution to the transistor, and the other, shared with Cooper and Schrieffer, for superfluidity. Their published account made no mention of BEC – which is one reason London remains a hero for the BEC community.

Thus, unlike BEC, the history of superconductivity and superfluidity is a history of experiment before theory from 1911 to 1957. And this relation continues: No theory explains a phenomenon known since 1986, high temperature superconductivity. In the case of BEC, there was only theory. There is a rich history of theorizing, especially in the Soviet Union, with men such as Lev Landau (1908-1968) and Nikolai Bogoliubov (1909-1992). We got a Bose condensate only in 1995. That required radical advances in cooling atoms, and the laser was the tool.

B3 Laser cooling: the 1980s

A hot bath gets cold because the most energetic water molecules evaporate, and so carry energy away with them. Atoms can be confined in a bath, or rather a trap, using electric and magnetic fields. Trapping technologies improved enormously in the 1970s, and tricks to speed up evaporative cooling followed suit. Very crudely, laser light is produced in a range of suitable frequencies, and this will excite some atoms to the point where they leave the trap, thereby cooling the remaining trapped atoms. Hydrogen was often the preferred element, and a number of teams were producing very cold hydrogen by evaporative cooling. Unfortunately, although this produced a whole range of new experimental skills, the apparatus became more and more complex. Increasingly attention was focussed on bosons of alkali metals, such as potassium, lithium, and, in the first successful experiment, rubidium. This was a choice encouraged by nature and society. Nature, because alkali metals have a single electron in the outer shell, and so they are easier to interfere with in a systematic way by using laser light of a frequency corresponding to the spectrum of the metal. Rubidium became the metal of choice, in many start-up labs, for a wholly social reason. The name rubidium, a substance identified by its spectrum in the middle of the nineteenth century, is derived from the Latin *rubidus* meaning dark red: its spectral lines are dark red. The cheap mass-produced lasers that are used in CD players and the like use dark red light, so one could buy cheap off-the-shelf lasers. If you want to work say with Calcium, with blue spectral lines, you still have to build your laser from scratch, as is being done for example in the laser physics laboratory in Hamburg by Andreas Hemmerich. (But his project is ambitious, to use BEC phenomena to produce as it were a laser of calcium atoms, that is, stimulated emission of a coherent wave of calcium atoms.)

How do you measure extremely cold temperatures in dilute gases? In essence, the standard is the ideal gas law, $PV = nRT$. One has atoms trapped by electrical and/or magnetic fields; turn off the fields, and the cloud of atoms will adiabatically expand. One takes a series of rapid 'photographs' of the expanding cloud, and the rate of expansion indicates the V and hence the T. Unless one has philosophical scruples, the photographs are just that, using devices very much like the digital cameras that millions of doting grandparents use for their children, or that even more millions of adolescents use on their mobile telephones. Laser light of suitable frequency is beamed through the cloud; a digital record of photons received is an absorption image of the cloud; successive images enable one to compute the velocities and hence the temperatures. As always I grossly oversimplify: there are intense workshops that debate exactly how to compute low temperatures.

In the mid 1980s there was a fundamental breakthrough: slowing atoms travelling in one direction by a beam of laser light of the right frequency, travelling in the opposite direction. This produced a variation on the Doppler Effect, whereby the relative velocity of the atoms was lowered, and hence they had less energy. Three laboratories shared the Nobel Prize of 1997 for their successful laser cooling: those of Claude Cohen-Tannoudji in Paris, Steven Chu in Stanford, and William Phillips at NIST in Gaithersburg. (NIST, the National Institute of Standards and Technology, is the former United States Bureau of Standards, of which there was always one main centre in Gaithersburg near Washington D.C. NIST now has a second, Western, centre, in Boulder, Colorado, in close collaboration with physicists at the University of Colorado.)

The story of laser cooling provides an interesting philosophical lesson. Paris and Stanford got Doppler cooling to work exactly as theoretical analysis had predicted. But the NIST group – part of an institution whose mission had always been the most precise measurement possible – was sceptical. They did not get values that agreed well with calculation. One thing that they noticed was that it made a difference where one placed the detector with which one observed the expanding cloud of atoms after the trap had been switched off. It is no mean feat to get all one's equipment in the neighbourhood of a tabletop. For convenience we humans tend to stack things up, so that we put the detector above the trap. But recall that these are very cold atoms! They have very little energy. They are lethargic. Normally gravity has no discernible effect on the motion of an atom. Remember Galileo: very cold atoms will fall, just like cannon balls. I have been told of one physicist who had been teaching his introductory mechanics course all his working life, but had never actually *used* the standard formula, $s = {}^{1}/_{2}\, at^2$, until he had to do computations involving falling atoms. Lesson: it will make a difference to your measurements whether you put your detector above or below the expanding cloud of atoms, because the atoms below a detector will be accelerating away from it, and those below one will be accelerating towards it – all in almost free fall.

The upshot was a surprise: experimenters routinely assume that their apparatus is not going to work as well as it ought to. But Doppler cooling worked much better than it should have. The groups of Chu and Cohen-Tannoudji had been content with results that pretty well fit predictions, but more delicate measuring showed that the cooling was much better than predicted. This is not a story one finds presented in this way in the printed record; I owe it to Paul Lett of NIST, who worked in Phillips' team.[13] Back to the drawing board: Cohen-Tannoudji and others realized there was another phenomenon at work, Sisyphus cooling. The name is apt. One has the picture of the beam of coherent light being like a washboard or other corrugated surface. An atom confronting the beam has to go up each corrugation, expending energy, and then do it all over again and again. That is the crude explanation of why laser cooling is so effective.

B4 Bose-Einstein condensation in the laboratory: Boulder and MIT, 1995

All the pieces were in place, and yet in 1994 Steven Chu was quoted as saying: 'I'm betting on nature to hide Bose condensation from us. The last 15 years she has been doing a great job.'[14] Nature unveiled herself a few months later. Victory went to the team with the simplest experiment and the simplest apparatus – that of Carl Wiemann and Eric Cornell, in June 1995, using rubidium-87.[15] This was published in *Science* with the note: received 26 June 1995, accepted 29 June 1995. Evidently the community was impatiently waiting! All simplicity is relative, but the Boulder group really did reverse the ever-increasing complexity of the hydrogen experiments. 'Previous laser traps involved expensive massive laser systems and large vacuum chambers for atomic beam precooling. [...] However in the first JILA [Boulder] magnetic trap experiment our lasers were simple diode lasers, the vacuum system was a small glass vapor cell, and the magnetic trap was just a few turns of wire wrapped around it. [...] If we wanted to modify our magnetic trap it only required a few hours winding and installing a new coil of wires. This was a dramatic contrast with the hydrogen experiments that, like all state of the art cyrogenic experiments, required an apparatus that was the better part of two stories, and the time to modify it was measured in (large) fractions of a year.'[16]

A few months later Wolfgang Ketterle's group at MIT succeeded with Sodium. In a photograph of the team taken in 1996, you see Ketterle and six others, which, as remarked in § A3, is a typical size for a BEC laboratory. A less typical scene dated 2001 shows 19 co-workers. To the first snapshot was added a photomontage of four men who were essential to the background of all this research. One of them was David Pritchard of MIT, who pioneered many of the techniques, and who is the veritable grandfather of Bose condensate in the laboratory.[17] Ketterle was his postdoc and Cornell his graduate student. But the network is more extended than that. The genealogical tree of *Doktorvaters* goes back to I. I. Rabi (1898-1988), who was awarded the Nobel Prize in 1944 for his work on the interactions between atoms and electric fields.[18]

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[13] One of the published papers on the NIST work is P. D. Lett, R. N. Watts, C. I. Westbrook, W. D. Phillips, P. L. Gould and H. J. Metcalf, 'Observation of atoms laser cooled below the Doppler limit', *Physical Review Letters* 61 (1988): 169-172.

[14] G. Taubes, 'Hot on the trail of a cold mystery', *Science* 265 (1994) p. 184-6.

[15] M. H. Anderson, J. R. Enscher, M. R. Mathews, C. E. Wiemann and E. A. Cornell, 'Observation of Bose-Einstein condensation in a dilute atomic vapor', *Science* 269 (14 July 1995): 198-201.

[16] Cornell and Wiemann, 'Bose-Einstein condensation' (see above, n. 3), p. 83, p. 87.

[17] For photographs, see Wolfgang Ketterle, 'When Atoms Behave as Waves: Bose-Einstein Condensation and the Atom Laser', Nobel Lecture 8 December 2001, <http://nobelprize.org/nobel_prizes/physics/laureates/2001/ketterle-lecture.html>, figures 16 and 22.

I have been unbelievably superficial, and given no idea of the intricacies of the apparatus and techniques, especially for evaporative cooling. There are excellent summaries of the innumerable tricks of the trade, of which the most useful may be Ketterle's.[19] There is a more philosophical question about the 'signature' of BEC, namely: What is taken to be the decisive sign that one has succeeded in producing a condensate? The phenomenon is metastable, which does not mean stable but rather unstable in the ordinary way of thinking. You can maintain it for a few seconds (as opposed to milliseconds) and at most (at present) for a few minutes. So how do you know that you have succeeded? How do you convince your colleagues, let alone your rivals, that you have produced Bose condensate in your laboratory?

In principle the proof is easy. Here is the idea. You have a cloud of atoms – bosons of one element – trapped in a vacuum, which you have cooled first by evaporative cooling, then by laser cooling and then by evaporative cooling again. Temperature is measured by the ideal gas law, $PV = RT$. You take a photograph of your condensate. Then you turn off the electric and magnetic traps. The gas now expands adiabatically, that is, at constant pressure. You wait say 8 milliseconds, and take a new photograph to see how much the gas has expanded and how far the bosons have travelled during that time. From that you deduce a distribution of velocities.

Needless to say, things are not that simple. For example, a photograph of the cloud is made by shining laser light through the cloud. That is very destructive: it shakes up the atoms, causes them to absorb photons, emit electrons, and whatnot. No matter how short the exposure, that destroys the structure in the cloud. As Heraclitus might have said, you cannot step into the same cloud twice. So you try to prepare many clouds under as similar conditions as possible. You photograph some of the clouds before expansion, some after an expansion of a given time, some after an expansion at a later time, and you assume that all the clouds to start with are pretty much the same, failing evidence to the contrary. Thus your final result will be based on averaging over very similar clouds at various stages in their life histories.

Let us go through the steps in the production of images in the early days, in 1995. Each digital 'snapshot' of the cloud of gas is a number attached to a pixel, which can then be turned into a two-dimensional display. Each number is the value of the charge that is induced by the light to which the pixel is exposed. But each such snapshot of a cloud of bosons is itself made from a composite of 3 exposures. In the first exposure, a laser beam is strobed over the cloud for a very few microseconds. The atoms absorb some energy, creating a shadow. The shadow is focussed on the back pane of the camera, and that is the first exposure, the *shadow* frame. Then all the atoms in the trap are 'dumped' – caused to exit by turning off the trapping fields. Shine the same strobed light through the trap, giving a *bright* frame. Finally make a third exposure with no laser, the *dark* frame: this is not wholly dark, thanks to the unwanted junk, such as leaks from the CCD array, or, who knows, daylight getting into the camera.

Next subtract the dark frame from both the bright frame and shadow frame. Finally combine these two frames, pixel by pixel, so that for example the spatial variation of the laser intensity will

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[18] *Ibid.*, figure 10.

[19] W. Ketterle, D. S. Durfee and D. M. Stamper-Kurn, 'Making, probing and understanding Bose-Einstein condensates', Number 9904034 v2 in the on-line archive of all condensed matter papers since April 1992, see <http://arxiv.org/abs/cond-mat/9904034>. For an entertaining popular explanation of the field, see the 'Atomic Lab' on-line at <http://www.colorado.edu/physics/2000/bec/>.

be cancelled out. If there were no atoms in front of a given pixel, the numerical reading ought to be 1.0, no absorption. Otherwise the intensity on the pixel is the result of a column of light passing through a distance. The light is decaying exponentially as it gets absorbed by atoms. One wants a quantity that is linear along each column, and this is represented for each pixel by the negative of natural logarithm of the measured value at that pixel. Thus the absorption pattern is turned into what can be called the *optical depth* frame.

That gives a picture of one cloud in one state. Condensates are repeatedly prepared: nowadays an established lab does this almost wholly automatically, and produces a condensate every two minutes. It is important that the digital information can be very quickly turned into a visual presentation. The computation is so quick that the results are available in the minutes before the next condensate is produced. If a human operator can take in the information at a glance, she can immediately use it for tuning the experiment for the next condensation. Hence the digital information must be transformed into a picture that the human eye and hand can immediately understand.

Successive clouds are produced, some photographed as produced, and others photographed at various times after the trap has been turned off. The clouds expand, and assuming that your preparation procedure is constant with constant results, you can deduce the velocity, and hence the energy, of the atoms. If you really have a BEC, you will have a bimodal distribution of energies. One lot of atoms will be in the lowest energy state, while the energies of the rest will be thermal with a Gaussian distribution. Elementary computer programming enables you to represent the result in all sorts of artificial images that make the point most clearly. The first lab to succeed turned the data into what has become the trademark of BEC. There are three images of the distribution of atom velocities in false colours, with the height of the graph indicating density and the colours indicating velocities. We see this transformation of the data (a) before BEC appears, (b) just as the condensation is starting, and (c) after a lot more evaporative cooling, when there is a peak of low velocities, surrounded by a vaguely Gaussian-looking distribution of higher velocities (see figure 1). This sequence is not something that one observed in a single condensate as it got colder – for as we have seen, every single snapshot destroys the structure of a condensate. It is rather a composite representation, a representation of what a single condensate does, but derived from the behaviour of a succession of many condensates.

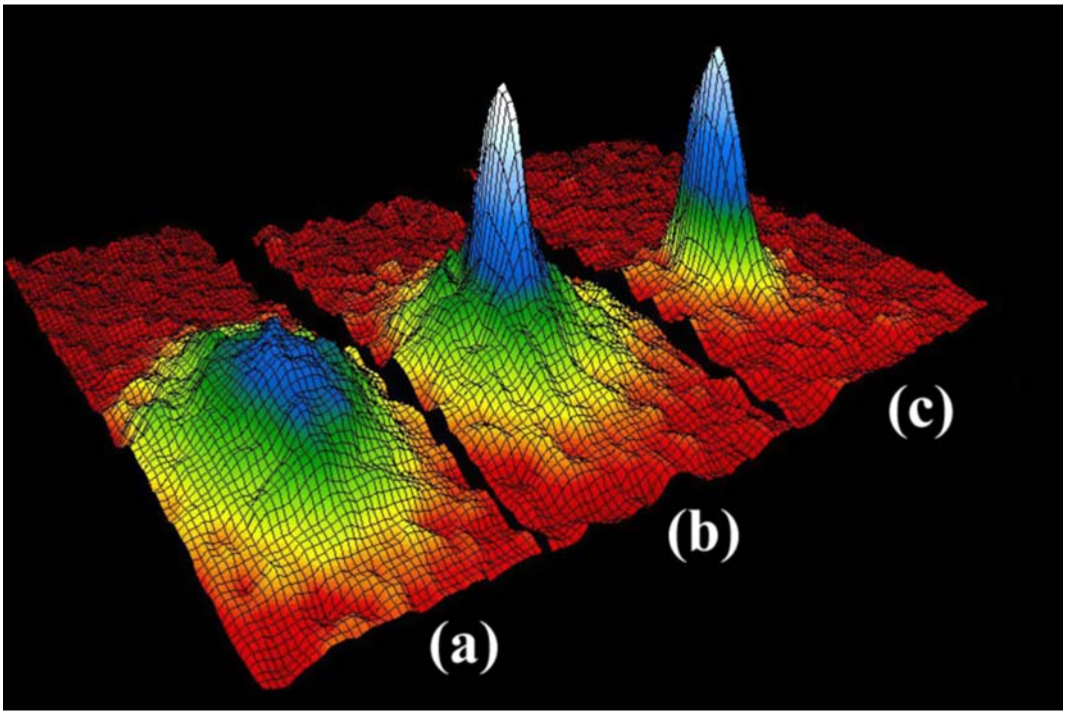

Fig. 1: Velocity distributions of the cloud of Rb atoms: (a) just before BEC, (b) the appearance of BEC, and (c) nearly pure BEC.
(Source: <http://www.colorado.edu/physics/2000/bec/three_peaks.html>)

At least until recently, it seemed as if almost every lab wanting to show that it had produced Bose condensate, displayed its version of the Three Peaks. A question arises here. To what extent was the choice of images determined by the nature of things, and to what extent was it a mere historical accident that Boulder got there first and chose to present their results this way? We have a very neat example of a question that arises in the debates about so-called social constructionism in physics. Nature or Society? Here it is a very small question, but correspondingly precise. This is further discussed in § C4 below.

B5 The present

In the beginning there were two laboratories, Boulder and MIT. Then 4, 10, 16. In February 2005 when I took on this interest, I was told that some 40 groups could now produce Bose condensate. In June, 70. In September 2005, 100. There are now dozens of research programmes all over the world. It is the thing to do. I have met sceptics who think that too many people are chasing too few ideas. I do not think so. There will be plenty of questions for everyone. These cold atoms are an open door to the manipulations and hence the understanding of atoms on an entirely new scale.

The analogy is the old explorers. A continent, new to the explorers, is found. They explore, establish colonies, find minerals to exploit, take plants home (and mostly kill off the original inhabitants). With the exception of the last clause, it is pretty much like that with Bose condensate. To begin with there are variations on the original theme, even with the workhorse isotope, Rubidium-87. Try to make a more long-lived condensate, with a far greater density of cold atoms, or on the other hand with very few. The Boulder condensates began with about 2000 atoms. Now rubidium condensates are ten thousand times larger. But also one plays with extremely dilute gases, what is called number-squeezing.

One very active field is cold fermions, partly because of the analogy with Cooper pairs (of electrons, which are fermions) that are at the heart of the Bardeen-Cooper-Schrieffer theory of superfluidity. Fermions, recall, have half-integral spin. You cannot make a BEC of fermions, but a pair of fermions is a boson, since spins are additive. So a gas of paired fermions should turn into a Bose condensate. Deborah Jin, a former student of Cornell's, was able to do this in 2003.[20] Is a condensate of fermion-pairs a superfluid? Everyone thought so, but the proof was left to MIT, using Lithium-6, in June, 2005. Because there is no friction, a superfluid condensate should produce a regular lattice of vortices on its surface.[21] Rudolf Grimm, director of the BEC laboratories at Innsbruck, hailed this result as a quantum revolution, comparable to the first production of Bose condensate in 1995.[22] It has also been observed that there is a pun here, that the quantum revolution is the spinning of the vortices! At any rate Grimm's enthusiasm knew no bounds:

> The spectacular observation of vortices in a Fermi gas heralds the advent of a new era of research reaching far beyond Bose-Einstein condensation. As an immediate experimental step, interfering light fields can be used to simulate a crystal lattice, providing a unique tool for solving problems in condensed matter physics. [I return to this at § C5.] And the amazing level of control demonstrated in the work [at MIT] can be extended to more sophisticated systems – mixed Fermi systems could be used to simulate a nucleus of protons and neutrons, or exotic semiconductors. This final proof of superfluidity in a Fermi system opens fantastic new prospects for many different fields of many-body quantum physics.

Recall the Bardeen-Cooper-Schrieffer theory that explains superfluidity. At present one speaks of two regimes, BEC and BCS, and of the BEC-BCS crossover as the material conditions in which one is produced, become close to those in which the other is produced. The standard conjecture is that the same physics is involved in both cases. The analogies between Cooper pairs and pairs of fermionic atoms leads many investigators to think we are getting closer to understanding the foundation and nature of both types of phenomena.

Those matters touch the present heart of our subject. But there is no lack of more peripheral and more speculative work. I shall just mention four examples that I have recently encountered, not because they are the most important, but because they are rather varied, and range from the relatively routine clearing up of old problems to bizarre investigations of new topics.

(a) What happens when light passes through glass? Sounds like a simple enough question that ought to have been answered long ago, and indeed as soon as there were photons in physical theory – 1905 – there were accounts of what happens when a photon passes through a dielectric.

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[20] M. Greiner, C. A. Regal and D. S. Jin, 'Emergence of a molecular Bose-Einstein condensate from a Fermi gas', *Nature* 426 (4 December 2003): 537-540.

[21] M. W. Zweierlin, J. R. Abo-Schaer, A. Shirotzek, C. H. Schunk and W. Ketterle, 'Vortices and superfluidity in a strongly interacting Fermi gas', *Nature* 435 (23 June 2005): 1047-1051.

[22] Rudolf Grimm, 'Low-temperature physics: A quantum revolution', *Nature* 435 (23 June 2005): 1035-1037.

The trouble is that there have been two competing models in circulation ever since the first decade of the twentieth century: call it the *Abraham-Minkowski confrontation*. One is due to Einstein's teacher, Hermann Minkowski (1864-1909) and the other to Max Abraham (1875-1902). The latter analysis is in effect more complex than the former, and adds an extra physical effect. It has been proposed for some time that on Minkowski's account, a beam of photons passing through glass should produce a force acting against the direction of the beam, while if Abraham is right, the force should be in the direction of the beam. This has the virtue of being a qualitative effect, even if it is a subtle one that has thus far never been detected. Is the glass nudged to the left or the right? Paul Lett is undertaking an experiment at NIST using a Bose condensate as the dielectric. A positive outcome may be hoped for in a couple of years.

(b) Cold atoms have almost no energy so, as we have said, they fall just like cannon balls. If only we could free them from the effects of gravity, we could study them with less effect from that external influence. Their wave function would be even more readily observable, and we would have a more 'macroscopic' quantum phenomenon than ever before. So, why not try to make *BEC in a laboratory in space*? Such a programme is envisaged. The first step is to make more robust lasers that stand up well to free fall. So they are now being dropped 146 metres from the Bremen *ZARM-Fallturm*, where they are in a controlled and measured state of free fall for 4.7 seconds. The dropping tower in Bremen can do three drops a day if the experimenter's measuring equipment is up to it. One likes to think of it as being admired by Galileo's ghost.

(c) One of the most dramatic of quantum effects is quantum tunnelling. Electrons cross an energy barrier, now they are on one side of it, now they are on the other. It is a well-established phenomenon that follows directly from theory, and yet it remains in a way wholly mysterious. What *does* happen in that instant when something 'tunnels' through a barrier? It is not obvious that the question makes sense, for we may be simply making a macroscopic demand – asking a question that makes macroscopic sense – in a domain for which it is in principle inappropriate. But everything happens so slowly in the ultracold, that one can ask what *ultracold quantum tunnelling* looks like. This is a project that Aephraim Steinberg hopes to undertake in the quantum optics laboratory at the University of Toronto.

(d) To conclude with something more mainstream, the laser, what is now the common-or-garden laser, but would never be taken for granted, produces an intense beam of coherent light which is an extraordinary tool for endless applications. For some years now there have been what, by a dubious analogy, are often called '*atom lasers*'. These produce beams of cold atoms going in exactly the same direction with exactly the same energy. These are developed at NIST for its traditional role, metrology. One envisages exact measurements based on interference patterns using these highly collimated beams of atoms.

C MORE LOCAL PHILOSOPHICAL THEMES

C1 Experiment and theory

§ B2 noted how BEC and superfluidity between them illustrate opposite sides of what philosophers have sometimes called the inductivist/deductivist divide. Deductivists think that theory always comes first, inductivists that observation and experiment always come first. BEC is a fine case for deductivism: seventy years of rich theorizing before any experimental confirmation worked. Superfluidity and superconductivity are great examples for the inductivist: decades of phenomena and phenomenology before the BCS theory of 1957, and even now high-temperature superconductivity is a fact of the laboratory and potentially of industry, that no theory well explains.

Philosophers of science tend to write as if 'the' relation between theory and experiment was a timeless aspect of the scientific endeavour. Philosophical accounts tend to reflect the state of science at the time the accounts are given. I love the passage from Humphry Davy (1778-1829) that I quoted at length in *R&I* (p. 152). It begins,

> The foundations of chemical philosophy are observation, experiment, and analogy. By observation, facts are distinctly observed and minutely impressed on the mind. By analogy, similar facts are connected. By experiment, new facts are discovered; and, in the progression of knowledge, observation, guided by analogy, leads to experiment, and analogy confirmed by experiment becomes scientific truth.

He wrote that at the beginning of his textbook on chemistry, published in 1812, fully a state-of-the art exposition. Incomparable lucidity and apt description – for the chemistry of his day. Turn to 1863 and Justus Liebig (1803-1873), an equally great chemist whose contributions to agriculture changed human civilization and refuted Malthus's gloomy doctrines about overpopulation. He, as quoted (on p. 153), was a Popper ahead of his time, 'in science, all investigation is deductive or *a priori*. Experiment is only an aid to thought, like a calculation: the thought must always necessarily precede it if it is to have any meaning'. In a single generation – Liebig was born 25 years after Davy – chemistry itself had been radically changed, first by the work of the French chemists with whom Liebig studied, and then by the upsurge of German science and technology. Liebig was saying that no matter how valuable are the applications of chemistry, we need ever deeper theories to generate the applications. Don't neglect to fund the theorist!

Popper was less motivated by the needs of research support than Liebig, but his *Logic of Scientific Discovery* furnished the epitome of all future deductivisms: 'the theoretician must long before have done his work [...] It is he who shows the experiment the way'. (Also quoted, p. 155.) Popper was fixated on the radical events of his youth, the theories of relativity and the quantum theories. The special theory of relativity was a model of pure thought; the general theory of relativity surpassed it, and pointed the way for Eddington's observers to go and look at the perihelion of mercury. So physics, *as Popper saw it*, made perfectly obvious that theory precedes experiment. But had he looked at another bit of Einstein's career in the *annus mirabilis*, 1905, he might have noticed that Einstein's photons (or grains of light, as he called them) were invoked to explain von Lenard's experimental results of 1901-3. Indeed the quantization of light, as opposed to matter, was something that Einstein did not, over the course of his life, even believe in. That has not infrequently been called a paradox of Einstein's philosophy, that he made the full quantum theory possible, but did not believe that theory was more than a temporary expedient. Maybe it was because photons were invoked almost *ad hoc* to explain an experimental phenomenon?

In the case of the photoelectric effect, Popper could still have had his way. The sceptical experimenter, in this case Robert Millikan, had to give way to the insight of the theorist. 'I spent ten years of my life testing the 1905 equation of Einstein, and contrary to all my expectations, I was compelled in 1915 to assert its unambiguous experimental verification in spite of its unreasonableness, since it seems to violate everything we know about the interference of light.'[23] The relation between theory and experiment in the case of the photoelectric effect is even more interesting than that. For there was always a minority who suspected that light does not have to be quantized, and that quantized matter combined with classical radiation theory could give a full account of the photoelectric effect and many other phenomena, perhaps all relevant phenomena known even into the 1980s. Indeed as Peter Milonni asserted in a lecture, Einstein's own paper relies far more on thermodynamic reasoning that on quantization.[24] Einstein had no confidence in the very existence of photons, and was contemptuous of people who professed to understand them. There was a persistent minority research programme of trying to explain known phenomena, such as the photoelectric effect, by quantized matter but using classical radiation theory. The wonderful iconoclast E. T. Jaynes worked out many of the details in the 1960s – when he was also, against the stream, proposing a thermodynamic foundation for Bayesian statistical inference.[25] Willis Lamb, who established the Lamb-shift for which he won the Nobel Prize, delivered a diatribe against photons at the end of his career: he called it simply 'Anti-photon'.[26] Could we have invented the laser without the idea of photons? Yes, replied Lamb in this talk, here is how. There is a more radical question that concerns us directly. Problems about photons and the black-body radiation prompted Bose's statistics, which in turn suggested Bose statistics for a dilute ideal gas, which in turn suggested Bose-Einstein condensation. Without photons, would we ever have got to Bose condensates?

A number of striking thoughts ensue. Suppose that there had been an early, non-quantized, classical solution to the questions posed in the early twentieth century. How would the quantum theory have evolved? Would we have had the new or second quantum theory of 1926-7? Is this a case in which we might have had an alternative or equally good physics, as is suggested by what I call the contingency thesis, as advocated by Andrew Pickering?[27] I quoted the science dictionary, which defined the photoelectric effect as the result of photons causing the emission of electrons. Well, the phenomenon would still be there, but we would not define it *that* way.

In this imagined turn of events, the phenomena would, it seems have been left unaffected by the change in theory. For example von Lenard's results would have persisted in any alternative theory, although they would have been understood differently. The way in which the energy of the emitted electrons depends on the frequency of the light, not its intensity, is a fact, together with the fact that the number of the electrons depends on the intensity, not the frequency. The facts would persist, although their explanation would change. This gives a new slant on how 'experiment has a life of its own'. Indeed it well illustrates Peter Galison's observation, in *How*

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[23] Robert A. Millikan, 'Albert Einstein on his seventieth birthday', *Reviews of Modern Physics* 21 (1949): 343. Millikan's original article was 'A direct photoelectric determination of Planck's *h*', *ibid.* 7 (1916): 355.

[24] Peter Milonni, 'An Einstein-Year Symposium on the Nature of Light', University of Toronto, 16 November 2006.

[25] The entire corpus of his published and unpublished papers is found on-line at the E. T. Jaynes website <http://bayes.wustl.edu/etj/etj.html>.

[26] W.E. Lamb, Jr., 'Anti-photon', *Applied Physics B* 60 (1995), 77-84.

[27] Ian Hacking, *The Social Construction of* ***What*** *?*, Cambridge, MA: Harvard University Press, 1999, p. 129.

Experiments End,[28] that theory, experiment and instrumentation can well proceed parallel to each other but at their own pace.

The example is slightly unnerving because von Lenard himself always said that the credit ascribed to Einstein, including the belated Nobel Prize of 1921 for photons, truly belonged to von Lenard. This was not just jealousy, for he thought that quantized photons just did not exist. (He was angry, as was his wont, even though he had received the prize in 1905 for his own experimental work.) Now imagine a physics where we got along without photons, and used only a revamped classical radiation theory. Such possible histories are unnerving because von Lenard rejected all of Einstein's discoveries not only on scientific grounds, which was not so unusual in the early days, but also on personal and racist grounds. He became an ardent Nazi, honoured as the Head of the organization of Aryan Physicists, dedicated to the extirpation of all Jewish science.

C2 A thesis due to Pierre Duhem made general

A century ago, in the course of his masterpiece on the philosophy of physics, Pierre Duhem presented his famous argument that an observation made with instruments can never refute a theory.[29] An astronomical theory predicts that a celestial phenomenon will be observed at a certain place and time. Using an appropriate telescope one sees nothing. Refutation! It is time to revise the theory? Not necessarily, one can always modify the current theory of how the telescope works.

This was an argument about physics. Duhem did not apply it to experimental physiology, for which Claude Bernard, author of the definitive French text on experimental medicine, was his model.[30] Bernard, Duhem argued, was in fairly direct contact with the phenomena. Indeed Duhem did not believe that his argument even applied to the chemistry of his day. Philosophers who appear not to have read Duhem now write about the Quine-Duhem thesis, but that is a confusion. Quine wrote about holism and the revisability of any part of a conceptual scheme in the light of a recalcitrant experience. He wrote about the *logical* possibility of saving any belief whatsoever. Duhem wrote about the possibility in real-life experimental physics of saving a hypothesis challenged by observations made using instruments.

Andrew Pickering and I have generalised Duhem's thesis.[31] You can modify the theory of how a particular type of telescope works, but you can also modify the instrument itself. I produced an inventory of three kinds of inhabitants of the laboratory, each in turn divided into five sorts. *Ideas* (including various levels of theory), *Things* (including different types of apparatus), and *Inscriptions* (including data and the analysis of data). The idea was that these all need to be brought into harmony with each other in order to have a stable investigation. You can, Popper-style, revise

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[28] Peter Galison, *How experiments End*, Chicago: University of Chicago Press, 1987.

[29] Pierre Duhem, *La Théorie physique, son objet et sa structure*, Paris: Chevalier & Rivière, 1906. 2nd edition, revised, 1914, facsimile edition Paris: Vrin, 1970. Translated as *The Aim and Structure of a Physical Theory*, Princeton: Princeton University Press, 1956.

[30] Claude Bernard, *Introduction a l'étude de la médecine expérimentale*, Paris: J. B. Baillière, 1865. Translated as *An introduction to the study of experimental medicine*, New York: Macmillan, 1927.

[31] Ian Hacking, 'The Self-Vindication of the Laboratory Sciences', in *Science as Practice and Culture*, ed. by Andrew Pickering, Chicago: University of Chicago Press, 1992, pp. 29-64; Andrew Pickering, *The Mangle of Practice*, Chicago: University of Chicago Press, 1995.

top level theories, yes. You can, Duhem-style, revise theories about how the apparatus works. But you can, as Pickering made clear with excellent examples, also modify the apparatus itself. In fact you can adapt any of my fifteen elements, and more. They are all what Pickering called *plastic resources* that are moulded to form the experimental-theoretical conclusions.

The history of laser cooling just described affords a splendid example. We begin with an arrangement of atom traps, lasers and so forth, but after the work of William Phillips, Paul Lett, and their team at NIST, we modified both the apparatus (moving the detectors around) *and* the theory of how it worked (the Sisyphus effect).

C3 On conceptual change: what is a molecule?

The name 'atom' has an elegant lineage through the French *atome* and Latin *atomus* – which also meant 'the twinkling of an eye' – back to ancient Greek. In contrast 'molecule' is modern. In 1674 the French *molécule* was explained as *partie très petite d'un corps*. It was used in discussion of the Cartesian philosophy. The *OED* cites an 1869 chemistry textbook in which atoms are distinguished from molecules, 'a group of atoms mechanically indivisible' but that distinction was not fully firm until the twentieth century. Molecules were groups of atoms held together by the chemical bond (to mimic the title of Linus Pauling's famous textbook).

I had thought of that usage as permanent, but no. One very active present field of research (noted in § B5) is on ions that are cold fermions. There is a strong analogy with Cooper pairs of another type of fermion, namely electrons. The bond in Cooper pairs is very weak, compared with the chemical bond. So too is the bond between pairs of fermions that form a boson – indeed the nature of this bond is not very well understood yet. It has become increasingly common for ultracold atomic physicists refer to Cooper pairs as molecules, and likewise for fermion pairs. Far from this being a merely extended usage, one hears the proposal that it is time to rethink the nature of the chemical bond. In 2003 Deborah Jin and her colleagues wrote that fermion pairs are molecules 'in the ordinary sense of the word'.[32] This was a change in heart, for in publications up to 2002 she had been resisting the idea, saying that fermion pairs should not be thought of as molecules.

Here we may have an elegant example of conceptual change going on before our eyes. The traditional discussions, provoked by the scintillating assertions of Paul Feyerabend and Thomas Kuhn, debated whether theory change produced conceptual change, not to mention meaning change. As is so often the case, looking at science in action gives us a richer perspective. We may at the moment be rethinking what molecules are. Yes, this may produce a new or revised concept of the molecule, if you prefer to talk that way, and think you have a good understanding of what it is for two related concepts to be the same, different, or modified. But such linguistic or logical playfulness should not make us turn away from the more complex issues of what is involved in what I have just called 'rethinking what a molecule is'. It involves, among other things, new models for what holds items of a certain size together. Such thinking has to be influenced by analogies with the profound concepts of strong and weak forces in subatomic physics.

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[32] Greiner, Regal and Jin, 'Emergence of a molecular Bose-Einstein condensate' (see above, n. 19).

C4 Signatures of BEC: written by nature or chosen by society?

During the 1990s there was a lot of racket about the social construction of scientific knowledge, including established physics. The Sokal affair led to pinnacles of passion. Aside from prejudice, polemics, and a parody of C.P. Snow's *Two Cultures*, serious philosophical questions were in play. I distinguished three legitimate 'sticking points' that separated constructionists from the traditional realist attitude of most physicists.[33] Each of the three is a version of an old philosophical chestnut for which there is no agreed resolution – for and against nominalism, for example. One reason why the parties to the constructionism debate could never agree, was that they were unwittingly hung up on philosophical issues that are never truly settled. Cold bosons furnish us with a more interesting case, in which a modest constructionism could be opposed to a modest realism, and where the question, being fairly sharp, and concerned with fairly (but not wholly) empirical matters, might be settled. Or rather, where the issue brings to light an interesting question about the proportions of nature and society that we might care to attribute to certain scientific facts and practices.

After 1995 there was a standard 'signature' of Bose-Einstein condensate, mentioned in § B4. To summarize what was said there, a cloud of bosons is trapped. Has condensation occurred? Turn off the traps and photograph the gas as it expands – or rather, average a series of photographs of successive condensates in various states of expansion. Deduce the energy of the atoms from the rate of adiabatic expansion. If Bose-Einstein condensation has occurred, some of the gas will be a condensate in lowest energy state, and the rest will be thermal with an ordinary Gaussian distribution of energies. The Boulder laboratory transformed the state into a vivid three-dimensional picture. A sequence of three images in false colours of atomic velocity distribution was produced. The height of the graph indicates density, colours indicate velocities. We 'see' the very cold gas (a) just before BEC appears, a pretty Gaussian looking curve, then (b) with a peak appearing above the curve, just as the condensation is starting, and (c) after a lot more evaporative cooling, when there is a peak of low velocities, surrounded by a Gaussian-looking distribution of higher velocities. Icy blue and then ice-white were the colours fittingly chosen for the slowest, coldest atoms. The 'Three Peaks' – albeit with different colour schemes – have become the standard image for a lab to display, often on-line, in order to announce that it has succeeded in making BEC. To what extent is the choice of this image a historical accident? To what extent is it imposed by 'natural' constraints?

The best short answer may be a remark made by Eric Cornell, in answer to this very question. '*There get to be standards of evidence, and standards of presentation, that grow up around a field*'.[34] The standards of evidence and presentation, with the Three Peaks as the criterion, did grow up very quickly. Before describing exactly how, two things need saying. In the early 1990s a great many technologies, using a variety of bosons, were being tried out in numerous cutting-edge

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[33] Ian Hacking, *The Social Construction of **What**?*, Cambridge, Mass.: Harvard University Press, 1999, chapter 3.

[34] Eric Cornell, in a long e-letter dated 1 August 2006, for which many thanks. I had asked him how 'inevitable' it had seemed to him, in early 1995, that this was the way to present results, and in retrospect how inevitable it seemed to him now, that this was the way to do it. Much of the information in the text comes from his answer to my query, and the occasional phrase or sentence in quotation marks is directly taken from his letter.

laboratories. The signature of a successful technology would depend to a great extent on the technology itself. Nevertheless in retrospect Cornell writes; 'From very early on in our project, Carl and I were convinced that we were looking for a central density peak to be our main signature for BEC.' This does not imply that the community as a whole had this expectation, but Cornell draws attention to a paper going back to 1987.[35] A diagram shows a density peak looking like a central bump, but that is partly an artefact of the projection being in logarithmic scale. That was a theoretical picture: the actual spatial resolution depended on being able to use cold alkalis. One may say: there was a widespread although by no means universal expectation of density peaks arising from some but not all technologies. To that extent, the final signature, if not exactly imposed by nature, was fairly robust across technologies.

Now let us continue the story of § B4 from the digital snapshots – already a distillate of three raw images – and on to the presentation of the results. Data analysis had transformed the photographs of clouds of condensed bosons into velocity distributions. In early 1995 the experiments were constrained by available technology that could be bought or, as Cornell puts it, 'scrounged'. Colour monitors did not cost much more than black and white, but ones that used other means of presenting data – shades of grey, say, that may in fact be better for representing two-dimensional information – were costly. So false colour was the medium of choice. In 1995 Cornell himself wrote the programme that turned the numerical data into real-time colour images on the monitor, although nowadays that is what graduate students do. The point to emphasize here is the need for a human being to make a snap decision about which parameters to change. It could be as simple as turning up the voltage, but there is not much time for deliberation. And one has to do this over and over again, 'without breaking stride'. Thus the images began as tools for perfecting the condensate. Later they became a way to presenting results.

The spring of 1995 was a time in Cornell's life when most people would be preoccupied by immediate family concerns, in this case involving more than one generation. But an announcement was planned, first for an International Conference on Laser Spectroscopy in Capri (which was to be the occasion when the newly married Cornells took their honeymoon) and then for the biennial BEC meeting in Strasbourg directly afterwards. No time for reflection: just get the pictures in order. For the monitor pictures, any old 'lurid' colour contrast would do, but now there was a question of choosing the colours that would best convey the information to an audience prepared to be sceptical. A presentation by contour lines, as on a topographical map for hikers, is hard to take in at once. Once again colours help us understand, so the model is more that of an atlas where increasing altitudes are displayed in different colours. Cornell prepared three images for Capri taken from the data acquisition images. These are the picture that appeared in midsummer.[36] This image does not have the three-dimensional look of the Three Peaks, but it does show the background thermodynamic situations as a circle which turns into an ellipse, as a condensate is produced. This was important, and part of what convinced the second audience, in Strasbourg, that Boulder had Bose condensate.

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[35] R. V. E. Lovelace and T. J. Tommila, 'Theory of Bose-Einstein condensation of atomic hydrogen in a dynamic trap', *Physics Reviews A* 35 (1987): 3597-3606.

[36] Figure 2 of the article in *Science* (14 July 1995), see note 26.

Why was the ellipse so important? Wiemann and Cornell emphasized, at the presentation, that they did not expect it, it was not what they thought they were looking for. Conventional philosophy of science, especially in the Popperian model, teaches that for an experimental result to be valid evidence, it must be predicted before the experiment takes place. So here is a counterexample: the ellipse was what convinced the audience in Strasbourg, precisely because it was not expected, but had an excellent explanation. It could not be cast aside as an artefact of the experiment. Or rather, it was an artefact of the experiment that proved to be decisive.

One asks: why should the condensate have an elliptical shape? And that is exactly the wrong question. One should ask: why should the thermal distribution be spherical? The point is that the trap itself is not isotropic – not the same in all directions. That is technically feasible, but in the early days there was absolutely no interest in doing that. Hence the atoms in lowest state would be distributed according to the anisotropies of the trap, and form a roughly elliptical pattern. But atoms in higher energy states would be moving randomly, and so would tend to lose the structure of the trap. Hence one sees an elliptical condensate surrounded by a circular thermal cloud.

After everyone had been convinced, there remained the more general question of how to display the results for a less savvy audience than the one gathered in Strasbourg. A graduate student, Michael Mathews, was now writing a programme to transform this flat picture into one that gave a three-dimensional impression. This does not add information, but enables human eyes and brains to process the information more quickly, especially if they are not the experimenters who have been living with these pictures day and night, but the critical audience of colleagues from around the world. In the first stages the colours were pretty gross. (The Three Peaks had colours chosen by chance from available programmes which in the end made them look distressingly phallic.) *Science* magazine asked if it could have a more dramatic picture for its end-of-the-year cover. For the first time some attention was paid to aesthetics.

The group liked the idea that the condensate itself would show at the top of the peaks in blue and white, as if it were ice. But this work was so much the topic-of-the-day in physics journalism, that other media wanted the picture. A new consideration entered. Who owned the picture in the *Science* article? The research team, or *Science*? If the research team, then that part of it created by those who work for the U.S. Federal Government, such as Cornell, cannot be copyright. It is by law in the public domain. But there were non-federal co-authors. *Physics Today* wanted an image *now*. So a new set of images with better colours was designed, copyrighted, and simultaneously made available free to anyone interested.

For the next decade the Three Peaks were the gold standard. The choice of colours was not always the same: Indeed the MIT group used different colours for their publication at the end of 1995. And there was a counterexample that proves the rule. It came from Hamburg, where Klaus Sengstock's team put their pictures on line on 23 September 2002. Why did you use different images, I asked. 'Because at the time we did not have a programme to transform our pictures into the canonical form. We wanted an immediate announcement and didn't have time to write the programme.' And Sengstock's pictures – in the form of a series of two dimensional coloured topographic maps – did have that increasingly elliptical core surrounded by the spherical form of the uncondensed thermal gas.

The use of the Three Peaks as proof of success by start-up labs might come to an end, not because of what is happening in that ever expanding group, but because of work done in the long established laboratories. § B5 mentioned all too briefly the current fundamental research on the so-called BEC-BSC crossover. The first success was by Cornell's former student, Debbie Jin working in Boulder, cited in note 19 above. The title, 'Emergence of a molecular Bose-Einstein condensate from a Fermi gas', itself indicates what had to be established: that a certain procedure leads from a gas described by the Bardeen-Cooper-Schrieffer analysis to a Bose condensate. The published results boldly show the false colour peaks to establish that this had been done. This paper was a great success indeed: the most cited paper in the field from then until the time of writing (August 2006). But the next major publication, the 'vortices' of the MIT lab cited in notes 20 and 21, no longer needed to show that one had BEC, but rather that one had superfluidity. There a two dimensional greyscale presentation conveyed the phenomenon better.

We are now in a position to address the general questions: To what extent is the choice of images a historical accident? To what extent are they imposed by 'natural' constraints? The question arises in many contexts. Phenomena or what we call effects are often associated with characteristic signatures. When fermion pairs condense, the (presumed) superfluidity will cause vortices to appear on straightforward photographs of the gas. This is regularly referred to as the *smoking gun* that establishes what is happening.[37]

It appears that the choice of a signal as definitive for a phenomenon is partly social, partly natural. Our models of the apparatus and of the phenomenon are produced in society, a community of experimenters, phenomenologists and theorists. That is trivial, and of course the community needs money and less material incentives. Often social construction theses amount to little more than an ideological gloss on such trivia. When I say that the choice of a signal is partly social I do not mean anything like that. It is rather that we choose a signal, and a way of formatting the signal (the colourful Three Peaks, the pretty symmetric arrays of vortices) so that it strikes the human eye. Between social and natural are also the choices of instruments, their history, their relative cost. That is partly a history of the evolution of the subject, but also a contingent history of particular laboratories with their own instrumental traditions. Every lab acquires skills at both building and using some kinds of apparatus, and is a klutz with others used to good effect elsewhere.

In his masterful study of high-energy physics, Peter Galison wrote of 'the "golden event": the single picture of such clarity and distinctness that it commands acceptance'.[38] He listed some famous ones, from Anderson's picture of the positron in 1932 to the 1970s picture of the single-electron neutral-current event in the 1970s. Note that golden events, in this way of speaking, are *pictures.* But increasingly images are malleable. The old confidence, that a photographic record tells it exactly as it is, has gone forever. The Boulder team translated their data into a striking series of images where one can see the peak (high density) of very unenergetic atoms (the part of the graph coloured blue). That this image, the Three Peaks, becomes cemented in the mind of the

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[37] Referring to the research in Boulder (see above, n. 31), *Physics World*, March 2004, writes under the headline, 'Fermionic first for condensates': '[...] the JILA result is the "smoking gun" of a fermionic condensate'.

[38] Peter Galison, *Image and Logic: A Material Culture of Microphysics*, Chicago: University of Chicago Press, 1997, p. 22.

community appears to a remarkable collaboration of the social and the physical. This in itself gives no incentive to anti-realism about the phenomenon of BEC. The phenomenon is real, and it produces the signal by which we recognize it. But the form in which that signal is made present to us, the extended community, is somewhat contingent on a tradition that is only a few years old, and which was inaugurated during 1995. A tradition that in retrospect seems inevitable. Thus we are furnished with a clear example of the contingent assuming the guise of the inevitable.

Thus far we are realists. But anti-realism is nigh. Not in the form of constructionism but of the old-fashioned positivism of Auguste Comte. We have the Three Peaks image. It is the colourful computer transformation of a series of digital photographs of adiabatic expansion taken every 8 milliseconds. We have the photographs of vortices indicating superfluid condensed fermion pairs, as published on the pages of *Nature*, 23 June 2005. The positivist says: that is all we have, various kinds of imaged data.

Now these images are not the phenomena of phenomenology, the private sensory experiences with which cognition starts. They are wholly public material entities, circulated by mass distribution on paper, and readily available for downloading from the Internet. I say that they are signals that tell us when a certain phenomenon has occurred. But a modern positivist – constructive empiricist – such as Bas van Fraassen will say that the phenomena are precisely these pictures. They are described in a theory-loaded way, which does not trouble him. We say, here is the peak of lowest-velocity atoms, or, here are the vortices of spinning fermion pairs, a veritable tornado. (The French use the same word for both, *tourbillon.*) But in the positivist or extreme empiricist doctrine, these are not signals that a cloud of gas has reached lowest energy, or that there are real, symmetrically arranged tornados of fermions in the cold gas. They are just – phenomena. In this case, public photographs. When I say, 'look at this vortex of fermions, it is strangely different from the one next to it', van Fraassen would say, I am using the language of theory in an economic way to talk about a picture. I am not implying that there is, in my little trap, a vortex of fermions.

There seems to me to be a fundamental difficulty with this approach to phenomena. The phenomena are not the images. The same phenomenon is exhibited by the two dimensional topographic display in which the Bose condensate is an ellipse, as the one that is exhibited in the Three Peaks. Perhaps one could argue that the phenomena are equivalence classes of images constructed by information technology out of an array of say 24 x 24 or 512 pixels. But why this equivalence class, if it is not because of its link to what the physicist would call the phenomenon, the condensate?

At *this* level of discussion, restricted to the barren dining halls of theory and observation, there is as usual nothing to choose between the positivist anti-realist and the physical realist. To repeat the proposal stated at the end of *R&I*, when we start to use 'atom lasers' (see § B5 (d)) derived from Bose-Einstein condensates say for measuring minute differences in the gravitational field in order to locate oil reserves underneath a desert, which cannot be detected by current geophysical engineering, then we are beginning to take for granted the reality of the condensates. Yes, that is one project of applied BEC technology, and yes, I have already said enough about those matters 25 years ago in 'the experimental argument for scientific realism'.[39]

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[39] Ian Hacking, *Representing and Intervening*, Cambridge: Cambridge University Press, 1983.

C5 On analogical research: From the cold laboratory to cold stars; From crystals to optical lattices and back again

'The foundations of chemical philosophy,' wrote Humphry Davy in 1802, 'are observation, experiment, and analogy.' Where philosophers of the 20th century wrote about induction, those of the 19th discussed analogy. A. A. Cournot dismissed induction as a mere matter of extrapolation, which, as Hume had shown, is a matter of custom and habit. Scientific reasoning, he taught, proceeds instead by analogy, which passed from the observation of relations between things to reasoned models of those relations.[40] During the twentieth century analogy almost disappeared from the philosophy of science, with the notable exception of Mary Hesse, who connected analogy with modelling, and thereby presaged the subsequent attention to models as opposed to theories.[41] Amusingly, her first chapters go back to an earlier era, for she began her book with an imaginary dialogue between two philosopher physicists, Pierre Duhem and Norman Campbell.

I would not argue that the same idea of analogy runs from Davy through Cournot to Hesse. Analogy surely means different things in different eras. There is however the fairly constant and core idea of similarity in some structural respects between things that are otherwise dissimilar, suggesting that they may be alike in other structural respects as well. Undoubtedly there is a tradition, albeit largely forgotten, of analogy in pure logic, but I am concerned with material analogies that lead on to further conjectured similarities of structure and organization. For a first example, there is the thought that the fermion pairs in the ultracold laboratory have a remarkable interest for nuclear astrophysics. 'Ordinary' luminous stars like our sun are mostly made up of protons – hydrogen atoms – that are turned into helium by nuclear fusion. The heat of the fusion stops these stars from collapsing: the thermal expansion and the gravitational attraction balance. Neutron stars such as white dwarfs are composed mainly of neutrons. Neutrons are fermions. Fermions resist being compressed too tightly. There is a rather cute reason for this. According to the Heisenberg uncertainty principle, severely constrained position entails great uncertainty in momentum. So most of the locally constrained fermions must be moving around very fast, which produces what is called the degeneracy pressure. This counteracts gravity and keeps the blob of neutrons from collapsing. Since we cannot experiment on white dwarfs, we have little direct evidence telling us how to model fermions under these conditions, but it is now widely speculated that they exist as fermion pairs forming a superfluid. In the laboratory we shall be able to determine experimentally, by manipulation of conditions, a great many of the properties of fermion pairs that we have hitherto been unable to model. Thus by analogy we may pass from a more thorough knowledge of the ultracold to an understanding of how and why neutron stars exist. And perhaps I was wrong about the phenomenon of BEC existing only in labs. Maybe there are pockets of Bose-Einstein condensate in neutron stars!

To take another example of analogical research, in § B5 we quoted Rudolf Grimm's reaction to a 2005 MIT result. 'As an immediate experimental step, interfering light fields can be used to

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[40] Antoine A. Cournot, *Essai sur les fondements de nos connaissances et sur les caractères de la critique philosophique*, 2 vols., Paris: Hachette, 1851, §46-§49.

[41] Mary Hesse, *Models and Analogies in Science*, London: Sheed and Ward, 1963 (expanded edition, University of Notre Dame Press, 1966).

simulate a crystal lattice, providing a unique tool for solving problems in condensed matter physics.' Here is what he had in mind. A crystal is a solid with a well defined geometrical form, characterised by a regular three-dimensional arrangement of atoms. A typical form is a three-dimensional lattice. An optical lattice can be thought of as an artificial 'crystal', composed not of atoms but, in a sense, of light. If you send two beams of laser light having the same frequency against each other in opposite directions, interference produces periodic dark and bright bands, which have half the wave length of the laser beam. Hence with three pairs of opposed lasers one can produce a lattice of points in three dimensions. Such a lattice can be used to trap atoms of cold quantum gases. The mathematics of such a lattice is simply taken over from a standard crystal lattice.

But now we can reverse the learning process. We took our knowledge of crystal mathematics and applied it to optical lattices. We can use easily manipulated optical lattices to investigate the structural properties of crystals that we cannot manipulate. This is really interesting. There is no good theory about high temperature superconductivity of certain artificial crystals, and no easy way to interfere with those crystals to find out more about them. So we can now by analogy transfer questions about these remarkable crystals to laboratory work on optical lattices.

These examples share a striking feature. It is more apparent in the case of crystals than in the case of neutron stars. We are no longer in the realm of *argument by analogy*. We have turned to what might be called *analogical research*, we investigate X, which is not susceptible to laboratory purification and manipulation, by an analogical substance Y, which is easily investigated in the laboratory. The essence of the laboratory is controlled interference and the production of new phenomena. When we cannot conveniently create a laboratory for asking one set of questions, we may be able to create the analogical laboratory where we pose parallel questions, and then see if the answers, by analogy, do not transfer back to the subject that in the first instance aroused our interest: neutron stars or high temperature superconductivity.

In conclusion: a beginning

What can philosophers learn from Bose-Einstein condensation? There are three kinds of lessons, which I shall call, with some presumption, *epistemological*, *socio-epistemological*, and *physical-metaphysical*. I suspect that many other experimental examples would teach the epistemological lessons just as well as BEC, but for the physical-metaphysical, Bose-Einstein has special merits.

1. Epistemological topics. *(a) Relations of theory and experiment.* A new conjecture: We have moved to a regime where, in physics, they are mutually inextricable. You cannot do experiment without detailed theoretical plotting of the possible outcomes. But also you cannot do theory without experiment telling you numerical values to incorporate in models. The latter is a fundamental observation for the current generation of students, who, following Rom Harré and Nancy Cartwright, study models as opposed to theories. § C1.

(b) Experimental realism continues to counter positivism and constructive empiricism. 'Experimental work provides the strongest evidence for scientific realism', or so I said in the final chapter of *Representing & Intervening.* I did not say that we are never entitled to assume scientific theories are true. Only that 'entities that in principle cannot be "observed" are regularly

manipulated to produce new phenomena and to investigate other aspects of nature,' – and that this is the strongest kind of evidence for the existence of the entities, without requiring any strong commitment to any particular theoretical model of the entity in question.

(c) Analogical research is an update on the old idea of argument from analogy. We do not just argue and infer using analogies, we manipulate items of one class to find out their behaviour, and then map that on to analogical items of another class. § C5.

(e) Theory-change, meaning-change. The old debates prompted by philosophical theories about incommensurability get replaced by real-life discussions, for example, about changes in the very concept of a molecule. § C3.

2. Socio-epistemological topics. *(a) Big science cedes to little science.* The norm in a physics laboratory becomes, once again, about 6 or 7 workers. § A3.

(b) Social construction issues get real. Rubidium 87 was the isotope of choice (i) because of its nature – because it is a boson with easily manipulated properties. And because of a social fact, (ii) red lasers were cheap. § B3.

(c) Nature and society. What determines the signatures of phenomena? § C4.

(d) The contingency thesis is social constructionism. Could we have got on without photons, using classical radiation theory plus the quantum mechanics of matter? In that allegedly alternative physics, would anyone have thought of Bose condensate, ever? § C1.

3. Physical-metaphysical topics. *(a) Leibniz's Identity of Indiscernibles as physics not logic.* Why do half the kinds of things in the universe, namely bosons, reject the principle, while the other half, fermions, obey a hyper-form of the same principle, namely Pauli's exclusion principle? § A1.

(b) Interaction. Every schoolchild is familiar with Einstein's equation connecting energy and matter. But the phenomena connecting light and matter, from the photoelectric effect on through the laser to the present state of experimental art brings home the way in which two intuitively different categories, light and matter, interact all the time.

(e) Do we see quantum phenomena? My colleague Serge Haroche insists that atoms and ions have ceased to be merely theoretical entities since he and his colleagues see small numbers of them in his ultracold laboratory[42] But now we want to go further. Have we changed forever the gap between the microscopic and the macroscopic? Have we ended the dichotomy between Eddington's two tables? There are now groups who claim to be working towards an ultracold version of Schrödinger's cat, which is in two quantum states at the same time. § A2.

To be continued: A longer version of this paper discusses further applications. Under 'epistemological', the way in which the 1960s idea of a scientific revolution should be replaced by the concept of surprise in experimental and theoretical physics. Under 'socio-epistemological', how apparatus becomes black-boxed. Under 'physical-metaphysical', the question of whether we create phenomena in the laboratory, or merely produce and purify them.

In short, serious reflection on current experiment continues to enlarge our philosophy of the sciences.

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[42] Serge Haroche, 'Vérité et réalité du monde quantique', in *La Vérité dans les sciences*, ed. by Jean-Pierre Changeux, Paris: Odile Jacob, 2003, pp. 93-108.

Material Experimental Traces

Ursula Klein

Introduction

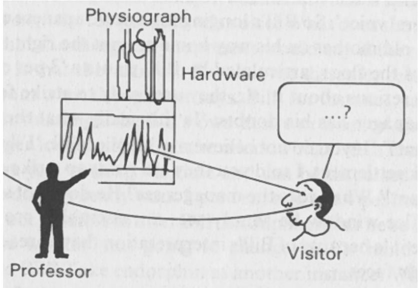

Fig. 1: Physiograph and inscriptions (after Latour, *Science in Action,* p. 71).

The drawing above nicely epitomizes our common understanding of experimental tracing. When experimenters study a scientific object they often use self-registering instruments, such as the physiograph. The physiograph, depicted here, consists of a set of electronic hardware and a glass chamber in which material transformations take place (such as the disturbance of the regular contraction of a piece of guinea pig gut by adding certain kinds of chemicals). The electronic hardware records the electronic pulses arising from these material transformations and transforms them into an inscription: the graph you see in the drawing. According to Bruno Latour the physiograph is an exemplary laboratory instrument: "I will call an instrument (or **inscription device**) any set-up, no matter what its size, nature, and cost, that provides a visual display of any sort in a scientific text. [...] the set-up provides an inscription that is used as the final layer in a scientific text."[1]

The view that laboratory instruments, as a rule, visually display experimental effects (in the case of instruments that are not self-registering) or provide pictures, counts or other kinds of inscriptions (in the case of self-registering instruments) has been largely accepted by historians, sociologists and philosophers of science, not only with respect to the twentieth and twenty-first centuries but also further back in history. I will argue in the following that the immediate experimental traces produced in the dominant laboratory science of the eighteenth and nineteenth centuries – chemistry – were neither transitory visual displays nor inscriptions but things: the kind of things you see in the jars depicted in figure 2. Chemists' material experimental

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[1] Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society*, Cambridge (Mass.): Harvard University Press, 1987, p. 68. Latour seems to equate laboratory instruments with "inscription devices." In the case of instruments that are not self-registering, such as a thermometer or eighteenth-century chemists' balances, this implies the claim that the experimenter's registering (or writing) is part of the inscription device: "A thermometer, a watch, a Geiger counter, all provide readings but are not considered as instruments as long as these readings are not used as the final layer of technical papers" (ibid.).

tracings via the production of chemical substances have consequences for our understanding of the process of representation – which in classical chemistry went hand in hand with material production – as well as for the technoscientific productivity of the laboratory sciences. The enormous technoscientific productivity of classical chemistry was to a large extent the unintended consequence of its mode of experimental tracing. I will present two examples of material experimental tracing in classical chemistry, the first one referring to chemical analysis in the 1780s, and the second one referring to studies of organic-chemical reactions in the 1830s. Both examples are typical of the interconnectedness of representation and material production in classical chemistry, that is, chemistry in the period from c. 1750 until c. 1950.

Fig. 2: Chemical substances (courtesy of *The Chemical Museum/Leeds University*).

1. Material experimental tracing I: the analysis of water

In winter 1783/84 Antoine-Laurent Lavoisier and Jean-Baptiste Meusnier performed an experiment on the chemical analysis of water that has been celebrated as a hallmark in the history of experimental sciences.[2] They inserted an iron gun barrel in an inclined position through a furnace, so that it was surrounded by burning coals (fig. 3). From a funnel connected to the elevated end of the barrel, they allowed water to flow slowly into the hot barrel. The lower end of

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[2] For the following account see Frederic L. Holmes, *Lavoisier and the Chemistry of Life: An Exploration of Scientific Creativity*, Madison: University of Wisconsin Press, 1985, pp. 211-13. See also James Riddick Partington, *A History of Chemistry*, 4 vols., New York: St Martin's Press, 1961-70, vol. III, pp. 445-447; Jean-Pierre Poirier, *Lavoisier: Chemist, Biologist, Economist*, Philadelphia: University of Pennsylvania Press, 1993, pp. 150-152.

the barrel was connected to a bell jar over water, where gases that developed during the experiment could be collected. (The spiral tube and bottle immediately attached to the barrel collected the water that had escaped decomposition.) In their experiment the two chemists found a large amount of gas which they identified as “inflammable air” or hydrogen. When they examined the iron gun barrel afterward, they further observed that its inner surface was corroded. Subsequent experiments led to the identification of the corroded material as a kind of iron calx or iron oxide. Based on the isolation and identification of the two reaction products, hydrogen and iron oxide, Lavoisier and Meusnier interpreted the experiment as follows: water was not a simple element, as had been assumed for centuries, but could be decomposed by the iron into hydrogen and oxygen; the hydrogen was collected in the bell jar, and the oxygen combined with the iron into iron oxide.[3]

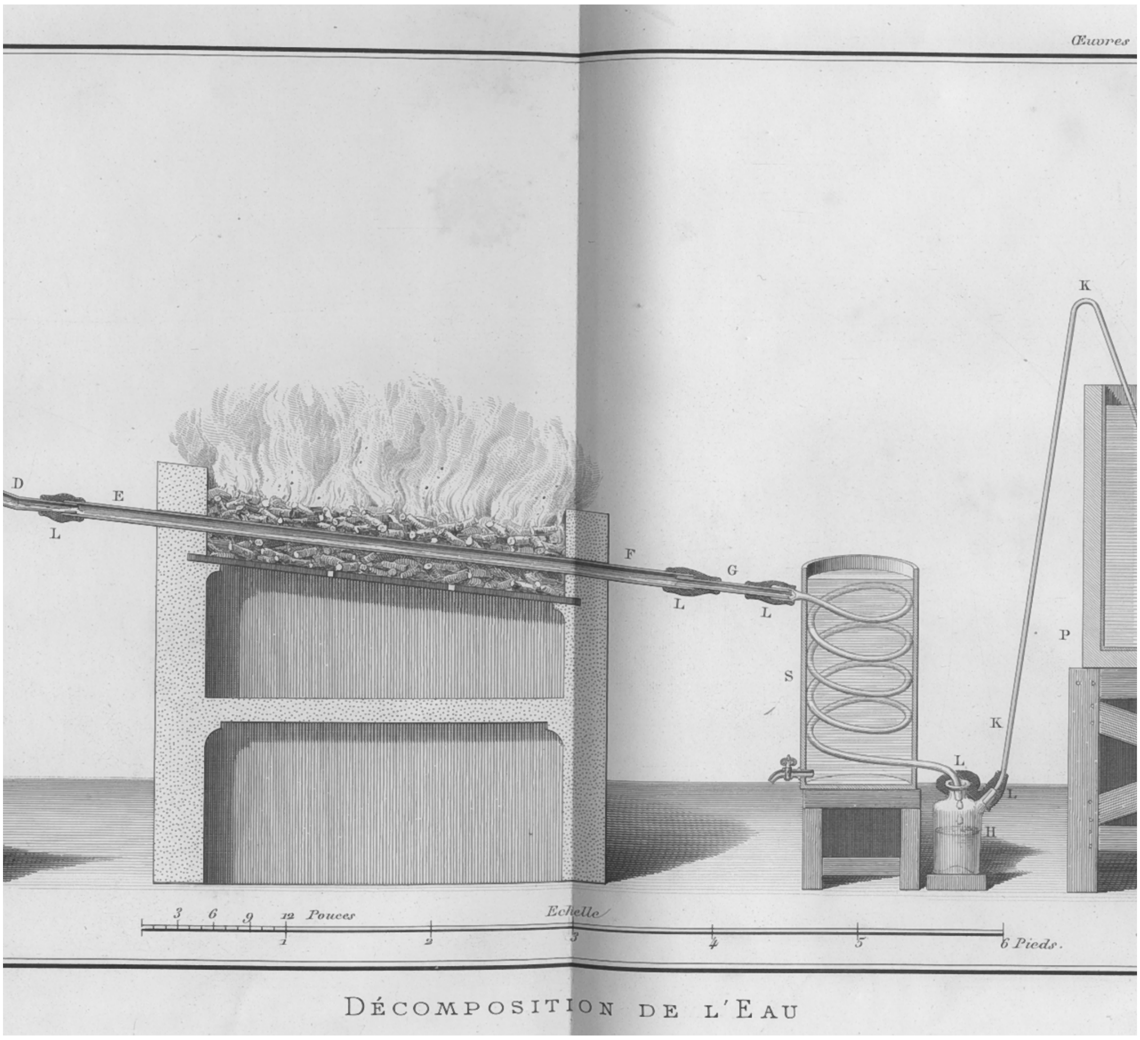

Fig. 3: Lavoisier and Meusnier’s apparatus for “gun barrel” experiments to decompose water (Lavoisier, Ouevres 2: plate 3).

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[3] See Antoine-Laurent Lavoisier, *Œuvres de Lavoisier publiées par les soins de son Excellence le Ministre de l’Instruction Publique*, 6 vols., Paris: Imprimerie Impériale, 1862-93, vol. 2, pp. 360-373.

In spring 1785, Lavoisier and Meusnier repeated the experiment in the presence of more than thirty witnesses, most of them members of a commission named by the Paris Academy of Sciences.[4] They then also supplemented the analysis of water by resynthesizing it from hydrogen and oxygen. Beginning in the middle of the eighteenth century, the resynthesis of a compound from its analytical products was a methodological requirement of chemical analysis that proved that the products of analysis were the true, that is, chemically untransformed, components of the original compound. Based on the analysis and resynthesis of water, Lavoisier and Meusnier finally proclaimed that "water is not an element, that it is on the contrary composed of two very distinct principles, the base of vital air and that of hydrogen gas; and that these two principles enter into an approximate relationship of 85 to 15 respectively."[5] Earlier in the eighteenth century, water had been considered a simple element, that is, one of the four Aristotelian elements. In the decades before 1783, chemists had already questioned two other philosophical elements: earth and air. They had subdivided earth and air into different kinds of earth and air. Now water, too, became deprived of its privileged ontological status.

Lavoisier and Meusnier's experiments ended with a publication, a text, and new analytical knowledge that contributed to the "chemical revolution." Yet inscriptions and knowledge were not the only results of the experiments. The immediate results and the first experimental traces that constituted the starting point for the final publication were materials: iron oxide and hydrogen. The chemical analysis of water actually took apart samples of water, thereby producing two substances: hydrogen and oxygen, which combined with iron. All interpretation of the experiment and all statements, numbers and other inscriptions finally published in a text relied on these two material reaction products, their physical isolation, their further experimental examination and their identification as specific kinds of substances and components. The two materials stood at the beginning of a chain of inscriptions or representations that began with registering names and weights of the reaction products and ended with a full-fledged text for publication, but the substances were themselves not inscriptions but material experimental traces.

My example is characteristic of chemical analysis in the entire period of classical chemistry from c. 1750-1950. The acquisition of knowledge about the imperceptible composition of chemical compounds and the representation of that imperceptible scientific object went hand in hand with material production: the products of the reaction were understood to be the chemical components of the analyzed substance or compounds containing such components. What does representation mean in this case? In his *Toward a History of Epistemic Things,* Hans-Jörg Rheinberger mentioned a meaning of "representation" that is expressed only in the German chemical term *Darstellung*. The *Darstellung* of a substance means both the representation of a chemical kind or species of a substance and its actual local production in the form of a sample of that kind. As Rheinberger observed, in the *Darstellung* of a chemical substance "the meaning of 'representation of' is gone, and instantiation in the sense of production of a particular substance has taken over."[6] If we distinguish between an individual sample of a substance, produced in a local experiment, and the general kind of substance that is instantiated, production did not

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[4] See Poirier, *Lavoisier*, pp. 150-152.

[5] Quoted after Poirier, *Lavoisier*, p. 151. See also Lavoisier, *Œuvres*, 5, pp. 320-334, p. 333.

[6] Hans-Jörg Rheinberger, *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube*, Standford: Standford University Press, 1997, p. 103.

perhaps totally "take over." In addition to production, individuation and identification of the sample are epistemic activities that cannot be reduced to production. But representation became inseparably tied to material production. In the analysis of water and in almost all classical chemical analyses from the middle of the eighteenth century until the middle of the twentieth century, when modern methods involving physical analytical apparatus and spectroscopy replaced the classical mode of chemical analysis, representation started with material production. As we will see below in my second example, the coupling of production and representation extended to most forms of experimental tracing in classical chemistry, even though in slightly different forms.

The interconnectedness of representation and material production is a characteristic feature of classical chemical experimentation, which is significant not only for the historical epistemology of experimentation and representation but also for our understanding of the relationship between the laboratory sciences, technology and society. The substances produced in chemical analysis and other kinds of classical chemical experiments were not merely experimental traces. Their use was not exhausted by their representational function. Rather, they also had a life of their own as material things. First, if they were hitherto unknown substances they immediately aroused chemists' curiosity and became new objects of inquiry whose chemical properties and potential for reaction were further explored. For example when Joseph Priestley analyzed the red calx of mercury in 1774, one of the two products of analysis created a sensation in the community of chemists that resulted in dozens of new experiments; this analytical product was dephlogisticated air, later renamed oxygen. Second, if the products of analysis were very reactive substances chemists applied them as reagents, that is, technical tools of experimentation, as in the case of new acids obtained by analyzing salts. Third, in the eighteenth century most of the products of analysis left the laboratory to become applied in technology and society. Lavoisier and Meusnier's analysis of water also exemplifies these technoscientific consequences of classical chemical tracing, as it "perfected a method for a large scale production of hydrogen to be used for inflating aerostats."[7]

In the eighteenth and early nineteenth centuries the technoscientific productivity of chemical experimental tracing played out mostly in chemical analysis. Especially in their analyses of plants and raw materials stemming from plants, chemists actively pursued dual goals: the acquisition of knowledge about the composition of plants and the production of materials used, in particular, as remedies, food, and dyestuffs. When, from the middle of the eighteenth century onward, the chemical analysis of plants concentrated on the separation of compound components of plants, scientific and pharmaceutical practices and goals became almost undistinguishable. Compound components of plants, such as fatty oils, essential or aromatic oils, extracts, resins, gums, wax, sugar, and essential salts, were obtained by expression, extraction with solvents, or wet distillation below the boiling point of water. Chemists and apothecaries considered all materials obtained by these analytical methods to be not only compound components of plants but also excellent chemical remedies. Hence, chemical analysis and analytical goals coincided with goals of pharmaceutical application. This dual technoscientific agenda of plant analysis continued well into the nineteenth century. Chemists' plant-chemical experiments focused on the extraction and examination of compound components of plants, which contributed to knowledge about the composition of plants and at the same time yielded applicable materials.[8]

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[7] Poirier, *Lavoisier*, p. 151.

2. Material experimental tracing II: the study of chemical constitution and reactions in early carbon chemistry

Now I come to my second example. During the nineteenth century, the number of pure chemical substances increased from approximately 800 around 1800 to approximately 100 000 around 1900.[9] This exponential growth of the number of pure chemical substances was caused mainly by the increase in production of *organic* substances. Whereas c. 1800 the number of pure organic compounds was fewer than one hundred, in 1872 Pierre E. Marcellin Berthelot already reported the impressive number of more than 10 000 pure organic compounds.[10] Some thirty years later, the number of pure organic compounds had increased to approximately 90 000. I argue that the chemists' mode of experimental tracing considerably contributed to their production of new organic compounds. In the following I will provide an example for this kind of experimental productivity, which stems from the 1830s, that is, from the period of the formation of the new experimental culture of organic or carbon chemistry. I will not go into details of the experiment itself, but summarize its most important results, focusing on the mode of experimental tracing.[11]

In 1832, the German chemists Friedrich Wöhler and Justus Liebig performed a series of experiments with the oil of bitter almonds, a natural organic material extracted from bitter almonds.[12] Their goal was to identify this specific natural plant substance clearly and to demarcate it from other kinds of vegetable oils via the examination of its elemental composition, chemical reactions, and chemical constitution. The two chemists had obtained a sample of oil of bitter almonds from their French friend and colleague Théophile Jules Pelouze. After first performing several experiments with solvents and reagents to test the purity of the oil, they subjected a sample of it to quantitative elemental analysis.[13] Having registered the analytical data and further transformed the data into a chemical formula, they then proceeded to the study of the chemical reactions and chemical constitution of the oil. Chemical "constitution" referred to the more

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[8] On the analysis of plants in the eighteenth century and the application of the analytical products, see Ursula Klein, "Shifting Ontologies, Changing Classification: Plant Materials from 1700 to 1830," *Studies in History and Philosophy of Science* 36/2 (2005), pp. 261-329.

[9] See Joachim Schummer, "Scientometric Studies on Chemistry I: the Exponential Growth of Chemical Substances, 1800-1995," *Scientometrics* 39/1 (1977), pp. 107-123.

[10] See Pierre Eugène Marcellin Berthelot, *Traité Elémentaire de Chimie Organique*, Paris: Dunod, 1872, p. V.

[11] For the formation of the experimental culture of organic chemistry in the period from the late 1820s until the early 1840s, see Ursula Klein, *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century.* Stanford: Stanford University Press, 2003.

[12] See Friedrich Wöhler and Justus Liebig, "Untersuchungen über das Radikal der Benzoesäure," *Annalen der Pharmacie* 3 (1832), pp. 249-282. In modern terminology, the main component contained in oil of bitter almonds is benzaldehyde.

[13] As it was broadly accepted at the time that organic substances consisted of the same kinds of elements – for the most part, carbon, hydrogen, and oxygen – the elemental analysis of the oil had to be performed in a quantitative way. Since Lavoisier, the overall method of quantitative analysis of organic compounds was to burn a weighed sample of the organic substance in a closed vessel and capture the combustion products carbonic acid (or carbon dioxide) and water, then weigh the combustion products, and finally calculate the composition of the organic substance, based on the comparison of its weight to the weights of the reaction products. Like Lavoisier and Meusnier's analysis of water, discussed above, elemental "analysis" of organic substances also meant the actual decomposition of the substance into its chemical components or reaction products of the components. But in the case of organic compounds, the same reaction products were almost always obtained, namely carbon dioxide and water. That is, this kind of analysis was not technoscientifically productive in the sense exemplified above.

compound components of a chemical substance. At the time, chemists assumed that chemical compounds that consisted of more than two simple elements were not composed directly of the simple elements but of more compound components made up of the simple elements. The method of studying the compound components of a substance consisted of studying its chemical reactions. Depending on what kind of reaction products chemists found in different reactions, they drew conclusions concerning the kind of compound components involved and how they were reorganized during the chemical reaction. Like the chemical analysis of the elemental composition of chemical compounds – exemplified above by Lavoisier and Meusnier's analysis of water – studies of chemical reactions and of the constitution of substances first yielded material experimental traces: reaction products.

In one of their studies of the chemical reactions of the oil of bitter almonds, Liebig and Wöhler used chlorine as a reagent. As a result of this experiment, they found two reaction products, one of which they could identify easily; it was hydrochloric acid, which was a very well-known substance. The second product, which seemed to be an unknown organic material, was more difficult to identify. The two chemists carefully isolated this product and then subjected it to quantitative elemental analysis. The analytical results confirmed their opinion that it was a novel organic compound that contained definite proportions of carbon, hydrogen, and oxygen – as most organic compounds did – and, much more surprisingly, also chlorine. They then transformed their analytical results into Berzelian formulae, which they used to compare the composition of the reaction products with that of the oil of bitter almonds. Based on this comparison, they gave the following explanation of the reaction:[14]

> Benzoyl hydrogen [oil of bitter almonds] consists of
> (14 C + 10 H + 2 O) + 2 H.
> Due to the effect of the chlorine, the 2 atoms of hydrogen combine with 2 atoms of chlorine to form hydrochloric acid, which is released. 2 atoms of chlorine take the place of this hydrogen, according to the following formula:
> (14 C + 10 H + 2 O) + 2 Cl.

Liebig and Wöhler's explanation of the reaction between oil of bitter almonds and chlorine consisted of a reconstruction of the regrouping of the components of oil of bitter almonds into the reaction products. This explanation went hand in hand with a statement about the constitution of oil of bitter almonds and of the organic reaction product. According to Liebig and Wöhler the two organic compounds consisted of two building blocks, one designated by the more complex partial formula written in parentheses – (14 C + 10 H + 2 O) – and the other one being hydrogen or chlorine, respectively. The two formulae visually represented the binarity of the constitution by separating the two components via parentheses and the plus sign. These formula models of binary constitution were coupled with a very simple, though at the time challenging, explanation of the reaction. The more complex building block (14 C + 10 H + 2 O) was preserved in the reaction, and only the second component of oil of bitter almonds, that is, hydrogen, was replaced by

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[14] See Liebig and Wöhler, *Radikal*, p. 263. For the chains of inscriptions constructed in this way and the role that was played by Berzelian chemical formulae see Klein, *Paper Tools*.

chlorine in such a way that two proportions or atoms of hydrogen were replaced by two proportions of chlorine; at the same time the replaced hydrogen combined with an equivalent proportion of chlorine into hydrochloric acid.

With this result, Liebig and Wöhler had reached their original goal, to identify the oil of bitter almonds clearly and to distinguish it from other kinds of organic oils. Oil of bitter almonds was benzoyl hydrogen, unambiguously denoted by its formula $(14\ C + 10\ H + 2\ O) + 2\ H$. The final result was an inscription, a chemical formula that represented both the elemental composition of oil of bitter almonds and its binary constitution. By contrast, the first experimental results were not inscriptions but rather substances: hydrochloric acid and the new organic reaction product "benzoyl chlorine." Seen from the perspective of the original goal of Liebig and Wöhler, the reaction product benzoyl chlorine was nothing more than an experimental trace that served to elucidate the reaction taking place between oil of bitter almonds and chlorine. Yet this new substance was almost as curious as Priestley's dephlogisticated air had been some sixty years earlier. A substance that appeared to be organic but contained an element, chlorine, which never occurred in natural organic compounds challenged the existing boundaries between the organic and inorganic realm. As this substance further was also enormously reactive, it was immediately subjected to further experiments. In other words, it became a new scientific object.

As a consequence, despite the fact that Liebig and Wöhler had achieved their original goal, they did not stop their experiments, but went on to study the properties and reactions of benzoyl chlorine. In so doing, they used a whole series of reaction partners, among them four metal compounds. In all experiments performed with these metal compounds, two reaction products were created, a metal chloride (which was well known) and a new kind of organic compound containing bromine, iodine, sulfur, or cyanogen, respectively. Based on their experimental results and the transformation of analytical data into chemical formulae, Liebig and Wöhler then proposed that in all of these reactions the chlorine contained in benzoyl chlorine was replaced by an equivalent proportion of bromine, iodine, sulfur, or cyanogen, which had been contained in the original metal compounds.[15] As a result, four new carbon compounds were created which consisted of the component "benzoyl," labeled by the formula $C14H10O2$, and bromine, iodine, sulfur, or cyanogen.[16]

At the end of their series of experiments, in an originally unintended and unforeseen way, Liebig and Wöhler had produced five new carbon compounds, of which three, benzoyl chlorine, bromine and iodine, were entirely novel types of compounds. They had started their experiments with a quite familiar natural plant material – the oil of bitter almonds – and had completed them with compounds that were totally unknown in nature. Unlike the vast majority of organic substances studied in plant and animal chemistry prior to c. 1830, carbon compounds containing chlorine, bromine and iodine had never been found in plants and animals. Furthermore, as chlorine, bromine and iodine were elements typically contained in inorganic compounds, the new carbon compounds blurred the existing distinction between the organic and the inorganic. Experiments like these contributed considerably to the formation of the new culture of organic or

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[15] Simultaneously, the chlorine set free in the reaction combined with the metal, contained in the original metal compound, and formed a metal chloride.

[16] Liebig and Wöhler, *Radikal*, p. 266.

carbon chemistry between the late 1820s and the early 1840, in which "organic" substances were no longer defined by natural origin but chemical composition.[17]

Like chemical analysis, studies of chemical reactions and chemical constitution first yielded substances – reaction products – as traces of the invisible objects of inquiry. These material experimental traces were submitted to quantitative analysis and then translated into inscriptions: first, into names and data reporting the qualitative and quantitative composition of the reaction products, then into chemical formulae, and finally into formula equations that balanced the weights of the original substances with the weights of the reaction products and further displayed the regrouping of components taking place in the chemical reaction. Like the products of chemical analysis, the products of other kinds of reactions, too, had a life of their own, independent of their signaling function. They were of interest to Liebig and Wöhler, and to all chemists of the time, not only as experimental traces of the imperceptible reaction process and the compound components of the substances involved in the reaction, but also for their own sake as new chemical materials. In particular, when a reaction product showed in tests of its chemical properties that it was highly reactive, the interest of chemists was aroused immediately. Further experiments would follow in which chemists studied the reactions of the new reaction product, by which new reaction products would be created, and so on. This type of "game" was potentially endless. Not only did each experimental investigation of a chemical reaction produce traces of the invisible reaction, but at the same time these traces were also material objects which spurred chemists' interest so that the entire game began anew. A large part of the thousands of new carbon compounds created in organic chemistry after 1830 were the result of this "material logic" of chemists' collective way of experimental tracing.

But there are also differences between my first and second example. Since the middle of the eighteenth century, chemists had accepted that chemical analysis required the actual separation and isolation of chemical components and, if feasible, the resynthesis of the original compound.[18] As we have seen in the example of the analysis of water, the component hydrogen was produced and isolated directly, whereas oxygen was isolated indirectly in the form of iron oxide, which yielded oxygen in subsequent experiments. The invisible scientific object – composition or components of water – was made visible by the *Darstellung* of the components. Production and representation of that invisible object converged when the produced samples of a substance were individuated and identified. Chemists then asserted that the material traces or products of the experiment "were" the components of water, for example, and they tried to prove that statement by resynthesizing the original compound from the isolated components.

In our second example, representation was also tied to material production, but the relation between production and representation was different in this case. First, there was no experimental production of the compound component designated "benzoyl." The existence of that stable building block was inferred from the formula model of the reaction.[19] In the new culture of carbon chemistry, work on paper with chemical formulae became an intrinsic part of the

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[17] See Klein, *Paper Tools.*

[18] Before the middle of the eighteenth century, chemists often drew conclusions about the presence of a distinctive type of components – the simple elements or principles – without having actually separated these components.

[19] It should be noted, with hindsight, that this inference is mistaken.

interpretation of chemical reactions and conclusions about the constitution of chemical compounds.[20] Chemists' modeling of a chemical reaction by means of formulae displayed possibilities of compound components and of their regroupings, which had to match experimental results but also implied an epistemic and semiotic surplus. Second, there was no *Darstellung*, that is, no convergence of production and representation, with respect to chemical processes such as chemical reactions. Rather, with respect to the latter type of imperceptible objects of inquiry in classical chemistry, the material experimental traces (the reaction products) were effects of that object of inquiry. They were, in the language of the philosopher Charles Sander Peirce "indices," that is, signs physically connected with their object.[21] Moreover, as these indices were incomplete in organic chemistry – chemists were not able to isolate all reaction products of an organic-chemical reaction and isolate them in a quantitative way – the use of chemical formulae as paper tools for modeling chemical reactions had to fill in the gaps left after experimental tracing.[22] In this way material experimental tracing became intertwined in a very distinctive way with work on paper. The representation of the imperceptible object of inquiry was completed only when a formula equation of the reaction had been constructed.

In order to explain what is going on here, I compare the entirety of the chemical equipment employed to produce and further analyze the material experimental traces of organic-chemical reactions (reaction products) with self-registering inscription devices, highlighted by Latour. A self-registering physiograph translates Peirceian indices – the electronic pulses stemming from the material transformation process that takes place in the glass chamber – into an inscription. Analogously, in the quantitative analysis of reaction products chemists weighed the components of reaction products and then registered the analytical data, that is, translated the indices into "symbols" (Peirce) or inscriptions. However, whereas in the case of the physiograph the inscription was complete, or taken to be complete, it was incomplete in the case of experimental studies of organic-chemical reactions. Chemical formulae had to substitute for the missing weights of the reaction products and the part of the initial substances that remained untransformed in the reaction. In this way chemical theory, embodied in chemical formulae, became implemented within representation on a very early stage.[23]

3. Conclusion: the shape of experiment

The type of experimental tracing described in my two examples continued well into the twentieth century, both in organic and inorganic chemistry. It characterized a long period of a chemical culture from c. 1750 to 1950 that may be designated "classical chemistry." New, alternative styles of chemical experimentation and experimental tracing gradually became accepted when novel types of physical instruments were introduced in the twentieth century.[24] In the 1930s

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[20] For further details see Klein, *Paper Tools*.

[21] See Charles Sanders Peirce, *Collected Papers of Charles Sanders Peirce*, 8 vols., Cambridge (Mass.): Harvard University Press, 1931-58, vol. 2, pp. 156-173. Likewise, the electronic pulses recorded by a physiograph were indices that then were transformed into signs or inscriptions. Yet unlike reaction products, these latter kinds of indices were ephemeral events, that is, they were not stabilized as technical objects.

[22] The problem is explained in more detail in Klein, *Paper Tools*, pp. 233-240.

[23] For the chemical theory embodied by chemical formulae, see Klein, *Paper Tools*, pp. 14-23.

spectroscopic analytical methods – infrared spectroscopy, X-ray diffraction, nuclear and paramagnetic resonance, mass spectroscopy – began to replace the traditional, productive analytical tools, both at scientific and industrial sites.[25] Some decades later, laser-based optical chemistry supplemented the traditional chemical mode of tracing of chemical reactions. These "instrumental revolutions" transformed the shape of chemical experiment, especially in the domain of analytical chemistry.[26] Spectrographs and spectrometers generated physical signals through physical interaction with a sample of a substance that became transformed into some form of inscriptions.[27] The immediate output was inscriptions rather than materials. In other words, the apparatus applied in analytical and preparative chemistry drifted apart.

In the 1940s spectrometry rooms that could compete with the equipment of big science became established in chemical institutes. "When one enters a modern analytical laboratory," the analytical chemist John Taylor observed in 1986, "one is surrounded by equipment so that the analyst may be dwarfed by the instruments at his or her command."[28] Precise temperature and humidity control and, most importantly, cleanliness became a major concern in these modern analytical laboratories. By contrast, the classical chemical laboratory was smelly, full of rotten vessels, corroded surfaces, and reagents all over the place. It was equipped with retorts, beakers, flasks, test tubes, pipets, filters, crystallization dishes, furnaces or Bunsen burners and so on (fig. 4). Devices to supply water and fuel were indispensable. Shelves and cabinets contained hundreds of bottles and jars filled with chemicals. Balances, thermometers, barometers, eudiometers and additional small-scale physical instruments were mostly kept separate from the chemicals.

Fig. 4: A mid-nineteenth-century chemical laboratory (from Morfit 1850, 24).[29]

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[24] See the papers contained in Peter J. T. Morris, *From Classical to Modern Chemistry: the Instrumental Revolution*, Cambridge: Royal Society of Chemistry, Science Museum, London, Chemical Heritage Foundation, 2002; Davis Baird, *Thing Knowledge: a Philosophy of Scientific Instruments*, Berkeley: University of California Press, 2004; John K. Taylor, "The Impact of Instrumentation on Analytical Chemistry," in: John T. Stock and Mary V. Orna (eds.), *The History and Preservation of Chemical Instrumentation*, Dordrecht: Reidel, 1986, pp. 1-10; Frederic A. White, *American Industrial Research Laboratories*, Washington: Public Affairs Press, 1961.

[25] See White, *Laboratories*, pp. 127-155.

[26] For the conceptionalization of that transition as an "instrumental revolution" see Morris, *From Classical to Modern Chemistry* and Baird, *Thing Knowledge*, p. 89.

[27] On the distinction between spectrometers and spectrographs see Baird, *Thing Knowledge*, pp. 70-80.

[28] See Taylor, *Instrumentation*, p. 1.

In the classical period of chemistry, analytical instruments and instruments applied in chemical preparation and synthesis of substances were the same type of instruments. Because analysis of elemental composition, constitution, molecular structure and reactions went hand in hand with the production of chemical substances, it was performed with the same type of vessels, instruments, and operations as chemical preparations and syntheses were. Chemists performed experiments for the most part on tables, placed under a fume hood. Their experiments made use of dissolutions, distillations and sublimations, precipitations, evaporations, crystallizations, both for chemical analysis and for preparation and synthesis. An experiment typically combined these different types of operations into a whole series of operative steps. The shape of experimentation in classical chemistry was conditioned by these common operations and the instruments necessary to perform them. The astonishing uniformity and stability of the laboratory equipment of classical chemistry and the overall shape of classical chemical experimentation went hand in hand with a way of experimental tracing that combined analysis and synthesis, production and representation. Moreover, the technoscientific productivity of classical chemistry, which was a major condition of its persistent interconnectedness with the arts and crafts and industry, was spurred to a considerable extent by that mode of material experimental tracing.

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[29] Campbell Morfit, *Chemical and Pharmaceutic Manipulations: A Manual of the Mechanical and Chemico-Mechanical Operations of the Laboratory*, London: Thomas Delf, 1850.

Balzac's Electromagnetic Alchemy in The Quest for the Absolute

John Tresch

1. Expanding the shape of experiment

In order to trace the shape of experiment, sometimes we have to consider other sites than those in which experiments physically take place. Scientific research draws on motivations which come from a wider culture; the meaning of the objects it investigates is forged not only in laboratories but also in philosophical and literary discourses aimed at various audiences. In addition, crucial historical layers contributing to the moral and metaphysical implications of both specific experiments and the "experimental life" are imposed by the popular press and artistic and literary representations of the sciences in a given place and time. These wider contours of experiment are particularly important if we are studying the physical science of the early nineteenth century and the fields which emerged from Laplacean studies of imponderable fluids. Merely internal analysis of these sciences would skew our attention away from what made these phenomena so interesting in the first place, and from the ontological – even metaphysical – significance of various experimenters' desire to move beyond Laplacean mechanism.

Nowhere is this as true as in the case of electromagnetism. The visible and material effects of electricity and magnetism can suggest the action of immaterial spirit; dynamic and dangerous, alive with potential, these phenomena have persistently drawn ideas and images from distant and neighboring fields into their atmospheres. Successful efforts to limit, define and operationalize them as scientific and technical objects have not managed to sever their symbolic, psychological, imaginary and religious associations. Such connections were strong in France in the 1820s and 30s, when the interactions between electricity and magnetism were definitively established and systematically explored by André-Marie Ampère, "the Newton of electrodynamics." In 1820, François Arago announced the discovery of the Danish natural philosopher H.C. Oersted that when an electrified wire was brought into proximity with a magnetic compass needle, the needle would move. Because this discovery directly contradicted a fundamental assumption of the terrestrial physics of Laplace – that light, heat, magnetism, and electricity were distinct, independent substances – and because of a distrust of the *Naturphilosophie* with which Oersted was associated, many in France dismissed Oersted's claim as "more German dreams." But Ampère, with help from his collaborators and institutional allies Arago and Augustin Fresnel, immediately set about reproducing the Dane's effect, and over the course of the next three years planned and conducted a series of experiments and equations which secured the basic principles of electrodynamics. He continued to elaborate and defend his theories until his death in 1836.[1]

This paper considers the context for electromagnetic research in post-revolutionary France; along with writings in natural history, philosophy, and politics, it focuses on one text by Honoré de Balzac, *La recherche de l'absolu* [*The Quest for the Absolute*] (1834), in which Ampère's major research foci, electrochemistry and electromagnetism, were given prominent attention. As such it prepares the basis for a broader understanding of Ampère's experimental work, whose internal details have been abundantly studied both in their own right and as a case study for various historical and philosophical arguments about the nature of experiment.[2] In this paper my very limited goal is to provide a background for a reconsideration of Ampère's electromagnetism, a study which will have to await another setting. To adapt a phrase of Gaston Bachelard, my aim is to sketch in the outlines of electromagnetism in this period when it is considered as a *cosmic substance*: as the locus of moral, emotional and symbolic values, forged in a cauldron of intuitions, attitudes and expectations drawn from both scientific and extra-scientific channels.[3] Ampère has been identified as one of the possible inspirations for the main character of Balzac's *The Quest of the Absolute*, a major novel about modern science from this period. Like Ampère, whose mixture of Newtonian mathematics and force laws with romantic and nature-philosophical interests in the ether and a vision of the universe as united by fundamental, dynamic forces, Balzac has posed difficulties for historical classification. His works' emphasis on passion and the emotions, his larger-than-life characters, the "organic" themes in his "natural history of society" *The Human Comedy*, the uncontainable energy in his writing and his life all have earned him recognition as a definitive romantic author. But at the same time, his work has been praised by authors from Marx to Lukacs to Sartre for its extreme *realism* and unflinching analysis of the underlying mechanisms of capitalist society. Directly influenced by the sciences of the time, including studies of electricity, chemistry, and magnetism, devoted to patient descriptions and analysis of the material conditions of existence, while at the same time leaving openings for the fantastic and mysterious throughout his works, Balzac was sacred monster not unlike Ampère.

Neither Balzac nor Ampère worked in isolation. Both thrived on historically specific forms of sociability and the often fickle enthusiasms of Parisian salons and circles, and their social and intellectual milieux significantly overlapped. Consideration of Balzac's major themes and representations of contemporary scientific theory and practice thus help us trace broader outlines of the shape of experiment at the start of the industrial age, leading us into domains underexplored by even the most thorough and insightful studies of Ampère's electrodynamics, as it makes explicit ambitions and horizons which can only appear implicitly in scientific research during this period, marked by emerging norms of specialization, impersonality, and refusal of "speculation." From this point of view, Ampère and Balzac's work, both of which demonstrate a fascination with convertible fluids, machines which realize their transmutations, and the centrality of technology in improving nature and human society, were directly in line with a major line of cosmological thought in post-revolutionary France. Running through the exact and qualitative sciences as well as politics, philosophy, literature and the arts, this constellation of ideas and practices combined

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[1] Classic and recent texts on Ampère and electrodynamics include Christine Blondel, *A.-M. Ampère et la création de l'électrodynamique (1820-1827)*, Paris: Bibliothèque nationale, 1982; Olivier Darrigol, *Electrodynamics from Ampère to Einstein*, Oxford, New York: Oxford University Press, 2000; P. M. Harman, *Energy, Force, and Matter: The Conceptual Development of Nineteenth-Century Physics*, *Cambridge History of Science*, Cambridge, New York: Cambridge University Press, 1982; Mary B. Hesse, *Forces and Fields; the Concept of Action at a Distance in the History of Physics*, Westport, Conn.: Greenwood Press, 1970; James R. Hoffman, *André-Marie Ampère*, Oxford, UK: Blackwell, 1995; L. Pearce Williams, "Ampère, André-Marie," in: *Dictionary of Scientific Biography*, edited by Charles C. Gillispie, New York: Charles Scribner's Sons, 1970, pp. 139-47.

[2] Honoré de Balzac, *La Recherche de l'absolu*, in: idem, *La Comédie humaine*, vol. 2, Nouvelle édition publiée sous la direction de Pierre-Georges Castex, Paris: Gallimard, [Bibliothèque de la Pléiade], 1976-1981. Quotes used in this paper are taken from the translation by Ellen Marriage, *The Quest of the Absolute*, Sawtry, Cambridge: Dedalus European Classics, 1986.

aspects of romanticism *and* mechanism. We might call it *mechanical romanticism* – an apparently paradoxical name which highlights the polarized treatment usually given to the first half of the nineteenth century by historians of ideas. Romanticism is usually linked the mind, imagination, subjectivity, and with vague, intuitively or speculatively grasped powers of nature. While presented in histories of art and literature romanticism as the naïve, primitivist, or subjectively tormented predecessor of modernism, in the history of science, *Naturphilosophie* is often taken as a dead end. I would not deny that certain projects of romantic visionaries were overly optimistic, solipsistic, crazy, and often wound up in tragic ends or renunciation by their older, wiser selves. While I can't make any claims about romanticism in general here, what I wish to show are two cases in which the subjectivist, imagination-heavy, immaterial, delirious and doomed view of romanticism simply does not hold up. In the cases of Balzac and Ampère, romantic impulses and themes were connected with attention to the precise, concrete, practical, predictable, and *mechanical.*

My analysis has been greatly helped by Madeline Fargeaud's magnum opus, *Balzac et "La recherche de l'absolu,"* but its overall direction is slightly different than hers and that of many other studies of science and literature. Instead of using scientific texts to explain the origins of certain literary images or an author's flights of the imagination, here the literary text broadens our perspective to better understand the implications of the period's scientific research. Accordingly, this detour through the works of one of Ampère's literary contemporaries aims at a perspective different from many admirable studies which have focused on the mathematical and experimental innovations of Ampère. Giving a sense of contemporary views of the personal and cosmological stakes of scientific research in order to show the broader, "non-scientific" notions which informed these investigations, it provides a different framework for understanding Ampère's research. The aim is to provide a solid sense of what a fantastically powerful, strange, and fluid phenomenon 'electromagnetism' was at the start of the 19th century and what made it particularly attractive as an object for experimental study – even, or especially, for the scientist we now celebrate as having provided it with a "rational" form.

2. Ampère and Balzac make the scene

Though a considerable literature exists on the formal institutional settings for scientific practice at this time, as well as, increasingly, the histories of specific journals and printing houses, for a long

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[3] Gaston Bachelard, *Le rationalisme appliqué*, Paris: Presses universitaires de France, 1949, p. 223. In a case study in the history of chemistry and electricity, Bachelard recounted how researchers at the start of the nineteenth century had noted an unusual smell produced when oxygen is exposed to electric sparks. In 1839, Schonbein, one of François Arago's correspondents, claimed to have identified the cause of this smell: ozone. According to Bachelard, ozone soon suffered a "cosmic overvaluation." Led by his adherence to a two-fluid theory of electricity, Schonbein suggested an analogous substance created by *negative* electricity, "antozone," which was then identified as an enabling cause for epidemics. Ozone and its phantom sister became central objects in a wide-scale though short-lived hygienic movement to map their appearance and absence as indicators of insalubrity. "In these conditions," Bachelard writes, "it would be a long and difficult task to bring into the laboratory this 'cosmic substance'" (p. 223). Ozone was an object entangled with too many disorganized phenomena and cultural expectations to reckon it according to a true proportion or *ratio*; for Bachelard, a "cosmic substance" is necessarily *an irrational object.* My use of the term suggests that these extra-rational associations are crucial for understanding the actual meaning and intentions going into the scientific research of such objects.

time the informal and interstitial sites in which less predictable social interactions took place have been neglected. But as new attention to the cultural settings of ideas has developed (especially concentrating on the French Revolution), we have greater historical insight into some of these milieux. The remarkable circulation of ideas which defines this period went hand in hand with a lively ferment of salons, circles, academies, reading rooms, and journals, both in Paris and the provinces; in these changing scenes, politicians, artists, scientists, dandies and ladies of fashion mingled and exchanged perspectives. The paths of Ampère and Balzac through these dense networks of people and ideas in the French capital overlapped at many points.

Despite an image of Ampère as a marginal and eccentric outsider there is abundant evidence that he thrived on intellectual exchange in diverse settings. It is well known that he was a major contributor to the theoretical and institutional opposition to Laplacean science in the Academy of Science and the Ecole Polytechnique. Along with "general Arago," Ampère championed and contributed to the wave theory of light of Fresnel (who lived in Ampère's home for several years). For his first decisive electromagnetic experiments, his audience was a list of anti-Laplacean dissenters: Humboldt, Fourier, Fresnel, Arago. This opposition to Laplace, one of Napoleon's favorite figures during the restoration, also intersected with open opposition to one of the most prominent natural historians under the empire, Georges Cuvier. In the famous Cuvier-Geoffroy debates of 1830, Arago openly took the side of Geoffroy Saint-Hilaire, who was often portrayed in the press (much like Arago) as a scientist of the people, the defender of speculative originality, and the enemy of sterile specialization. Though Ampère was at times on friendly terms with Cuvier, his first candidacy to the Institut was shot down by him and Laplace, who awarded the post to Cauchy. As Arago demonstrates at length in his 1836 éloge, from the 1820s Ampère launched various criticisms against Cuvier's claims about the fixity of the species and his divisions of kingdoms and embranchments, in concert with Geoffroy's highly detailed arguments about the existence of "a single animal" and the unity of form.

Ampère formed many of his closest intellectual friendships early. As a young man in Lyon, he participated in meetings of the Society of Arts and Letters (where his interest in electricity was reinforced by a visit from Volta). Along with a close-knit group of friends, some of whom would also attain fame in their own right, he was one of the founders of a "Société chrétienne." From before the revolution, Lyon had been a center for illuminist thought and remained so throughout the twentieth century; it was home to many disciplines of Saint-Martin and later in the century could claim spiritualist Allan Kardec as one of its natives.[4] Theological and mystical themes were a constant topic of interest for him and his friends, who included the anatomist and Director of the Royal Veterinary School of Lyon, Brédin, the anthropologist and linguist De Gerando (one of the founders of the Societé de l'Observation de l'Homme), and Pierre-Simon Ballanche, who, as will be discussed later, had a stellar career as romantic prophet of politics and religions. When Ampère arrived in Paris in 1804 after the death of his first wife, Ballanche and DeGerando gave him an entry into various social and intellectual circles. Degerando notably presented him to the circle of philosophers around Maine de Biran, one of the first French Kantians, with whom Ampère began a long correspondence on metaphysics. Ampère also had ongoing conversations

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[4] J. Buche, *L'école mystique de Lyon, 1776-1847. Le grand Ampère, Ballanche, Cl.-Julien Brédin, Victor de Laprade, Blanc Saint-Bonnet, Paul Chenavard*, Paris: F. Alcan, 1935.

about the classification of the sciences with Frédéric Cuvier and the polymath chemist Eugène Chevreul.

Restoration Paris offered many other occasions for literary and scientific worlds to meet. Those with an interest in mysticism and theosophy were drawn to the salon of Madame de Krudener, frequented by Ampère's friend from Lyon, Ballanche, and where Dr. Koreff – the Prussian student of Mesmer who initiated E.T.A. Hoffman into animal magnetism – was also a fixture; there is evidence that Ampère attended this salon, which featured discussions of illuminism and theosophy and performances of magnetism. Ballanche later became the confidante of the legendarily beautiful Restoration hostess, Julie Récamier, in whose salon oppositional politics and liberal Catholicism were mixed with romantic literature. Ballanche acted as patron and chaperone for Ampère's son, Jean-Jacques, who despite setbacks as a playwright and suitor to the much older Mme. Récamier, later attained great fame as a historian and literary critic for romanticism's key journal, *The Globe.* [5] Madeleine Fargeaud, in her magnum opus, *Balzac et la Recherche de l'Absolu*, suggests a strong possibility that Ampère – who composed verses of nature poetry in French and Latin throughout his life – may have met Balzac in the company of Jean-Jacques, whose many friends included Prosper Mérimée and sons of other notable savants: Adrien and Alexis de Jussieu and Fulgence Fresnel.[6] Jean-Jacques later wrote, "I take great pride in two things: I knew M. de B[alzac] while he was thin, and unknown." Other famous salons of this period included Charles Nodier's at the Arsenal, which was the center of romanticism under the Empire and Restoration, and that of Madame Merlin where Arago was a habitué; Georges Cuvier also hosted a salon which both Ampères frequented along with literary friends. It was Ampère's hope for several years to arrange a marriage between Jean-Jacques and Cuvier's daughter, a possibility which appears to have sent Jean-Jacques in flight across Europe. Socialité savants were vectors for the ideas developed in cabinets of *physique* and *histoire naturelle.* In this kaleidoscope of social and intellectual scenes, one could shine as brightly with scientific as with literary *bons mots.*

Furthermore, in the offices and on the pages of the new mass-circulation of literary journals – made possible by steam printing and periodic relaxations of censorship – such juxtapositions and meetings took a written form and were made available for broader consumption. *The Globe,* the *Revue des Deux Mondes,* and the *Figaro* (one of whose founders was Arago's brother, Etienne), are a few of the longer-lasting of the new journals of this period; they combined reviews and excerpts of romantic literature, liberal political arguments, and discussions of the newest sciences. Evening orations, among them Arago's weekly public lectures on *Popular Astronomy*, were also a site for the mingling of romantic luminaries like Balzac, George Sand, Victor Hugo with liberal and reformist political actors and an interested public.

For Balzac, other opportunities for scientific socializing arose while he lived on the Rue Cassini in the 1820s and 30s. Fargeaud suggests that the physiognomy of *The Quest for the Absolute's* central character may have been modeled on that of Arago, the Observatory's director. A short walk brought Balzac to the Observatory's residence, where he was a frequent guest thanks to his

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[5] Auguste Viatte, *Les sources occultes du romantisme: illuminisme, théosophie, 1779-1820,* Paris: H. Champion, 1969.

[6] Madeleine Fargeaud, *Balzac et "La recherche de l'absolu,"* Paris: Hachette, 1968, p. 179.

friendship with Etienne Arago – François' brother, a Carbonarist conspirator, Vaudevillian playwright, and mayor of Paris in the Second Republic, and with whom Balzac wrote one of his first published works. These remarkable neighbors presented him to the physicist Mathieu (Arago's brother-in-law) as well as Félix Savary, a student of Ampère's to whom Balzac dedicated his fantastic novel *La Peau de chagrin*. They also connected him with the Observatory's opticians, who took time from off from the fabrication of astronomical lenses in the summer of 1834 to make the novelist "*une lorgnette divine*" – a divine pair of opera glasses.[7]

These contacts provided Balzac with excellent guides for his readings in the current debates in chemistry and physics, especially as he wrote *The Quest*. The project obsessively pursued by the hero Balthazaar Claës – to fabricate a diamond electrochemically by applying electricity from a massive Voltaic battery to carbon in combination with other substances – had in fact been the topic of debate and the dream of many inventors discussed in the Academy of Sciences in recent years, with Arago and Davy showing keen interest in the possibility. One of these inventors, Thillorier, had also pursued with Arago's support a new kind of metal made of carbonic acid which was expected to revolutionize steam engines; Balzac refers to carbonic acid repeatedly and to the procedures of electrochemistry. In addition, Fargeaud has located passages from the eight volume *Treatise of Chemistry* by Berzelius which Balzac appears to have lifted with minor modifications, including one in which the Swedish chemist refers to the hopes of the alchemists in showing how certain flowers produce out of their own substance new compounds of metals, demonstrating the equivalence of organic and inorganic substances – the alchemical vision of living matter. Likewise, Claës research turns around the dynamic powers of electricity and its relation to chemistry, topics of great interest to Ampère throughout his life. As much as he created the myth of the obsessed scientific researcher reproducing nature's rarest treasures, Balzac adapted it from the examples offered by the leading scientific lights of the day.

3. Electricity and the fluid imaginary

What kind of *thing* was electricity in the first half of the nineteenth century? It was considered variously as a fluid, as two fluids, as a state of matter, as a modification of the ether. It was also frequently associated with other equally elusive fluids. Surveying the sea of discourses on other invisible, imponderable, and possibly unreal fluids in France, which included light, heat, electricity, magnetism, caloric, nervous fluid, gases and miasmas, oxygen, and "vital fluid" in the first half of the nineteenth century, one is inclined to speak of a *fluid imaginary*: a reservoir of notions which melded with troubling ease from one into another, whose distinctions, intersections, metaphysical bases and ontological statuses were extremely difficult to pin down.

Claims of the existence of a single substance which undergoes modifications to produce the diverse forms of matter and souls has been traced back to Presocratic cosmologies and to the Stoics' notion of *pneuma*. It has also been cited in the works of alchemists and early modern natural philosophers like Gilbert (who took magnetism as a cosmic principle of matter and motion) and Maxwell, and informs discussions of electricity and magnetism throughout the early modern period. Enlightenment-era variations on this theme can be found in vital materialists like

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[7] Fargeaud, *Balzac et "La recherche de l'absolu,"* p. 98.

La Mettrie and Diderot: thanks to the cultural relays of figures like the Grimm brothers, the *philosophes* "philosophie de la nature" seized the imagination of many in the late German Enlightenment, providing a materialist vision of a world animated by dynamic forces and contributing along various pathways to *Naturphilosophie.*[8] *Naturphilosophen* and their fellow-travellers held to a view of the universe in terms of fundamental powers in opposition – forces, not substance, a precursor to the philosophy of energy; such an idea has been shown to be operative in the writings of Oersted, a demonstrated precursor for many of Ampère's physical views, as well as in the works of Schelling, Ritter, Davy, and Faraday.

The history of the (re)reception in France of such ideas was closely entwined with the changing fortunes of mesmerism ("le magnétisme" or "le magnétisme animal"). In the last decades of the eighteenth century, cosmological abandon in identifying the correspondences and conversions between invisible fluids and matter reached its wildest extremes in the works of Mesmer and his disciples. In 1784, a commission by the Académie des Sciences was widely read as denying the existence of a single "mesmeric fluid" which penetrated matter and had affinities to thought, will, and celestial phenomena. However, both the title and argument of Darnton's influential *Mesmerism and the End of the Enlightenment in France* suggest that the wide-ranging theories of the magnetists had been brought under rational control by 1800. Most historians of French physics have reinforced such claims by focusing on the internal histories of specific fields or phenomena and processes of experiment and debate taking place primarily within the institutions legitimated under the Empire and Restoration. The position advanced by Laplace, presented in the first two decades of the century as Newtonian orthodoxy, was that *light, heat, electricity,* and *magnetism* were independent, weightless (or imponderable) fluids which, despite their lack of mass, were subject to Newton's laws of attraction. These fluids were assumed to consist of microscopic particles which repelled each other (hence the tendency of light to diffuse itself when unfocused) but which at a macro level had attractive powers: Coulomb's earlier, exemplary discovery that the inverse square law applied to electric attraction was cited as a key justification of these claims. Volta's creation of the same effects as Galvani by means of his famous battery was seen by many as a further step towards rationalizing electricity.

Outside the intra-Academy debates between Laplacean orthodoxy and its heterodox opponents, the possibility of identifications between the imponderables and living forces was still a source of great interest. In 1806 natural historians sought to clarify matters by reducing the proliferation of fluids in the sciences:

> The multiplicity of names that savants have given to the universal electric principle has thrown confusion into the diverse applications that they have made of it: some have called it *elementary fire*, others *nervous fluid*, some of them *animal magnetism*, some of them *vital air (air vital)* or *oxygen gas [gaz oxigène]*: but it is obvious that all of these names indicate the same agent which exists in earth as much as in air, which all bodies, especially living bodies, have the property of condensing."[9]

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[8] Peter Hans Reill, *Vitalizing Nature in the Enlightenment*, Berkeley: University of California Press, 2005.

[9] J.J. Juge Saint-Martin, ancien professeur d'histoire naturelle. *Théorie de la pensée.* 1806, quoted after: Fargeaud, *Balzac et "La recherche de l'absolu,"* p. 147.

Even while Volta was refuting Galvani's notion of a distinct substance of bioelectricity, one of his nearest allies in Paris, Étienne Gaspard Robertson, was in 1800 stressing a spooky vitalist reading of electricity both to the Académie des Sciences and to popular audiences. To the former he reported, "Couldn't this extraordinary fluid be the first of the acids available in nature? Couldn't it be the first agent of the living movement, that the ancients called *nervous fluid*? Couldn't it be a veritable poison?"[10] To the latter he offered demonstrations of the electrical effects of the city's most powerful batteries as the first act of his famous "Phantasmagorie" spectacle, held in an abandonded convent near the Palais Royal. Although these demonstrations took place in a well-lit room, with audience members encouraged to inspect the scientific equipment on display, this open-handed "rationalistic" unveiling was profoundly ambiguous in a way comparable to much popular science of this time. With the second act, spectators were ushered into a pitch black room to the eerie sounds of a glass harmonica, and were terrified/moved/delighted/amused by the immaterial images of disembodied specters: a murdered Caesar, Marie Antoinette, and Robespierre. However rational and evenhanded was Volta's presentation of the electric battery, its potential for fantastic – or phantasmagorical – readings was always present.

Historians of the physical sciences have largely ignored the massive revival that animal magnetism underwent in France in the 1820s and 30s. Mesmer's main disciple, Puységur, continued to teach his doctrine of a universal fluid, taken up briefly by the charismatic polytechnician Alexandre Bertrand, and the eminently respectable Deleuze at the Muséum d'Histoire Naturelle wrote a series of patient and modest books giving an explanation of new mental states produced and observed by Dupotet at the Hôtel Dieu and by R. J. Georget and hygienist Léon Rostan at the Salpêtrière, fifty years and more before the better-documented experiments of Charcot and Janet.[11] What is the nature of this fluid, whose motions appear to be at the root of these astonishing psychological conditions? What is its connection to other fundamental substances? Deleuze inventoried the possibilities:

> Is it the same as light? Is it a single thing variously modified by the channels that it runs through? Is it composed of many different fluids? Electricity, caloric, mineral magnetism, the nervous fluid, etc. ... are they its modifications? Is it subject to the law of gravity? What is its movement, and what causes direct its movement? We do not know.[12]

New societies and journals devoted to the study of the medical uses of magnetism and to its principles of action appeared. Again and again, the idea that this fluid might be at the basis of many other phenomena, a kind of *prima materia* like that of the alchemists, was uttered: *Archives du Magnétisme animal,* we read, "The more research we do, the more we discover that the means of nature are simple. The *electric, magnetic-mineral, or organic* fluid, consists perhaps in a single elementary fluid, which is modified in different manners."[13] This public interest led to a new

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[10] Robertson in Giuliano Pancaldi, *Volta: Science and Culture in the Age of Enlightenment*, Princeton, N.J.: Princeton University Press, 2003, pp. 230-231. Pancaldi notes Napoleon's enthusiasm for the Galvanic reading of the Voltaic battery, as well as his announcement of a significant prize for discoveries in electricity which motivated Ampère to begin research in the field as early as 1805.

[11] For a helpful overview of the situation of animal magnetism in France after Mesmer, see Alan Gauld, *A History of Hypnotism*, Cambridge: Cambridge University Press, 1992, pp. 111-140, 163-178.

[12] *Histoire critique du magnétisme animal.* Paris, 1813, Tome I, p. 81. quoted in Fargeaud, *Balzac et "La recherche de l'absolu,"* p. 146.

Commission on Animal Magnetism, sponsored by the Academy of Medicine, with the participation of physicist François Arago and Bailly begun in 1824.

The connection between the two sorts of *magnétisme* was more than a homonym. In the debates about magnetism at the Academie de la Médecine, the analogy between the action of magnets and the effects described by proponents of animal magnetism was strong enough to produce a motion in even the sharpest minds, nearly reversing the long-held opinions of one of the commission's most distinguished savants. According to the transcription of the discussion at the Academy of Medicine, Bailly "recalled the profound impression that the report of M. Husson had made and expressed the regret of being obliged to oppose it, put forth that *at one moment he was strongly shaken in favor of the belief in an animal or organic magnetism:* it was when he received knowledge of the experiments by means of which M. Arago managed to impress a rotatory movement onto a copper needle by means of a piece of magnet which he made to turn at some distance, despite the interposition of a sheet of paper. He was astonished that the magnetizers had not taken advantage of this fact in favor of their doctrine."[14] One of the series of experiments inaugurated by Ampère and followed out variously by Arago and Ampère made this distinguished savant, who had taken part in the commission of 1784, turn his head.[15] Strong words were exchanged in the discussion, with some making an equation between the disputed magnetic fluid and nervous fluid which, though given scientific credence, had never been observed in an autopsied brain. Lines were drawn here as they had been in 1784, though the reference to the necessity of having *faith* and exercising their *will* to bring about a successful cure (as emphasized in Puységur's work), the key words on the one hand of the religious restoration and on the other of the "post-revolutionary self" put forth by Maine de Biran and Victor Cousin, suggested a mutation of the debate into explicitly post-enlightenment terms: neither reason, nor simple sensationalist observation, would explain mesmerism's action. A deeper source is required, either in the hidden structures of human psychology, or in the divine.[16]

Balzac closely studied these contemporary debates about animal magnetism in which light, heat, magnetism, and electricity were closely associated with life, thought, and will. There are direct relations between his interest in the physical "fluids which are known only by their effects" as he describes them in the "Preface" to the *Human Comedy*, the mesmeric fluid, and the fluids rampant in materialist psychology (Cabanis' nervous fluid) and natural history (Lamarck's view of transformation and adaptation via "canalization" of fluids into new organs, and the interest, witnessed especially in Humboldt as well, on the fluids which served as the external milieux for all

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[13] Quoted after Fargeaud, *Balzac et "La recherche de l'absolu,"* p. 150.

[14] Note however that Bertrand argued strongly against the fluid interpretation of animal magnetism, advocating instead that it was a means of bringing about cures by acting on the imagination of the patient – the effects it brought about through suggestion were nevertheless real.

[15] Ultimately, however, Bailly refused to recognize the existence of a magnetic fluid, citing a fear of exposing the Academy to "the ridicule which is attached to all of those who concern themselves with animal magnetism," and of being associated with "the jugglers" who have already taken advantage of investigation. To avoid this mockery, he does not rule the question out of court; instead he suggests that the study animal magnetism be placed in the hands of licensed physicians "as is done with all other subjects."

[16] Discussion de l'Académie de Médecine sur le Rapport de M. Husson, p. 517, in: Alexandre Jacques François Bertrand, *Du Magnétisme Animal en France, et des jugements qu'en ont portés les sociétés savantes.* Paris: J.B. Baillière libraire-éditeur, 1826. On nervous fluid, see p. 521.

organisms, to say nothing of contemporary theories of miasma and salubrious vs. insalubrious atmospheres). A major clue is the way in which, shortly after the publication of *The Quest*, Balzac made frequent mention of Geoffroy Saint-Hilaire's notion of "unity of composition," a single animal structure unfolded to different degrees in all living forms; as suggested earlier, Geoffroy's opposition to Cuvier, as the representative of an establishment science built on stability and eternal-fixed species was a rallying point for anti-Laplacean and politically progressive scientists in the 1830s.

And indeed, this interest in fluids which combined and transformed themselves to produce life, growth, and thought had resonance beyond the sciences. Strong connections were made by various other actors between the desire for a single substance to unite all of physics and a vague but compelling desire for "unity" at the level of politics and ideology. The pendulum swings of successive regimes since 1789, the perceived specialization of the modern sciences, the social upset brought by changing modes of production, and the rise of a sterile and egotistic individualism brought by the abolition of the corporations led many to see the present as mere anarchy. A search for unity was in harmony with political projects of reform, from those of a reactionary theosophists like de Maistre to those of utopians like Fourier, the Saint-Simonians, or Comte. It also fueled research ambitions in both marginal and mainstream sciences.[17]

Dreams of remaking the world from the smallest particles of matter up to the heavens often turned to the past for inspiration. A trope which united many of these projects of unification was *alchemy* which assumed the connectedness of all existence and looked to the external labor of purifying matter as a means of learning, through analogy and sympathy, the steps of the internal work of purifying the spirit. Hénin de Cuvillers, writing a history of animal magnetism, suggested that "alchemy ... is the mystical part of chemistry."[18] The notorious Polish scientist Hoëné Wronski, mathematician, engineer, translator of Kant, and probable con-artist, gave his prophecies alchemical accents in a text from 1818:

> Absolute reason, this verb inside of us, being considered as virtual reality, can not itself be considered except by the principle of all reality – that is, by the absolute, which it must in fact create to give itself its own reality. And there lies the great *Mystery of creation* which messianism must unveil.[19]

Such talk – in which German idealism, mysticism, mathematical science, and industrial millenarianism were combined – was hard to avoid in France in the first half of the nineteenth century. These kinds of discourses set the background for the rise of romantic literature and for the political and ideological controversies which led up to 1848. Despite the well-built defenses of the official sciences, discourses also informed scientific debates wherever issues including

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[17] See J. Goldstein, *The Post-Revolutionary Self: Politics and Psyche in France, 1750-1850*, Cambridge, MA, London, England: Harvard University Press, 2005 on "horizontal fragmentation" as the problem faced by the post-revolutionary self. See P. Bénichou, *Le Sacré de l'ecrivain, 1750-1830. Essay sur l'avènement d'un pouvoir spirituel laïque dans la France moderne*, Paris: J. Corti, 1973 and P. Rosanvallon, *Le Moment Guizot*, Paris: Gallimard, 1985, both of which argue that a search to re-establish the "spiritual power" to unite society was the definitive political issue of this period: a philosophical and intellectual consensus which could be achieved by more fluid instruments than a one-way state apparatus of command and obedience.

[18] Hénin de Cuvillers, *Le Magnétisme animal retrouvé dans l'antiquité*, 2e ed., Paris, 1821, p. 94.

transformism, progress, utility, or invisible fluids were at stake. Electricity and magnetism often served discursively as a cosmic glue for projects which sought to harness invisible powers to reforge social unity and to make ideas real. As Ampère's close friend Ballanche put it in *Palingénésie Sociale*, the mission of man was "to exercise the intellectual magnetism which tends to spiritualize matter." In Balzac's work, like that of many others in this period, electricity and its analogous (or conterminous) fluids was a symbol and, it was hoped, an instrument for the actualization of spirit in nature.

4. Science through an alchemical glass: Balzac's quest

When we consider the intense intellectual and social exchanges in Paris during the uncertain period between Napoleon's fall and the Revolution and 1848, it comes as no surprise to find that a novelist like Balzac would find scientific research an engaging topic. For many, science was a key plank for projects like those mentioned above for rebuilding an individual, social, and natural unity. His novel, however, shows science in an unusual and rather thrilling light. Setting out on the topic in the summer of 1834, he finished it in less than a month. Conceived as part of his series of novels on "the private life of the 19th century," he hoped it would repeat the success of the sentimental drama *Eugénie Grandet*. According to Fargeaud's reconstruction, however, Balzac's original interest in a family drama was soon overtaken by the opportunity to develop his longstanding philosophical ambitions. The result is a dramatic portrayal of the social dimensions of scientific research, the nature and significance of electricity, and reflections on the legitimate scope of human activity in its technical interventions in to nature. The tale itself is a cosmogram, a representation of what there is in the universe and the proper relations among its parts.[20]

The *Quest for the Absolute* begins in the recent past, 1812, in the city of Douai in Flemish Northern France. Into a peaceful background of tradition and propriety, an element of *novelty* and *modernity* is introduced when Claës, the paterfamilias of a noble household, returns from Paris after studying chemistry with Lavoisier. As one of the town's leading inhabitants, he leaves science aside in favor of family life, his beloved wife, and the obligations of social respectability. This domestic bliss collapses with the visit of a Polish émigré, a former artillery officer and amateur natural philosopher, who tells Claës of his recent discoveries, and initiates him into the secret of his long scientific quest for *the absolute.* Knowledge of the action of this fundamental principle of matter and spirit, he claims, will grant its discoverer powers far greater than those hoped for by alchemists.

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[19] Wronski, *Philosophie absolue de l'histoire*, prémière partie, p. 1818, quoted after Fargeaud, *Balzac et "La recherche de l'absolu,"* p. 73. Despite public scandals in which he was accused of swindling credulous students out of thousands of francs in exchange for "the absolute," he miraculously retained a reputation in progressive scientific circles into the 1830s. *Metempsychosicist* Jean Reynaud wrote an appreciation of Wronski's political mathematics in *palingeneticist* Pierre Leroux's *Revue Encyclopédique***,** while Fargeaud suggests that François Arago encouraged the Pole to publish a "new theory of machines" at the moment when the Perpetual Secretary of the Academy was making a case for the French origins of the steam engine. For a bizarre and impassioned debunking of Wronski which includes an interview with his widow and makes use of a newly "rationalized" orthography, see Alexandre Erdan, *La France Mistique: Tableau des Excentricités Réligieuses de ce tems*, T. 1, Paris: Coulon-Pineau, 1855.

[20] See John Tresch, "Cosmograms." Interview with Jean-Christophe Royoux, in: *CosmogramI*, Melik Ohanian and Jean-Christophe Royoux,(eds.), New York and Berlin: Lukas and Sternberg, 2005, pp. 67-76.

Claës takes up the quest for the absolute like a man possessed. In an isolated attic room of his manor, armed with costly chemicals, metals, and scientific machines, he begins physical and chemical researches which over the course of years will consume his energies, his fortune, and his family, from whom he progressively withdraws, staring at invisible objects and murmuring obscure formulae. After a few years, his devoted, loving and simple wife Josephine discovers to her shock that both her fortune and that of her husband have gone up in the smoke which puffs at all hours from the attic chimney. Creditors demand payment for chemicals and instruments. To the pleas of his daughter and wife for him to stop his research and avoid ruin, he replies “You shall be rich again when I wish it. When I find a solvent of carbon, I will fill the parlour downstairs with diamonds, but even that is a pitiful trifle compared with the wonders I am seeking.” (p. 159). Devastated by his inability to renounce science and recognize his family’s suffering, his wife falls ill and dies.

Remorse briefly awakens Claës from his trance, but he soon returns to his attic and his experiments. One by one the family’s treasures are sold off: the silver and china, the Old Master portraits in the household gallery, the lands from which they had received a guaranteed income, and, finally, the most treasured family heirloom, the “Claës Tulip,” a hybrid bulb with all the colors of the rainbow, whose bulbs have been passed down for centuries. In the face of this dissolution, the plucky eldest daughter, Marguerite, takes matters in hand with the unspoken support of the protégé of her mother’s confessor, Emmanuel, who has a “heart like a diamond.” Marguerite brazenly exiles her father to Normandy. Through various financial contortions she refinances the family lands and restores a number of portraits to the gallery. The two daughters get married off to local notables, and the youngest son, Gabriel, is sent to Paris, where he enters the eminently respectable Ecole Polytechnique.

The tale’s ending stages the tragic futility and essential grandeur of the solitary scientific search. Racked with convulsions, the aged Claës takes to his bed. Unable to speak, “his thoughts seemed to blaze from his eyes;” in his final moments, his daughter mentions a news article about a Polish mathematician’s search for the absolute. The old man rose up: “[A] breath of inspiration passed over his face and made it sublime. He raised a hand, clenched in frenzy, with the cry of Archimedes: EUREKA! I have found it!” before collapsing, an expression of despair frozen on his face.[21] Unable to communicate or test his ultimate discovery, Claës realizes the ambition of his life at the moment of death. We never learn if his search had at last brought him the absolute, or if his deathbed discovery was yet another illusion; what we see, however, is the pathetic wreckage of a man who abandoned his life and the love of his family in the search for the secrets of nature. A struggle and resolution between polarities which underlies the hero’s philosophy of nature also plays out in the plot: Claës’ titanic ambition to understand and master nature clashes with the piety of his devout wife. The cathartic result is the portrait of a cosmos restored to balance, represented in Douai by the couple of Marguerite and her theologian-turned-accountant husband, and in Paris by the inheritor of the Claës lineage, the son trained at the Ecole Polytechnique, where the mysteries of energy conversion were given practical and civic applications as part of with a vocation of duty and collective sacrifice.

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[21] Balzac, *The Quest*, p. 226.

If the narrative dismantles and reforges unity at the level of its structure, at the level of content one of its central themes is a cosmic unity of different kind: the search for a single principle or substance underlying all phenomena. In the physical theory presented within the book, electricity was closely connected to heat, light, and matter; their combinations were understood to produce the phenomena of life and especially thought, a notion Claës elaborates in conversation with his wife:

> Man, representing the highest point of intelligence, is a piece of mechanism which possesses the faculty of Thought, one-half of creative power. And combustion is accordingly more intense in man than in any other animal organism ; its effects may in a measure traced by the presence of phosphates, sulphates, and carbonates in the system, which are revealed by analysis. What are these substances but traces of the action of electric fluid, the life-giving principle? Should we not look to find the compounds produced by electricity in greater variety in man than in any other animal? Was it not to be expected that man would possess greater faculties for absorbing larger quantities of the Absolute Element, far greater powers of assimilating it, an organization more perfectly adapted for converting it to his own uses, for drawing from it his physical force and his mental power? I am sure of it. Man is a matrass.[22]

The human being is a combination of materials in an alchemist's test-tube, a "matrass" – a vessel in which materials are burnt and transformed, through processes of electrochemical combustion. Appalled by what appears here to be a monstrous reductionism, his wife asks:

> 'What! My love for you is – '
>
> 'Matter etherealized and given off, no doubt the secret of the universe.'

Balzac's concern here and in other novels was the relations between matter, ether, electricity, thought, love, and will; his hero, Claës, searches for the material techniques to realize thought in nature, to locate the ground at which mind and spirit become things.[23] This quest is frequently associated with alchemy. The Polish scientist who mesmerized Claës into his scientific fixations (a character directly modeled on Hoëné Wronski, mentioned above) claimed to be pursuing the same goal as "all great seekers of occult causes" and students of alchemy, "that transcendental chemistry" (p. 79). His ambition, to produce a diamond artificially, goes even further down this path: "the alchemists themselves, who thought that gold could be resolved into its different elements, and made up again from them, would have shrunk in dismay from the attempt to make the diamond." The "absolute" – the hidden principle of all of matter and life – is the lowest common denominator of the electrical fluids. Knowledge of this fundamental principle will grant mastery over all of nature. The fluid of electricity here plays the same role as the alchemical "*prima materia:*"

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[22] A *matrass* is a long, straight-necked glass vessel in which substances are heated in pharmaceutics and chemistry.

[23] p. 82. For a view of Balzac's bifocal epistemology of vision, see A. Goulet, "'Tomber dans le phénomène': Balzac's Optics of Narration," *French Forum* 26/3 (2001), pp. 43-70.

> THE PRIMITIVE ELEMENT must be an element common to oxygen, hydrogen, nitrogen, and carbon; the AGENCY must be the common principle of negative and positive electricity. If after inventing and applying test upon test you can establish these two theories beyond a doubt, you will be in possession of the First Cause, the key to all the phenomena of nature ... the last word of creation. (p. 78).

Mastery of the absolute hands the researcher a power like that of God. It is the key to creation, a principle underlying all the transformations witnessed in recent electric and chemical researches. Yet the book acknowledges that in the present there was a clear stigma on alchemy and that it could be used as a term of abuse; at the end of the book, Claës' final humiliation is to be cursed as an "alchemist" and pelted with stones by children.

It is as if Balzac needed this excursion into the historical and imaginative resources of alchemy in order to show the complexity of the the drama he perceived within modern science, whose self-presentation increasingly laid emphasis on impersonality, specialization, and the rejection of superstition. Balzac was not alone in making this detour. Alchemy was a central point of reference for many romantic authors and artists. Spurred by translations in the 1820s and 30s of E.T.A. Hoffman and of Goethe's *Faust* which cloaked anxieties about modern technoscience in Medieval and Renaissance garb, fantastic works like Gautier's and Esquiros' *Le Magicien*, and Nerval's stories of madness, magic, and the occult sciences offered French versions of these themes. Visual imagery of alchemical and magical symbolism could be seen in both serious and playful forms, from Paul Delaroche's celebrated painting of alchemist Bernard de Palissy to Grandville's allegorical and metamorphic engravings of *Un Autre Monde*. Furthermore, as previously mentioned, a revived interest in animal magnetism – closely linked to alchemy and illuminism in the cosmic philosophy of the discredited but still influential Mesmer – took on such strength that a new Commission to investigate its claims was launched by the Academy of Medicine in 1834.[24] Discussions of the Kaballah and the newly "rediscovered" Egyptian Tarot were on the rise, as shown in works of Eliphas Lévi, Paul Erdan (*La France Mistique*), and the liberal Deputy, Eusèbe de Salverte, whose history of the occult sciences featured a preface by Arago. Perhaps more surprising are the ways in which the legitimate sciences showed themselves susceptible to this fad. The chemist Jean-Baptiste Dumas began his lectures on *Philosophie Chimique* of 1836 with a lengthy and largely admiring appreciation of the alchemical roots of his science; in the same text he expressed in contemporary terms a claim that resembled one of alchemists, that "the molecules of diverse simple bodies may well be constituted by the condensation of a single, unique matter."[25] Initiated by Deleuze into Animal Magnetism in 1812, chemist Eugène Chevreul also maintained an extensive alchemical library and in the 1860s wrote a vast exposition of the subject. Related themes from Renaissance natural magic appeared elsewhere in the sciences: Giordano Bruno's discussions of multiple worlds found an echo in astronomical discussions in the popular lectures given by Arago at the Observatory, one key site for legitimate science, as well as in oppositional discourses like the cosmologies of Blanqui and Fourier, in which multiple worlds often took Swedenborgian forms.

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[24] For discussion of the Commission, see Gauld, *A History of Hypnotism.*

[25] *Philosophie Chimique,* 1836; *Mémoire sur les equivalents des corps simples* 1857, quoted in Fargeaud, *Balzac et "La recherche de l'absolu,"* p. 320.

In political terms, the reference to alchemy was equivocal: it could nostalgically represent a lost, long-for, and unchanging past, or suggest a time of radical innovation which resembled and anticipated the changes perceived in the present. Around 1800 counter-revolutionary authors like De Bonald and the theosophist De Maistre had presented the middle ages as a highly desirable world of stasis, a paradise lost with the Reformation and whose ultimate disappearance was tragically punctuated by the French Revolution; alchemy and occult science could here be seen as neglected modes of knowledge which worked in harmony with faith. On the other hand, for liberal historians including Guizot, Thierry, and especially Michelet, a version of the Renaissance that focused on innovation and change could suggest an alternate historical trajectory, one which grounded the new social and economic forms that grew out of the Revolution in a distinguished past.[26] In this reading, alchemy and magic reflected post-revolutionary hopes of unleashing powers which seemed to fulfill the ambitions of medieval and Renaissance mages.[27] In both interpretations, however, the reference crystallized concerns about the power of the human will and intellect to overthrow a seemingly eternal order of society. As recent discussions of alchemy show, the analogy between modern technology and science was not without basis: like modern scientists, alchemists manipulated invisible fluids to identify and harness the underlying principles of nature; both placed a heavy emphasis on labor and craft; in both epochs, human subjectivity and morality were seen as closely entwined with the external world through symbols, technologies, and ritual practices.[28]

5. Moral and metaphysical dimensions of modern alchemy

These uses of alchemy give us some idea of its appeal for Balzac. Ambivalent images of modernity seen through the lens of alchemy run throughout *The Quest*, which raises but does not resolve fundamental questions about science and technology. The alchemical notion of "the absolute" (or "the One") itself makes explicit a rarely acknowledged ambition still lurking within the increasingly specialized sciences: the search for a single principle or force underlying the varied phenomena of the cosmos, mastery of which would unlock nature's secrets. This hope was part of what kept the public reading the weekly *feuilleton scientifique* and attending public lectures in which such broad questions could be indulged. We might attribute some of the ferocity of anti-Laplacean sentiment to disappointment with gravity or "universal attraction" as failed candidates for just such a cosmically harmonizing principle, which some had seized upon as the possible basis for a secular religion; as will be discussed further below, electricity and the ether were invested with similar cosmic hopes. [29]

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[26] See L. Orr, *Headless History: Nineteenth-Century French Historiography of the Revolution*, Ithaca: Cornell University Press, 1990. The classic definition of the Renaissance by Burckhardt was strongly influenced by Michelet's *Histoire de la France*.

[27] The scientific and technological advances of this period appeared within an emergent cosmological references. and point toward neglected cosmological configuration emerging at this time.

[28] See Andrew Pickering, "Science as Alchemy," in: Joan Scott and Deborah Keates (eds.), *Schools of Thought: 25 Years of Interpretive Social Science*, Princeton: Princeton UP, 2001. What Pamela Smith has called an "artisanal epistemology" in the case of Paracelsus was echoed in the prominence given to instrument makers in the 1830s and 40s.

Alchemy presupposed a close interaction between the individual practitioner and the objects worked upon; the power it claimed, that of modifying and recreating nature, lent it an air not only of mystery but of moral uncertainty. The threat posed to society by the magical practitioner Claës' quest for knowledge is paradoxically connected and opposed to a wider society. In one way, nothing could be less social than his pursuits: "He had mounted the winged steed of science, and was far from the actual world" (p. 80). [30] His distraction puts him at odds with those around him and devastatingly reverses the natural social order. Yet in the very damage it causes, Balzac shows science as thoroughly enmeshed with personal destinies and character as well as the intimate social order of the family. He shows the scientific quest in interaction with a wider world of calculation and the rise and fall of social and financial capital; Claës' tragedy is as much economic as it is moral, and his overreach has the same consequence as that of other ambitious characters ruined by Parisian intrigues. The names of his accountants, Chiffreville and Protez, remind us that the nation and its capital city are built on the fragile basis of numbers or *chiffres* and the protean shapeshifting of raw materials, labor, commodities, and money.

The book's central theme thus opens out into a reflection on the place of technology and science during the industrial takeoff of the 19th century. One widespread reaction to these new, nearly demiurgic powers of modern science is uttered by his devoutly religious wife just before her death. Her simple but profound faith and nearly mute devotion serves throughout the novel as a foil to Claës' abstraction and distraction. When Claës reveals to her that his experiments aim at nothing less than taking up the work of creation, her response is utter condemnation: "Accursed science! Accursed fiend! You are forgetting, Claës, that this is the sin of pride by which Satan fell! You are encroaching on God!" (p. 82). Eating of the tree of knowledge will grant us the powers of the Creator. As later evoked with equal fascination and disgust in Baudelaire's "Litanies of Satan," such powers were often seen diabolical.[31] The issue of *creating a new nature* was as much a source of reflection at this time as it was in early modern alchemy.

The question of the proper limits of modification is central to the tale. The goal of Claës' electrochemical experiments was to manufacture, grow, or artificially create a diamond. Like crystals, dead matter which appears to grow organically, his experiments – of which we see only a

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[29] As late as the 1820s Laplace was seeking such a principle as an explanation for all material phenomena in Newton's laws and in universal gravitation; Saint-Simon and especially Fourier extended this principle of attraction to explain social and psychological life. Key disciples of these theorists retained the imperative of unification, but questioned the possibility of a single mode of explanation for all phenomena. Often in discussions of the concept of *association* Comte, Considérant, and Leroux promoted notions of connection (between individuals, classes, vocations, or scientific fields and their objects) in which individual differences are preserved.

[30] His relationship with the one person who is with him throughout his research, his valet Lemonquilier, only underlines the point: towards the end there are hints that Claes is using money lent him by the servant, and that the social inferior has some unwholesome, improper grip on his master. Again, science is seen shown as a threat and a reversal of the proper social order.

[31] Baudelaire's poem concludes, "Gloire et louange a toi, Satan, dans les hauteurs/ Du Ciel, où tu regnas, et dans les profondeurs/ De l'Enfer, où, vaincu, tu rêves en silence!/ Fais que mon âme un jour, sous l'Arbre de Science/ Près de toi se repose a l'heure où sur ton front/ Comme un Temple nouveau ses rameaux s'epandront!" (Glory and praise to you, Satan, in the heights of Heaven, where you reigned, and in the depths of Hell, where, you dream, defeated, in silence! Grant that one day, beneath the Tree of Science, my soul will rest at your side, at the moment when over your head, like a new temple, its boughs will grow forth!"). For references to "la science" as implying primarily occult sciences in the works of Saint-Martin, Ballanche, and others, see Viatte, *Les sources occultes du romantisme.*

glimpse – involve a kind of mineral fertilization. One of the most resonant ironies of the novel comes when Claës returns to the family home after being expelled by his daughter. Though humbled by his daughter's generosity and talent, Claës cannot resist taking a glance at the attic laboratory in which he had left his experiments unfinished. What he discovers is both wondrous and appalling. During his long absence, he left his experimental apparatus near a window, exposed to daily sunlight. With this uncontrolled energy source, far greater than any battery, his experiment succeeds where his own feeble attempts had failed: coal left in a flask has been transmuted, without his lifting a finger, into a diamond the size of an egg; as it occurred without his observation, however, he cannot recall the exact conditions in which he had combined the materials. The success is devastating as there is no way for him to repeat it. Here the plot seems to conspire to agree with Claës long-suffering wife: only nature can (and should) produce novelty in the natural order.

But this apparent moral is equivocal. Despite his faults, Balzac's hero is presented in tones marked by awe and admiration. As we learned earlier, to produce a diamond would mean going further than the "philosopher's stone" by which base metals are turned to gold. The steps of the alchemical process are identified by a rainbow of colors: ruby red, raven black, peacock green. Claës' most cherished heirloom, the rainbow tulip, is an analogous symbol of natural perfection. His degradation is complete when he is forced to sell it in order to feed his habit. But while we can see this as a symbol of the traditional harmony which had reigned before the "accursed fiend" of science invaded the home, the rarity of the breed reminds us that like many of the flowers of the Low Countries, this tulip is a *human creation*, the product of early modern bioengineering. As a symbol of wealth, however legitimate and traditional, it may also remind us of the great speculative bubble which grew up around tulips – like the railroad fever which raged while Balzac wrote – and the catastrophic crash brought by irrational exuberance around these "natural" objects. While at one level Balzac's narrative moves us from a state of harmony associated with a natural order of tradition and stable wealth – presenting science and technology as the sinful usurpers of this order – one of the central symbols of this natural order, Claës' rainbow tulip, is a product of the same logic of innovation, modification, and speculation against whose dangers it appears to warn.[32] Certain forms of modifying nature are presented as comparatively safe and hallowed by tradition; others, though sublime, lead inevitably to destruction.

Similar tensions unfold in the book's presentation of his scientific instruments: these machines are at once the tools of a deadly reductionism and vessels of the supernatural, a duality which gives them an uncanny power. Instruments enter the tale as costly talismans which demand Claës' constant ministrations, glimpsed uncomprehendingly by all the other characters, who fear their obscure and inscrutable powers. At the same time, Claës' obsession itself is represented as having turned him into a machine: the Polish philosopher speaks in a mechanical voice, and when Claës is in the clutches of the Absolute he has a "listless, mechanical way of walking" (p. 74). This imagery resonates with the language of mesmerism, which appears throughout the book in

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[32] Many commentators have noted Balzac's attachment to the countryside as a nostalgic refuge from the corruption of his central subject, the French capital of Paris; yet just like the artificiality of the "natural" rainbow tulip, his demonstrations of machinations of a nearly Parisian degree of venality in small towns undermines the assumption of the provinces as the preserve of the pure and good. (as in *Lost Illusions, Cousin Pons*).

references to vibrations as a means of emotional communication and to light and fire blazing from characters' eyes.[33] As theorized by Puységur, the magnetic trance involved the transmission of the will of the magnetizer on to that of the subject, making the latter into a passive instrument, one associated with automata in texts from Hoffmann, Gautier, and the stage magic of Robert-Houdin. Claës is enchanted by the very machines of instrumental rationality which would later be seen as playing a part in the disenchantment of the world. To Claës, however, these devices are not his masters but his servants, and will give him power over life itself; this inversion is paralleled by his relation with his servant Lemonquilier, whom we realize by the end of the tale has an *un*natural power over his nominal master. The tale's central tension is the scientist's belief in his mastery over forces which all others see as mastering him: it is the question of whether his attempt to know the divinely-ordained plan of the universe does not lead him to mistake himself for a divinity. With the deathbed revelation at the book's close, the author leaves the struggle undecided.

The alchemical metaphor deployed in the text applied not only to science but, reflexively, to Balzac's own practice as a novelist in a growing marketplace of mass-produced cultural goods. Caught entirely in the cycles of rising and falling fame via the fluctuations of public sentiment and the constant flow of written words, he was one of the first generation of authors to earn his living entirely by exposure to the marketplace. He wrote within a heavily saturated world of journalistic representations made possible by steam printing and by the periodic relaxation of censorship since the empire. In his mad scramble to maintain an extravagant lifestyle he speculated on commercial ventures for the exploitation of new technologies: a printing shop, railroads, and the "acclimation" of plants imported from the colonies: his first fortune was lost in a misguided project for a pineapple plantation near Paris. *Illusions perdues* develops this reflexive parallel most strongly, detailing the techniques of printing and the intrigues of the publishing world in terms analogous to Claës' research: the printer as the stove, the paper and words as the vessel: these are the instruments that allow his thought and will to act upon and transform the world. But Balzac had already assimilated his own composition of *The Quest* to the self-destructive acts of ambitious creation it described. While writing it he confesses, "The book is killing me. It is an immense subject, the finest book I am capable of writing," and indeed it is difficult to separate the travails and torments of Claës from those of his equally obsessed author, whose caffeine-fueled search for methods to create lasting impressions and vital effects brought him to an early demise.

Balzac's novels turned the vague but historically salient concept of *unity* into a cosmological and compositional principle. The highest aim of a science was to identify the ultimate principles of the universe, in which life, matter, and thought would all be addressed and shown in their interactions. Some of his works depicted attempts to realize this aim, in its scientific or mystical dimensions (*Séraphîta*, *Louis Lambert, Ursule Mirouët*). This unifying position was neither a materialism nor a spiritualism, but rather a way of viewing the mental and the physical as closely bound up with each other and in constant exchange and conversion. He was a romantic, but one with little faith in the imagination alone or in a pristine unmodified nature. His books portray a constant search for the proper medium or instrument to harness and convert the fundamental forces of nature and mind: these include magical talismans (as in *La Peau de Chagrin*), steam presses and literary genres (*Illusions perdues*), or scientific instruments (*La Recherche de l'Absolu*).

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[33] See Viatte, *Les sources occultes du romantisme.*

Far from the enemy of romantic ideals, he saw the sciences and new technologies as the obligatory means to realize these ideals.

In the first half of the nineteenth century, *modification* and *transformation* were newly seen as the fundamental mode of relation of humans to themselves, society, and nature. As Guizot put it, "man metamorphosizes things and puts on them the imprint of his personality, transforming them into simulacra of liberty and intelligence;" his one-time interlocutor Auguste Comte likewise aimed at a modification of self and nature through various ritual practices which took as their model the Catholic Church as well as a new order of knowledge and society more in keeping with our true needs.[34] Balzac and Ampère's work, far from isolated or idiosyncratic, was directly connected to these new ways of thinking about humans' relations to each other and to the natural world. In these and similar projects devised to adapt to modernity, several themes were held in common: the existence of a single, primary substance underlying all matter, the unity of nature via a complex net of associations and relationships, and the power of the human mind, in conjunction with specific techniques and technologies, to modify the natural order. In these defining themes of the modern age of industry and reason, many saw not only novelty, but a reflection of the much older arts of alchemy.

6. Conclusion: The concrete cosmos

In an essay which links Ampère and other students of electricity and magnetism – Oersted, Ritter, Day – to the broad movement of *Naturphilosophie*, L. Pearce Williams suggested that one of this movement's goals was nothing less than a change in the conceptual frame for understanding the cosmos:

> Kant, and more particularly the *Naturphilosophen*, attempted to substitute a new cosmic metaphor. The world of the eighteenth-century *philosophe* was a machine; the *Naturphilosophen* insisted it was an organism. Its laws were laws of development; its basic theoretical paradigm was field theory in which the connections between parts were as important as the parts themselves. Organisms live because they are informed by Spirit, and the *Weltseele* [world soul] was the ultimate substratum of physical reality. Only spirit can understand spirit; science, then, is spiritual in its essence.[35]

This is a familiar reading of romanticism, and one not without ample justification in writings found in the first half of the nineteenth century. In the cases of Balzac and Ampère, however, this familiar opposition between machine and organism, between knowledge by technological manipulation and knowledge by intuition, between "objective" and "subjective" approaches to nature is much harder to find. Instead we see these strands woven together in a variety of combinations. For all the variety of this period, as well as its abundant fascination with contradiction and paradox, something like a shared *zeitgeist* was forming precisely out of these unusual mangles in science, politics, and literature: a modern metaphysics which incited movement and syntheses between opposed views of a single object – spiritualist, emotional,

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[34] Guizot, *Histoire de la civilization dans l'Europe*, 1828, quoted after Goldstein, *The post-revolutionary Self*, p. 228.

[35] L. Pearce Williams, p.17.

mechanistic, utilitarian – and saw everywhere opportunities for a morally ambivalent but highly desirable modification of nature.

The search for analogies and correspondences in the ideas of Ampère and Balzac (or his fictional double, Claës) may smack of the hasty generalizations involved in describing the "worldview" of intellectual figures as found in traditional history of ideas. This field has stood in some disrepute at least since Foucault in *The Archaeology of Knowledge* blasted its use of "the categories of cultural totalities (whether world-views, ideal types, the particular spirit of an age)." Equally annoying to Foucault was the concept of "influence," a notion "too magical ... to be very amenable to analysis," which, he continued with erudite sarcasm, "links, at a distance and through time – as if through the mediation of a medium of propagation – such defined unities as individuals, *œuvres*, notions or theories."[36] Although I believe that the strong analogies between Ampère and Balzac identified here make a case for a shared cosmology (leaving space for considerable individual and disciplinary variation), to construct this intellectual object there has been no need to rely on magical explanations of cultural causality. These ideas and attitudes spread without need for action at a distance, thanks to the overlap of these actors' pathways through salons, circles, institutions, journals, and networks of friends and associates, *milieux* which served as the material "medium of propagation" for these cultural forces and unit-ideas, to use Lovejoy's term. The aim, then, has been to arrive at the hermeneutic notion of a cultural whole, but to ground this totalizing abstraction in the local, particular, material practices and social connections through which actors described and realized it. This "holistic" aim might appear "romantic," yet the method is "mechanical:" grounded on direct interactions, concrete settings and observable practices. One object of this paper has been a constellation of ideas and practices called mechanical romanticism; the same name could apply to the method it seeks to develop.

Each in its own way, these projects of utopia and totalization from this period – attempts to present the entire *Globe*, indeed the entire order of nature in a single representation, whether a linked series of novels, a new encyclopedia, a total history, or a complete system of human knowledge – were located in the same cosmological streams whose ripples reached Ampère and Balzac, and which many saw vibrating in sympathy with the concerns of alchemy. This is not to suggest that the *differences* between these emerging fields, like the difference between the *savants* of the nineteenth century and the actual practices of early modern alchemists, are not also significant. As Sainte-Beuve wrote in an anonymous review of *The Quest* in 1834, "M. de Balzac seems to believe that there is but a step between a taste for alchemy and the lessons of Lavoisier, when there is an abyss."[37] But Balzac was not the only one to span this abyss, or to refuse to defer to it. The shared ground in which exchanges and identifications across this divide could appear self-evident – not just in a set of shared ideas, but in networks of acquaintances, social forms and modes of sociability, and ways of *thinking* about practice. As further discussion of Ampère's work will show, his pursuit of the secrets of electromagnetism and Claës' quest for the fundamental substances of nature were symmetrical and closely entwined quests for the absolute, nourished in the same material and intellectual milieux. These obsessed researchers – one real, one fictional –

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[36] Michel Foucault, *Archaeology of Knowledge*, London, New York: Routledge 2002, pp. 17 and 24.

[37] In the *Constitutionnel*, Revue Littéraire, 9 October 1834, quoted in Fargeaud, *Balzac et "La recherche de l'absolu,"* p. 496.

provided the early industrial age with important metaphors and formulae to help understand and realize about the transformation of society and the natural world.

Sciences of Falling Down: Time, Body, and the Machine, 1750-1900

Henning Schmidgen

In the last paragraphs of the *Birth of the Clinic*, Michel Foucault refers to Hölderlin's drama fragment on Empedocles, the Greek philosopher and physician who, according to the legend, threw himself into the mouth of the volcanic Mount Etna. Foucault writes: "[It] is the death of the last mediator between mortals and Olympus, the end of the infinite on earth, the flame returning to its native fire, leaving as its sole remaining trace that which had precisely to be abolished by his death: the beautiful, enclosed form of individuality."[1] After the *Death of Empedocles*, Foucault continues, the world is placed under the sign of finitude. In this perspective, the advent of clinical medicine appears as one of the decisive developments confronting modern man with the "obstinate yet reassuring face of his finitude." In the same context, however, Foucault also speaks about the "technical world" constituting "the armed, positive, and full form of finitude."[2] What has occupied me during the last weeks is the question whether or not the physiological and psychological time experiments I am investigating belong to this technical world of finitude. Can, in this respect, physiological psychology be compared with clinical medicine? Are the experimental machines that Hermann von Helmholtz, Adolphe Hirsch, Wilhelm Wundt and other such scholars constructed in order to investigate the time relations given in the human brain and nervous system 'machines of finitude'?

I start with an image. I encountered it when reading a short text by Peter Sloterdijk. It is a drawing by the mannerist Hans Bock the Elder (fig. 1). Male human beings, perhaps former angels, are depicted as falling down from heaven. Sloterdijk argues that these various forms of falling down tell us everything about what is to be seen on this image: "Each body falls, but at the same time each body is a case [*ein Fall*]. Despite the fact that these plastic individuals fall in a group, we do not encounter them in wild decomposition. To the contrary, everyone is falling down in a specific way, each body falls in its separate space contained in the total of falling down, and everyone knows how to fall in a specifically interesting way."[3] However, it is not so much the "dialectic tension" between individual fallings-down and the overarching disaster that got me interested. Rather, I started wondering about the time it takes to fall. I was struck by the fact that Bock's drawing dates from the same period as the famous series of trials with solid bodies and inclined planes that paved the way to the modern discovery of the laws of fall. In *De Motu,* Galileo even refers explicitly to the falling down of human bodies in order to discuss the falling of wooden balls and other solid bodies, quoting common experiences of divers and swimmers of being "thrown down" into the water.[4]

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[1] Michel Foucault, *The Birth of the Clinic: An Archaeology of Medical Perception*, transl. by A. M. Sheridan Smith, New York: Vintage Books, 1994, p. 198.

[2] Ibid.

[3] Peter Sloterdijk, "Jeder Körper ist ein Fall," in: Fritz J. Raddatz (ed.), *ZEIT-Museum der 100 Bilder*, Frankfurt am Main: Insel Verlag, 1989, pp. 364-368, quotation on p. 367.

[4] Galileo Galilei, "De Motu" in: idem, *On Motion and on Mechanics*, transl. by I. E. Drabkin and S. Drake, Madison: University of Wisconsin Press, 1960, pp. 13-131, quotation on p. 102.

Fig. 1: Hans Bock the Elder, “Fall of Angels” (1582), Department of Printings and Drawings, Kunstmuseum Basel.[5]

The connection between time, bodies and falling down re-surfaced in a different context in the mid-19th century. When biologist Karl von Baer discussed the time required for the conscious perception of sensations in his 1860 lecture on our appropriate conception of living nature, he referred to the experience of falling down. "I believe that with 1/6 or 1/10 of a second, and especially with the latter figure, I have given the more or less precise measure of the time required for a common sensory perception. This claim is underscored by the common experience that persons who are knocked down by a stroke do sense the stroke but not their falling to the ground [...]."[6] Baer continued by reporting two situations of being knocked down from his personal experience. Or take an example from philosophy, Bergson's theory of laughter. To Bergson, the paradigmatically comic situation is a man running along the street, stumbling and falling. As a consequence, Bergson's book deploys a theory of laughter, but also one of falling down. Let's assume the man on the street stumbled because he hit a stone. Bergson argues that to avoid falling down, the man would have to have stopped or at least modified his movement. Instead, he went on in a machine-like manner. As Bergson puts it: "the muscles continued to perform the same movement when the circumstances of the case [sic] called for something else."[7] Similar to von Baer's lecture, falling down refers here to specific time relations incorporated in organic individuals, even if Bergson does not give any precise numbers.

The case studies that I have conducted (and let's not forget that the word "case" derives from *cadere*, to fall) are roughly situated between von Baer and Bergson, i.e. they cover the period between 1860 and 1900, including a recent excursion to cybernetics. They mainly concern time experiments in human beings, and falling down does play a crucial role in them. In fact, the standard version of one my experiments relies on a precision timer calibrated by means of physical trials with solid bodies that are falling down. For this purpose, the so-called Hipp chronoscope was equipped with "dropping down-" and "bouncing-apparatuses." However, these apparatuses were also used for presenting acoustic stimuli to human test subjects. In other words, it was by movements of falling bodies that human beings became practically involved in these experiments. In the following I want to trace something like the genealogy of such machinic assemblages, mainly focusing on some of their basic elements, their initial distribution and gradual "conretization" as well as their changing positions and interaction.[8] The focus will be on three dates: 1750, 1800 and 1850. Special attention will be paid to interfaces, intervals, and interruptions of flows.

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[5] Reproduced from Emil Maurer, *Manierismus: Figura Serpentina und andere Figurenideale*, München: Fink, 2001, p. 95.

[6] Karl Ernst von Baer, "Welche Auffassung der lebenden Natur ist die richtige? und wie ist diese Auffassung auf die Entomologie anzuwenden?" in: idem, *Reden, gehalten in wissenschaftlichen Versammlungen und kleinere Aufsätze vermischten Inhalts, Erster Theil: Reden*, St. Petersburg: Verlag der Kaiserlichen Hofbuchhandlung H. Schmitzdorff (Karl Röttger), 1864, pp. 237-284, quotation on p. 257.

[7] Henri Bergson, *Laughter: An Essay on the Meaning of the Comic*, transl. by C. Brereton and F. Rothwell, London: MacMillan, 1911, p. 9.

[8] On the "concretization" of technological objects, see Gilbert Simondon, *Du mode d'existence des objets techniques*, 3rd ed., Paris: Aubier, 1989, pp. 19-49.

1. The King is dead

On November 30, 1718, Charles XII of Sweden was killed. At this point in his career, the Swedish King had established his native country, by military means, as a major power in Europe. Charles was still fighting against Russia and Denmark. But peace negotiations with Russia were already under way, and the Swedish troops were in the midst of attacking a crucial Danish fortress blocking the way to Oslo. In late November, the final assault was under preparation. On the evening of November 30, Charles was supervising the digging of the most advanced trench from a position in the next trench behind it. His head and shoulders were visible above the breastwork. The Danish troops had illuminated the field and were firing heavily. Charles did not care. About 10 p.m., a bullet hit the King, piercing his hat and head (fig. 2). Until today, it remains unknown whether the shot came from the Danish troops or if somebody from the Swedish side committed murder.[9]

Fig. 2: The mask modeled from the dead face of Charles XII. in 1718, by Simon Josse.[10]

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[9] See, for example, Ragnhild M. Hatton, *Charles XII of Sweden*, London: Weidenfield and Nicolson, 1968, pp. 495-509.

[10] Reproduced from ibid., between p. 492 and p. 493.

In 1731, Voltaire published his biography of Charles XII. Concerning the sudden death of the King, Voltaire states that the King was killed by the shot of a Danish canon. More precisely, he notes: "L'instant de sa blessure avait été celui de sa mort; cependant il avait eu la force en expirant d'une manière si subite, de mettre par un mouvement naturel la main sur la garde de son épée, et était encore dans cette attitude."[11] The first biography of the Enlightenment thus confronts us with some final incoherence. According to Voltaire, Charles XII, as a human being, was already dead while briefly continuing to live as a soldier. The difference between biological and political being – between sudden death and natural movements – is even reinforced when Voltaire depicts the transporting of the corpse. Covered with somebody else's coat and dressed in a different wig and hat, the King was carried away while officials pretended it was somebody else. Thus, one could actually *see* the dead King being carried through the camp, but one would only *know* of it retrospectively.[12]

In 1749, Buffon refers to Voltaire's biography in his *Histoire naturelle de l'homme*. The context is provided by Buffon's discussion of the question whether or not the moment of death implies physical pain. In the chapter "De la viellesse et de la mort," Buffon stresses the unity of life and death: "Nous ne commençons de vivre par degrés, et nous finissons de mourir comme nous commençons de vivre."[13] Even in the case of a violent and sudden death, no excessive pain is felt, and as Buffon explains, the reason for this lies in the temporal structure of our experience. He then writes: "Lorsque Charles XII reçut le coup qui termina dans un instant ses exploits et sa vie, il porta la main sur son épée : cette douleur mortelle n'était donc pas excessive, puisqu'elle n'excluait pas la réflexion; il se sentit attaqué, il réflechit qu'il fallait se défendre, il ne souffrit donc qu'autant que l'on souffre par un coup ordinaire."[14] In other words, the director of the *Jardin du Roi* contradicted Voltaire. The movement of Charles' hand was by no means "natural," but relied on a conscious decision. In Buffon's eyes, soldier and human being, body and mind were not separated. On the contrary: until the very last moment, the King's mind and body remained united.

It is precisely here that Buffon presents an argument that recurred in 19th-century physiology and psychology: "deux idées qui se succèdent, ou qui sont seulement différentes l'une de l'autre, ont nécessairement entre elles un certain intervalle qui les sépare; quelque prompt que soit la pensée, il faut un petit temps pour qu'elle soit suivie d'une autre pensée, [...]. [I]l faut un certain temps pour passer de la douleur au plaisir, ou même d'une douleur à une autre douleur [...]."[15] As Buffon adds, the duration of this interval "n'est point arbitraire ni indéfinie," since it refers to the nature of our soul and the organization of our body which can only move with a certain velocity. Of course, Buffon did not give any numbers. To him it was sufficient to have pointed to

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[11] Voltaire, *Histoire de Charles XII*, édition critique par Gunnar von Proschwitz, Oxford: Voltaire Foundation, 1996 [=Les oeuvres complètes de/The complete works of Voltaire; 4], p. 541.

[12] Ibid.

[13] *Oeuvres complètes de Buffon, avec la nomenclature linnéenne et la classification de Cuvier, Revues [...] et annotées par M. Flourens, II: L'homme – Les quadrupèdes*, Paris: Garnier Frères [1854], p. 80.

[14] Ibid., p. 83.

[15] Ibid., p. 82. On Buffon's psychology, see Jacques Roger, "Diderot et Buffon en 1749," *Diderot Studies* 4 (1963), pp. 221-236, and Paul Mengal, "La psychologie de Buffon: À travers le traité *De l'Homme*," in: *Buffon 88: Actes du Colloque international pour le bicentenaire de la mort de Buffon*, ed. by Jean Gayon, Paris, Montbard, Dijon: Institut Interdisciplinaire d'Etudes Epistemologiques, 1988, pp. 601-612.

the interval that prevented King Charles from a painful death. It was the difference between the swift movement of the bullet and the rather slow movement of body and soul that was decisive for him.

One has often noted that Buffon marks the point where biological (and, one should add, geological) time enters the epistemology of the modern life sciences.[16] In my recursive reading, Buffon's reflections on the death of humans represent the initial encounter of components that were to reappear in physiological and psychological laboratories, albeit in a highly controlled, mediated and so to speak neutralized manner: the trajectory of a bullet, the movements of an organic individual, and the idea of intervals constitutive of human experience and behavior. As I want to show in the next section, questions concerning the relation of fall, life and time remained important in the meantime.

2. The blade

Georges Canguilhem once noted that, around 1800, the history of the modern brain- and neurosciences connected itself with the history of a machine that can be qualified as as terrible as it was emblematic, a machine that was perhaps most effective in fighting the difference between citizens and Kings: the guillotine.[17] With respect to the medical and physiological debate on the Guillotine, Canguilhem (and other science historians after him) has placed much emphasis on the question of localization.[18] I want to draw your attention here to the eminent role of time. First, the guillotine was the concrete, material prerequisite for countless, quick and quasi-industrial executions in post-revolutionary France. Historian Daniel Arasse even writes: "The guillotine, that product of the Enlightenment, was [...] one of the first machines considered in economic terms, the cost-effectiveness of its output being evaluated according to the time taken."[19] Second, because of its technological efficiency, this execution device confronted contemporaries with fundamental asynchronicities calling into question established concepts of the mind-body-relation. In Buffon, the interval between two thoughts or feelings guaranteed the unity of organism and consciousness. To the physicians engaged in the debate about the guillotine, the precisely falling blade constituted a rupture with irreversible disunifying effects. On the one side, authors such as Sömmering, Oelsen und Sue claimed that consciousness continued for a while in heads that were separated from the body, and speculated wildly about the afterthoughts and afterimages of executed humans. On the other side, Cabanis, Wedekind and Léveillé denied the existence of remaining consciousness shortly after execution, postulating a temporally structured hierarchy of physiological and psychological functions that prevented conscious pain. Thus on both sides, the unity and synchronicity of organic life was contested.[20]

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[16] See, for examle, Wolf Lepenies, *Das Ende der Naturgeschichte: Wandel kultureller Selbstverständlichkeiten in den Wissenschaften des 18. und 19. Jahrhunderts*, München: Carl Hanser Verlag, 1976; Bernard Balan, *L'ordre et le temps: L'anatomie comparée et l'histoire des vivants au XIXe siècle*, Paris: Vrin, 1979, pp. 117-147; Hans-Jörg Rheinberger, "Buffon: Zeit, Veränderung und Geschichte," *History and Philosophy of the Life Sciences*, 12 (1990), pp. 203-222.

[17] Georges Canguilhem, "Le cerveau et la pensée," in: *Georges Canguilhem: Philosophe, historien des sciences*, Paris: Albin Michel, 1993, pp. 11-33, in particular p. 12.

[18] See, for example, Michael Hagner, *Homo cerebralis: Der Wandel vom Seelenorgan zum Gehirn*, Berlin: Berlin Verlag, pp. 185-193.

[19] Daniel Arasse, *The Guillotine and the Terror*, London etc.: Allen Lane/The Penguine Press, 1989, p. 27.

It is interesting to see what kind of rationality was connected to the construction and inner dynamics of Guillotin's execution device (fig. 3). The historical documents often allude to the physical laws of falling bodies. But when it came to actually constructing the machine, a practice of probing and testing prevailed. Precise calculations or measurements are not reported. In this respect, the bill that Guillotin introduced to the Constitutional Assembly on December 11, 1798, is quite characteristic. Its central message was condensed into two laconic sentences: "The criminal shall be decapitated. Decapitation is to be effected by simple mechanism."[21] It would take three years before the actual construction of the machine was tackled. Antoine Louis, secretary of the Academy of Surgeons, was charged with considering the medical and technological aspects of the project. Similar to Guillotin, he favored carrying out execution "by invariable mechanical means." However, to Louis, it was decisive that one was able to establish "force and effects" of these means precisely. But with respect to the required height of the guillotine's frame, he just vaguely noted that "the force increases with the height from which it falls."[22] No wonder, then, that the next step, taken in April 1792, was carrying out a series of test trials with human corpses in a small yard at the Bicêtre. Three years later, the German physicist Georg Christoph Lichtenberg, in his short account of the history of the guillotine, also alluded to the possibility of mathematically representing the interaction of "the extended blade, the large weight, and the massive fall."[23] But Lichtenberg wanted to spare the readers of the *Göttinger Taschenkalender* this kind of calculation. Similar to what had happened within the French context, he considered the actual construction of the guillotine as a problem of "practical mechanics."[24]

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[20] On the debate, see Dorinda Outram, "The Guillotine, the Soul and the Audience for Death," in: idem, *The Body and the French Revolution: Sex, Class, and Political Culture*, New Haven and London: Yale University Press, 1989, pp. 106-123, Jürgen Martschukat, "Ein schneller Schnitt, ein sanfter Tod!? Die Guillotine als Symbol der Aufklärung," in: *Das Volk im Visier der Aufklärung: Studien zur Popularisierung der Aufklärung im späten 18. Jahrhundert*, ed. by Anne Conrad, Arno Herzig and Franklin Kopitsch, Hamburg: Lit, 1998, pp. 121-142, Regina Janes, "Beheadings," *Representations* 35 (1991), pp. 21-51, Ludmilla Jordanova, "Medical Mediations: Mind, Body and the Guillotine," *History Workshop*, 28 (1989), pp. 39-52.

[21] Quoted after Arasse, *The Guillotine and the Terror*, p. 11.

[22] "Doctor Louis' proposal concerning methods of decapitation," in: Arasse, *The Guillotine and the Terror*, pp. 186-187, quotation on p. 187.

[23] Lichtenberg, "Ein Wort ueber das Alter der Guillotine," *Göttinger Taschen-Calender* 20 (1795), pp. 157-165, quotation on p. 163. A highly instructive commentary on this short text is provided by Gerhard Neumann, "Georg Christoph Lichtenberg als Begründer eines sozialen Topos," in: *Lichtenberg: Streifzüge der Phantasie*, ed. by Jörg Zimmermann, Hamburg: Dölling und Galitz, 1988, pp. 84-111.

[24] Lichtenberg, "Ein Wort ueber das Alter der Guillotine," p. 163.

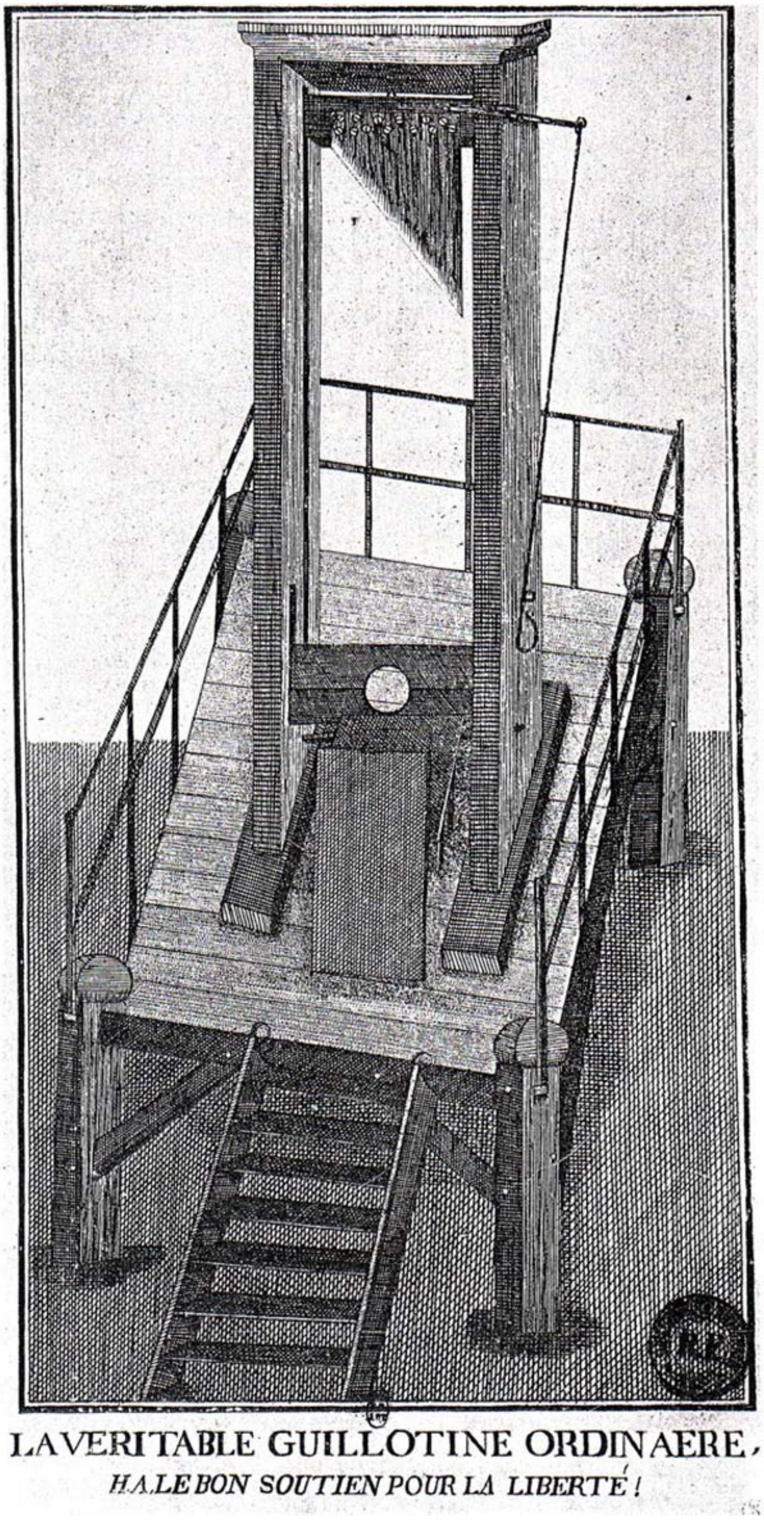

Fig. 3: "The authenic ordinary guillotine," Musée de Carnavalet, Paris.[25]

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[25] Reproduced from *La guillotine dans la revolution*, ed. by Valérie Rousseau-Lagarde and Daniel Arasse, Château de Vizille: Musée de la Révolution Française, 1987, p. 124.

Interestingly enough, it was one of the physicians involved in the discussion of physiological aspects of guillotine executions who went one step further. Georg Wedekind, physician at the Strasbourg military hospital, was interested in the precise dimensions of the machine and explicitly in the problem of time. According to Wedekind, "the heavy blade falls down so rapidly from a height of at least twelve shoes [i.e. roughly 5 meters] that nothing more than one second of time may be counted."[26] As a consequence, he argued, the cutting of the neck happens within the 24th part of a second, since the neck is not thicker than half a shoe. The Strasburg physician was convinced that within such a short time no pain could be accounted for – a conviction he bolstered by referring to his clinical experiences with injured militaries. Cabanis and Léveillé proceeded in a similar manner, relying largely on clinical observations.

Wedekind's calculations hardly impressed those physicians who argued that the cut-off head kept its consciousness for a given period of time. Sömmering, Oelsner and Sue simply insisted on the fundamental difference between the objective duration of the execution and the subjective duration of being-executed. Similar to Buffon, Oelsner underlined in this context: "Notre esprit mesure le temps sur le nombre et le genre des sensations qu'on éprouve."[27] In this perspective, the decisive issue was not precise measurements of time but the structural organization of the human body. To Oelsner, Sömmering and Sue, physiological and psychological functions were so intimately connected to organic structures that a sudden dissolution of the overall organization of these structures would not affect the respective functions. As Sue put it: the brain as the "workshop of the principle of thought" continued its activities even when the blood supply was interrupted.[28]

In contrast to this highly spatialized conception, Cabanis and Léveillé stressed more general aspects of the relation between structures and functions. To them, consciousness was a function relying on the interaction of virtually all bodily structures. As Léveillé put it: "the laboratory of life is active insofar it is distributed over the whole of the body."[29] This conception by no means implied the idea that with the falling blade, all manifestations of life came to an immediate end. Rather, it was argued that there was no conscious suffering after the execution. The basis for this argument was in itself temporal. Cabanis and Léveillé assumed that the hierarchy of physiological and psychological functions had structured itself over the course of organic development. Apart from the guillotine, time thus functioned as a parameter that allowed one to distinguish various levels of vital activity. In other words, Cabanis and Léveillé were less skeptical concerning the use of the guillotine because for them time had already turned into an analytic means for investigating organic functions – similar to the pathological anatomy Bichat developed shortly later. As we will see now, it is exactly this analytical notion of time that paved the way for the time experiments

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[26] Georg Wedekind, "Ueber den Tod durch die Guillotine, wider die Behauptungen der Hrn. Sömmering und Sue," *Humaniora* 3 (1797), pp. 63-78, quotation on p. 75.

[27] Konrad Oelsner, "[Présentation de la] Lettre du professeur Soemmering, sur le supplice de la guillotine," *Mémoires de la Société Médicale d'Emulation* 1 (1796), pp. 266-269, quotation on p. 268.

[28] Jean J. Sue, "Über den Schmerz, der nach der Enthauptung fortdauert," in: *J. J. Sue's physiologische Untersuchungen und Erfahrungen über die Vitalität: Nebst dessen Abhandlung über den Schmerz nach der Enthauptung, und den Abhandlungen der Bürger Cabanis und Léveillé über denselben Gegenstand*, ed. by Johann Christian Friedrich Harleß, Nürnberg: Kaspersche Buchhandlung, 1799, pp. 73-116, quotation on p. 111.

[29] Jean B. F. Léveillé, "Wird die Empfindung in dem Augenblick gänzlich vernichtet, in dem der Kopfe vom Rumpf getrennt wird?," in: *J. J. Sue's physiologische Untersuchungen und Erfahrungen über die Vitalität*, pp. 136-148, quotation on p. 145.

conducted by Helmholtz, Wundt and others in the 1850s and 60s. There, time measurements (*Zeitmessungen*) functioned as a sort of knife (*Messer*) dissecting complex organic functions into their basic elements without severely hurting human beings.

3. Dropping down apparatuses

In 1852, the Karlsruhe-based physicist Wilhelm Eisenlohr published the sixth, improved and extended edition of his *Textbooks of physics for the use in lectures and self-instruction*. A whole section was devoted to recent applications of electromagnetism. Besides various kinds of telegraphs, Eisenlohr presented and discussed the role of electromagnetism in the communication and determination of time. An illustrated paragraph dealt with the chronoscope constructed by the German clockmaker and mechanic Matthäus Hipp (fig. 4). As already mentioned, this precision timer should become a standard device in late 19th-century labs for psycho-physiological research. The chronoscope consisted of a mechanical, weight-driven clockwork mounted on four classical columns. The clockwork was controlled by a steel tongue, a sort of miniature tuning fork, making one thousand vibrations per second. The main feature of the Hipp chronoscope was the separation of the clock work and the dials. Only when measuring time, the hands of the clock were pulled into the running clockwork by means of electromagnetism and thrown out of it in the same way when the measurement was stopped. Eisenlohr had checked the precision of the Hipp chronoscope by conducting trials with solid bodies that he dropped from specific heights, comparing the obtained results with the times he had calculated on the basis of the laws of fall. Convinced by the precision of the results, Eisenlohr then suggested two possible applications of the Hipp chronoscope. First, learned physicists could use this device for empirically demonstrating the laws of fall in classroom lectures. Second, the chronoscope might be used for ballistic trials concerning the speed of projectiles. Eisenlohr's presentation of the chronoscope was as neutral as one might expect from a physics textbook. However, it was not free of associations to contexts seemingly far removed. Thus when describing the "dropping down apparatus" as an integral part of the chronoscope, Eisenlohr said that the metal ball (at K) was attached to a "wooden gallows."[30] Every trial with a falling body thus was evocative of an execution.

Procedures of precision time measurement profoundly transformed the research practices of mid 19th-century physiologists and psychologists. As is well known, the emergence of these procedures was connected to physics *and* ballistics.[31] In his pioneering experiments on the propagation speed of the nervous impulse in frogs and human beings, Helmholtz made use of Pouillet's galvanometer method, a method explicitly inspired by Benjamin Robin's ballistic pendulum.

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[30] Wilhelm Eisenlohr, *Lehrbuch der Physik zum Gebrauche bei Vorlesungen und zum Selbstunterrichte*, Sechste verbesserte und vermehrte Auflage, Stuttgart: Krais und Hoffmann, 1852, p. 624.

[31] Hebel E. Hoff and Leslie A. Geddes, "Ballistics and the Instrumentation of Physiology: The Velocity of the Projectile and of the Nerve Impulse," *Journal of the History of Medicine and Allied Sciences* 15/2 (1960), pp. 133-146; idem, "The Technological Background of Physiological Discovery: Ballistics and the Graphic Method," *Journal of the History of Medicine and Allied Sciences* 15/4 (1960), pp. 345-363.

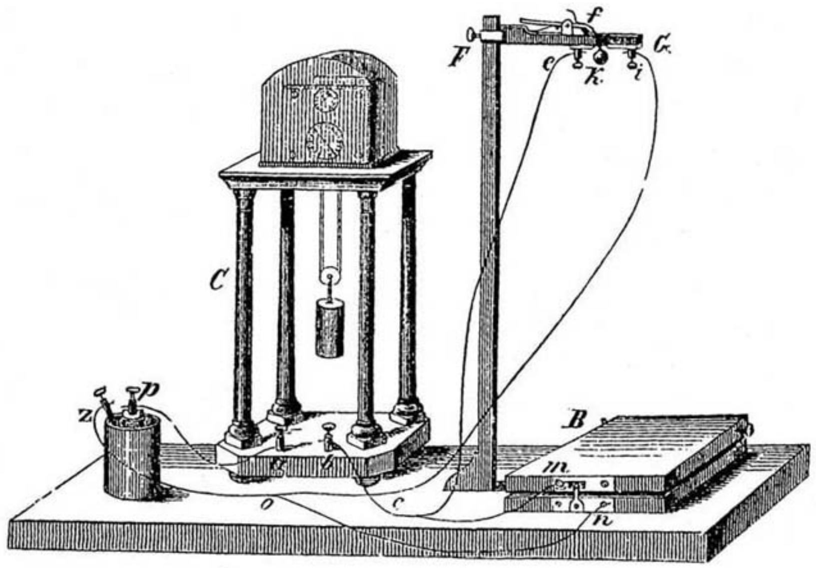

Fig. 4: Hipp chronoscope with falling down and bouncing apparatus.[32]

Moreover, the model for Hipp's precision timer was the electro-magnetic chronoscope constructed by Charles Wheatstone in the early 1840s. Both instruments were explicitly designed to serve ballistic purposes. But already in Wheatstone, trials with falling bodies were the principal method of checking and guaranteeing the precision of all time measurements. In this respect, the ballistic trials with chronoscope as they were performed in Germany in the 1850s can be seen as applied experiments with falling bodies. Instead of using the dropping down apparatus for trials with small metal balls, it was rotated horizontally, then reinforced and extended so that canon balls or rifle bullets might be shot and measured with respect to time (fig. 5).

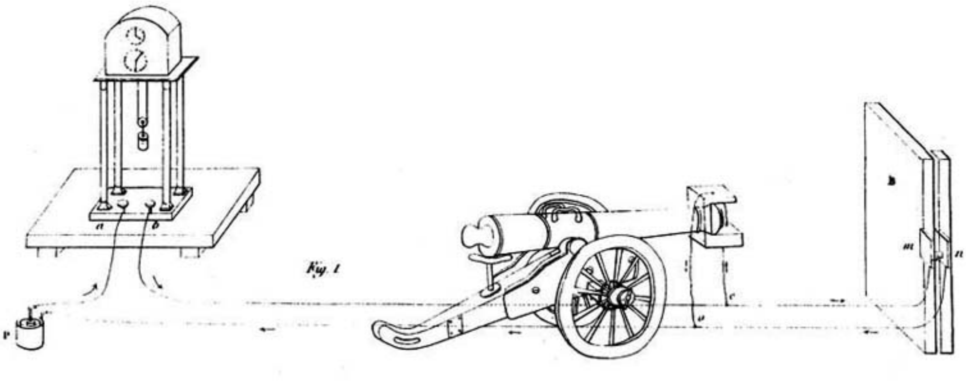

Fig. 5: Experimental set-up for conducting ballistic experiments with a Hipp chronoscope.[33]

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[32] Reproduced from ibid.

[33] Reproduced from Martin de Brettes, "Etudes sur les appareils électro-magnétiques destinés aux expériences de l'artillerie en Angleterre, en Russie, en France, en Prusse, en Belgique, en Suède etc., etc. (III)," *Journal des Armes Spéciales et de l'État-Major* 13/2 (1853), pp. 5-32 and pp. 89-98 (image from accompanying plate).

Besides canons and rifles, human beings could also be integrated into the circuit of the chronoscope. This was precisely what the Neuchâtel astronomer Adolphe Hirsch suggested in the early 1860s in order to measure what he called the "physiological time" of human beings.[34] Hirsch proceeded in the following manner. First, he used the chronoscope and its dropping down apparatus to perform trials with falling bodies. The aim of these trials consisted in checking and calibrating the instrument. Then he re-wired the whole set up in such a way that the time could be measured for a test subject to react to the sound of the ball striking the lower part of the dropping apparatus. Upon hearing the ball striking the board, the subject was asked to operate a telegraph key as quickly as he or she could. Two decades later, this use of the chronoscope had become a common practice in psycho-physiological research (fig. 6). At the same time, uncounted variations were introduced. Psychologists dropped metal, wooden and other balls, large or small, using modified models of the dropping down apparatus, but they also presented optical and tactile stimuli, asking for telegraph key, microphone and other kinds of reactions. No longer was there a question of life and death, instead a whole industry of stimulus-response-couplings emerged.

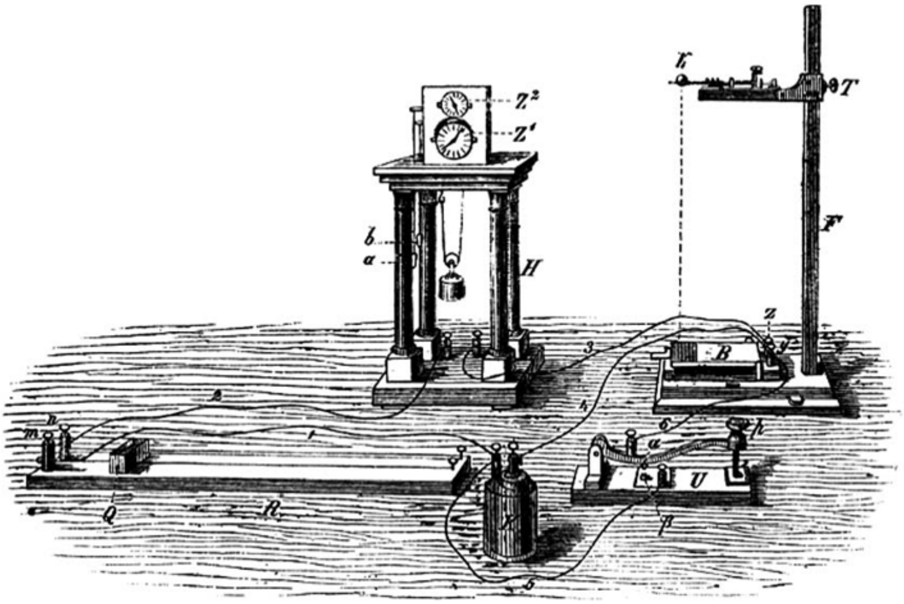

Fig. 6: Reaction experiment with a Hipp chronoscope.[35]

The majority of these machinic assemblages functioned against the background of an epistemology of analytic experimentation. According to John Pickstone,[36] this form of experimental activity does not primarily aim at deciding between well-stated hypotheses. Rather, it serves to elaborate principles for reconstructing complex phenomena from their basic elements.

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[34] Adolphe Hirsch, "Expériences chronoscopiques sur la vitesse des différentes sensations et de la transmission nerveuse," *Bulletin de la Société des Sciences Naturelles de Neuchâtel* 6/1 (1862), pp. 100-114.

[35] Reproduced from Wilhelm Wundt, *Grundzüge der physiologischen Psychologie*, Leipzig: Engelmann, 1874, p. 770.

[36] John V. Pickstone, *Ways of Knowing: A New History of Science, Technology and Medicine*, Chicago: Chicago University Press, 2001, pp. 83-134.

Like chemists, Helmholtz, Hirsch, Wundt and others thus set out to trace compound phenomena situated on the border between physiology and psychology (feeling, thought) back to their basic components (sensations, representations). However, despite the fact that their machines followed similar principles, their functioning clearly differed from the analytical procedures cultivated by organic chemists, botanists and pathological anatomists during the first third of the 19th century. Neither balances nor microscopes were needed in order to set these analytical machines into motion, and the elements they isolated were hardly as tangible as tissues or cells. There was no body that had to be opened and sectioned, no organs or members of the body that had to be cut out or cut off for preparation. What the reaction time experiment did instead was to cut out sequences from a flow of movements or behavior. To be connected to the circuit of the chronoscope, the experimental subject had only to sit still and – following half-technological, half-physiological orders – to react as swiftly as possible to stimuli of all sorts.

The practice of these experiments led to a far-reaching deterritorialization of the human body. The focus was on time measurements, not on concrete anatomy as the basis for the reception, propagation and transformation of stimuli. Of importance was not the precise route a specific excitation took through the body. The main interest was in the time required for taking this route, for accomplishing these processes. Repetitions of trials were meant to improve the reliability of results, and variations of stimuli *and* responses served to detect differences – in time, not in space. The organic body thus only intervened as a flattened entity, an envelope offering multiple access points. Only the distances *on* this envelope contributed to trace the differences in time.

This deterritorialization of the body was connected to and perhaps contradicted by a reterritorializing practice of conceptual distinctions. These distinctions served to analyze the reactions, or behavioral sequences, that were circumscribed by way of experimentation. Based on their respective anatomical and physiological knowledge, Helmholtz, Hirsch and Wundt began to reconfigure the body that was connected to the timing devices. Helmholtz and Hirsch assumed that four partial processes fitted into the span between excitation and reaction.[37] Wundt postulated that five processes were involved, two of them physiological, three of them psychological.[38] The Dutch physiologist Franciscus Donders even counted twelve.[39] No doubt that the ideal of this conceptual analysis was completeness. Empty intervals had to be avoided. But still, sharp distinctions were not be reached – one reason being that the time intervals that were measured proved to be highly variable, something Buffon probably would not have expected. Factors such as the attention of the test subject, the room and body temperature, even the humidity of the laboratory continuously threatened the reliability of results.

Laboratory experts tried to cope with these unexpected findings by re-organizing the technological and biological components of their experimental set-ups. They began to distribute the components of these set-ups over various laboratory rooms that were re-connected by means of telegraphy and telephone; for the experimental subject they built sound proof rooms where the

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[37] Hermann von Helmholtz, "Ueber die Methoden, kleinste Zeittheile zu messen, und ihre Anwendung für physiologische Zwecke," *Königsberger naturwissenschaftliche Unterhaltungen* 2/2 (1851), pp. 169-189, in particular pp. 186-187; Hirsch, "Expériences chronoscopiques," in particular pp. 103-104.

[38] Wilhelm Wundt, *Grundzüge der physiologischen Psychologie*, Leipzig: Engelmann, 1874, pp. 727-728.

[39] Franciscus Donders, "Die Schnelligkeit psychischer Processe: Erster Artikel," *Archiv für Anatomie, Physiologie und wissenschaftliche Medicin* 9 (1868), pp. 657-681, in particular p. 664.

only disturbance was the noise emitted by the subject's body; and they taught their subjects how to focus attention. Around 1900, a younger generation of psychologists criticized these practices as leading to results that were psychologically irrelevant.[40] As a consequence, these new psychologists did away with established standards and, when measuring time, returned to everyday settings. Philosophers (Bergson is a case in point) started to insist on the fundamental uncertainty implied by the span between stimulus and reaction. Other scholars continued, however, to measure reaction times in the Wundtian sense, often triggered by technical improvements. But no doubt that at this point the rather civilized and controlled interaction between falling balls, ticking clocks, and well trained reaction gestures had irrevocably opened a distance, a span, an interval that continues to spread an "experimental night" over us.[41]

Conclusion

This paper started with a Foucault quotation, but by now it will be no surprise that I would like to finish with a quote from Deleuze and Guattari. In the appendix to their seminal *Anti-Oedipus*, the authors comment on their rather idiosyncratic use of the term "machine." As is well known, they define machine, in a quite general manner, as a "*system of interruptions* or breaks (*coupures*)."[42] Here is how they explain: "The object is no longer to compare man and the machine in order to evaluate the correspondences, the extensions, the possible or impossible substitution of one for the other, but to bring them into connection in order to show how man is a component or part of the machine, or combine something else to constitute a machine. The other thing can be a tool or even an animal or other men."[43] I have hardly any doubts that the history of experiments can be incorporated, in its specificity, into a general history of machines in this sense, a history that following Deleuze and Guattari covers such diverse installations as the mega-machine of pyramid construction, the horse-man-bow-ensemble of certain nomad tribes, the factory system as depicted by Marx, and the kinetic sculptures of Jean Tinguely. I would even say that experiments are particularly good examples of machines in this sense, given their material heterogeneity and semiotic productivity, their focus on connecting and cutting, conjunctions and disjunctions, flows and interruptions.

I am less convinced, however, that it will be sufficient, for this purpose, to focus on the technological contexts and the material culture of experiments. Besides spatialization there is a need for temporalization, in synchronic as well as in diachronic perspective. On the one hand, there is the temporal management concerning the "together with" and "against each other" of machinic components, i.e. the actual assembling of processes involving organic and unorganic, material as well as semiotic components. On the other hand, there is the historical process of bringing together and separating machine parts, *movement-moments* that is, a process that aims at the construction of new machines. This process can manifest itself as the rather slow development of concretization, but it also can imply rapid mutations.

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[40] Alfred Binet, *Introduction à la psychologie expérimentale*, Paris: Félix Alcan, 1894, p. 113, p. 119

[41] Gilles Deleuze, *Cinema 2: The Time-Image*, Minneapolis: University of Minnesota Press, 1989, p. 201.

[42] Gilles Deleuze and Félix Guattari, *Anti-Oedipus: Capitalism and Schizophrenia*, Minneapolis: University of Minnesota Press, 1983, p. 36.

[43] Gilles Deleuze and Félix Guattari, "Balance Sheet – Program For Desiring-Machines," *Semiotext(e)* 2/3 (1977), pp. 117-135, quotation on pp. 117-8 (emphasis in the original).

The three scenes I have dealt with in this paper may be understood as frames in the gradual emergence of a machinic assemblage that eventually presented itself as an experiment on the time relations given in the human brain and nervous system. A core element of these machines are movements of falling down; these movements occupy different positions, they are accelerated and slowed down and referenced to one another in different ways. The striking feature of the development of these machines is the disappearance of pain and death, or rather its displacement, since physiologists had and probably still have a well-established practice of decapitating frogs.[44] But although intimately connected to ballistic research, psychophysiological short time measurements did and do not shoot human beings. Perhaps one can say, they transform the end of life into small fractions of time that get lost here and now, every minute, every second. Hence the answer to my initial question. Yes, I guess the history of physiological psychology *can* be referred to Foucault's history of clinical medicine. The time experiments of Helmholtz, Hirsch, Wundt and others do indeed contribute to shape "the armed, positive, and full form of finitude" that Foucault speaks of. But in my eyes this "technical world" needs a different kind of historiography. We cannot rely here on space, language and the gaze, as Foucault did. What is at stake is time, machinic time that is, constituted by transitory couplings between organisms and tools or instruments. Thus what we have to deal with are machines of finitude, machines that in themselves have a specific duration, but also confront us with our own ending by discretely introducing temporal intervals in what we take to be our individual and hence indivisible experience.

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[44] On this issue, see Henning Schmidgen, "Enthauptet und bewußtlos: Zustände der lebenden Maschine in der Psychologie um 1900," in: *Identifikation und Repräsentation*, ed. by Alfred Schäfer and Michael Wimmer, Opladen: Leske+Budrich, 1999, pp. 151-167.

Experimental Readings

Hans-Jörg Rheinberger

What I would like to do in this paper, is to explore the reading of traces in experimental systems. In *Toward a History of Epistemic Things*, I have argued that in a general way, the primary products of experimental setups can be regarded as traces.[1] These traces are then configured into tentative patterns, and in their peculiar materiality, represent the epistemic things at issue in a particular experimental system. Epistemic things thus would be traces or configurations of traces from the very beginning, they were, to speak with Jacques Derrida, graphematically constituted, composed of elementary grammata or graphemes.[2] As is well known, in Derrida the concept of trace is associated with the idea of writing in general and thus, as my title says, I assume that there is something to read or that something renders itself to reading in experimental systems.

Science studies of the past two decades as well have operated, in their laboratory studies, within the space of the metaphor of writing. First and foremost it were Bruno Latour and Steve Woolgar who, in their analyses of Laboratory Life, have brought into circulation the concept of "inscription."[3] However, soon after, Latour joined the critics of what they perceived as the "semiotic turn" [4] and has tried to re-define the products of the laboratory in the sense of his actor-network theory as circulators, as "immutable mobiles."[5] And Ian Hacking, too, has preferred to address the primary outcomes of the laboratory activity in a more neutral sense as "marks."[6]

I will not follow these differentiations here, although it might be worth at some point to study in more detail the efforts undertaken by science studies, philosophy of science and history of science alike to subvert – and I think that is basically with what we have to do here – to subvert the problem of representation. What I would like to do instead is to have a somewhat closer look at a particular experimental procedure and to explore whether it lends itself to bring the concept of experimental trace into sharper relief. The experimental technique is the method of "radioactive tracing." We see that the English expression – in contrast to the German "Markierung" – directly makes use of the notion of trace.

In order to do so, I need to start with a brief historical reminder. It was known since the beginning of the twentieth century that radioactive substances could produce light flashes, so-called scintillations, on especially prepared screens, and people like William Crookes and others soon realized that counting these flashes could be used as a measure for the intensity of the radiation of radioactive elements. One of the first to realize that radioactive elements could be

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[1] Hans-Jörg Rheinberger, *Experimentalsysteme und epistemische Dinge. Eine Geschichte der Proteinsynthese im Reagenzglas*, Göttingen: Wallstein, 2001.

[2] Jacques Derrida, *Grammatologie*, Frankfurt/M.: Suhrkamp, 1974, p. 21 etc.

[3] Bruno Latour and Steve Woolgar, *Laboratory Life. The Construction of Scientific Facts*, Princeton: Princeton University Press, 1986.

[4] Bruno Latour, *Nous n'avons jamais été modernes*, Paris: La Découverte, 1991.

[5] Bruno Latour, "Drawing things together," in: Michael Lynch and Steve Woolgar (eds.), *Representation in Scientific Practice*, Cambridge, MA: MIT Press, 1990, pp. 19-68.

[6] Ian Hacking, "The self-vindication of the laboratory sciences," in: Andrew Pickering (ed.), *Science as Practice and Culture*, Chicago: The University of Chicago Press, 1992, pp. 29-64.

used as "indicators" in the metabolic analysis of organisms was the Hungarian physico-chemist George von Hevesy.[7] He followed the transport of radioactive heavy metals, in particular of lead, in plants. The expression that Hevesy used, "indicator," points to the fact that in this case, a particular substance functions as a kind of pointer in a twofold sense: First, the radioactive decays of the isotope, that were measured since the 1920s predominantly with a Geiger counter, mark the path that a substance takes on its way through the body. Second, however, the characterization of the method as an indicator also refers to the fact that, as a rule, comparatively minute admixtures of the radioactive element were enough to do the job. Thus the indicator principle essentially rested on the possibility of still getting a signal while at the same time minimizing the damage to the tissue through the indicator.

Elements like lead and other heavy metals, however, that spread through the body and eventually become enriched in certain organs, usually still acted as poisons and were essentially restricted to in vivo assays on intact organisms to which the isotopes were fed or into which they were injected. In order to render the procedure more broadly applicable, also for in vitro experiments, and biologically more meaningful, two things were necessary. First, the elements most characteristic for organic matter had to be represented by suitable isotopes. The production of radioactive phosphorus, sulfur, hydrogen, and finally carbon was realized in principle with the advent of cyclotron technology during the 1930s. But only since the early 1940s did they become available in larger quantities as a byproduct of reactor technology and the first piles in the United States.

As a consequence, more complex, radioactively labeled molecules could be produced biochemically and introduced biologically. Now the possibility was given to mark molecules such as the building blocks of proteins, that is amino acids, or the building blocks of nucleic acids, that is nucleotides with radioactive labels. Given a suitable half-life – for radioactive carbon it amounts to somewhat over 5000 years – these molecules behaved, until their decay, exactly like their unlabeled counterparts. What physiologists had consequently in their hands was a kind of masked probe. They could be introduced into the metabolism, and there they participated in the corresponding reactions, and when they decayed, they released a signal. That is, they left a trace at the place of their breakdown.

Second, it was important that the isotopes radiated as weakly as possible upon their decay. Preferably, they released electrons – that is, β-rays – that had a short action range and with that, a small damage potential. This was a decisively important condition for the biological components of an experimental system into which radioactive labels were to be introduced. With these weak isotopes, the biological action range of radioactivity technology became generic. In principle, it comprised all possible biological reactions and test systems.

But the availability of these biologically relevant, weakly radiating isotopes had also an impact on measurement technology. They could no longer be reliably measured with the traditional Geiger-Müller tubes. For β-radiators, Geiger tubes required concentrated, dry samples that were usually difficult, if not impossible to prepare. I will not go into detail here of the history of "liquid

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[7] Georg von Hevesy, "The absorption and translocation of lead by plants: a contribution to the application of the method of radioactive indicators in the investigation of the change of substrate in plants," *Biochemical Journal* 17 (1923), pp. 439-445.

scintillation," a technology which made the generic use of the new isotopes possible. I have a whole paper on that for those who are interested.[8] Here, I will only briefly sketch the principle. In physical terms, the development of these machines after World War II rested on the transformation of the slow electrons of the weak radiators into photons in certain liquid organic substances, followed by amplification via photomultipliers. From a biological perspective, this measurement technology provides, so to speak, a "wet" intersection for test tube experiments involving organic material. For the liquid scintillation counter could accommodate fluid samples containing even water and with that, to render most biological samples ready for radioactive measurement. The liquid scintillation counter technology developed thus in parallel and intertwined with the revolutionization of biological chemistry by radioactive tracers in the course of the 1950s. In addition, through rendering the sample counting automatic, it made possible experiments on a scale and of a design that would have been simply unthinkable on the basis of the earlier counting technologies.

The introduction of radioactivity into the laboratory cultures of biology had, however, also consequences for laboratory architecture and for laboratory life as a whole. The possibility to measure radioactive traces of minimal strength in biological samples decisively depended on the supposition that the environment of the experiment remained uncontaminated. This sounds as trivial as it was all important for the new mode of experimentation. The spread of traces of radioactivity had to be avoided at all costs if the potential signals were not to disappear behind contaminating background radiation. That did not only result in a completely new laboratory regime, but it had also massive effects on the very design and form of the experiments. These few remarks may suffice to indicate that the technology of radioactive tracing cannot be reduced to an instrument. Rather it represents a structure that with its components penetrates and permeates a whole experimental culture. First, it introduced an indicator principle into the analysis of metabolic processes and with that, it oriented biological chemistry into the direction of an in vitro experimental culture that without tracer technology itself could not have developed. For the production of radioactive traces in the test tube also meant the possibility of bypassing a long-hold principle of chemical measurement which meant that chemical substances, in order to be measured at all, had to be rendered in pure form and in sufficient amounts for micromeasurement. In contrast, radioactive measurement could be performed before the impure background of a mixture of all sorts of cellular components. The only thing that had to be granted was that the radioactive probe was molecularly determined in an unequivocal way. Second, as already mentioned, it was the driving force for the development of new measurement technologies, whose integration into the experimental systems of molecular biology did not only alter their size, but also their structure and disposition. Third, it became the material point of mediation between the know hows of biologists, chemists, and physicists. It was thus a technology that in its material structure itself displayed a kind of interdisciplinarity. And finally, it demanded new standards and rules for daily laboratory life, including the disposition of radioactive waste. The generation of traces and the avoidance of traces at the same time showed themselves as the two inseparable sides of one coin.

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[8] Hans-Jörg Rheinberger, "Putting isotopes to work: Liquid scintillation counters, 1950-1970," in: Bernward Joerges and Terry Shinn (eds.), *Instrumentation Between Science, State and Industry*, Dordrecht: Kluwer Academic Publishers, 2001, pp. 143-174.

Of course, visualization and readability are the keywords here. The radioactive trace is the visible remainder of events that otherwise remain in the unseen and do not become graspable. Two examples out of the history of the life sciences of the past half-century will exemplify this. At the same time, they display two completely distinct forms of visualization and readability, one of them in the realm of biological structure, the other one in the realm of biological function.

Toward the end of the 1950s, the small community of protein synthesis researchers had learned to prepare homogenates of bacterial cells and to incorporate amino acids in proteins in the test tube. In the course of these experiments it had turned out that the bacterial homogenate could be separated into two parts, a microsomal sediment and an enzyme supernatant, which were both inactive for themselves, but if mixed together again, the restored the activity of the so-called amino acid "uptake." In addition, it had become evident that in this reaction, two different kinds of ribonucleic acids played a role that soon came to be known under the terms of transfer RNA and messenger RNA. All these findings had come about in an in vitro system, and the picture of the process of amino acid incorporation that emerged, and with it, the picture of the process of protein synthesis, essentially rested on the application of radioactive nucleotides with which to mark the ribonucleic acids, and of radioactive amino acids, whose trace could be followed in the proteins synthesized in the test tube. In the course of about ten years, a slowly differentiating picture of the process took shape. It is schematically depicted in this figure (Fig. 1). Here it can be seen how the radioactive tracer slowly makes visible a dynamic process and piece by piece visualizes a synthetic pathway which in the end completely changed the picture of protein synthesis within the cell. If at the beginning biochemists had preferred the enzymatic imagery of

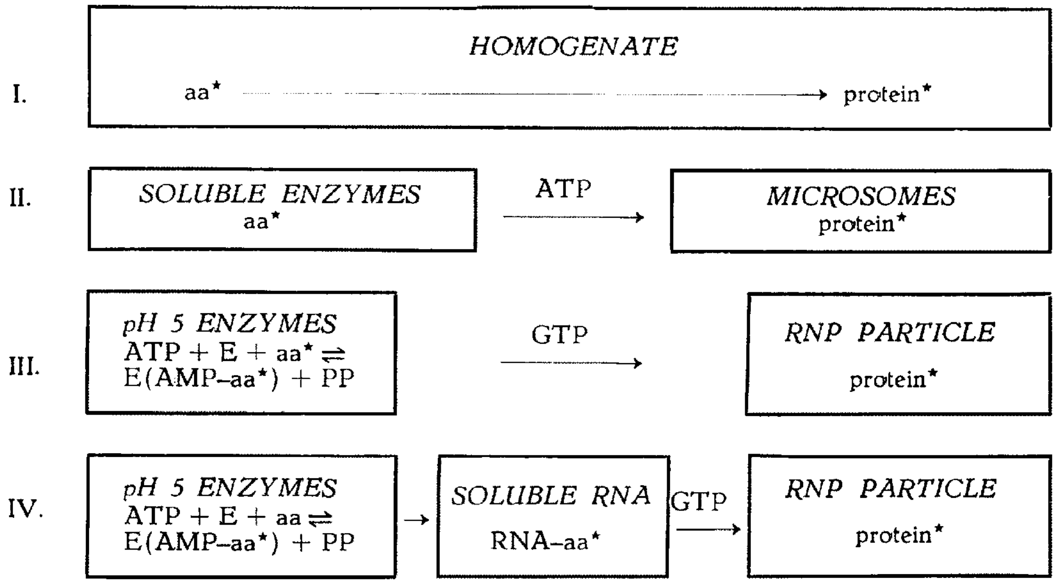
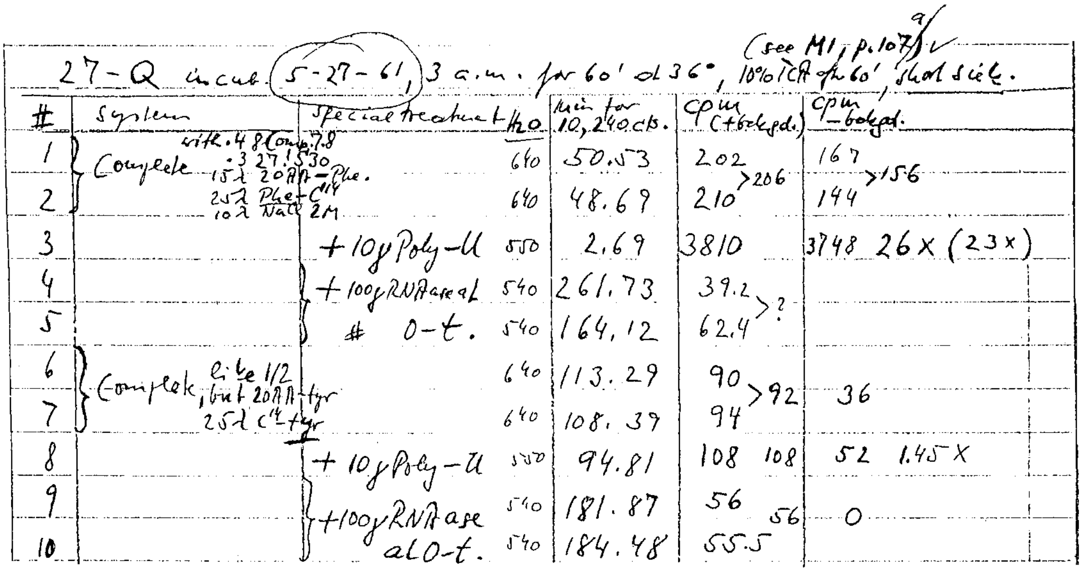


Fig. 1: Stages of dissection of the system of cell-free amino acid incorporation into rat liver homogenates. From Mahlon Hoagland, On an enzymatic reaction between amino acids and nucleic acid and its possible role in protein synthesis. Recueil des Travaux Chimiques des Pays-Bas et de la Belgique 77 (1958), 623-633, there Fig. 1 (Fig. 9.10 in Rheinberger 2001).

protein synthesis as a reversal of protein lysis – and the latter process was already known to be enzymatically mediated – what emerged at the end was the picture of a process directed by molecular templates.

That such a system is capable of creating an experimental space in which completely unexpected perspectives can emerge is shown by the appropriation of the protein synthesis system to solve the riddle of the genetic code my Marshall Nirenberg and Heinrich Matthaei, a story that has been described in detail by Lily Kay.[9] At the beginning, Nirenberg and Matthaei had investigated the stimulation of amino acid incorporation in proteins of the bacterium Escherichia coli by different fractions of nucleic acid. In doing so, they had realized that the test system functioned best if the endogenous ribonucleic acid components were used up in a pre-incubation, before the effect of the newly added components was measured. This was a small, but a decisive twist of the system. In the course of this work, the two researchers of the National Institutes of Health also used artificial ribonucleic acids produced by their colleague Leon Heppel of the neighboring lab. They first used them as negative controls and then came to realize that they also could be used as artificial templates of known composition in order to see whether they stimulated the incorporation of particular amino acids. Proceeding along these lines, they were finally able to correlate certain amino acids with certain combinations of nucleotides and to decipher the first code words. The following figure (Fig. 2) shows the key experiment that led to the identification



(see M1, p.107) a/

27-Q incub. 5-27-61, 3 a.m. for 60' at 36°, 10% TCA for 60', shot sick.

#	System	Special treatment	H2O	Min for 10,240 cts.	cpm (+bckgd.)	cpm −bckgd.
1	Complete with .4 γ Comp. 7.8 .3 27.1 530 15λ 20AA − Phe. 25λ Phe-C14 10λ NaCl 2M		640	50.53	202 >206	167 >156
2			640	48.69	210	144
3		+10γ Poly-U	550	2.69	3810	3748 26x (23x)
4		+100γ RNAase al	540	261.73	39.2 >?	
5		# 0-t.	540	164.12	62.4	
6	Complete, like 1/2, but 20AA−tyr 25λ C14-tyr		640	113.29	90 >92	36
7			640	108.39	94	
8		+10γ Poly-U	550	94.81	108 108	52 1.45X
9		+100γ RNAase	540	181.87	56 56	0
10		at 0-t.	540	184.48	55.5	

Fig. 2: Protocol of the experiment leading to the identification of the first genetic code word. From the laboratory notebook of Heinrich Matthaei, Experiment 27A, 27. May 1961 (Fig. 12.1 in Rheinberger 2001).

of the very first of them, namely a stretch of uracil bases coding for the amino acid phenylalanine. This protocol gives us a nice picture of the intricate system of controls that was necessary in order to stabilize one particular signal. We could also say: to turn a noise into a trace. Experiment 1 and

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[9] Lily Kay, *Who Wrote the Book of Life? A History of the Genetic Code*, Stanford: Stanford University Press, 2000.

2 shows the activity, as it says, of the "complete system" with a full complement of 20 different amino acids of which, however, only one was radioactively labeled: phenylalanine. One sees that there is a residual endogenous "activity" of the system that later becomes subtracted from the other samples as a "background." A 23-fold enhancement of the activity is observed in experiment 3 upon the addition of polyuridine, an artificial homopolymer completely composed of uridine nucleosides. If however at the same time in addition RNase is added (experiments 4 and 5), an enzyme thus that degrades ribonucleic acids, then there is no activity, which in turn allows the conclusion that the measured differential goes indeed back to the addition of the nucleic acid. One also sees here that the controls are usually run as double determinations. Experiments 4 and 5 show, that discrepancies between such double determinations can be considerable, as the question mark behind the two experiments emphasizes. In this case, however, they do not put the result in question. As additional systems controls are added experiments 6 to 10, in which everything remains the same except that instead of radioactive phenylalanine, radioactive tyrosine is added as amino acid. The result shows that tyrosine does not provoke the synthesis of protein in the presence of poly(U). This is a good case of experimental reading, as I would like to put it. We see that the radioactive signal becomes a trace only in a network of controls and so allows the conclusion that a stretch of uridines – later it turned out to be three – leads to the selection of the amino acid phenylalanine. We also see on this example that if radioactive "tracing" at large pervades a whole experimental culture, it also pervades the microcosm of the setup of a particular experimental arrangement.

What was measured in this case, as we can take from the protocol, was "cpm," that is, "counts per minute." These are counting events per minute in a scintillation counter of the kind described before, and as they were at hand at the beginning of the 1960s in proficient laboratories of molecular biology. In order to be able to do these measurements, however, each sample has to be treated in a special way. In this case, it is straightforward: One separates the incorporated radioactivity from the not incorporated one by acid precipitation and a following filtration. The product retained by the filter is placed in the scintillation liquid and counted. Liquid scintillation machines like the then famous TriCarb of Packard were, in principle at least, able to separate the energy spectrum of radioactive phosphorus, hydrogen, and carbon and in this way to allow for performing triple-label experiments. This means that one could follow three different radioactive traces through a particular metabolic pathway at one and the same time.

At the end I would like to briefly discuss a second form of visualization through the generation of traces, connected to the use of radioactively labeled substances. This is the technique of autoradiography. *Grosso modo*, two different variants of it can be distinguished. In the first, an organism receives a radioactive substance. Following this, its incorporation into certain cells or cellular structures is observed. The tissues, cells, or cellular components are prepared and covered with a radiation sensitive film. After incubation, the film is developed and shows a pattern of more or less blackened regions. What can be seen on the following autoradiogram, or better, radioautograph, is the distal segment of the salivary gland chromosome C of the fly *Rhynchosciara angelae* in three stages of larval development (Fig. 3).[10] The larvae of the insect received an

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[10] Hubert Chantrenne, *The Biosynthesis of Proteins*, New York: Pergamon Press, 1961, Fig. 37.

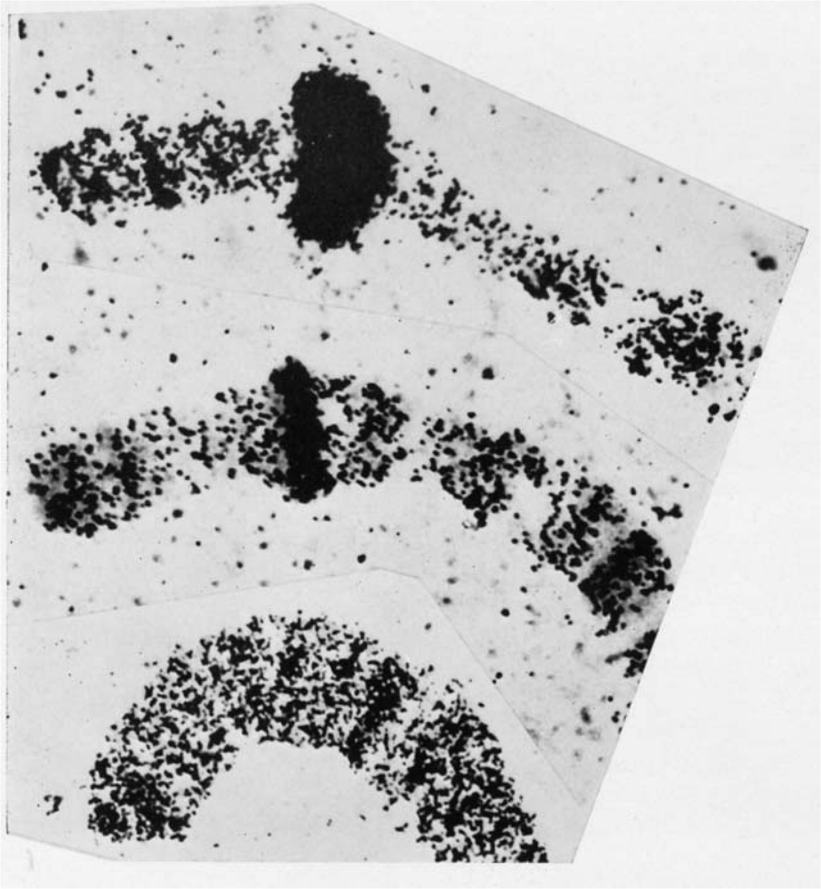


Fig. 3: Radioautograph of the distal segment of chromosome C from the salivary gland of *Rhynchosciara angelae* in three stages of larval developmend. From Adriane Ficq, C. Pavan und Jean Brachet, Experimental Cell Research, Supplement 6 (1959), 105 (Fig. 37 in Chantrenne 1961).

injection of the nucleic acid building block thymidine labeled with radioactive hydrogen. After 24 hours, the chromosomes were removed and fixed. The growing black region around one of the puffs of the giant chromosome indicates a growing nucleic acid turnover at this local point of the chromosome. It can be interpreted as a differential local gene duplication connected to a particular developmental stage of the larva of the insect. Jean Brachet and Adriane Ficq concluded from the work, done the end of the 1950s at the University of Brussels, that embryonic development included a differentiation of the nucleus. With this technique, it became possible to mark metabolic activities *in situ*, either in whole tissues or in particular cellular components, and to fix the traces of these activities also in their temporal flow. The concept of radioautograph says that it is the radioactively doped probe itself that delivers its imprint. The traces it leaves on the photo plate are taken as indicators for processes going on in the depth of the cell and embedded in a complex metabolic network that, importantly, is not destroyed by using this technique.

The second variant is best displayed with a DNA sequence gel (fig. 4 a and b). Here we have a

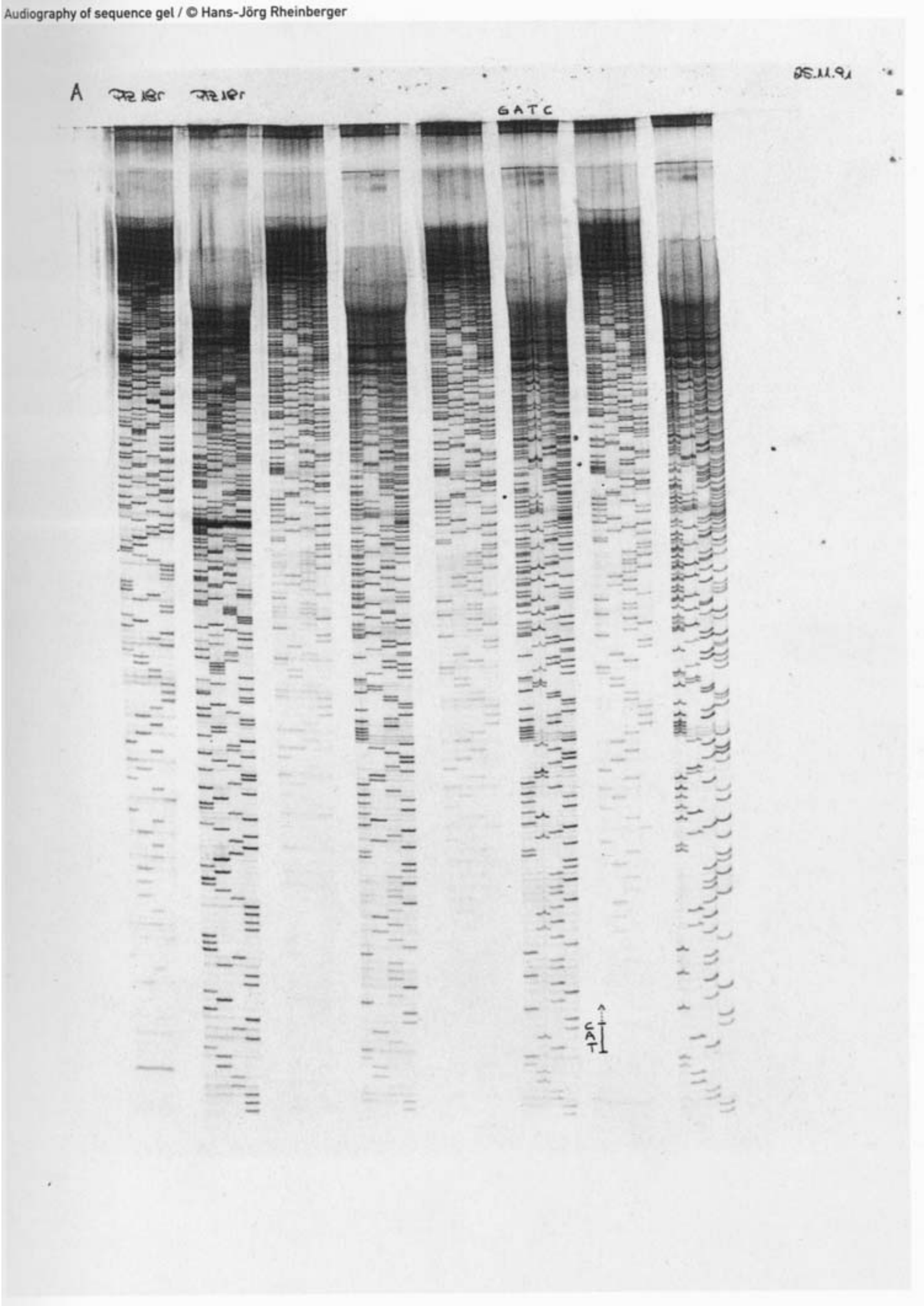
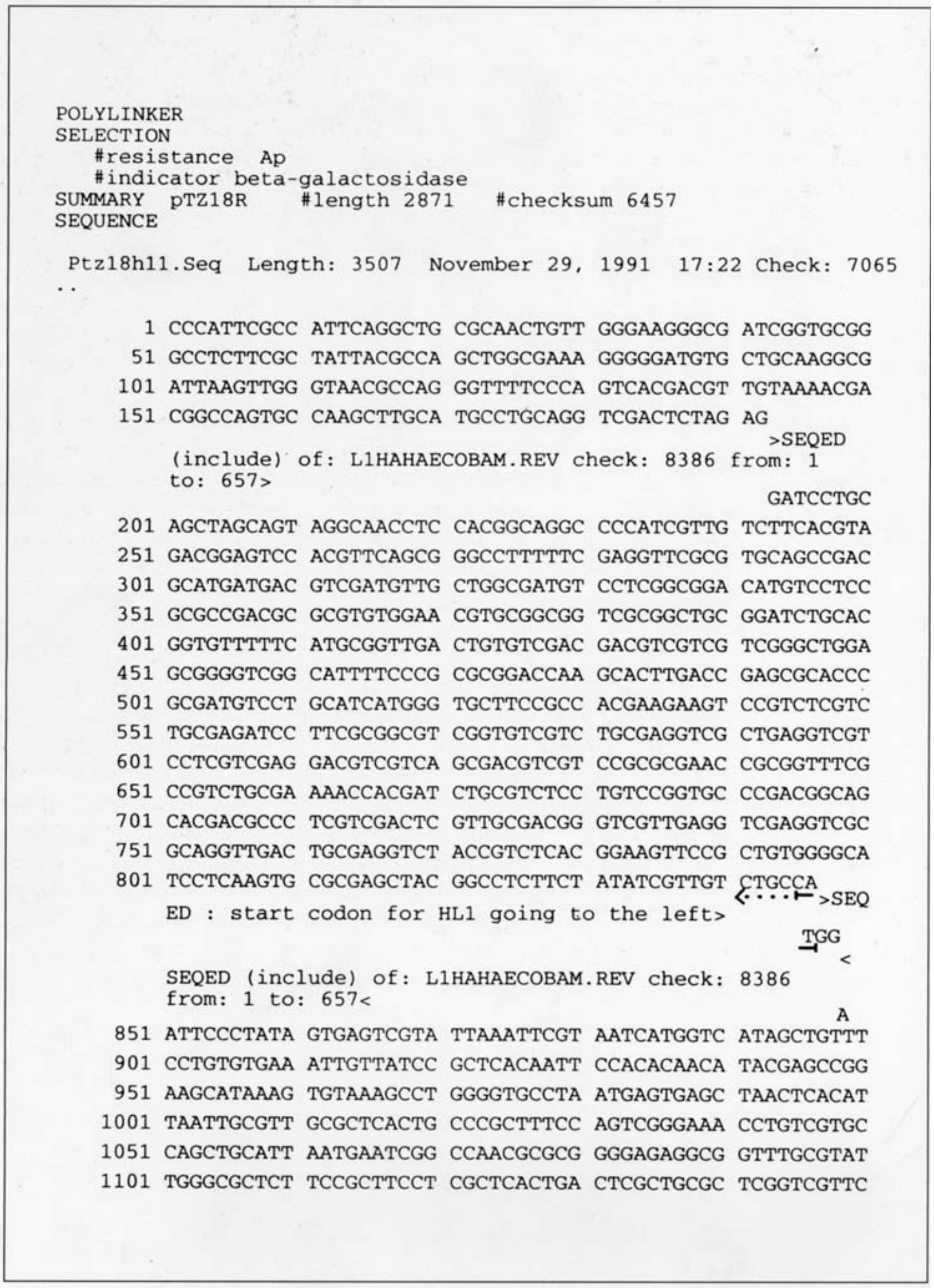

Audiography of sequence gel / © Hans-Jörg Rheinberger

Fig. 4a

Fig. 4: Part of a sequence of the gene for ribosomal protein L1 of *Halobacterium halobium*. With kind permission of François Franceschi, Max Planck Institute for Molecular Genetics, Berlin (taken from Hans-Jörg Rheinberger, "Alles, was überhaupt zu einer Inskription führen kann", in: Norbert Haas, Rainer Nägele and Hans-Jörg Rheinberger, Liechtensteiner Exkurse I, Im Zug der Schrift. Fink Verlag, München 1994, 295-309).

chromatographic procedure, as it is called, that must not only be viewed as a system of recording traces, but as a system of producing traces. With it, the sequence of a nucleic acid can be represented. The procedure was developed by Frederick Sanger in the late 1970s and has revolutionized DNA research. It marks, so to speak, the birth hour of genomics. The principle is as follows. You have a DNA probe, whose sequence you would like to know, doubled by an enzyme called DNA polymerase. Statistically, after each addition of a radioactively labeled nucleic

Nucleic acid sequence / © Hans-Jörg Rheinberger



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POLYLINKER
SELECTION
   #resistance  Ap
   #indicator beta-galactosidase
SUMMARY  pTZ18R    #length 2871   #checksum 6457
SEQUENCE

 Ptz18h11.Seq  Length: 3507  November 29, 1991  17:22 Check: 7065
..

       1 CCCATTCGCC ATTCAGGCTG CGCAACTGTT GGGAAGGGCG ATCGGTGCGG
      51 GCCTCTTCGC TATTACGCCA GCTGGCGAAA GGGGGATGTG CTGCAAGGCG
     101 ATTAAGTTGG GTAACGCCAG GGTTTTCCCA GTCACGACGT TGTAAAACGA
     151 CGGCCAGTGC CAAGCTTGCA TGCCTGCAGG TCGACTCTAG AG
                                                      >SEQED
         (include) of: L1HAHAECOBAM.REV check: 8386 from: 1
         to: 657>
                                                        GATCCTGC
     201 AGCTAGCAGT AGGCAACCTC CACGGCAGGC CCCATCGTTG TCTTCACGTA
     251 GACGGAGTCC ACGTTCAGCG GGCCTTTTTC GAGGTTCGCG TGCAGCCGAC
     301 GCATGATGAC GTCGATGTTG CTGGCGATGT CCTCGGCGGA CATGTCCTCC
     351 GCGCCGACGC GCGTGTGGAA CGTGCGGCGG TCGCGGCTGC GGATCTGCAC
     401 GGTGTTTTTC ATGCGGTTGA CTGTGTCGAC GACGTCGTCG TCGGGCTGGA
     451 GCGGGGTCGG CATTTTCCCG CGCGGACCAA GCACTTGACC GAGCGCACCC
     501 GCGATGTCCT GCATCATGGG TGCTTCCGCC ACGAAGAAGT CCGTCTCGTC
     551 TGCGAGATCC TTCGCGGCGT CGGTGTCGTC TGCGAGGTCG CTGAGGTCGT
     601 CCTCGTCGAG GACGTCGTCA GCGACGTCGT CCGCGCGAAC CGCGGTTTCG
     651 CCGTCTGCGA AAACCACGAT CTGCGTCTCC TGTCCGGTGC CCGACGGCAG
     701 CACGACGCCC TCGTCGACTC GTTGCGACGG GTCGTTGAGG TCGAGGTCGC
     751 GCAGGTTGAC TGCGAGGTCT ACCGTCTCAC GGAAGTTCCG CTGTGGGGCA
     801 TCCTCAAGTG CGCGAGCTAC GGCCTCTTCT ATATCGTTGT CTGCCA
                                                 <····⊢ >SEQ
         ED : start codon for HL1 going to the left>
                                                       TGG
                                                          <
         SEQED (include) of: L1HAHAECOBAM.REV check: 8386
         from: 1 to: 657<
                                                          A
     851 ATTCCCTATA GTGAGTCGTA TTAAATTCGT AATCATGGTC ATAGCTGTTT
     901 CCTGTGTGAA ATTGTTATCC GCTCACAATT CCACACAACA TACGAGCCGG
     951 AAGCATAAAG TGTAAAGCCT GGGGTGCCTA ATGAGTGAGC TAACTCACAT
    1001 TAATTGCGTT GCGCTCACTG CCCGCTTTCC AGTCGGGAAA CCTGTCGTGC
    1051 CAGCTGCATT AATGAATCGG CCAACGCGCG GGGAGAGGCG GTTTGCGTAT
    1101 TGGGCGCTCT TCCGCTTCCT CGCTCACTGA CTCGCTGCGC TCGGTCGTTC
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Fig. 4b

acid building block, a stop of synthesis can occur. This is achieved by adding to the synthesis mixture not only the building blocks A, T, C, and G, but also a certain amount of analogs of them. Whenever an analog is picked, the chain can no longer be prolonged. The result is a mixture of DNA stretches that differ from each other in length by one nucleoside each. After the reaction this DNA mix becomes separated on a polyacrylamid gel plate in four lanes according to the four different bases. From this plate, like in the first variant of radioautography, a photo replica is made. The radioactive bars inscribe themselves on the photo plate and make the sequence readable as a discrete succession of the four letters A, T, C, and G. In the present case, the sequence is part of the gene for a bacterial ribosomal protein.

In a recent paper on "preparations" in the life sciences, I have explored in more detail the particular form of indexicality of such molecular biological procedures.[11] What I would like to emphasize here is that the graphematic configurations through which, in radioactive tracing, the elementary structures and functions of the living and its molecules are visualized and rendered readable – that these graphematic configurations have created a space for possible traces in which the game – and sometimes also the drama – of the molecular representation of epistemic things was played for several decades, and in resonance with the symbolism of the atomic age. The decisive experimental systems of the molecular biological revolution after World War II would not only have been un*doable*, but also un*thinkable* without the procedures of radioactive tracing. They created a new horizon for the "game of the possible," as François Jacob would have said.[12] In the meantime, many of these radioactive visualization procedures have been replaced by other ones. They now belong to the historical sediment of those traces on which all experimental science is built. With that, they themselves are transformed into a trace for the history of science, a trace however that characterizes and punctuates a whole epoch of the life sciences.

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[11] Hans-Jörg Rheinberger, "Präparate – 'Bilder' ihrer selbst," *Bildwelten des Wissens*, Kunsthistorisches Jahrbuch für Bildkritik 1/2 (2003), pp. 9-19.

[12] François Jacob, *Le jeu des possibles*, Paris: Fayard, 1981.

Purifying Objects, Breeding Tools: Observational and Experimental Strategies in Nineteenth-Century Gas Discharge Research

Falk Müller

The alchemy of light

> Cela dit, mon oncle prit d'une main l'appareil de Ruhmkorff suspendu à son cou; de l'autre, il mit en communication le courant électrique avec le serpentin de la lanterne, et une assez vive lumière dissipa les ténèbres de la galerie. Hans portait le second appareil, qui fut également mis en activité. Cette ingénieuse application de l'électricité nous permettait d'aller longtemps en créant un jour artificiel, même au milieu des gaz les plus inflammables. [...] La lumière des appareils, répercutée par les petites facettes de la masse rocheuse, croisait ses jets de feu sous tous les angles, et je m'imaginais voyager à travers un diamant creux, dans lequel les rayons se brisaient en mille éblouissements.[1]

In his *Voyage au centre de la terre* from 1864 Jules Verne equipped his adventurers with a specific light source: Stored in a leather bag was a battery connected to a Ruhmkorff inductor, which would produce several hundred volts. The battery and induction coil were then used to ignite gas residues in small, spiral glass tubes. This special arrangement of instruments had been suggested as a safety lamp for miners in 1862,[2] and Verne may have seen it when it was shown at a fair as a novelty. In his story, the minerals of a cave exploded into a fancy kaleidoscope of colours and shapes when the pale light of the electric tube was switched on. In reality the tube's light was not very bright; only in the twentieth century decisively improved discharge tubes were used for illumination. However, Verne may have seen experiments with other evacuated glass vessels in the workshop of the instrument maker Heinrich Daniel Ruhmkorff in Paris.

Ruhmkorff had introduced his induction coil around 1850 and soon collaborated with the physicist and chemist Jean A. Quet in investigating the colourful and curiously shaped luminous effects in a so-called electric egg.[3]

This research was not only for personal pleasure. The coil and the spectacular electrical effects it produced were used as marketing instruments. They were shown at fairs and scientific meetings,[4] and in 1855, Ruhmkorff sponsored a scientific publication that further popularised his

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[1] Jules Verne, "Voyage au centre de la terre," in: ibid., *Les voyages extraordinaires*, Paris 1977, pp. 96 and 115.

[2] One of these devices (purchased in 1863) can still be seen in Haarlem's Teyler Museum; see Gerard L'E. Turner, *The Practice of Science in the Nineteenth Century: Teaching and Research Apparatus in the Teyler Museum*, Haarlem 1996, p. 296 for an image and description of the device and a short note on the two inventors.

[3] Until the mid-nineteenth century these were used as standard instruments for investigating discharge effects in gases at variable pressures.

[4] The physicist Otto Lehmann reported that in 1858 Ruhmkorff came to Karlsruhe to show his induction machine and the "marvellous discharge phenomena it produced" on the occasion of a meeting of the *Association of German Naturalists and Physicians*. The local sovereign was so enthusiastic about the demonstration that he decided to purchase such a machine for the physics department of the Karlsruhe polytechnic school – against the vote of the school's director (Otto Lehmann, *Die elektrischen Lichterscheinungen oder Entladungen bezeichnet als Glimmen, Büschel, Funken und Lichtbogen in freier Luft und in Vacuumröhren,* Halle 1898, p. 550).

device.[5] In 1858 Ruhmkorff seems to have incorporated another recent technical innovation into his laboratory – vacuum tubes. These tubes were not produced by Ruhmkorff himself. He bought them from a friend, the glass blower and instrument maker Heinrich Geissler in Bonn.[6] Most of these so called "Geissler tubes" were artfully shaped; in some cases several layers of glass were nested inside each other, resulting in complex architectures with cavities partly filled with fluorescent liquids.

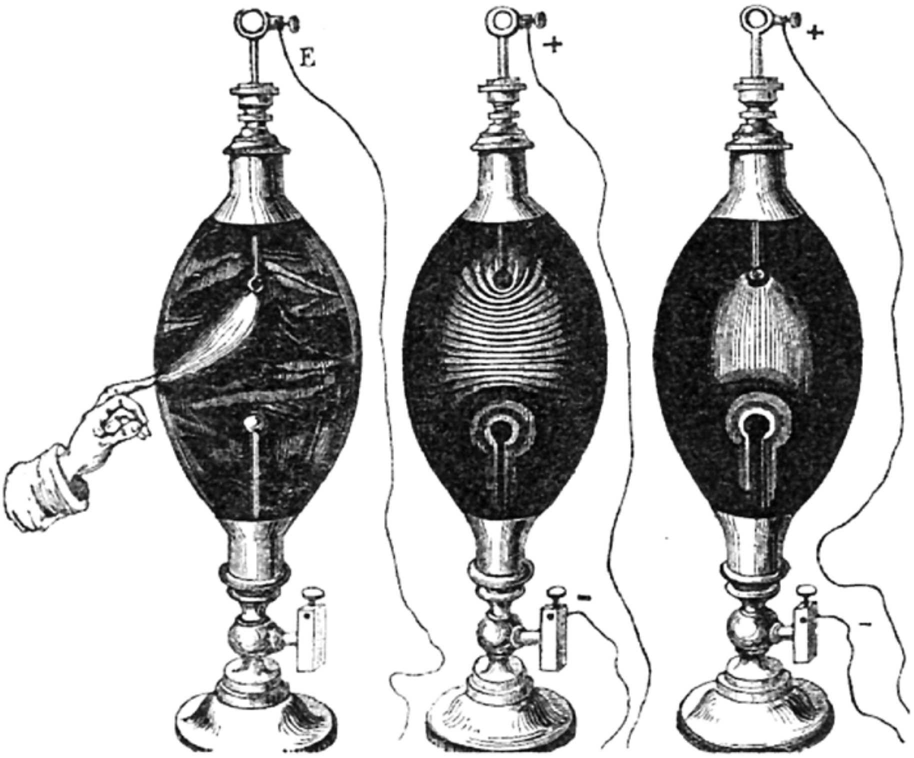

Fig. 1: Experiments by Ruhmkorff and Quet showing stratifications of the positive light in an "electric egg."

Geissler's tubes were primarily produced for a commercial market, and most innovations he introduced were meant to enhance the aesthetic attraction – a fact that annoyed several researchers who would rather have had more scientific and less sophisticated instruments.

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[5] The book was published by Théodore Achille Louis du Moncel, *Notice sur l'appareil d´induction électrique de Ruhmkorff,* Paris 1855. In the preface of the German edition (translated and extended by C. Bromeis and J.F. Bokkelmann, Frankfurt/Main 1857) the authors report that during their stay in Paris, Ruhmkorff called their attention to the French original.

[6] Heinrich Geissler (1814-1879) was originally from Thuringia, a region with a long tradition in glass blowing and home to many nineteenth-century German instrument makers and their dynasties. After Geissler learned his father's trade he worked as a glass blower in several German and Dutch cities before he settled for good in Bonn in the early 1850s (see Karl Eichhorn, *Heinrich Geißler(1814-1879). Leben und Werk des Thüringer Glasinstrumentenbauers und Pioniers der Vakuumtechnik,* in: Jahrbuch 1995 des Hennebergisch-Fränkischen Geschichtsvereins, Bd. 10, Meiningen/Münnerstadt). In a letter to Justus von Liebig from February 2, 1858, Geissler reports a shipment of fifty tubes to Ruhmkorff (Bayrische Staatsbibliothek, München, Liebigiana II B, Geißler, H.). Eichhorn, *Heinrich Geißler(1814-1879).*

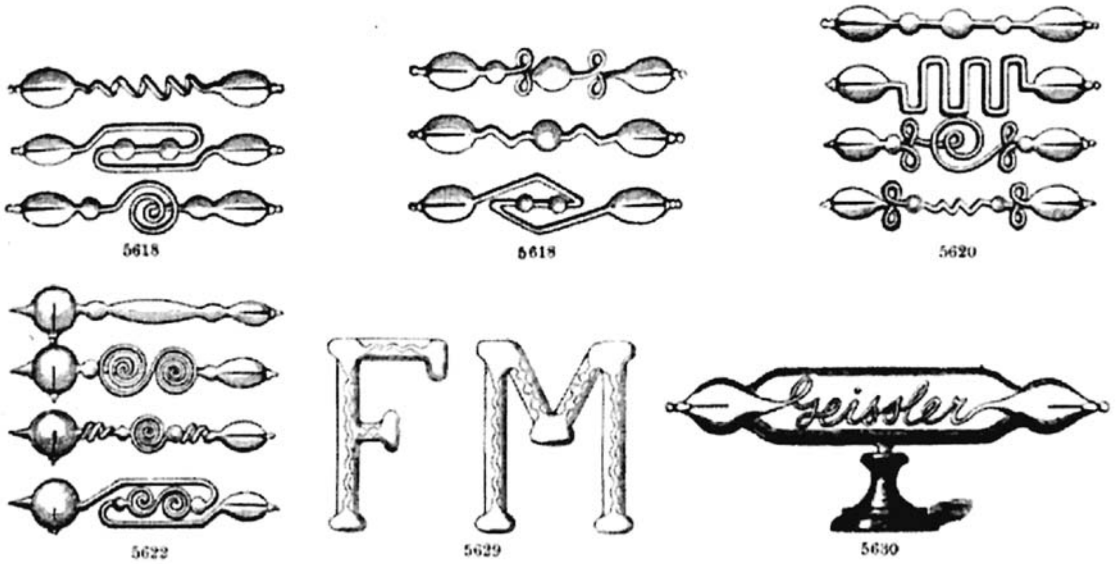

Fig. 2: Images taken from a catalogue of the company Franz Müller, Geißler's successor, from 1904 showing tubes that were still produced in the fashion Geissler had introduced in the late 1850s.

As early as the eighteenth century artfully shaped and evacuated glass tubes were used to literally "inscribe" a meaning, as can be seen from a description of some discharge experiments by Johann Heinrich Winkler from 1745:

> If tubes are shaped in the form of letters, they will shine brightly if approached by strongly electrified metal in a dark room. [...] Distinguished people to whom I had the honour of showing my experiments were especially amused when the initials of the most serene name AUGUSTUS REX were hanging in the air brightly lit or virtually burning in a completely dark room, and when they saw the letters suddenly filled with a waving flood of electric light.[7]

Similar tubes were later produced by Geissler for advertising – although with platinum wires as electrodes, more durable evacuations, and filled with traces of various gases. In the 1890s William Crookes combined the shape of tubes with light in a specific kind of performative "speech" act or inscription: "I also noticed," Lord Rayleigh later recalled, "on the wall of the laboratory, what one might regard as the parent of the modern neon sign – a vacuum tube bent to spell the word 'electricity'."[8] According to Marshall McLuhan "electric light is pure information," a medium without a message, "unless it is used to spell out some verbal ad or name."[9] If McLuhan therefore emphasises that the "content" of any medium is always another medium it may be pointed out

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[7] J.H.Winkler, *Die Eigenschaften der electrischen Materie und des electrischen Feuers, aus verschiedenen neuen Versuchen ercläret, und, nebst etlichen neuen Maschinen zum Electrischen beschrieben*, Leipzig 1745, p. 66.

[8] Lord Rayleigh (Robert John Strutt), "Some reminiscences of scientific workers of the past generation, and their surroundings," *The Proceedings of the Physical Society* 48 (1.3.1936), pp. 217-246, quotation on p. 238.

[9] Marshall McLuhan, *Understanding Media. The Extensions of Man*, New York 1964, p. 8.

that in the case of 19th century gas discharge research the intersection of media and message turned out to be much more complicated. While the case of luminous letters is quite simple, Geissler and other researchers and instrument makers of the nineteenth century added stratifications, cathode rays and various other embodiments of the "electric light" that could possibly serve as a medium. Many researchers hoped that the luminous effects were direct manifestations of obscure and hidden processes and causes, e.g. specific disturbances of the physical ether. Many saw the tubes as a good opportunity to investigate the interaction of various "forces" (light, heat, electricity, magnetism, ether etc.). There were many possibilities to implement and inscribe meaningful structures, and researchers applied many tools such as revolving mirrors, spectroscopes, magnets, photographic plates and galvanometers, and they constructed complex apparatus to explore the possible and precipitate something significant.[10] The vacuum tubes served as a core for the development of a rich material culture and of an experimental system that entered and connected several areas of research.

A first account of experiments conducted with Geissler tubes in Bonn – a small book that was probably financed by Geissler – was published by the curator of the university's physics cabinet, Theodor Meyer, on the occasion of the 1857 meeting of the *Versammlung Deutscher Naturforscher und Aerzte* in Bonn. As a distinctive feature, the book contained coloured plates, which allowed a wider audience to get at least some impression of what the luminous effects were about. A mere exemplary excursion, as Meyer complained: "I have seen about 40 to 50 tubes many times: what peculiar diversity and intrinsic individuality. To this day I have not observed two tubes that have a single detail in common although they were produced under the same circumstances."[11] To capture the nuances and contours of colours in their descriptions, the researchers used flowery terms, like those found in a chart by the French scientist Morren who introduced colours like "light rosy, glossy" and graduations like "entirely and absolutely white" or "decidedly white" or subjective utterances like "sky-blue, very beautiful."[12]

Most researchers in the 1860s and 70s concentrated on the colourful and beautiful light of the "positive column" (the light around or near the anode) and the complicated shapes of its stratifications. This approach possibly reached the highest state of perfection in experiments performed by the British chemist Warren de la Rue and his colleague Hugo Müller in the late 1870s. With the help of high voltage batteries consisting of several thousand elements, de la Rue and Müller managed to produce and stabilise some remarkable shapes or so called "entities" inside their tubes. The metamorphoses of phenomena caused by changes of voltage or pressure were registered and described in "histories" of these phenomena which were accompanied by diagrams and documented by sequels of photographs.

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[10] New instruments opened up new perspectives and exposed hidden details. For the engineer and spiritualist Cromwell Varley the photographic plate turned out to be an important extension of his senses in his experiments with gas discharge tubes: "The eye and the collodion-plate do not, however, tell the same tale." C. F. Varley, "Some Experiments on the Discharge of Electricity through Rarefied Media and the Atmosphere," *Proceedings of the Royal Society of London* 19 (1871), pp. 236-242, quotation on p. 238.

[11] W. H. Theodor Meyer, *Beobachtungen über das geschichtete electrische Licht sowie über den merkwürdigen Einfluß des Magneten auf dasselbe nebst Anleitung zur experimentellen Darstellung der fraglichen Erscheinungen,* Berlin 1858, p. 14.

[12] A. Morren, "Ueber die elektrische Leitungsfähigkeit der Gase unter schwachen Drucken," *Poggendorfs Annalen der Physik und Chemie* 130 (1867), pp. 612-636, quotation on p. 629.

> 12 very similar luminosities, but on introducing 500,000 ohms into the circuit they changed to *arrow-headed entities* [...] the introduction of 1,000,000 ohms changed them into parallel *worm-like entities* [...] With 4,500,000 ohms resistance all the luminosities disappeared, a nebulous light reaching from the positive half-way towards the negative shrank in a few seconds up to the positive and disappeared, leaving a faint glow on the positive terminal.[13]

Fluorescenz der Röhrenwand	Farbe	
	der Aureole	des Lichts in der Röhre
	Wasserstoff	
Sehr groſs	Rosiges Weiſs	Sehr lebhaft purpurroth in engen Röhren
	Stickstoff	
Sehr groſs und sehr glänzend	Ganz und rein weiſs	Kupferroth, glänzend in kleinen Röhren, sanft in groſsen
	Sauerstoff	
Sehr wenig bedeutend	Weiſs, grau rosig	Sehr sanftes Rosenroth
	Kohlensäure	
Sehr sanft	Flachsblau, zart und sehr rein	Weiſsliches Grün
	Kohlenoxyd	
Sehr sanft	Entschieden weiſs	Weiſsliches Grün
	Schweflige Säure	
Ziemlich sanft	Weiſs	Himmelblau, sehr schön
	Atmosphärische Luft	
Noch geringer	Veilchenblau	Hell rosig, glänzend

Fig. 3: Chart taken from Morren's article.

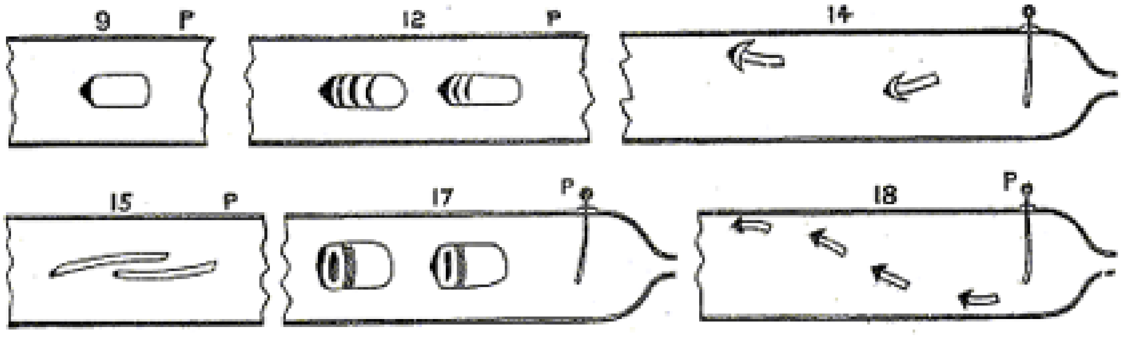

Fig. 4: Sketch of "entities" in de la Rue and Müller's experiments.

Some of these experiments were shown on a Friday-Evening lecture at the Royal Institution and contemporary observers where impressed by the complexity and beauty of the produced shapes.[14]

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[13] Warren de la Rue and Hugo Müller, "Experimental Researches on the Electrical Discharge with the Cloride of Silver Battery," *Transactions of the Royal Society* 169 (1878), pp. 55-121 and 155-241, p. 198.

For romantic characters these hieroglyphic figures waited for a decryption. But: if these appearances were signs or symbols, what was their meaning?

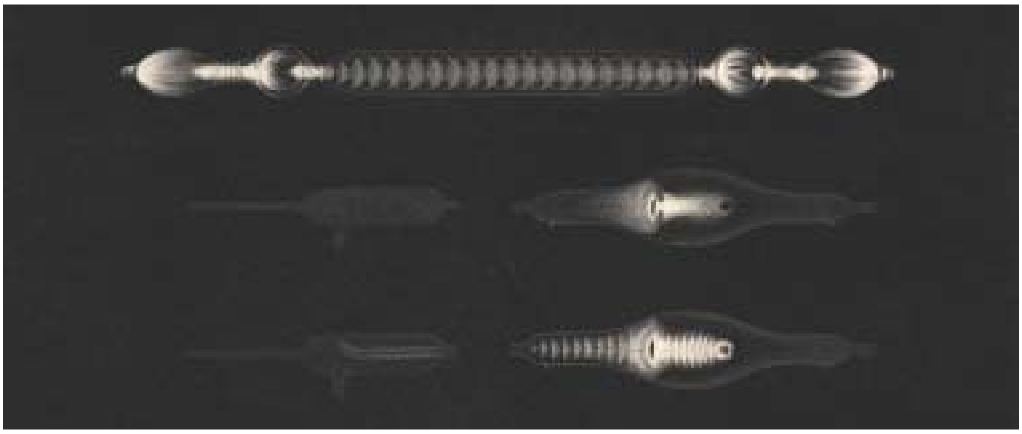

Fig. 5: Photographs of discharge phenomena taken by Warren de la Rue and Hugo Müller.

The outcome of the application of new instruments and the development of new approaches was an overwhelming multitude of shapes, signs and signatures – and the remaining question if these experimental findings could possibly be merged into a coherent whole, if one was dealing with a single phenomenon or a superposition of several.

An attempted geometrification

As soon as Geissler had shown the tubes and a newly constructed mercury air pump at Bonn University's physics cabinet in 1857, he started a cooperative investigation with the local professor of mathematics and physics, Julius Plücker. Plücker was educated as a mathematician, and one would have expected that he would start a mathematical study of these objects as soon as possible. His publications, however, give only occasional evidence that this was the case. Plücker started his research with experiments on the nature of the stratifications of positive light. Soon additional research foci followed, namely the investigation of a new class of phenomena that had already been noticed by Michael Faraday in the 1830s but which had never aroused as much attention as the sparkling effects of the positive light: a bright blue light that was localised close to the surface of the cathode. This "negative light" spread into the surrounding space when the evacuation was enhanced; Geissler's new mercury air pump turned out to be of crucial advantage here. In 1876 Eugen Goldstein would term this light "cathode rays."

The diversity of all these appearances intrigued Plücker, and here we may even find a bridge to his mathematical research.[15] Plücker was looking for figures that could be compared to mathematical or geometrical shapes. In his comparative investigation of the behaviour of "negative" and "positive" light under the influence of a strong magnetic field, he found that

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[14] "The 'Friday Evening' at which [de la Rue] gave account of them was on a heroic scale. The preparations of the experiment occupied, I believe, nine months. He set up in the institution, for his lecture, a battery of 14,000 cells. [...] It was rumored that he spent many hundrets of pounds on its preparation." J. J. Thomson, "Reminiscences of Physics and Physicists," *Science* 80 (24.8.1934), p. 171.

negative light, if extending from a wire into the surrounding space, would be bent in agreement with Faraday's "lines of magnetic force," as he enthusiastically wrote to Faraday in 1857:

> I can, in a few words give no better account of them but by saying, that I am enabled by means of the electric light, to *render luminous your lines of magnetic force.*[16]

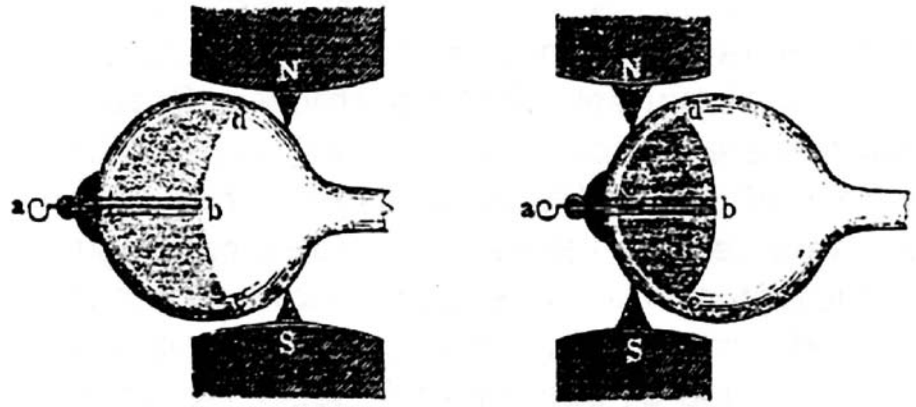

Fig. 6: "Negative light" bent according to the "lines of magnetic force."

While, as Plücker emphasized, "[h]itherto only filings of iron enabled us to give in peculiar cases an imperfect image of these curves,"[17] now the electric light could serve as a more suitable medium:

> It equally represents the form which a chain of infinitely small iron needles, absolutely flexible and not subjected to gravity, would assume, if attached with one of its points in the point of the negative wire.[18]

The bulky material that had been used before could now be substituted by infinitely small, absolutely flexible elements – attributes more suited to an idealised or mathematical world than a

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[15] The mathematician Alfred Clebsch wrote about Plücker's mathematical style: "Plücker's [...] way of thinking was of a more productive than analytical manner. It offered him the complete satisfaction of the richness of new shapes and formations fed by the productive fertility of his unfailing fantasy." A. Clebsch, "Zum Gedächtniss an Julius Plücker," *Abhandlungen der Königlichen Gesellschaft der Wissenschaften zu Göttingen* 16 (1871), pp. 1-40, quotation on p. 5.

[16] Plücker to Faraday, December 27, 1857, in: L.P. Williams (ed.), *The Selected Correspondence of Michael Faraday. Vol. II 1849-1866,* Cambridge 1933, p. 891. Here one is tempted to quote Maxwell's hymn to Faraday's "mind's eye:" "Faraday, in his mind's eye, saw lines of force traversing all space where the mathematicians saw centres of force acting at a distance: Faraday saw a medium where they saw nothing but distance: Faraday sought the seat of the phenomena in real actions going on in the medium, they were satisfied that they had found it in a power of action at a distance impressed on the electric fluids." Maxwell, James Clerk, *A Treatise on Electricity and Magnetism*, Vol. 1. New York 1954, p. IX.

[17] Julius Plücker, "Abstract of a Series of Papers and Notes Concerning the Electrical Discharge through Rarefied Gases and Vapours," *Proceedings of the Royal Society* 10 (1858/59), p. 259.

[18] Ibid, p. 258.

material one. The electric light could thus be used to visualise the distribution of the magnetic force in those cases where “the mathematical analysis fails;”[19] for Plücker, this kind of analysis could even serve as a possible alternative to a mathematical treatment.

Plücker henceforth called the negative light “magnetic light.” Its performance in a magnetic field stood in sharp contrast to the behaviour of the positive light, which twisted like a spiral, comparable to the behaviour of a thin metallic conductor under the same conditions. The positive light was therefore termed “electrical light” or “electrical stream of light.”

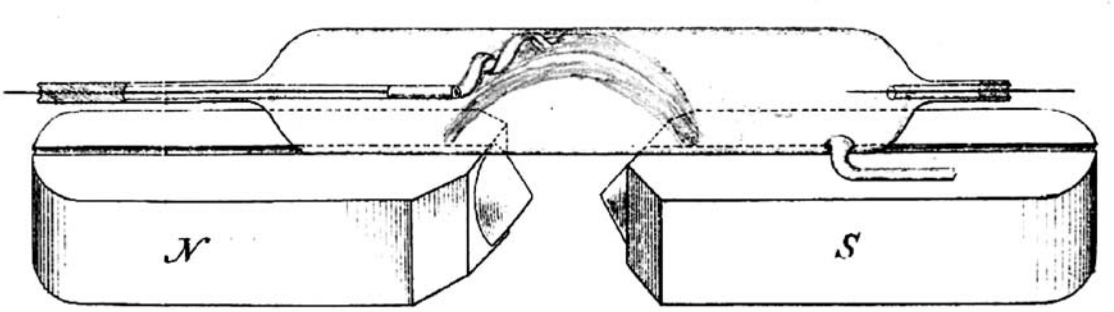

Fig. 7: Demonstration of the behaviour of the luminous effects in a magnetic field in one of Wilhelm Hittorf’s publications.

Another area that absorbed Plücker’s attention more and more was the prismatic analysis of the luminous effects. He soon noticed that, seen through the prism, the nuances of light could be reduced to a few characteristic lines of a “mathematical sharpness” that seemingly only changed in intensity when conditions changed. As he wrote:

> But whatever colour impression the eye experiences, tubes containing the same gas will show the same arrangement of spectral colours, and their intensities will vary in a specifiable way. While the eye, whose judgement is, moreover, influenced by the varying conditions of the external light, gives no further information, the spectrum will undoubtedly identify the kind of gas or vapour contained in the tube.[20]

For this research, Geissler constructed special tubes with spacious bulbs around the electrodes and a narrow capillary tube in between. Using a burner, various elementary and compound substances could be evaporated. In 1858 – more than a year before the official birth of spectrum analysis – Plücker had already tried to introduce a spectroscopic approach that aimed far beyond the identification of chemical substances. He wanted to establish a dynamic approach that would allow the observation and investigation of chemical decomposition: a new branch of science which he called “micro chemistry.”[21]

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[19] Julius Plücker, “Mittheilung über eine neue physikalische Erscheinung,” *Verhandlungen des naturhistorischen Vereins der preussischen Rheinlande und Westphalens* 15 (1858), p. XXX.

[20] Julius Plücker, “Fortgesetzte Beobachtungen über die elektrischen Entladungen durch gasverdünnte Räume,” *Poggendorfs Annalen der Physik und Chemie* 104 (1858), pp. 113-128, quotation on p. 123.

[21] Ibid., p. 128.

In Plücker's understanding the luminous effects were a visual indication of the relation of electricity and matter. He was convinced, as Faraday was, that there was no electricity without matter and therefore no electric light per se. Any explanation of the electric light had to be accompanied by a more thorough understanding of the nature of matter as the medium of the electrical current:

> Considering the complete lack of knowledge regarding the constitution of material bodies and its influence on the constitution of the electric current, it is not surprising that we do not find a satisfactory explanation for the above-mentioned phenomena. New hypotheses, which currently can only take the form of pictures and symbols will not reach the status of a sound explanation until they involve more specific assumptions on the nature of the current and its medium.[22]

Due to oftentimes unconnected observations and experiments and a light-hearted devotion to new theories and explanations, it is difficult to outline Plücker's research. He described many examples of phenomena that were only discussed and seen as important much later. In most cases he was content with having suggested them – or as Erwin Hiebert interprets Plücker's approach:

> Problems of interpretation, and ideas on potential ways to secure deeper (theoretical?) insights, had a way of becoming translated, not so much into theoretical solutions, as into variant puzzlements that pointed to alternative modes of experimental attack. Not every puzzle was attractive or manageable, but certain puzzles, perennially coming into focus, would exhibit a sturdiness that was embedded securely within the realm of nature. Such a puzzle had a life of its own, not to be snuffed out or sidestepped.[23]

Johann Wilhelm Hittorf (1824-1914)

Plücker's "micro chemistry" met with a disappointing public resonance. The successful application of spectrum analysis by Bunsen and Kirchhoff intensified Plücker's feelings that his research and his contributions were not properly acknowledged. It became more and more clear that he had to find a partner with sophisticated skills in chemistry and experimental technology, and he invited his former student Johann Wilhelm Hittorf to join his research project.

Hittorf had left Bonn in 1847 after finishing his studies with a mathematical dissertation on conic sections. He went to the provincial Prussian city of Münster to become *Privatdozent* and later professor of physics and chemistry at the local college. In the 1850s he carried out a long and demanding analysis of electrolytic conduction. As a result he found out that different ions move with different velocities – results that were seen much later as supporting the theory of dissolution and free ion transport in solutions. Hittorf was driven by the hope of understanding the transformation between ionic and atomic states of matter, a process that obviously happened at the electrodes but was not understood. A main purpose was to show the applicability of Ohm's

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[22] Julius Plücker, "Ueber die Einwirkung des Magneten auf die elektrische Entladung," *Poggendorfs Annalen der Physik und Chemie* 113 (1861), pp. 249-280, quotation on p. 520.

[23] Erwin N. Hiebert, "Electric Discharge in Rarefied Gases: The Dominion of Experiment." Faraday, Plücker, Hittorf, in: A.J. Kox and D.M. Siegel (eds.), *No Truth Except in the Details*, Dordrecht 1995, pp. 95-134, quotation on p. 109.

law to electrolytic conduction. For Hittorf both Ohm's and Faraday's laws stood as outstanding examples of simple formalism that satisfied empirical and theoretical requirements without drawing on metaphysical propositions such as electrical or other "scholastic" fluids, which Hittorf ardently disliked.

These electrolytic studies had rendered Hittorf a skilled worker in investigating the electrical properties of solutions and solids at the boundary between physical and chemical research. In his cooperation with Plücker he took over the task of preparing the discharge tubes for the spectral analysis – evacuating them and filling them with various gases. He soon not only developed exceptional skills in purifying the substances, but he also introduced important improvements in vacuum technology. His mastery in preparing high quality and long-lasting evacuations soon exceeded even Geissler's skills.

Hittorf's improvements in vacuum technology were to a great extent due to the application of spectroscopy. The spectroscope made visible the finest cracks of the glass walls or disturbing effects of the used materials by detecting even the faintest quantities of nitrogen or other gases. While other researchers were quite content with or had to make do with the vacua they produced, Hittorf introduced into his apparatus a factor that forced him to go on with his search for leakage and hidden gases occluded in glass walls and electrodes. In an interesting way we find here that an epistemic thing became a technological object that was then reintroduced into the research process from which it originated. Hitherto discharge tubes had been used as mere engines for producing fancy effects, and as means for constituting and unfolding a phenomenological field. The identification and preparation of epistemic things and the introduction of technological objects initiated a progressive deconstruction and a gradual transformation of the discharge tube into a scientific instrument that became the central part of a sophisticated laboratory environment.[24]

While the technical and experimental achievements cannot be overestimated, the most important outcome at the conceptual level of Plücker and Hittorf's cooperation was the determination that one sort of gas could display up to three spectra that did not have a single line in common.[25] That one substance can show distinct spectra was a new and puzzling phenomenon and did not accord with the chemists' search for one-spectrum-per-substance correlations, which were so important for the application of spectroscopy in chemical practice. Plücker and Hittorf interpreted these variations as clues to allotropic modifications of gas molecules. These modifications, they imagined, were caused by the heating effect of the conduction process, which, according to Joule's law, would be proportional to the product of voltage, current and time. With the help of this concept, light, electricity, heat and material change were correlated within a single experimental system.[26] At this point their method of using discharge tubes to produce spectra proved its superiority to flame analysis since the variability of the electric current supplied an easy

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[24] For further information see Falk Müller, *Gasentladungsforschung im 19. Jahrhundert*, Berlin 2004, chapter 2.

[25] Julius Plücker and Johann Wilhelm Hittorf, "On the Spectra of Ignited Gases and Vapours, with Especial Regard to the Different Spectra of the Same Elementary Gaseous Substance," *Philosophical Transactions of the Royal Society* 155 (1865), pp. 1-29.

[26] This misconception was resolved in the mid 1870s when experiments by other researchers showed that temperatures inside the positive light and close to the anode were not exceeding 100° C.

way to widely change the temperature and appearance of the gas. The electric current could be seen as a powerful tool not only for enforcing chemical or physical changes but also for doing so in a controlled and calculable manner.

Early in his investigation, in an attempt to increase the brightness of the electric light, Hittorf noticed a peculiar electric resistance at the cathode that likely had the same origin as the negative light. Although he only returned to studying this effect in 1867, two years after Plücker had left the cooperation to complete his geometrical studies, the year 1862 can be seen as the starting point of a research project that absorbed Hittorf for more than 20 years. Since most of his colleagues believed the negative light to be a secondary effect of processes taking place close to the anode and in the positive light he remained alone in his investigation of the negative light for a long time. This long-term research project focused on the extension, re-figuration and purification of the rich but overly inconsistent experimental space he inherited from Geissler and Plücker. His overall strategy can be structured into three phases.

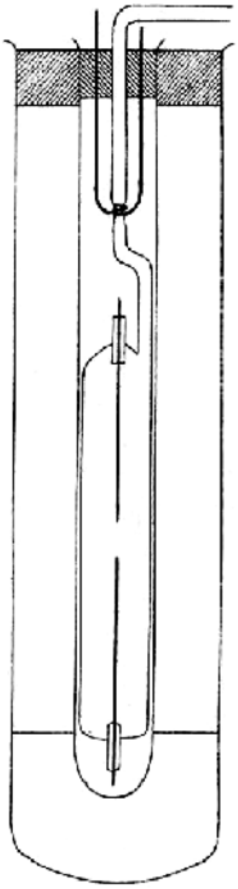

Fig. 8: Hittorf's apparatus for the preparation of high vacua. The inner tube was kept in boiling sulphur for several hours while the evacuation continued. At some point a galvanic current would heat a platinum wire looped around the tube's neck and seal the tube.

First a playful phase in which he discovered and described several new effects. This was not a mere tinkering but an attempt to grasp and measure the dimensions of the phenomenological field by exploring the structures and limits of the experimental space, for instance by varying certain parameters to extremes. In the mid-1860s he managed, for example, to construct and evacuate a tube that would not conduct or discharge the voltage of the strongest Ruhmkorff coil in Paris. These tubes were seen as a proof that a vacuum would not conduct electricity. They were produced and sold by Geissler as "Hittorf's resistance tubes" and advertised as containing a "perfect vacuum" (fig. 8).

In a second "explorative" phase, Hittorf tried to set up what I would like to call a "micro-laboratory." In this phase he constructed various instruments that were used for qualitative experiments and relative measurements.[27] These experiments can be seen as procedures to prepare and optimise the conditions for the third phase, which was reserved for exact and absolute measurements.

While the first phase resembled what had been done before and in most cases extended and clarified what Plücker and Geissler had already found, the second phase was connected with certain preconceptions and theoretical assumptions. Most important was Hittorf's belief that (under circumstances that had to be explored) gases would conduct electricity in a way comparable to that of metals and electrolytic solutions. Therefore the conduction process had to be treated as an electro-dynamic phenomenon. This was not a widely shared assumption since most other researchers would not accept that the discharge was continuous or they did not perceive gas as being essential to the discharge process; some even thought gases to be ideal isolators. Hittorf's discharge tubes became part of a rich network of theories and practices, which had developed out of the exploration of electric circuits and which could possibly be adapted to the investigation of gas discharge phenomena. There were, however, obvious differences between the electrical properties of gases and those of solids and fluids. Most notably, a very high voltage surge was needed to induce discharge. The current itself seemed to generate and define the conditions for its existence. Only if these conditions could be understood would Hittorf be able to control and investigate changes in the spectrum, in heat production or in the variation of electric resistance.

> However, the biggest nuisance is that at temperatures at which gases conduct [electricity] every known matter stops behaving like an isolator. Hence in gaseous media we are not able to confine the electric current to a restricted, geometrical path. Therefore we will possibly never reach the precision we easily accomplish in measuring the electric properties of metallic bodies.[28]

Hittorf had already learned to control the vacuum in his spectroscopic studies. The provision of an appropriate source of electricity was a similarly complicated task. For his purpose he needed a

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[27] Most of these experiments were published in Hittorf's most renowned paper on gas discharge phenomena: Johann Wilhelm Hittorf, "Ueber die Elektricitätsleitung der Gase," *Poggendorfs Annalen der Physik und Chemie* 136 (1869), pp. 1-31 and 197-234. In later articles he did not add many new observations but reported on iterations and the performance of similar experiments under continually enhanced conditions.

[28] Ibid., p. 224.

continuous source, not an intermittent one like the induction machine, which unfortunately only supplied a pulsed voltage. The only comparable continuous source of electricity was a high-voltage battery – a very expensive and time-consuming piece of nineteenth-century "big science." It took Hittorf several years to achieve a final number of 2,400 elements in a Bunsen battery and a voltage of almost 5,000 volts in 1883. In the meantime, before he was able to use the battery, he worked with the pulsed current. This, he remarked, should suffice to do qualitative experiments before he could start precision measurements (fig. 9).

To investigate the conductive properties of the tubes in connection with changes in the luminous effects, Hittorf used shunt circuits in which pairs of tubes with only minor aberrations were compared (fig.10). Other tubes were constructed to observe the behaviour of the positive and negative light at various pressures (fig. 11).

In all of these experiments, the luminous effects served as indicators that could be connected to changes of the electrical properties and which showed an experienced observer the slightest variations in the conditions. The knowledge of the sensitive correlation of the shape of the tubes, the electrodes and the electric properties was important for understanding the conditions that supported or prohibited the discharge process. The main outcome of these experiments was a sound understanding of the interdependencies of instrument design and electrical properties. On the technical side they lead to a considerable reduction in resistance at the cathode, which had been a major obstacle in maintaining continuous conduction.

Fig. 9: Picture of the remains of Hittorf's battery in the Deutsches Museum (Photo by Bernhard Taufertshöfer).

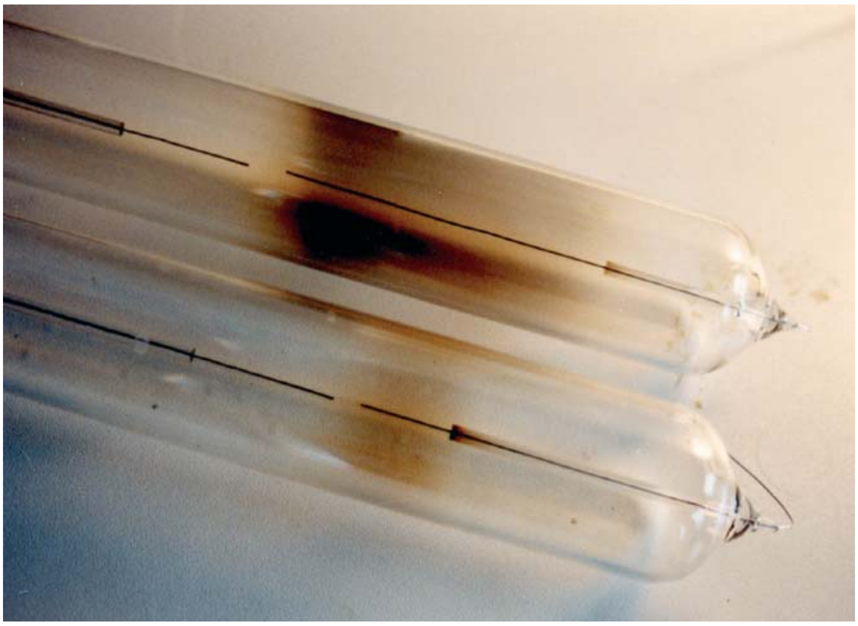

Fig. 10: Reconstruction of a pair of tubes with cathodes made of platinum wire of different lengths (photo by the author).

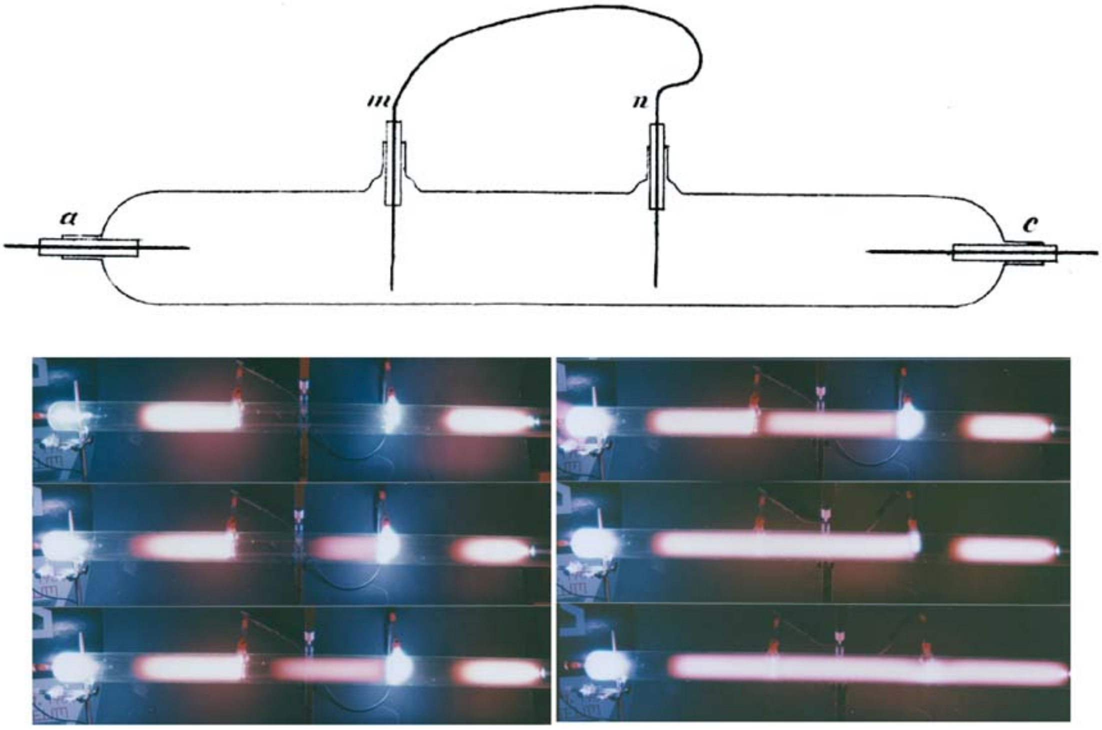

Fig. 11: Replication of a series of experiments to measure the electric resistance of the negative and positive light at varying pressures (photos by the author).

By the end of the 1870s Hittorf was able to vary vacuum, electric current, voltage and some elements of tube design almost continuously and over a wide range. Hittorf's motto was *natura non facit saltum* – "Nature does not jump." He disliked any singularities. The design of his experiments aimed at a morphological arrangement in a Goethean style that would link various states, manifestations or symptoms into a chain or a single phenomenological field.[29] In the case of gas discharge phenomena he was, however, dealing not with a single but many mutable strands. His conviction was that the phenomenon he was looking for would only emerge and could only be explored if all these strands were under control and interwoven into a single texture.

This desired status of the experimental system coincides with what I would like to call a "micro-laboratory." The micro-laboratory is a specific preparation of the experimental space and the phenomenological field of gas discharge by technical means. Comparable to Rheinberger's experimental system, it is an arbitrary, but (at the local scale) meaningful, formation that helps, for example, to define what belongs to the experimental apparatus, what constitutes the phenomenon, what is a primary and what is a secondary effect – it separates what is meaningful from what is merely possible. It can be seen as a laboratory inside the laboratory, which would mediate between various levels and sources of experience and which was established to control the flow of matter, energy, information and meaning between the microscopic and the macroscopic world. While single aspects of the underlying phenomena could be illustrated in demonstration devices or in formulas describing correlations and regularities (the latter would have been the purpose and a possible result of exact measurements), the phenomena themselves could only be conceived in the experimenter's imagination and perceived as a product of long-term experience. A micro-laboratory serves as an interface which not only entangles practical requirements, subjective experiences and imagination but also implements a specific economy and hierarchy into an array of epistemic practices.

Hittorf entered the third phase only in the early 1880s and never completed his research project because shortly thereafter he had to retire due to psychological problems. One of the few examples from the third phase is an instrument with which he started to map the variation of potential inside the tube.

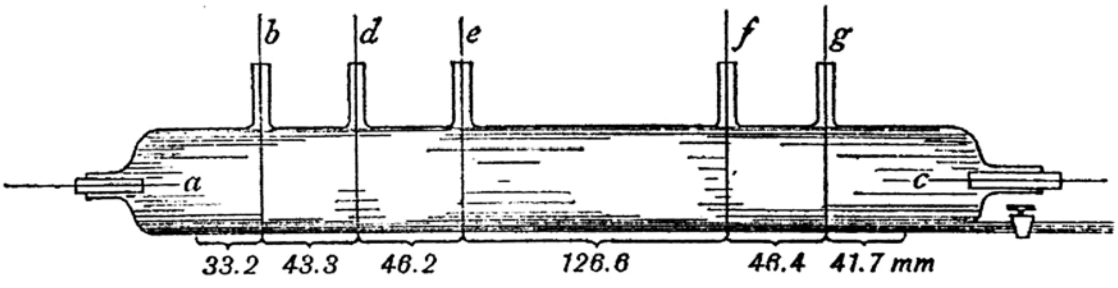

Fig. 12: Instrument for quantitative analysis of variation of potential.

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[29] For a discussion of the development of the "Seminar für die Gesammten Naturwissenschaften" at Bonn University where Hittorf was educated see Gert Schubring, "The Rise and Decline of the Bonn Natural Science Seminar," *Osiris* 5 (1989), pp. 57-93. The seminar was founded in 1824 by Christian Gottfried Nees von Esenbeck, a student of Schelling and friend of Goethe. It placed a special emphasis on *Naturphilosophie* as essential part of the curriculum. Traces of this education can still be found in Hittorf's style of research.

Hittorf's last publication on gas discharge issued in 1884 anticipated a final conclusion which he never presented. In my interpretation, this was because Hittorf suffered from a severe problem: The preparation of the experimental space was almost completed and he needed conceptual support. In his research he followed and tried to extend Faraday's electro-dynamical theory – for a mathematical elaboration he had to wait until Maxwell published his *Treatise* in 1873. As far as I can tell he did not find what he was looking for since Maxwell gave no answers to the problematic relation between electricity and matter in electrolysis and gas discharge. How Hittorf's desperate search for answers (and his problems with Maxwell's mathematics) affected his general constitution can be seen from the report of a colleague's wife:

> More serious was a break down that happened in the early 1880s. It was accompanied by a strange apathy toward everything and everybody. For days he would not say a single word, focusing on one and the same train of thought connected to problems in mathematical physics that even overstrained Hittorf's sharp intellect. These problems occupied him all the time, and the book (Maxwell's) accompanied him almost day and night. Thus he lost appetite, sleep, and finally his well-balanced mood. Gloomy and silent he would sit in his study reading [...]. We tried to take him for a walk, arranged theatre visits and even managed to invite him and his sister to see a comedy – in most cases his restlessness drove him home ahead of schedule where he again was absorbed by the incomprehensible Maxwell. Hittorf's weight loss was terrifying at this time, and when my husband joined him for a journey through the Harz Mountains his condition would not improve. Secretly he had taken along the Maxwell.[30]

William Crookes (1832-1919)

In 1878/79, roughly ten years after Hittorf had published his first account, the London chemist and businessman William Crookes publicly demonstrated the results of his own research on gas discharge phenomena in a series of famous lectures at the Royal Society, the Royal Institution and other places. Crookes began, in the early 1870s, to explore curious attractive and repulsive effects in evacuated vessels that seemed to act between cold or hot bodies and the pans of precision scales and other forms of sensitive balances. In 1875 he introduced a new instrument into his studies, the still-popular radiometer,[31] which consisted of a small rotating fly inside an evacuated glass bulb. Attached to the fly were vanes that were blackened on one side. The vanes would rotate if a light or heat source approached.

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[30] Recollections of Adelheid Sturm; cited from A. Heydweiller, "Johann Wilhelm Hittorf," *Physikalische Zeitschrift* 9/10 (1915), p. 175.

[31] For the relation between Crookes' "radiometer" and the "light mill" that was produced and sold by Heinrich Geissler, see Günther Dörfel and Falk Müller, "Crookes' Radiometer und Geißlers Lichtmühle – Kooperation oder Konkurrenz?," *N.T.M. (International Journal of History and Ethics of Natural Sciences, Technology and Medicine)* 11 (2003), pp. 171-190.

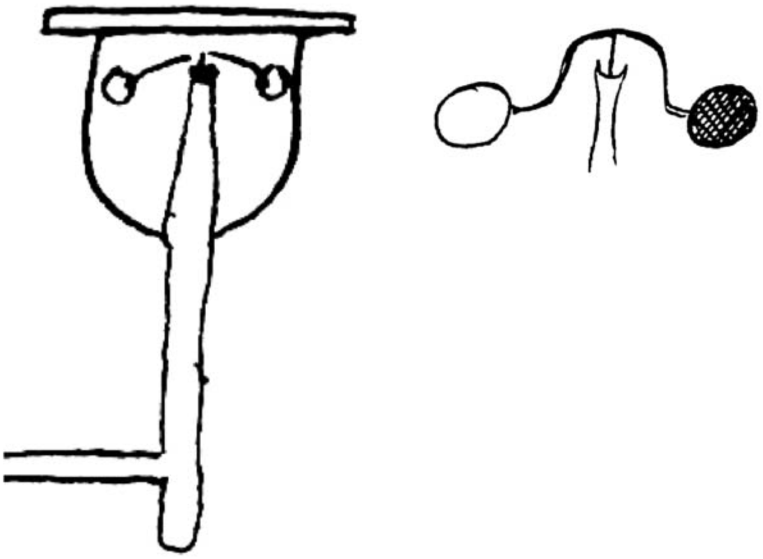

Fig. 13: Sketch of an early radiometer. Images taken from Crookes' Notebooks IV (p. 5, 2.7.1875), courtesy of the Trustees of the Science Museums, London.

After a number of interpretations had been suggested and tested, several physicists came to believe that an explanation for these effects could only be found in a research area just starting to emerge – the kinetic theory of gases. Crookes himself had good reason to oppose this interpretation: gas molecules could not play any role at all, since in his opinion, the mercury air pumps he used removed all gases from the glass bulb, thus generating a "perfect" vacuum. As illustrated in Hittorf's case, the practical problems with producing high vacua were barely recognized and hardly understood. It took some time and several suggestive experiments to persuade Crookes that his vacuum had to be improved and that traces of gas within his tubes were responsible for most of the observed effects.[32]

An important step on the way to a better understanding of the radiometer movement was to realize how complex the apparently so simple device actually was. In addition to the assumptions made about the important role played by the surfaces of the plates and gas molecules, the wall of the vessel was assumed to have a function as well. The Irish physicist George Johnstone Stoney summarized these new perspectives in two convincing imagery models.[33] The first account of his theory, published in March 1876, overstrained the imagination of most of his readers:

> Many persons have told me that they have found it difficult to understand the explanations that I offered in the March Number of the Philosophical Magazine.[34]

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[32] For further information see Falk Müller, *Gasentladungsforschung im 19. Jahrhundert*, Berlin 2004, chapter 5.

[33] George Johnstone Stoney, "Crookes's Radiometer," *The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science* 1 (1876), pp. 177-181 and "On Crookes's Radiometer, Part II," ibid., pp. 305-313.

[34] Ibid., p. 305.

In a second publication, a month later, he attempted to mediate between the abstract formalism he had used before and a common understanding. His aim was

> [...] to present a picture of the mechanism by which I conceive the pressure to be produced, in a form which will, I hope, be intelligible to persons who have not made a special study of the dynamical theory of gases.[35]

The "pictures" Stoney introduced turned out to be very influential and served as a basis for many contemporary researchers' and even for our modern popular understanding of these processes. The first one and most important was the image of an "active layer." Heated surfaces communicated an extra portion of "heat" in the form of increased kinetic energy to nearby molecules. "Crookes' layer" or "Crookes' force," as Stoney termed this excited layer of molecules, was not visible or experimentally detectable at atmospheric pressure. It only became relevant at pressures as low as those produced in Crookes' bulbs. At these pressures the mean free path of the molecules was comparable to the dimensions of the glass vessel. The two differently heated sides of the fly had to be seen as surrounded by layers of Crookes' force of different sizes, which would push aside other molecules approaching the surfaces. Normally, the resulting forces on both sides balanced each other; the balance, however, was disturbed as soon as one of the layers came into contact with the sides of the glass vessel where the molecules lost their energy. The resulting pressure difference was responsible for the movement of the fly.[36]

The second picture introduced by Stoney was a direct consequence of the first. Stoney perceived the radiometers as "little heat-engines"[37] where Crookes' pressure served as a medium for transferring energy. The fly was seen as the "heater of the little engine" where the molecules were energetically charged. The glass wall served as a "cooler" or condenser where they lost their energy.[38] Crookes and his assistant Charles Gimingham tried to implement Stoney's "pictures" in a variety of instruments. Furthermore the pictures served as suggestive models in Crookes' publications and explanations.

A new dimension was achieved when Crookes believed he could produce and "illuminate" "Crookes' layer" by electrical means. This "illumination" was the result of a slow convergence of radiometer and gas discharge research initiated or provoked by the idea that various forms of energy could be used to produce "Crookes' layer," and accompanied by the construction of several instruments in which these new applications could be introduced and compared. For this merger Gimingham constructed a tiny radiometer, called a "telltale," that was used to indicate and map these molecular layers and streams.

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[35] Ibid., pp. 305f.

[36] For further contemporary discussions on the theory of the radiometer and especially Maxwell's and Osborne Reynolds' contribution to an alternative understanding see S. G. Brush and C. W. F. Everitt, "Maxwell, Osborne Reynolds, and the Radiometer," *Historical Studies in the Physical Sciences* 1 (1969), pp. 105-125.

[37] Stoney, "Crookes's Radiometer," p. 180.

[38] Stoney, "On Crookes's Radiometer, Part II," p. 307.

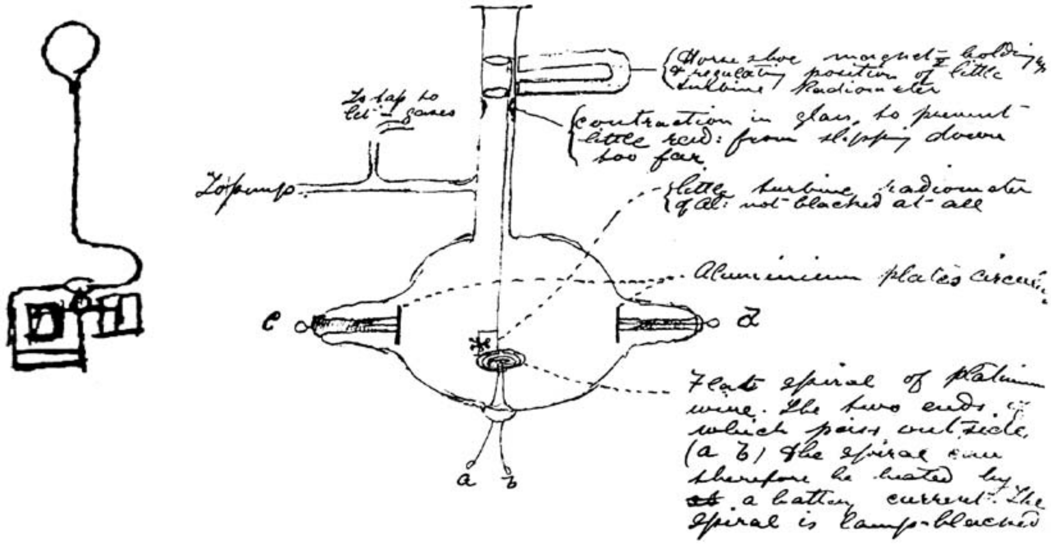

Fig. 14: A "telltale" and its application as a means to map and compare the "molecular streams" at various surfaces. Images taken from Crookes' Notebooks V (p. 104, 15.6.1878) and IV (p. 499, 2.10.1877), courtesy of the Trustees of the Science Museums, London.

For a final synthesis of gas discharge and radiometer phenomena Crookes and Gimingham started a systematic analysis of gas discharge phenomena in September 1878 with the construction of an electrical radiometer. This radiometer had metallic vanes and could be electrified by an external source of electricity.

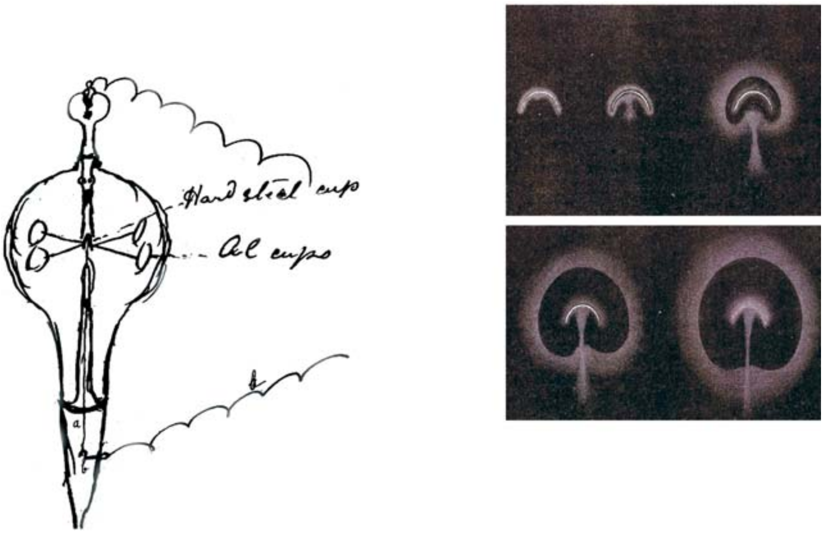

Fig. 15: "Electrical radiometer" and the "illuminated" extension of "Crookes' layer" at various pressures. Image taken from Crookes' Notebooks V (p. 159, 11.9.1878), courtesy of the Trustees of the Science Museums, London.

As soon as a potential was applied the fly started to turn on an irregular basis and in changing directions. Only when the luminous layer of the negative light reached the glass wall did the rotation stabilise and turn in the direction expected. With Gimingham's help William Crookes was able to test a wide range of instrumental variations. Although most instruments looked similar or alike, each embodied a specific epistemic configuration. Various forms of probes or indicators were introduced to visualise and map physical processes. Sometimes these devices were used to compare processes of seemingly different origin. In these cases single instruments or complex apparatus served as a means to fuse different fields of research, like gas discharge physics and molecular kinetics. In many ways the "electrical radiometer" can be seen as a "missing link" in Crookes' translation of radiometer and gas discharge effects.

This research was accompanied by a preparation of instruments and experiments for a series of lectures on the nature of cathode rays. In these lectures, brilliant experiments illustrated Crookes' hypothesis that cathode rays are made up of negatively charged particles that are expelled at a high speed at a right angle to the surface of the cathode. In Crookes' perception, molecules in such a condition constituted a new material state: the "fourth state of matter" or simply "radiant matter." At the end of the published version of a lecture given at the BAAS meeting in Sheffield 1879 he closed with a famous and often cited passage:

> In studying this fourth state of matter we seem at length to have within our grasp and obedient to our control the little invisible particles which with good warrant are supposed to constitute the physical basis of the universe. We have seen that in some of its properties radiant matter is as material as this table, whilst in other properties it almost assumes the character of radiant energy. We have actually touched the borderland where matter and force seem to merge into one another, the shadowy realm between Known and Unknown which for me has always had peculiar temptations. I venture to think that the greatest scientific problems of the future will find their solution in this Border Land, and even beyond; here, it seems to me, lie Ultimate Realities, subtle, far-reaching, wonderful.[39]

One of the tubes shown in the paper was supposed to demonstrate the theory of expansion of Crookes' layer in a very convincing way:

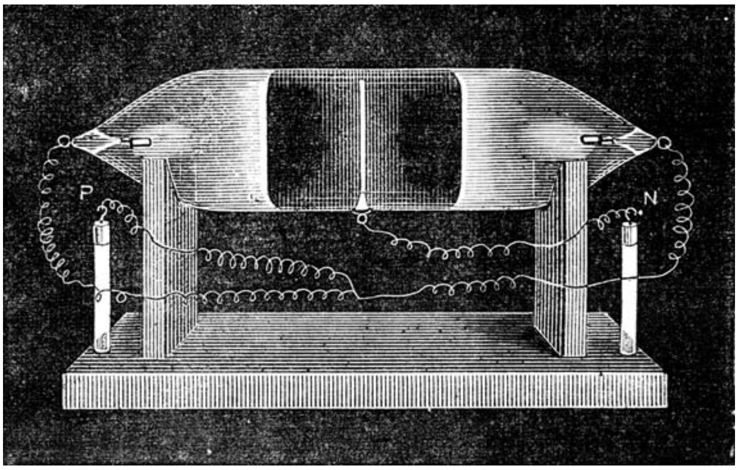

Fig. 16: Illustration from Crookes, "On Radiant Matter," p. 419.

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[39] William Crookes, "On Radiant Matter," *Nature* 20 (1879), 28.8.1879, pp. 419-423, and 4.9.1879, pp. 436-440: 439.

> I have long believed that a well-known appearance observed in vacuum tubes is closely related to the phenomena of the mean free path of the molecules. When the negative pole is examined while discharge from an induction coil is passing through an exhausted tube, a dark space is seen to surround it. This dark space is found to increase and diminish as the vacuum is varied, in the same way that the mean free path of the molecules lengthens and contracts.[40]

Crookes described what was occurring in the tube:

> As the one is perceived by the mind's eye to get greater, so the other is seen by the bodily eye to increase in size.[41]

This kind of suggestive correlation could be found in most tubes produced by Crookes and Gimingham, and in Crookes' understanding, the instruments enabled a mediated but clear perception and conception of the underlying phenomenon. The instruments shown in the lectures are only a few examples of a great variety of machines that were stored and archived in Crookes' laboratory, taken out for visitors, for public demonstrations or for further experiments.

Some of these instruments were soon copied, enhanced and sold by several instrument makers. The tubes exerted a strong influence on many scientists who took up this kind of research later on. The tubes were more than mere tools for manipulating phenomena; they served as epistemic tools for structuring perception. Crookes emphasized their importance when he wrote:

> In every step of this investigation, theory and observation have gone hand in hand, and at each point gained it has been my endeavour to permanently record such experimental proof in the convenient form of an instrument, so as to have it available for further examination.[42]

Discussion

> But even as we write our knowledge of the subject is extending, and we refrain from referring to more modern results; for historical sketching – a difficult task in any case – is unsafe in an open field like this, where some apparently insignificant fact may contain the germ of a great discovery.[43]

The early gas discharge research was dominated by attempts to produce and stabilize luminous effects inside tubes of various shapes and under varying circumstances. Comparable to old fashioned television sets, people were turning adjusting knobs in search for a program they liked or that made sense. Most researchers tried to capture something they could not grasp with an ever-growing and increasingly refined scaffold of instruments – some of these devices were used to further differentiate the experimental and phenomenological space, some were used to try to transform the object of their research into something more intelligible and manageable. The application of spectroscopic methods, for example, opened up a window into the world of

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[40] Ibid., p. 419.

[41] Ibid.

[42] William Crookes, "The Bakerian Lecture – On Repulsions Resulting from Radiation – Part V," *Transactions of the Royal Society* 169 (1879), pp. 243-318, quotation on p. 244.

[43] George Chrystal on "Electricity" in the ninth edition of the *Encyclopaedia Britannica*, discussing the progress of gas discharge research (Enc. Brit., 9. Edn. (1878), Vol. 8, p. 15).

microscopic processes. Spectroscopy seemed to point to a universal phenomenon and it marvellously reduced the chaotic behaviour of the luminous effects to characteristic and comprehensible changes of more or less simple and reproducible patterns of coloured lines. Used in such a way, the spectroscope could serve as a technological object and as a powerful tool to control the conditions and processes inside the tubes.

In many cases the application of new instruments or tools was accompanied by additional effects that further increased the field's complexity – especially when researchers demonstrated the flexibility of factors that formerly seemed solid and sufficiently understood. Especially in Hittorf's early experiments, the handling of materials and instruments turned out to be inexpedient and their perception fluid and blurred. While vacuum appeared as an unproblematic concept to most researchers, Hittorf's and later Crookes and Gimingham's experiments showed that the available experimental technology was fare from producing a "perfect" vacuum.[44] On the one hand, the concept of "vacuum" itself turned out to be ambiguous. On the other, subsequent investigations fostered the search for new concepts and practices and therefore helped to bridge the gap between an idealized concept of "empty space" and the apparent manifold of visual effects in gas discharge tubes. The new awareness of the crucial role played by residual gases in these processes and the introduction of models and formalisms taken from the kinetic theory of gases can be seen as one of the most important results of this development.

Those researchers that were not repelled by the field's complexity enjoyed participating in a rich material culture that soon gave birth to some path-breaking discoveries such as X-rays or J.J. Thomson's corpuscles – a material culture that would not have achieved such a diverse and sophisticated state at the end of the 19th century if the phenomena had not attracted so many researchers of various professions, forced them to cooperate and kept them in the field for a long time; Crookes played here an important role since he supplied strong patterns for the crystallization of a collective imagination.

Even after the introduction of the concept of the electron enormously enhanced the understanding of what the luminous effects of the tubes were about, a tension, an absence of clarity remained, something that again and again eluded the scientific grasp. We find here a good example of what François Jacob and Hans-Jörg Rheinberger have called an experimental machinery for "making the future:" gas discharge research arises as a field in which epistemic things repeatedly turn into technological objects and again decompose into a variety of new epistemic things and so on – or, as in Crookes' words: it took a long time until researchers left the "border land."

In 1930 Carl Ramsauer and Ernst Brüche, both distinguished physicists and leading scientists at the renowned research institute of the German electro-technical company AEG, arranged an 80th birthday celebration for Eugen Goldstein, the inventor of the term "cathode rays," whom they saw as a distinguished predecessor of their own professional focus: electron physics and electron

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[44] Geissler and Hittorf's innovations and improvements exerted a strong influence on the further development of vacuum technology. Even Crookes purchased instruments from Geissler's workshop. A comparison with the performance of his own instruments showed the poor quality of the highest vacuum he could attain in his laboratory. Until the late 1870s Geissler's apparatus served as a standard for Crookes and Gimingham's efforts to improve their vacuum; see Müller, *Gasentladungsforschung*, chapter 5, for further information.

optics. They were surprised by what they found when they visited Goldstein's former domain at Berlin's Institute of Physics and the observatory at Berlin-Babelsberg. In the attic of one building and the basement of the other they came across hundreds of discharge tubes "spilled into wooden boxes like potatoes" as they recalled or "piled up in a corner like coal."[45] In 1918 alone, Goldstein reported, he ordered 700 tubes. Some of these tubes were rescued and their cathodes were pinned down like species of bugs on a showcase that can still be seen in the Deutsches Museum in Munich.

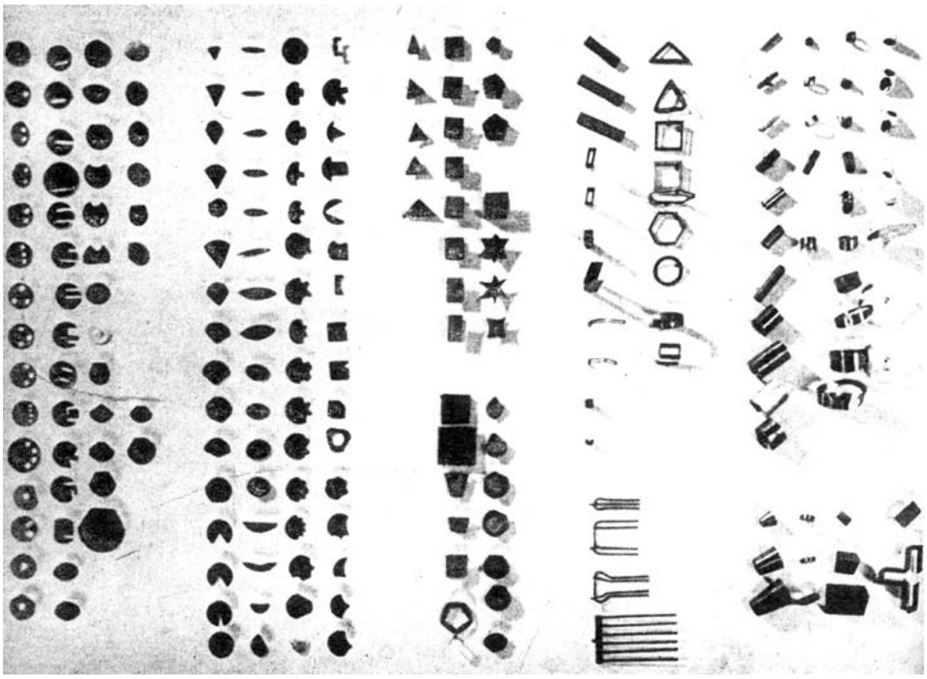

Fig. 17: Image of cathodes and anodes taken from Goldstein's tubes.

Ernst Brüche introduced a controversial but somehow apt metaphor when he adapted Hittorf's, Goldstein's and Crookes' work to his own field of study. In a process of "breeding," as Brüche called it, he and his collaborators found a specific form of cathode rays in the late 1920s, the *Fadenstrahlen*: a self-stabilising, visible ray, highly sensitive to changes in the magnetic field; it could be used to visualize the behaviour of charged particles in magnetic and electric fields. This tool stood within a tradition begun by Plücker and Hittorf. The rays can be seen as an attempt to use a physical process to suggest empirical solutions to problems that were otherwise connected to elaborate mathematical calculations. These auxiliary constructions were used as mediators between mental and material processes, between image and logic. They can be seen as an epistemic device, such as that mentioned by John Herschel in his *Preliminary Discourse* in a discussion of Chladni's experiments on sound and vibration:

> In such cases the inductive and deductive methods of enquiry may be said to go hand in hand, the one verifying the conclusions deduced by the other; and the combination of experiment

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[45] Carl Ramsauer, "Eugen Goldstein, ein extremer Experimentator," *Physikalische Blätter* 10 (1954), pp. 543-548, quotation on p. 547.

> and theory, which may thus be brought to bear in such cases, forms an engine of discovery infinitely more powerful than either taken separately.[46]

Finding such an "engine" is one thing. To further develop it into a technique around which a new experimental practice may crystallize is another. Integrating and encapsulating the technique into a network of different experimental practices, conceptual developments, and other levels of meaningful actions is what makes the shape of experiment so complicated.

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[46] John F.W. Herschel, *A Preliminary Discourse on the Study of Natural Philosophy*, with a new foreword by Arthur Fine, Chicago 1987, p. 181.

Science and Craftsmanship
The Art of Experiment and Instrument Making

Sven Dierig

The 1825 painting by the Berlin architect Karl Friedrich Schinkel, *Blick in Griechenlands Blüte* (fig. 1, see appendix) shows a temple in the process of being built.[1] Looking down from an elevation onto a Mediterranean city landscape, the beholder of the picture could imagine himself in the midst of a construction site: with muscled men and stone masons at work, cable winches, an iron jack, a cog-driven hoisting apparatus, and artisans and builders beneath a shading tent. This scene is a stark contrast to the visual expectations linked to antiquity in the age of neoclassicism.[2] While standard depictions of Greece usually show the monuments either in their finished state or as ruins, Schinkel here painted ancient architecture in the process of becoming [*Bildung*]: the cooperative work of construction. Central here were the creative artist and the role of hand craftsmanship in man's production of the beautiful. "In all things, man should form himself [*sich bilden*] beautifully, so that every activity exuding from him will be thoroughly beautiful in both subject and execution:" this was Schinkel's famous artistic imperative. For him, every act was an artistic task.[3]

In the foreword to *Vorbilder für Fabrikanten und Handwerker*, a collection of models for commercial artisans that Schinkel edited together with the Prussian industrial reformer Christian Peter Wilhelm Beuth, this artistic idealism was applied to practical concerns and transferred to then current needs. The models shown in the 1837 book, drawings of lamps, vases, furniture and other everyday items, were intended to show craftsmen "how necessary and useful it is to give their products not only technical perfection, but also the greatest consummation of form. Only an execution that unites the two can bring the work of the craftsman closer to artwork, moulding it with the stamp of formation [*Bildung*] and giving it a more enduring value than the costliness of the material from which it was made."[4] The aesthetic linkage between *Bildung* and craftsmanship intended by Beuth and Schinkel also proved well suited for understanding the use of craftsmen's tools in the laboratories of natural scientists around 1840 as a form-giving artistic task. The experimentation of the Berlin physiologist Emil Du Bois-Reymond represents an example of this. His *Untersuchungen über thierische Elektricität* seems like an attempt to provide alongside *Vorbilder für Fabrikanten und Handwerker* a model for the craftsmanship of experimental scientists.[5] If one replaces in the passage quoted above "the work of the craftsman" with "the work of the experimenter," it would read like a Du Bois-Reymondian instruction for how to work in the laboratory. In the sense of Schinkel and Beuth's *Vorbilder*, the *Untersuchungen* are the result of an

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[1] This essay will appear in *Les Comptes rendus de l'Académie des sciences Paris. Series Biologies.*

[2] I follow Adolf Max Vogt, *Karl Friedrich Schinkel: Blick in Griechenlands Blüte: ein Hoffnungsbild für Spree-Athen,* Frankfurt/M.: Fischer Taschenbuch, 1985.

[3] Andreas Haus, *Karl Friedrich Schinkel als Künstler. Annäherung und Kommentar*, München: Deutscher Kunstverlag, 2001, p. 249.

[4] *Vorbilder für Fabrikanten und Handwerker. Auf Befehl des Ministers für Handel, Gewerbe und Bauwesen herausgegeben von der Königlich Technischen Deputation für Gewerbe*, 2d ed. Berlin: Königlicher Staatsdruck, 1863, p. V.

art of craftsmanship directed at the mutual interpenetration of technical perfection and consummation of form. Engaged together in creating beautiful forms, as in Schinkel's painting, hand craftsmanship and experimental science found their way to one another at both the workbench and the laboratory table.[6]

Growing and becoming

Like his generation as a whole, Du Bois-Reymond was fascinated by phenomena of growth and development, in both a direct biological sense and in terms of categories of individual *Bildung* or historical growth.[7] When it came to the development and formation of individuals, children and vegetation – the children in the garden – was a typical emblem of the age. In 1845, in the style of the romantic artist Philipp Otto Runge, Du Bois-Reymond drew a kindergarten of the natural sciences, using it to illustrate the membership card of the *Physikalische Gesellschaft zu Berlin* (fig. 2), of which he was one of the co-founders. The drawing showed an exotic-looking imaginary plant. Between its stems and leaves there are young boys cavorting around the various branches of the natural sciences.[8] The image suggests that the children using their research instruments help to encourage the growth of natural scientific knowledge. The individual development of the researcher actively engaged with laboratory instruments is a prerequisite for this historical process. He experiences "pleasure" because he sees how he is "progressing," Du Bois-Reymond wrote at the beginning of his experiments to his friend Eduard Hallman. "I am growing, we want to see where to."[9] In a public lecture ten years later, he used a child as a model to illustrate the development of an experimental scientist:

> Observe a child in the tender age of development as it begins to discover the external world with a fresh gaze and to place the causes of his sensations outside himself. He sits at a table: he has been given a spoon to play with. Accidentally, the spoon reaches the edge of the table and falls clamorously to the floor. His small face is transfigured as often as one repeatedly raises the spoon for the child, it repeats joyously the same attempt; but he still did not know that bodies are heavy, that an unsupported body rushes toward the earth, how should it? Only

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[5] Emil Du Bois-Reymond, *Untersuchungen über thierische Elektricität*, Berlin: Reimer, 1848-1884. For summaries of this work, see Christoph v. Camphausen, "Elektrophysiologische und physiologische Modellvorstellungen bei Emil Du Bois-Reymond," in: Gunter Mann (ed.), *Naturwissen und Erkenntnis im 19. Jahrhundert. Emil Du Bois-Reymond*, Hildesheim: Gerstenberg, 1981, pp. 79-104; Gabriel W. Finkelstein, *Emil Du Bois-Reymond: The Making of a Liberal German Scientist, 1818-1851*, Ph.D. diss. Princeton University, 1996; idem, "M. Du Bois-Reymond goes to Paris," *British Journal for the History of Science* 36/3 (2003), pp. 261-300.

[6] For an integrative history of experimental physical sciences, which encompasses the history of craftsmanship see Heinz Otto Sibum's programmatic case study on M. H. Jacobi, "Experimentalists in the Republic of Letters," *Science in Context* 16/1-2 (2003), pp. 89-120.

[7] On Du Bois-Reymond's enthusiasm for "Bildung" see Dietrich v. Engelhardt, "Der Begriff der Bildung und Kultur bei Du Bois-Reymond," in: Gunter Mann (ed.), *Naturwissen und Erkenntnis im 19. Jahrhundert. Emil Du Bois-Reymond*, Hildesheim: Gerstenberg, 1981, pp. 173-186; Finkelstein, *Emil Du Bois-Reymond: The Making of a Liberal German Scientist.*

[8] M. Norton Wise gives a detailed iconographic analysis of this "Tree of Knowledge" in Chapter 5: "What's in a Line?", in M. Norton Wise, *Bourgeois Berlin and Laboratory Science* (in preparation).

[9] Estelle Du Bois-Reymond (ed.), *Jugendbriefe von Emil Du Bois-Reymond an Eduard Hallmann*, Berlin: Reimer, 1918, p. 93; Finkelstein, *Emil Du Bois-Reymond: The Making of a Liberal German Scientist,* p. 204.

> experience, some of it painful, will in the course of time impress this truth upon him so effectively greatly that he will think it self-evident.[10]

The spoon in the nursery corresponded to Du Bois-Reymond's galvanometer on his laboratory table. This precision instrument consisted basically of two magnetic needles hung one over the other on a silk thread beneath a glass cylinder. The lower needle floated inside a copper wire spool, the above one, visible from outside, over a scale divided into degrees. On the copper wire electrical current directed the magnetic needles around the axis of the thread. The operation of the sensitive galvanometer, which was highly subject to disturbances, required a significant degree of hand-eye coordination. In other words: a successful experiment required the experimenter's having gone through a rigorous process of physical development. Self-perfection or completion in using the instrument was rule number one in Du Bois-Reymond's laboratory. In January 1848, he wrote to his friend Carl Ludwig:

> Very soon I was also able to translate the pain that the burning of a frog's foot caused to the animal into electromagnetic motion, and with unfailing practice and perfection of the experimental technique I don't see why it should not ultimately also be possible to translate the change in the so-vital current of the *opticus* of a pike into a magnetic equivalent.[11]

The experimenter practicing at the galvanometer is figured in the branches of the science plant drawn by Du Bois-Reymond. One of the boys is shown doing chin ups on a magnet. Du Bois-Reymond here depicted himself twice: as gymnast and as researcher. In so doing, the use of the galvanometer was linked to the physical experience in the gymnasium that Du Bois-Reymond regularly visited while working on the *Untersuchungen.*[12] The drawing of the boy exercising on the magnet suggested that gymnastic equipment and laboratory instruments had a similar kind of relationship to the body working on or with them. The practiced gymnast and the practiced experimenter, both were the result of a physical self-perfection. Just like a gymnast on the bars or horse, the experimenter formed himself by exercising and perfecting himself on the laboratory equipment. The appendix to the *Untersuchungen* contains an illustration of the experimenter formed by laboratory work. In this depiction, Du Bois-Reymond gave himself the appearance of an ancient art figure: an idealized image of a beautiful youth, the classical symbol for physical perfection, works at an experiment using a galvanometer. The beholder was thus to understand

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[10] Emil Du Bois-Reymond, "Über tierische Bewegung. Im Verein für wissenschaftliche Vorträge zu Berlin am 22. Februar 1851 gehaltene Rede," in: Estelle Du Bois-Reymond (ed.), *Reden von Emil Du Bois-Reymond*, Leipzig: Veit & Co., 1912, Vol. 2, p. 29.

[11] Estelle Du Bois-Reymond (ed.), *Zwei große Naturforscher des 19. Jahrhunderts. Ein Briefwechsel zwischen Emil Du Bois-Reymond und Karl Ludwig*, Leipzig: Barth, 1927, p. 5.

[12] Du Bois-Reymond as a bodily trained physiologist is well known in the history of gymnastics because of his committed promotion of parallel bars during the "Barrenstreit" in the 1860s. In his two writings *Über das Barrenturnen und die sogenannte rationelle Gymnastik* (Berlin: Reimer, 1862); *Hr. Rothstein und der Barren: Eine Entgegnung* (Berlin: Reimer, 1863) Du Bois-Reymond argued from the standpoint of his own years of practical experience. See e. g. Johann Buomann, *Der Barrenstreit und das Stützproblem* (Dissertation: Universität München, 1932). For Du Bois-Reymond as a "Turner," see also Timothy Lenoir, "Laboratories, Medicine and Public Life in Germany 1830-1839: Ideological Roots of the Institutional Revolution," in: A. Cunningham and P. Williams (eds.), *The Laboratory Revolution in Medicine*, Cambridge: Cambridge University Press, 1992, pp. 14-71; Finkelstein, *Emil Du Bois-Reymond: The Making of a Liberal German Scientist.*

that experimentation in the laboratory is a form-giving physical art. If the exercising experimenter is an artist on his own terms, the trained experimenter himself is also a work of art.[13] Art historians consider Schinkel's *Blick in Griechenlands Blüte* to be the programmatic image embodying the spirit of Prussian neoclassicism. Du Bois-Reymond's view of the laboratory is in turn emblematic for the link between classicism and the natural sciences typical for his generation. For Du Bois-Reymond, modern technology and antiquity were not opposed to one another. While Schinkel's muscled workers use an iron machine in building their temple, the young Greek in Du Bois-Reymond's laboratory experimented with the newest tools and instruments.

Craftsmanship

In Du Bois-Reymonds years as a student, there were around 30,000 Berlin residents who were craftsmen of some kind. Around 200 of these were professionally categorized as *mechanische Künstler,* highly qualified jacks-of-all-trades in mechanics and optics. But a view of the mechanical artist as the upright citizen with an apron working at the workbench and vice is too limiting, as is shown by the example of Carl Philipp Heinrich Pistor. Du Bois-Reymond commissioned Pistor's workshop in the winter of 1840 with producing his first research instrument, a microscope. In 1816, Pistor had been the first to build a functioning steam machine in Berlin, along with the technician Georg Christian Freund, and in the 1830s provided the technical equipment for the optical telegraph line between Berlin and Koblenz.[14] But Pistor did not limit himself to the city's technical circles. Not only did he host the author Ludwig Tieck, he was also a guest at the literary salon of the publisher Georg Andreas Reimer. Reimer was the publisher of literary romanticism in Berlin, and his program in the 1840s included the writings of E.T.A. Hoffman, Jean Paul, Novalis, Ludwig Tieck, and Grimms' fairy tales.[15] It was also *Reimer Verlag* that published Du Bois-Reymond's *Untersuchungen über thierische Elektricität.*

The study of the contribution of craftsmen to the experimental work of the researchers of the nineteenth century is an issue of comparatively recent interest in the historiography of science. In the literature on Du Bois-Reymond's *Untersuchungen*, the mechanical artists have only been given a marginal treatment. In this way, over and over again the image of an autonomous experimenter has been conjured up; an experimenter who, at his own whim, relying on his own ability and own intuitions, drove forward his scientific work. But the opposite was the case. Du Bois-Reymond's experimental work on *Untersuchungen* was a shared undertaking, the result of a collaboration between the art of experimentation and the art of mechanics.

Johann Georg Halske (fig. 3) was the most important mechanical artist involved in *Untersuchunge*n. Better known as the co-founder of telegraphy workshop *Siemens & Halske* (along with Werner Siemens) in 1847, Du Bois-Reymond got to know him at the beginning of his

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[13] This argument has been developed in more detail in Sven Dierig, "Die Kunst des Versuchens. Emil Du Bois-Reymonds' Untersuchungen über thierische Elektricität," in: H. Schmidgen, P. Geimer, S. Dierig, (eds.), *Kultur im Experiment*, Berlin: Kadmos 2004, pp. 123-146, 384-391.

[14] On Pistor and his workshop see Jörg Zaun, *Instrumente für die Wissenschaft. Innovationen in der Berliner Feinmechanik und Optik 1871-1914*, Berlin: Verlag für Wissenschafts- und Regionalgeschichte Engel, 2002.

[15] Doris Reimer, *Passion & Kalkül. Der Verleger Georg Andreas Reimer (1776-1842)*, Berlin: de Gruyter, 1999. Includes a 1843 publishing catalogue on CD-ROM.

experimental work when Halske worked as an apprentice in the workshop of the mechanic W. Hirschmann in 1841. Between his apprenticeship under Hirschmann and his later collaboration with Siemens, Halske established with F. A. Boetticher a workshop that was located in walking distance from Du Bois-Reymond's laboratory. In his introduction to the *Untersuchungen*, Du Bois-Reymond expressly emphasized this profitable relationship with Halske and Boetticher. Without the work of the two mechanics, *Untersuchungen* would have been "nearly impossible."[16] This was thus more than just a simple relationship between a customer and a manufacturer. Du Bois-Reyomond did not just turn to mechanical artists in order to quickly have an order made, and then again leaving the workshop, returning later to pick up the completed apparatus. He remained in the mechanic's workshop as his instrument was being made, watching and participating in the process. It was in Boetticher's and Halske's workshop, for example, that the galvanometer mentioned above was built. The mechanics took charge of making the instrument's mechanics, while Du Bois-Reymond himself took on the task of winding the silk around the copper thread spool. *Untersuchungen* was not just an experimenters report on new findings in the area of muscle and nerve physiology. At the same time, Du Bois Reyomond presented himself as a mechanical artist who was able to build scientific instruments and was familiar with all sorts of tricks. In 1847, Hermann Helmholtz reported on the manufacture of the galvanometer in Halske's workshop:

> Dr. Dubois was insufferable all day: he was namely working with a mechanic on an instrument that he had himself ordered, carrying out an extremely tedious task, that is, winding copper wire 10000 times around a small wooden frame, because he believed that he would do this with greater care and regularity than the mechanic. He had already winded the entire morning, and wanted to spend the whole next day at it as well. He was so fogged up from his work also in the evening that I could not inform him about what I wanted to speak to him.[17]

Draughtsmanship

While Beuth and Schinkel were collecting *Vorbilder für Fabrikanten und Handwerker*, Ludwig Tieck was completing the novella *Der junge Tischlermeister,* also published by *Reimer Verlag* in 1837. A tract against the beginning industrial age, in this novel, the protagonist, the carpenter Wilhelm Leonhard, is a craftsman, an independent autonomous figure who saw himself reflected in his own products, and was repelled by all imitation and the factory-like. Tieck's Wilhelm Leonhard had nothing in common with the models that Beuth and Schinkel had suggested to the craftsman and manufacturer. A master of craftsmanship had to be his own draughtsman. The "relation of art, but without wanting to be art," Wilhelm Leonhard says in the novel, drew him to craftsmanship: "I thus dedicated myself to drawing untiringly."[18] Tables, armchairs, and chairs emerged first as "shapes," as "things" that floated about in his "imagination" and were "turned back and forth," then to be drawn and finally built. From the idea to the drawing, and from the

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[16] Emil Du Bois-Reymond, *Untersuchungen über thierische Elektricität*, Vol. 1, p. LII.

[17] Richard L. Kremer (ed.), *Letters of Hermann von Helmholtz to his Wife, 1847-1859*, Stuttgart: Steiner, 1990, pp. 6-7.

[18] Ludwig Tieck, *Der junge Tischlermeister*, Frankfurt/M.: Ullstein, 1996, p. 57.

drawing to the final product: in the workshop of the mechanical artists, design and construction before building were at this time still the exception, and considered the latest innovation. Craftsmen working according to plan raised scientific instrument making to a new level. A report by the head of Berlin's observatory, Johann Franz Encke, on the precision mechanic Carl Otto Albrecht Martins describes this modern type of the constructing craftsman:

> From the very beginning, Herr Martins made it his approach to make a detailed drawing the foundation of his work, and thus made it possible to form a rational judgement by improving each individual part. As unimportant as it might seem, I do believe it vital to place a great stress on this point, for I learned to treasure in Herr Martins a thoughtful artist who does not just try to discover something by trial and error, but gives his experimentation a sure foundation by making it completely clear to himself what he intends, and thus anticipating the problems that might hinder his intention.[19]

Like Martins or the literary protagonist Wilhelm Leonhard, Halske was a mechanic who also used draughtsmanship in order to explore the mechanics and the operation of the apparatuses that Du Bois-Reymond used in his laboratory. Du Bois-Reymond later reported: "Halske was much more than just a talented worker." To a "rare degree," Halske possessed a "constructive talent" and a "sure intuition" for finding the "simplest and best way" to solve the task at hand:

> It was a great pleasure that I often enjoyed half the night long to watch him with a pencil in hand approaching step by step the complete perfection of an idea for an experimental set up or a device.[20]

The conceptual construction of the laboratory apparatuses with the pencil in hand, in order to anticipate how what was assembled in the workshop would later function in the laboratory, making clear how an instrument emerges and what is intended with a mechanical device or an experimental arrangement, step by step approach, drawing, tinkering, and assembling: Du Bois-Reymond's actual laboratory consisted of the triumvirate work bench, drawing table, and experimental table. Like the workers, builders, and artists at Schinkel's construction site, Du Bois-Reymond took part in a shared process of construction and growth at Halske's workshop. In the mechanical workshop, Du Bois-Reymond assisted in building and conceiving the instrument, got involved, and watched the process of planning, learning how technical things took on shape and form. Step by step over time the apparatuses he needed for the laboratory developed. The development of technical things might well have enthused Du Bois-Reymond just as much as nature's own processes of development. The report on Halske's art of draftmanship was thus almost identical sounding to a report on the drawing abilities of his former teacher and mentor Johannes Müller. As Du Bois-Reymond remembered, he was a "master of drawing at the chalkboard:" "It was a great pleasure to watch him gradually taking an animal form in the process of development through a series of intermediate steps to the final shape."[21]

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[19] Jörg Zaun, *Instrumente für die Wissenschaft*, p. 41.

[20] Emil Du Bois-Reymond, "Johann Georg Halske," *Verhandlungen der Physikalischen Gesellschaft zu Berlin* 9/7 (1890), pp. 39-44, p. 40.

Mechanical beauty

The craftsman should not be misguided "to compose himself," as Beuth and Schinkel warn in their *Vorbilder für Fabrikanten und Handwerker*. Instead, the craftsman should limit himself to internalizing the spirit and taste of historical models and imitating such models. Craftsmen should not seek to be artists. In his novel *Der junge Tischlermeister*, Tieck drew the same dividing line: It was a "relation to art, but without wanting to be art" that constituted the aesthetic autonomy of craftsmanship. In Tieck's text, Baron Friedrich Elsheim asks the carpenter the question of why he became a craftsman: "I have always been surprised, my friend, that you with your open mind and varied scope of knowledge, your pleasure on all developed things [*allem Gebildeten*], that you did not prefer to chose the status of the artist." Wilhelm Leonhardt answers,

> That I did not fit the role of a scholar was something I realized very early, because I was more interested by things than thoughts, words, or formulars. I lack the enthusiasm of the artist, that striving, winged spirit, that can neglect and forget everything, that is at home in strange worlds, but not in our own: in contrast, my own spirit is quite limited, a truly bourgeois; my drive to work, my need to be useful, my pleasure in fixed and practical things; all of this convinced me early on that I was destined to become a craftsman.[22]

Wilhelm Leonhard chose craftsmanship over the sciences and art. While scholar, artist, and craftsman are clearly distinguished in Tieck – the scholar is an intellectual, the artist is not of this world, and the craftsman creates useful things – in Halske's workshop, in contrast, craftsmanship, science, and art all mingled with one another. In Du Bois-Reymond's view, Halske's creations were far more than just useful things: "Halske's fundamental attitude and goal was to make every piece as consummate an artwork as possible, up to the very last screw."[23] In the same way, Beuth and Schinkel demanded this for the products of craftsmanship, Halske's instruments united both technical perfection and a consummation of form. Du Bois-Reymond coined his own expression to describe the beauty of Halske's artworks: the scientific instrument possesses a "mechanical beauty" that "pleases," since it "rests on the unconscious impression of absolute functionality with the greatest possible simplicity."[24]

The beauty of Halske's instruments was thus something quite different from the beauty of the use-objects in the *Vorbilder* or those imagined by Tieck. Beuth and Schinkel, just like Tieck, relied on the forms of the past, albeit in a different way. While Beuth and Schinkel related on the aesthetic models of antiquity, Tieck turned to the middle ages. Wilhelm Leonhard wants "to ornament hard straight lines and square corners with flowers and garlands or with light figures that border on the arabesque" – the superfluous and unreasonable is what gives a work of craftsmanship beauty.[25] Mechanical beauty as understood by Halske and Du Bois-Reymond, was

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[21] Emil Du Bois-Reymond, "Gedächtnisrede auf Johannes Müller. Gehalten in der Leibniz-Sitzung der Akademie der Wissenschaften am 8. Juli 1858," in: Estelle Du Bois-Reymond (ed.), *Reden von Emil Du Bois-Reymond*, Leipzig: Veit & Co., 1912, Vol. 1, pp. 135-317, p. 272.

[22] Ludwig Tieck, *Der junge Tischlermeister*, p. 53.

[23] Emil Du Bois-Reymond, "Johann Georg Halske," *Verhandlungen der Physikalischen Gesellschaft zu Berlin* 9(7), 1890, pp. 39-44, p. 43.

[24] Emil Du Bois-Reymond, "Über tierische Bewegung, " p. 32.

[25] Ludwig Tieck, *Der junge Tischlermeister*, p. 60.

exactly the opposite: the beautiful is only what looks rational: the impression of beauty arises not by decorating the useful with classical or medieval forms, but by intentionally avoiding any superfluous decoration. There were no instruments placed on classical columns or featuring romantic ornamentation in Halske's workshop and Du Bois-Reymond's laboratory. Schinkel, the painter of *Blick in Griechenlands Blüte* and architect of Berlin neoclassicism, had by now taken also another route. Berlin's *Bauakademie*, built in the 1830s, was no Greek temple. The new functional building was built to fulfil its purpose, a modern, factory-like red brick building with iron window frames.

Acknowledgments

The author wishes to thank Brian Currid for English translation.

Figures:

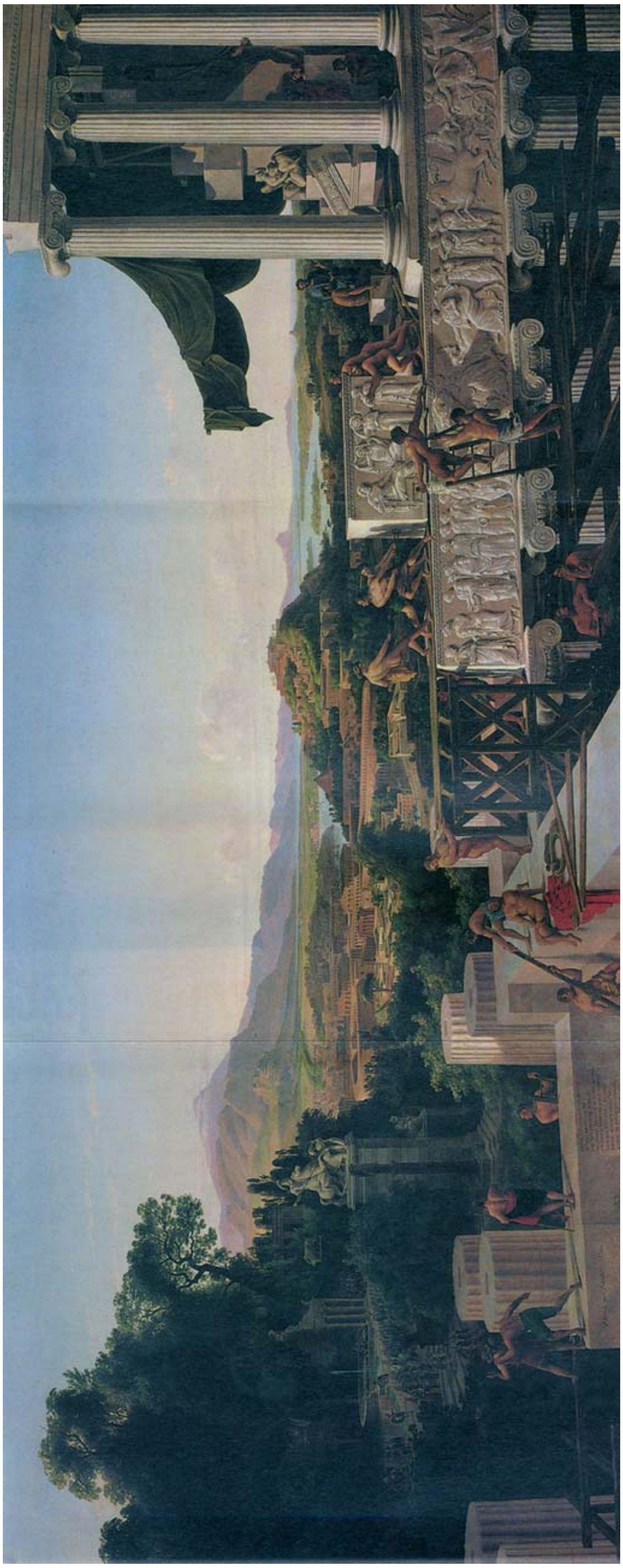

Fig. 1: Karl Friedrich Schinkel, Blick in Griechenlands Blüte.

Die phyſikaliſche Geſellſchaft
in Berlin

erwählte in ihrer Sitzung vom 14ten Januar

Herrn

Dr. Emil du Bois-Reymond

zu ihrem

ordentlichen Mitgliede.

Berlin, den 14ten Januar 1845

der zeitige Vorsitzende
G. Karsten

der zeitige Schriftführer
W. Beetz

Fig. 2: Membership card of the Physikalische Gesellschaft zu Berlin.

Fig. 3: Johann Georg Halske.

“Singing Flames”
On the Relation between Play and Experiment in the Nineteenth Century

Helmar Schramm

On 13 January 1875, Frederic Kastner’s newly invented instrument of physics and music, the pyrophone, was presented before the Royal Institution. As the climax to the presentation, in front of a gathering of physicists and music specialists, the hymn “*God Save the Queen*” was played in a timbre that had never before been heard.[1] The event was associated with quite an extraordinary resonance in the dual sense of the word. On the one hand this can be understood in an acoustic-musical sense concerning the range of these previously unheard tones. Their special feature consisted, however, not only in their relation to cosmic dimensions, the spherical and supernatural, but the secret, even shocking effect, arose from their bewildering closeness to the human voice. Both dimensions, the cosmic supernatural and the sensuousness of the human voice, seemed mysteriously blended together. Embedded in this very fusion is a deeper reference to the playful concepts of the *Gesamtkunstwerk* that had been discussed and tried out since about 1850.

On the other hand one can also speak of the extraordinary resonance of the first presentation of the pyrophone as an instrument of music and physics in terms of its enormous public effect. Numerous discussions, reports and rumours accumulated, spreading out around the new wonder like the ripples of a stone cast in water.

It must be admitted that as swiftly as the strange invention had become a spectacular event, so swiftly and thoroughly did it fall back into oblivion. From today’s perspective, it is notable that the word “pyrophone” is often missing in even quite reliable encyclopaedias and dictionaries. The same fate befell Frederic Kastner, who along with his definitive life work, has been almost totally forgotten. And his little booklet, in which he noted background, technical construction, and potential development possibilities for his invention, is also buried without a trace in the labyrinths of many libraries, or simply is not there anymore.

Fig. 1: W. Weissheimer playing the Pyrphone

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[1] Frédéric Kastner, *Le Pyrophone. Flammes chantantes*, quatrième édition, Paris: Dentu, 1875, p. 2.

Why then is the pyrophone, this strange instrument of music and physics, interesting and memorable in our context? In the following I want to clarify the way in which the pyrophone serves as an exemplary case to illuminate interferences between experiment and play (or games). Above all this concerns developments in experimental practice in the nineteenth century whose understanding requires reflecting on the development of new experimental arts since the seventeenth century. What is also of extreme interest in the short, furious flare of the effective history of the pyrophone is the absoluteness of its oblivion and its disappearance. I will come back to this briefly once more at the end of my commentary.

If one wants to conceive of the pyrophone as an instrument in the border region between physics and music, if one wants to understand its spectacular effect, it is important first of all to see it through the perspective of the history of art and the history of science, for the invention of the pyrophone is very tightly connected to specific aspects of a cultural history of experimentation. Let me sketch out a few pointers.

It is well known that around the year 1800 impressive changes were occurring in the relation between science and art. These changes can be succinctly seen in a fundamental transformation in the experimental attitude to fire. It is no coincidence that between 1750 and 1800 innumerable treatises on fire were produced. Worth mentioning here is Jean Paul Marat's 1782 publication *Physical Investigation on Fire* because in this the intensity with which ever new results from an experimental practise were pushed onto the market becomes clear. Typically enough, in the forward Marat expressly voiced his concern that the publication of his experimental results could be too late.

Why did so many experiments revolve around fire, what was so special and so new about these experiments? Coming into its own here, under a completely different rubric, was a grand tradition of experimental practise with fire in the context of the old chemistry – alchemy – which, in the seventeenth century, had formed an important, and equally ambiguous, background for the emergence of a new experimental culture. It is not a coincidence that John Peter Eberhard added to his 1750 publication, *Thoughts on Fire and Related Bodies of Light and Electrical Material*, an appendix on alchemical fire.

The endless work of alchemists, a practical form of philosophising with materials, on the "Great Work" was situated up until and into the seventeenth century, in a monumental theatrical collection, a work based on its own incomparable system of perception and recording, on traditionally inherited symbols, picture worlds, practises and instruments. Complicated allegorical picture sequences ran through the *Chemistry Works* of Nicolai Flamelli,[2] the *Quinta Essentia* of Leonhart Thurneisser[3] and the *Last Testament* of Basilius Valentinus.[4] The importance

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[2] Nicolai Flamelli, *Chymische Werke*, aus dem Franzoesischen in das Teutsche uebersetzt von J. L. M. C., Vienna: Kraus, 1751 [1399].

[3] Leonhart Thurneisser, *Quinta Essentia. Das ist die Hoechste Subtilitet / Krafft / und Wirkung / Beyder der Furtrefelichsten (und menschlichem geschlecht den nutzlischsten) Könsten der Medicina / und Alchemia, auch wie nahe dise beide / mit Sibschafft Verwandt. Und das eine On beystandt der andren kein nutz sei*, Munster: Ossenbruck, 1570.

[4] Basilius Valentinus, *Letztes Testament / Fr. Basilii Benedictiner Ordens. Darinnen die Geheime Buecher vom Grossen Stein der Uralten Weisen / und andern verborgenen Geheimnuessen der Natur. Auß dem Original, so zu Erfurt in dem hohen Altar / unter einem Marmor-steinen Taefflein gefunden / nachgeschrieben* [1626], Straßburg: Dolhopff, 1667.

of such picture elements was emphasised over and over again. The theatrical character of classical alchemy arose on the one hand from ritualised role play in the context of experimental procedures and demonstrations, and on the other hand, from the strategy of personification and dramatic formation of chemical processes. Fire was always of central importance in alchemical tracts and picture series, but its importance was especially true in the alchemists' practical work, which in large part concentrated solely on instrumentalised fire – the philosophers' oven (*athanor*).

Fig. 2: Tafel aus dem Mutus Liber. Nach: *Die Alchemie und ihr Stummes Buch* (Mutus Liber). Vollst. Wiedergabe der Orig.- Ausg. von La Rochelle 1677, Amsterdam: Weber, 1991.

Fire appears as the principle of movement par excellence, as a dynamic world principle creating connections between processes in the human body and in the cosmic world building. Fire is the core principle and object in revealing interrelatedness and in the distillation or transformation of materials and substances. Last but not least, in the bright darkness of its flickering ambivalence, fire is also a battery charged by the playful poetic-symbolic alchemical thinking that powerfully asserted itself especially in the fifteenth and sixteenth centuries and that was then carried through into the seventeenth century. As a result, immense processes of exchange took place with other realms of culture and religion in which fire has represented a kind of vault of symbolic strength since time immemorial.

Besides fire, it is music that so decisively determines the process, the life, the dynamics within the great alchemic works – music as world music and spherical harmony, music as rhythmic power.

Thus in the interplay of fire and music, the "Great Work" of alchemy becomes an original *Gesamtkunstwerk* in the truest sense of the word (providing us with extremely interesting bridges to the concept of *Gesamtkunstwerk* in the nineteenth century). Seen in this way, the treatises on fire between 1750 and 1800 seem to rest on a long, powerful tradition. Paradoxically, however, the very tendency that is associated with the downfall of alchemy from the seventeenth century on engenders consequences that provide completely new nourishment for the experimental interest in fire.

At the heart of the matter the following tendency can be observed: from the end of the seventeenth century, the practise of separation, purification and sublimation of substances, tried and tested over hundreds of years of tradition, was increasingly related to the alchemical written material itself. As can be seen reflected in the publication controversies, alchemy was caught up in a radical process of self-purification. In a tract in 1702, alchemical fire was even converted into the "Purgatorial Fire of the Art of Distillation,"[5] i.e. a fire meant to purge and purify the texts by eliminating the dark poetical surplus, the purpose-free, playful side of alchemy. However, along with the body of the texts, mother alchemy is systematically dissected, so that by 1777 Christian Wiegleb could observe that alchemy had finally fallen from its altar and lay there "to the mockery of all, with decapitated head and scattered limbs, and only the children and the common rabble toss about its mutilated torso back and forth."[6]

Ultimately this allegory, this process of self-purification, was nothing other than a symptom of the newly established and accepted practise of evidence production. Such practises, in diverse branches of knowledge that were increasingly systematically delineated from one another, were linked with very diverse concepts, methods and instruments. This was clearly exemplified by Leibniz when he compared the forms of truth production in the fields of theology, jurisprudence, historical writing, logic and natural sciences.[7] What should be taken from this, first of all, is the impression that striving for evidence is perhaps always connected with the deconstruction of holistic ideas (and thus indirectly provokes new inquiries). Considering this, as well as the very concrete localisation of experimental practise, it is also worthwhile keeping long-term historical processes in mind, thus allowing us to learn to what degree certain resolved questions turn out to be repressed problems that were really not put to rest, but that may break out again at different times under different guises.

If one wonders about the fate of the alchemical poetic surplus, it can be concluded that it was "accommodated" once more by romantics such as Schlegel, Novalis, Schelling and Ritter. Ritter wrote an essay on a history of chemical theories in which he doubts, with quite strong arguments, the matter-of-course use of sharp disciplinary borders.[8] In his essay *Physik als Kunst* (*Physics as Art)* he outlines a far-reaching counter model.[9]

Still, it should be emphasised once more that the new experimental interest in fire around the beginning of the nineteenth century can be explained principally as a product of the tendency associated with the decline in alchemy since the seventeenth century – the thorough rationalisation of chemical thinking and practise.

On this the following: in principle, it can be said of fire that on the basis of physical-chemical investigations on the properties of various gases, it became increasingly possible towards the end of the eighteenth century to systematically decompose the ambivalent totality of fire. We are dealing with a passion for dissection (and it would not be uninteresting to compare the separation of fire into its component parts with the practises of the *Theatrum anatomicum*).

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[5] Johann Anton Soeldner, *Keren Happuch, Posaunen Eliae des Kuenstlers / oder Teutsches Fegefeuer der Scheide-Kunst / Worinnen Nebst den Neu = gierigsten und groessten Geheimnuessen vor Augen gestellet Die wahren Besitzer der Kunst; wie auch die Ketzer, Betrieger / Pfuscher / Stuemper / und Herren Gern = Groesse*, Hamburg: Libernickel, 1702.

[6] Johann Christian Wiegleb, *Historisch-kritische Untersuchung der Alchemie oder der eingebildeten Goldmacherkunst; von ihrem Ursprunge sowohl als Fortgang, und was von ihr zu halten sey*, Weimar: Carl Ludolf Hoffmann, 1777, p. 379.

[7] Gottfried Wilhelm Leibniz, *Neue Abhandlungen über den menschlichen Verstand* (Nouveaux essais sur l'entendement humain), Frankfurt/Main: Suhrkamp, 1996 [1746], vol. 2., p. 491-539.

[8] Johann Wilhelm Ritter, "Versuch einer Geschichte der Schicksale der chemischen Theorien in den letzten Jahrhunderten," in: *Fragmente aus dem Nachlasse eines jungen Physikers. Ein Taschenbuch für Freunde der Natur*, edited by Birgit and Steffen Dietzsch, Leipzig: Kiepenhauer, 1984 [1800], p. 7.

[9] Ritter, "Physik als Kunst," p. 60.

Lavoisier, in particular, was responsible for the experimental shift away from the totality of fire and toward the construction and function of the flame and its constituent parts.

While the poetic-symbolic dimension of fire and especially its elementary relation to the whole, comprising nature and world, was demoted to the realm of aesthetics (there to be absorbed in such categories as light-dark, claire-obscure or the sublime), physical-chemical investigations concentrated not only on analysing flames in their different guises, but also in placing them in fully new contexts (such as electricity, lightning, and magnetism). On the basis of wave theory, it was possible to define completely new contexts and new realms that passed over the structures of human sense organs – their commonalities appeared calculable in a world unconcerned with the criteria of audibility and visibility.

Electricity, magnetism, light phenomena and acoustics entered into completely new relations with one another, and thus it is not surprising that the phenomena of the "singing flames" first discovered by Higgins in 1777[10] aroused intense interest and was the cause of the most diverse investigations of great scientists and experimental artists.

The list of names of those interested ranges from Scherer, De Luc and Chladni through Helmholtz, Sondhaus, and Schaffgotsch and on to Faraday and Tyndall.

Concerning the experiments with singing flames, I would now like to go into John Tyndall's lectures on sound, which were designed for maximum public effect. He had conceived them with close reference to Hermann von Helmholtz's *Theory of Sound Perception*, and in 1869, shortly before Kastner's invention of the pyrophone, he presented the culmination, as it were, of an almost century-old development. Incidentally, Helmholtz's view on the matter was that Tyndall with his "provident unification of a presentation, both clear and eloquent, with splendidly devised and convincing experiments" possessed "to an unusual degree the gift of making even the most difficult lesson in physics accessible to the educated public."

In my view what is conspicuous and important here is, first of all, a clearly recognisable influence of aesthetic concepts on experimental practise. Again and again aesthetic evaluations are made of experimental phenomena, forms are declared beautiful; Tyndall speaks of Chladni's famous acoustic "sand figures of extraordinary beauty" and thus sees musical instruments as instruments of physics, and physics instruments conversely as musical.

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[10] Frédéric Kastner, *Le Pyrophone. Flammes chantantes*, quatrième édition, Paris: Dentu, 1875, p. 23.

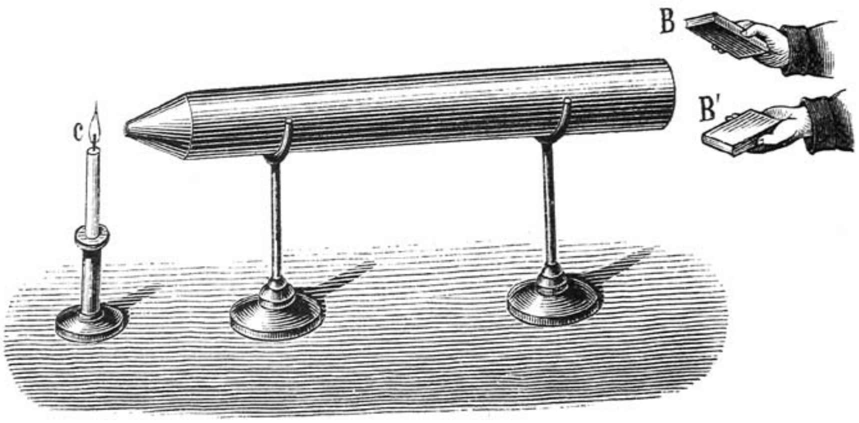

Fig. 3: Experiment on the propagation of sound waves.

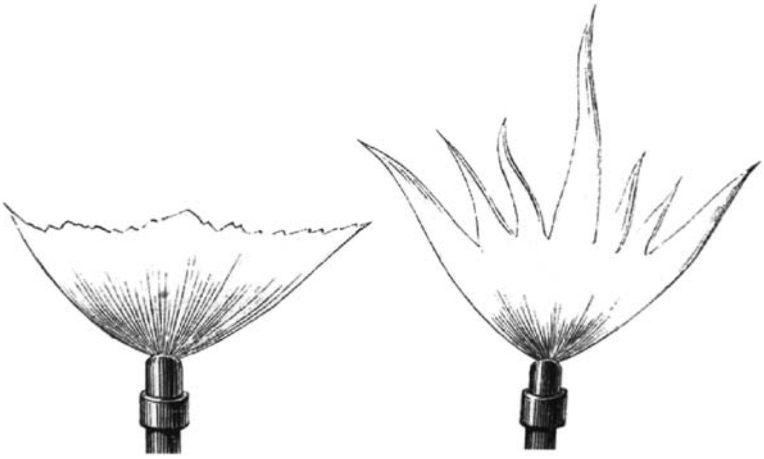

Fig. 4: Experiments on a bat's-wing burner.

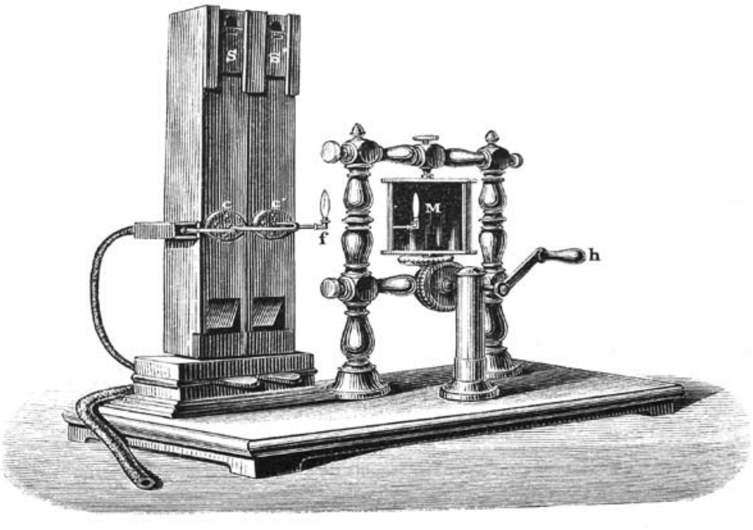

Fig. 5: Interference of waves from organ-pipes.

Fig. 6: Musical flames: rising and lowering the pitch of a flame.

One of the topics in his second lecture was the physical difference between noise and music. A musical tone is produced by regularly recurring oscillations, a noise by irregular ones. “If I shake this tool box, with its nails, bradawls, chisels, and files, you hear what we should call a noise. If I draw a violin bow across this tuning-fork, you hear what we should call music. The noise affects us as an irregular succession of shocks. We are conscious while listening to it of a jolting and jarring of the auditory nerve, while the musical sound flows smoothly and without asperity or irregularity.”[11] Noises “dash confusedly into the ear, and reproduce their own unpleasant confusion in our sensations. Music resembles poetry of smooth and perfect rhythm, noise resembles harsh and rumbling prose. But as the words of the prose might, by proper arrangement, be reduced to poetry, so also by rendering its elements periodic the uproar of the streets might be converted into the music of the orchestra.”[12] Tyndall also included observations typical for the time; the town was characterised as a great big noise instrument, the Paris stock exchange as an echoing tone instrument, the thunder of artillery in the battlefields as a sublime thunder concert.

Along with such aesthetic aspects of experimental observation and description, a reconnection of anthropological questions to an instrumental-technical basis attracts attention: the voice is presented as the greatest of instruments; interrelations between physiology and technology (communication technology) are brought into play. “The most perfect of reed instruments is the organ of voice” according to Tyndall.[13] And: “Were the apparatus of the voice that now addresses you examined, it would doubtless appear, either that the edges of the vocal chords are more or less serrated, or that they strike each other, or that they imperfectly close the slit during their vibration, the harshness of tone which you tolerate so patiently being thus accounted for.”[14] Tyndall not only cited the famous anatomist Johannes Müller who imitated the action of the vocal chords with rubber bands; he also referred in his demonstrations to the apparatuses of Kratzenstein and of Kempeln for imitating vocal tones, as indeed the re-enacting of such repertory experiments had great public appeal (and this especially due to their reference to the human voice). Tyndall described the effect of his performances when he says: “the word ‘mamma’ is heard as plainly as if it were uttered by an infant. For this pyramidal tube I now substitute a shorter, and with it make the same experiment. The ‘mamma’ now heard is exactly such as would be uttered by a child with a stopped nose.”[15] It was his opinion that, analogous to the elementary colours, mixtures of all possible timbres of all vowels were conceivable. After all, the tendency indicated that the voice would prove to be ultimately quite computable; it emerges through an apparatus; it is an instrument, perfectly imitable.

Experimentation moves here, so to speak, in the border zone between the playful creation and the disenchantment of a wonderful world. The playful-theatrical implications concern both the experimenter’s art of presentation as well as his relation with the public and the performance space. In this respect the experiments on the singing flame appear as the absolute highlight of a thoroughly stage-managed impact strategy.

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[11] John Tyndall, *Sound. A course of eight Lectures. Delivered at the Royal Institution of Great Britain*, London: Longmans, Green, and co., 1867, p. 49.

[12] Ibid., p. 50.

[13] Ibid., p. 195.

[14] Ibid., p. 196.

[15] Ibid., p. 198.

Having explained the physical basis of the flame-music to be heard in the appropriate pipe, Tyndall carried out various quite astonishing experiments; time and again expressions such as "extraordinary beauty" and "wonderful effects" were used. The singing flames became dancing flames through interferences with a siren employed for the purpose, and finally through the influence of the experimenter's own human voice. The performance proceeded to a real dialogue between the experimenter and the singing flames over a large distance (whereby a certain tone was essential). "By placing my finger for an instant on the end of the tube I stop the music; and now, standing as far from the flame as this room will allow me, I command the flame to sing. It obeys immediately. I turn my back towards it, and repeat the experiment..."[16] "The most marvellous flame hitherto discovered is now before you. It issues from the single orifice of a steatite burner, and reaches a height of 24 inches. The slightest tap on a distant anvil reduces its height to 7 inches. [...] The twitter of a distant sparrow shakes the flame down; the note of a cricket would do the same. From a distance of 30 yards I have chirruped to this flame, and caused it to fall and roar. I repeat a passage from Spenser [...] The flame picks out certain sounds from my utterance; it notices some by the slightest nod, to others it bows more distinctly, to some its obeisance is very profound, while to many sounds it turns an entirely deaf ear. [...] These figures are taken from photographs of the flame. In our experiments downstairs we have called this the 'vowel flame,' because the different vowel sounds affect it differently."[17]

The aspects outlined so far indicate that Tyndall's experiments were characterised by a very strongly developed consciousness of form, and I think for this very reason they could be of special interest when we are inquiring about "the shape of experiment" in the nineteenth century. How can the form of experiment best be conceived? I think it is especially appropriate here to pay particular attention to the relation of experiment and play /games.

Leibniz had astutely recognized the significance of this constellation. Thus it was not a coincidence that he suggested a clever mathematician might attempt to collect and catalogue all games in circulation, since in games the subtleties of human relations and the strength of the human spirit are revealed far more transparently than in any other field of human activity. What is remarkable in this is, first, the conviction that with incorruptible mathematical precision, the almost incomprehensible, elusive diversity of games may be grasped and systematised. Second, he was concerned with filtering out a beneficial usable element in order to then devote it to a better cause, namely the world of experiment and inventive art. Thus productivity should be transferred from the sphere in which it is especially strongly developed, through direct linkage with human desire, pleasure and satisfaction, to another which promises greater benefit. So certain game rules are beneficial and adoptable as models for increasing effectivity.

Games and experiment do in fact manifest many commonalities from questions of spatial arrangement (there is no game, no experiment without a frame) through dimensions of rule observation (there is no game, no experiment without rules) on to aesthetic aspects. Thus seen, it would undoubtedly be possible to conceive of a history of experimentation as a history of games or play.

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[16] Ibid., p. 228-29.

[17] Ibid., p. 240-41.

But what special consequences did this have concerning the development of experimental culture in the nineteenth century? Let us recall the pyrophone once more, that strange instrument of physics and music. It points beyond the blinkered perspectives of the time to meaningful interferences between science and art. And this is of crucial significance here. Experimental strategies and techniques have been developed and refined in both fields. Consequently a systematic examination of the experimental aesthetic around 1850 could make essential contributions to deciphering the "shape of (scientific) experiment." And the reverse is also the case. In the nineteenth century particularly, for many different reasons, such interrelations played a very important role, but were normally suppressed through excessively dominant cultural blinkers (or in Bachelard's words, epistemological obstacles). In the light of the constellation of experiment and games, it is possible, as I see it, to relativise such hermetic screening of experiments in art and science and to better understand the functioning of the said blinkers, who are related equally to anthropological, technical and political givens of perception, physical movement, language and thought. As a secondary effect they are inherent in the constitution of all cultures, and an intrinsic part of their nature is that they are extremely difficult to identify.

Undoubtedly there exist well-functioning cultural structures (such as science, art, politics and lifestyle) that cause downright deafness and blindness. This is one explanation for the reaction of advanced artists who develop a deep affinity for experimental strategies of destruction, interruption, dissonance and the conscious reversal of habitual perspectives. When, for Marcel Duchamp, the dust on an object becomes the truly interesting observational and breeding object, when Nam June Paik listens for the self-presence of the medium in broadband noise, when Joseph Beuys directs his energy to the secret of materials, a radical alienation of the observational situation is always involved.

Seen in this way, the avant-garde movement of the twentieth century can perhaps be described from today's perspective, with express reference to archaeological traces of knowledge, as an experimental set-up of grand scope directed at the radical questioning of culturally defining blinkers in art, politics, science and everyday life. It is exactly this conceptional core which remains open to development, pointing beyond all faded scandals, utopias and illusions to the urgency of a project still outstanding – the investigation of the ambivalent architecture of cultural borders with all their inherent dynamics. It is only within the framework of such an architecture of cultural borders that the shape of experiment can be understood and described.

A final word on the complete forgetting of the pyrophone: The pyrophone belongs to the large group of experiments that mark the striking *cul-de-sacs* of scientific and technical development. The pyrophones, based on the singing flames, were capable above all of imitating the human voice in a mysterious way and thus awakening spectacular public interest. In almost the same year, however, Edison brought his phonograph onto the market, which would not only make it possible to reproduce all imaginable voices, but also to record and reproduce all the sounds in the world. In this reproducibility of sounds, a new test set-up must be seen that would also fundamentally contribute to revising the relation between noise and music in the framework of avant-garde art experiments.

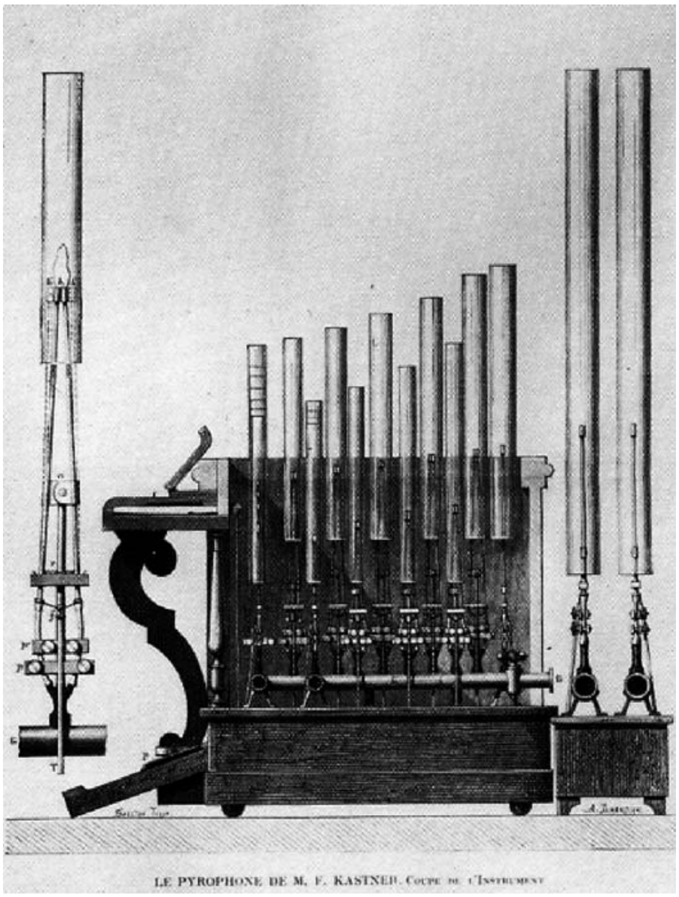
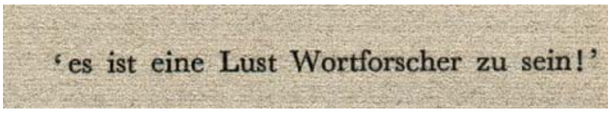


Fig. 7: Kastner's Pyrophone.

The POW Camp as Language Laboratory: Leo Spitzer's Epistolary Research

Andreas Hiepko



'es ist eine Lust Wortforscher zu sein!'

Before beginning with my illustrated narration of a memorable encounter with experiments in the annals of my science, I would like to thank Hans-Jörg Rheinberger and Henning Schmidgen for their invitation. It is a pleasure to speak, as a philologist, to an audience that is not only familiar with the emergence but also the disappearance of sciences.

It is common knowledge that philology is in a precarious situation. Not that we lack academics specializing in literature. Nor that people no longer speak of philology. Quite on the contrary, we are witnessing a strange renaissance of this notion in debates about the future of the humanities. Depending on which camp one adheres to, this is either an insult or holds promise. It seems that this debate repeats the polemic between Willamowitz-Möllendorff and Rhode on Nietzsche's *Birth of Tragedy* that led to the distinction between "Zukunfts-" and "Afterphilologie," between the philology of the future and pseudo-philology, with slightly scatological connotations that are not translatable into English. But there is an important difference. Up until this debate, philology believed to know what it is: a science of dead languages which seeks to correct and pass on the documents of these languages to following generations.

Today philology encompasses much more. First of all, philology deals with languages that are spoken by many people, i.e., languages that do not need to be passed on by philologists. They are themselves media of tradition.

Consequently in the contemporary debate those who want to return to philology often only wish to establish a conservative canon for their national history of literature. But this is nothing more than Lovejoy's "History of Ideas," which, according to Leo Spitzer, has nothing to do with the joy of being a "Wortforscher," a word researcher.

To illustrate this let me briefly recapitulate a book that was published two years ago under the promising title *The Powers of Philology*. In this context the philologist of Romance languages Hans Ulrich Gumbrecht seems to propagate a very strict definition of philology. According to him philology consists of five indispensable elements:

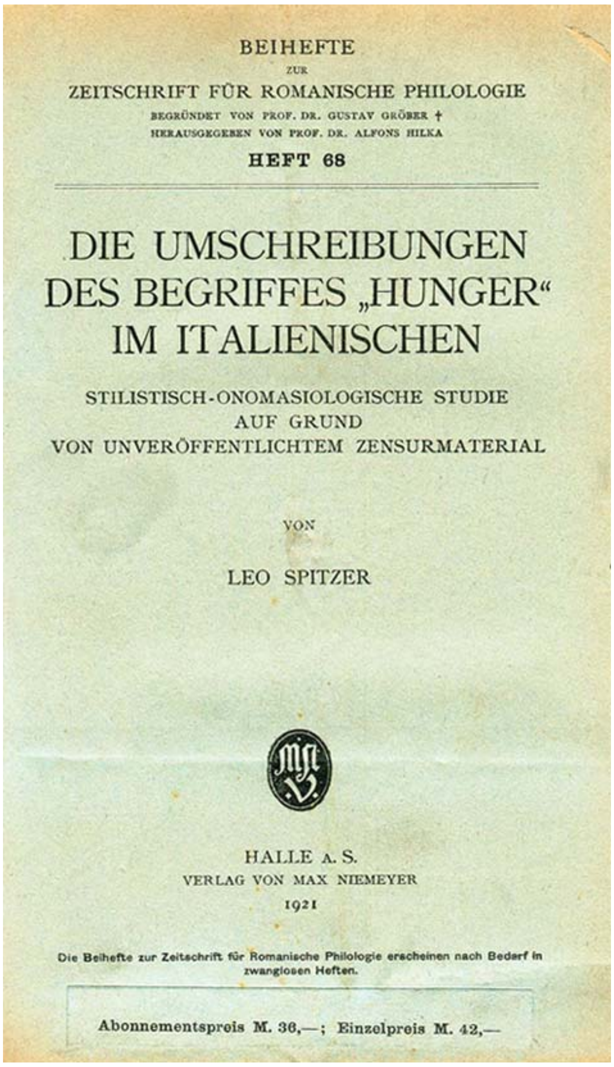
1. To identify fragments
2. To edit texts
3. To write historical [!] commentaries
4. To historicize things
5. To teach (complexity)

At first glance this appears quite concise. In the introductory chapter he even excludes Spitzer, whom I shall discuss as an exemplary philologist, from philology because he never edited a text.[1] On the other hand Gumbrecht adds to the first three duties, identifying, editing and commenting, which all would accept without contradiction, two further elements that are more controversial:

Of course, philologists must date texts from time to time, but do they have to historicize things? And what does teaching complexity mean? Isn't it rather a particularity that interests philologists?

Gumbrecht addresses the problem from the perspective of an historian of literature. That he favors the history of literature to the detriment of linguistics becomes evident when he speaks of the Spanish philologist Ramon Menendez Pidal, who in the 1890s prepared a monumental edition of the Castilian national epic *El cantar de Mio Cid*. Gumbrecht appreciates this only as a monumental work in the history of Spanish literature, or to be more precise, Castilian history of literature.[2] For Menendez Pidal I suppose this anonymous text served also as a document of the history of the Castilian language.

Thus I hope it is acceptable when I use Spitzer to exemplify the uneasy relationship between philology and experiment.



BEIHEFTE
ZUR
ZEITSCHRIFT FÜR ROMANISCHE PHILOLOGIE
BEGRÜNDET VON PROF. DR. GUSTAV GRÖBER †
HERAUSGEGEBEN VON PROF. DR. ALFONS HILKA
HEFT 68

DIE UMSCHREIBUNGEN
DES BEGRIFFES „HUNGER"
IM ITALIENISCHEN

STILISTISCH-ONOMASIOLOGISCHE STUDIE
AUF GRUND
VON UNVERÖFFENTLICHTEM ZENSURMATERIAL

VON

LEO SPITZER

HALLE A. S.
VERLAG VON MAX NIEMEYER
1921

Die Beihefte zur Zeitschrift für Romanische Philologie erscheinen nach Bedarf in zwanglosen Heften.

Abonnementspreis M. 36,—; Einzelpreis M. 42,—

Fig. 1: Spitzer, *Die Umschreibung des Begriffs 'Hunger' im Italienischen.*

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[1] Hans Ulrich Gumbrecht, *The Powers of Philology. Dynamics of Textual Scholarship*, Urbana, Chicago: University of Illinois Press, 2003, p. 2.

[2] Gumbrecht, *The Powers of Philology*, pp. 24-27.

In fact, I want to talk about the circumstances in which Spitzer elaborated one of his earlier monographs. In 1920 (!) he published his stilistic-onomasiological study on the basis of unpublished censorship material under the title "The Paraphrases of the Notion 'Hunger' in Italian" as a supplement of the *Zeitschrift für Romanische Philologie*.[3] This study opens with the following sentence: "The lack of the possibility of experiment in linguistics was regretted at all times. To state causal connection natural science can by systematic alteration of the conditions (that is by experiment) determine the results of this alteration, while language research always has to take notice of results and can only speculate as to the alterating factor." Admittedly, this is not a very complex definition of experiment. Nevertheless of interest here is the assertion that philology normally lacks the possibility of experiment, indeed, that philology cannot produce data but must register given ones. Probably no other field of research in linguistics is more concerned with this dilemma than etymology, and generally, historical linguistics, since they must deal with language change. In a case such as this we, like Spitzer, cannot accept a mechanical explanation that presupposes a set of phonetic laws that automates change like a program. Spitzer was always obsessed with the idea that language change is motivated; that something differing from language provokes this change.

Today it is common sense that etymology is, at least in the field of semantics, a pseudo-scientific method. Modern linguistics claims that the meaning of a word has nothing to do with its history, that its semantic cannot be derived diachronically, but is produced by a system of oppositions on a synchronic level. However, this shift from the history of language to synchronic structuralism, this de-historicizing of linguistics has caused a memorable effect on other historical disciplines. Literary history, for example, sees its task in reconstructing discourse formations, or self-contained epistemes in which literature functions as an indispensable element. This requires great efforts from both the historian of literature as well as the reader. But are these efforts, this detailed reconstruction of something that probably never existed, satisfying? It is the old problem of historicism and its narrative mode of "once upon a time." The more probable this narrative seems, the more useless it becomes to us.

As we can read in Gianfranco Contini's obituary for Leo Spitzer published in 1961 in *Paragone*, etymology has a totally different methodology, if this is a methodology at all:

> On the10th of February of last year Leo Spitzer wrote to me from Palo Alto, California, (where he was teaching at Stanford University) to congratulate me on a 'finding' that he was so kind as to call absolutely beautiful, concerning the etymology of *razza*, race.[4]

Contini was impressed that Spitzer, who claims in 1933 and 1948 that *razza* is a corruption of *Ratio*, congratulates the researcher, who by chance discovered a record that suggests another etymology that leads via *razzo* – *arrazzo*, 'to harass,' to the stud-farm and hence to the semantic

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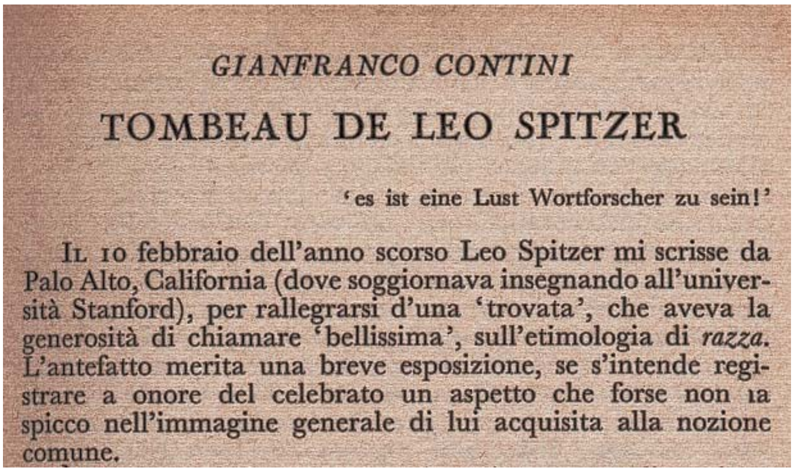
[3] Leo Spitzer, *Die Umschreibung des Begriffs 'Hunger' im Italienischen. Stilistisch-onomasiologische Studie auf Grund von unveröffentlichtem Zensurmaterial*, (Beihefte zur Zeitschrift für Romanische Philologie, Heft 68.), Halle: Max Niemeyer Verlag, 1920. One year later Spitzer published a second monograph on the basis of this censorship material: *Italienische Kriegsgefangenenbriefe. Materialien zu einer Charakteristik der volkstümlichen italienischen Korrespondenz*, Bonn: Verlag Peter Hanstein, 1921.

[4] Gianfranco Contini, "Tombeau de Leo Spitzer," *Paragone* 12 (1961), pp. 5-12, quotation on p. 5.

field of horse husbandry. What is interesting here is not Spitzer's generosity that Contini sought to illustrate with this anecdote but rather the description of the mechanisms that allow insights.



GIANFRANCO CONTINI

TOMBEAU DE LEO SPITZER

'es ist eine Lust Wortforscher zu sein!'

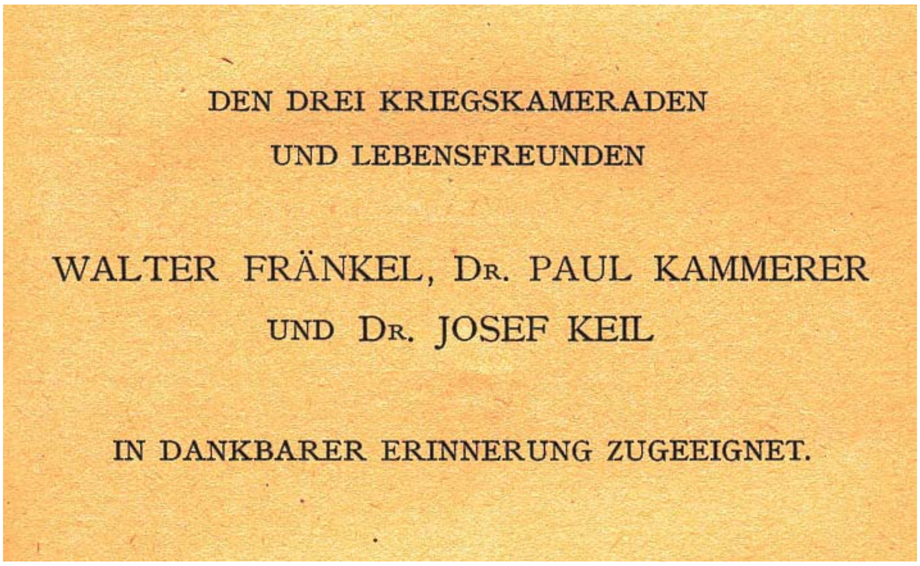
Il 10 febbraio dell'anno scorso Leo Spitzer mi scrisse da Palo Alto, California (dove soggiornava insegnando all'università Stanford), per rallegrarsi d'una 'trovata', che aveva la generosità di chiamare 'bellissima', sull'etimologia di *razza.* L'antefatto merita una breve esposizione, se s'intende registrare a onore del celebrato un aspetto che forse non ha spicco nell'immagine generale di lui acquisita alla nozione comune.

Fig. 2: Contini, "Tombeau de Leo Spitzer." The motto of Contini's obituary and my talk cites the last phrase of Leo Spitzer, *Romanische Stil- und Literaturstudien II,* Marburg: N. G. Elwert'sche Verlagsbuchhandlung, 1931, p. 285.

Whether insights are indeed gained is absolutely unpredictable; it is not the result of a systematical procedure. Results in etymology are lucky findings.

At the end of his life the philologist comes to terms with the fact that his science does not produce knowledge, but rather he, like his beloved words, must wander through a desert of texts until he occasionally stumbles over some revealing detail. He knows knowledge only as "objet trouvé."

But let us return to the 1920s when Spitzer still believed that there was a possibility for his science to become experimental. What had happened in order to raise such hopes?



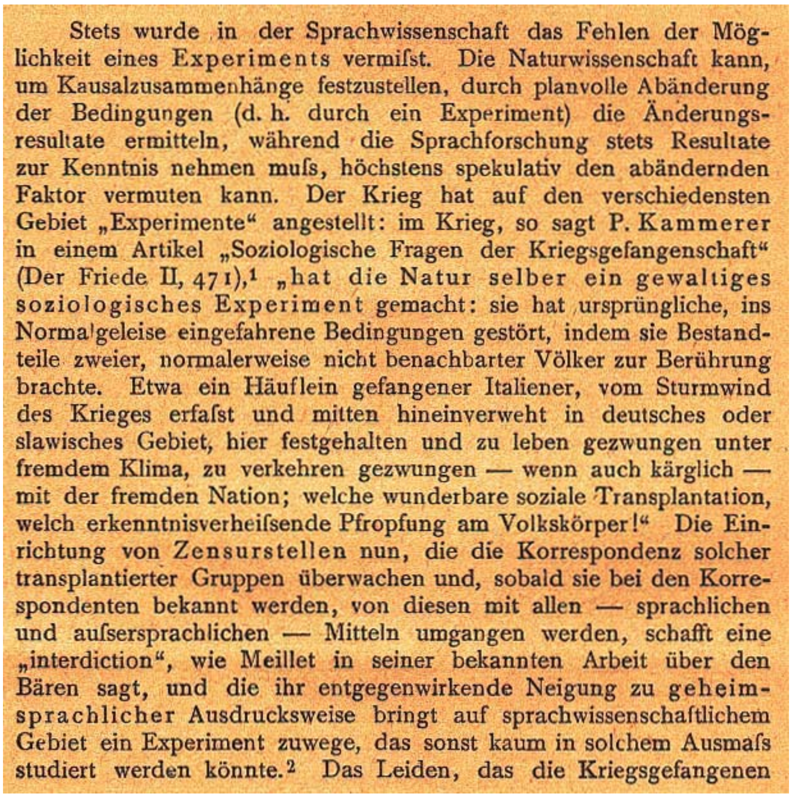
DEN DREI KRIEGSKAMERADEN
UND LEBENSFREUNDEN

WALTER FRÄNKEL, Dr. PAUL KAMMERER
UND Dr. JOSEF KEIL

IN DANKBARER ERINNERUNG ZUGEEIGNET.

Fig. 3: Spitzer, *Die Umschreibung des Begriffs 'Hunger' im Italienischen,* dedication.

A first hint to the answer of this question reveals a typical paratext which precedes Spitzer's study: a dedication to his wartime comrades and friends in (civilian) life. Besides Walter Fränkel, on whom I could not find any information, there is Dr. Josef Keil who later directed the excavations in Ephesus from 1926 to 1935, and Dr. Paul Kammerer, author of the famous book *The Law of Seriality* and a paradigmatic figure for faking scientific results. When an archeologist, a biologist, and a philologist work together and stay friends after completing their project, we may be tempted to imagine a successful interdisciplinary project. But let us examine what their work really entailed.



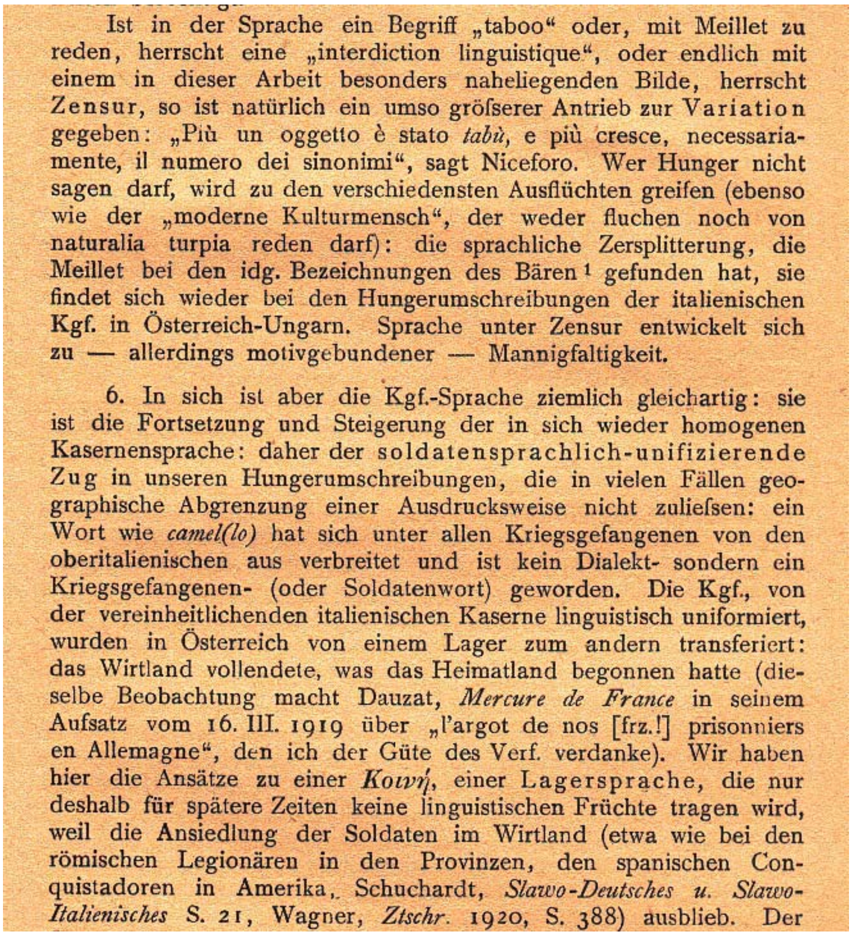
Stets wurde in der Sprachwissenschaft das Fehlen der Möglichkeit eines Experiments vermiſst. Die Naturwissenschaft kann, um Kausalzusammenhänge festzustellen, durch planvolle Abänderung der Bedingungen (d. h. durch ein Experiment) die Änderungsresultate ermitteln, während die Sprachforschung stets Resultate zur Kenntnis nehmen muſs, höchstens spekulativ den abändernden Faktor vermuten kann. Der Krieg hat auf den verschiedensten Gebiet „Experimente" angestellt: im Krieg, so sagt P. Kammerer in einem Artikel „Soziologische Fragen der Kriegsgefangenschaft" (Der Friede II, 471),[1] „hat die Natur selber ein gewaltiges soziologisches Experiment gemacht: sie hat ursprüngliche, ins Normalgeleise eingefahrene Bedingungen gestört, indem sie Bestandteile zweier, normalerweise nicht benachbarter Völker zur Berührung brachte. Etwa ein Häuflein gefangener Italiener, vom Sturmwind des Krieges erfaſst und mitten hineinverweht in deutsches oder slawisches Gebiet, hier festgehalten und zu leben gezwungen unter fremdem Klima, zu verkehren gezwungen — wenn auch kärglich — mit der fremden Nation; welche wunderbare soziale Transplantation, welch erkenntnisverheiſsende Pfropfung am Volkskörper!" Die Einrichtung von Zensurstellen nun, die die Korrespondenz solcher transplantierter Gruppen überwachen und, sobald sie bei den Korrespondenten bekannt werden, von diesen mit allen — sprachlichen und auſsersprachlichen — Mitteln umgangen werden, schafft eine „interdiction", wie Meillet in seiner bekannten Arbeit über den Bären sagt, und die ihr entgegenwirkende Neigung zu geheimsprachlicher Ausdrucksweise bringt auf sprachwissenschaftlichem Gebiet ein Experiment zuwege, das sonst kaum in solchem Ausmaſs studiert werden könnte.[2] Das Leiden, das die Kriegsgefangenen

Fig. 4: Spitzer, *Die Umschreibung des Begriffs 'Hunger' im Italienischen,* detail of p. 1.

Let me at least partially translate the first page of the introduction. I will begin with the passage cited above:

> The lack of the possibility of experiment in linguistics was regretted at all times. To state causal connection natural science can by systematic alteration of the conditions (that is by experiment) determine the results of this alteration, while language research always has to take notice of results and can only speculate on the changing factor. The war carried out 'experiments' in the most diverse areas: in the war, P. Kammerer states in his article "Sociological Questions of War Captivity," "nature itself has made an immense sociological experiment: she had disturbed normal conditions by bringing in contact selected groups of two not normally neighboring peoples. Take, for instance a small group of captured Italians, seized by the stormy wind of war and blown into German or Slavic territories, and forced to communicate with the foreign nation; what a wonderful social transplantation, what a

knowledge promising grafting of the people's body!" Hence, the establishment of censorship offices that control such transplanted groups and, as soon as they are known to the correspondents, they will be by-passed with all – linguistic and extralinguistic – means. This establishment produces an 'interdiction', as Meillet in his well-known article about the bear says, and the opposing inclination towards secret language accomplishes an experiment that otherwise could not be studied on such a large scale.



Ist in der Sprache ein Begriff „taboo" oder, mit Meillet zu reden, herrscht eine „interdiction linguistique", oder endlich mit einem in dieser Arbeit besonders naheliegenden Bilde, herrscht Zensur, so ist natürlich ein umso gröſserer Antrieb zur Variation gegeben: „Più un oggetto è stato *tabù*, e più cresce, necessariamente, il numero dei sinonimi", sagt Niceforo. Wer Hunger nicht sagen darf, wird zu den verschiedensten Ausflüchten greifen (ebenso wie der „moderne Kulturmensch", der weder fluchen noch von naturalia turpia reden darf): die sprachliche Zersplitterung, die Meillet bei den idg. Bezeichnungen des Bären[1] gefunden hat, sie findet sich wieder bei den Hungerumschreibungen der italienischen Kgf. in Österreich-Ungarn. Sprache unter Zensur entwickelt sich zu — allerdings motivgebundener — Mannigfaltigkeit.

6. In sich ist aber die Kgf.-Sprache ziemlich gleichartig: sie ist die Fortsetzung und Steigerung der in sich wieder homogenen Kasernensprache: daher der soldatensprachlich-unifizierende Zug in unseren Hungerumschreibungen, die in vielen Fällen geographische Abgrenzung einer Ausdrucksweise nicht zulieſsen: ein Wort wie *camel(lo)* hat sich unter allen Kriegsgefangenen von den oberitalienischen aus verbreitet und ist kein Dialekt- sondern ein Kriegsgefangenen- (oder Soldatenwort) geworden. Die Kgf., von der vereinheitlichenden italienischen Kaserne linguistisch uniformiert, wurden in Österreich von einem Lager zum andern transferiert: das Wirtland vollendete, was das Heimatland begonnen hatte (dieselbe Beobachtung macht Dauzat, *Mercure de France* in seinem Aufsatz vom 16. III. 1919 über „l'argot de nos [frz.!] prisonniers en Allemagne", den ich der Güte des Verf. verdanke). Wir haben hier die Ansätze zu einer *Κοινή*, einer Lagersprache, die nur deshalb für spätere Zeiten keine linguistischen Früchte tragen wird, weil die Ansiedlung der Soldaten im Wirtland (etwa wie bei den römischen Legionären in den Provinzen, den spanischen Conquistadoren in Amerika, Schuchardt, *Slawo-Deutsches u. Slawo-Italienisches* S. 21, Wagner, *Ztschr.* 1920, S. 388) ausblieb. Der

Fig. 5: Spitzer, *Die Umschreibung des Begriffs 'Hunger' im Italienischen,* detail of p. 281.

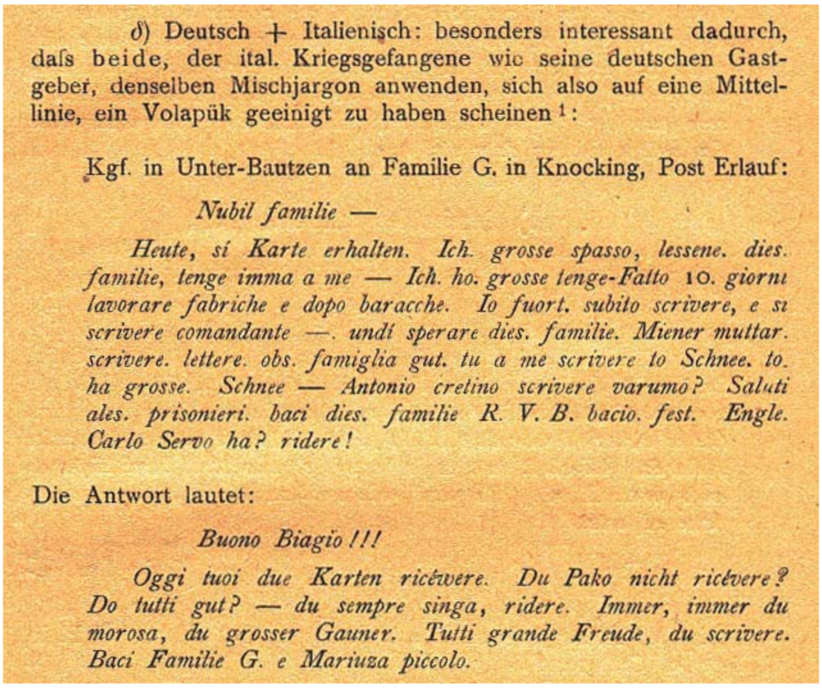
As we all know, almost all of Kammerer's experiments involved forcing various amphibians to breed in environments that were radically different from their native habitat. It is equally well-known that the results of these experiments were not really convincing and forced Kammerer to fake more convincing ones. While his experience with censorship of POW correspondence led the biologist Kammerer to manipulate the results of his experiments and even to commit suicide, the same experience, in the case of Spitzer, fitted into another fabulous philological career. The Italian department of the Central Censorship Office in Vienna which ordered every written complaint concerning malnutrition expressed by the Italian POWs in their correspondence with their families and friends to be made illegible represents the simplest form of experimental design. However, for the philologist the collected data were so abundant that he wrote many proposals to the Ministry of War in which he suggested transforming the office of censorship into an office of epistolary research. It is obvious that these proposals were never answered.[5] But what about this data source once the war ended, if, as Spitzer admits, the privacy of the letters can no longer be violated. His solution is simple: you only have to open the dead letter office, where uncountable records containing ordinary language rest unveiled, to linguists, folklorists, and sociologists.

The two main results of this abuse of a military institution for scientific purposes are described by Spitzer at the end of his 350 page book (fig. 5).

> If a notion in a language is 'taboo' or, to speak with Meillet, if an 'interdiction linguistique' exists, or finally with an image obvious for this work, if there is censorship, naturally there is a much greater impulse promoting variation. [...] Language under censorship develops into – obviously motivated – diversity.

On the one hand we have the very simple operation to impose upon one single notion – a taboo as a generator of language variation. But on the other hand the special situation of the POW camp has a unifying effect. It produces a *koiné*, an extreme type of *argot*, that results from a condensation of soldiers' slang and what Spitzer calls "Lagersprache," or camp language.

And finally, in the appendix of his study, we find a strange example of an emerging pidgin. This very strange *lingua franca* with its uncertain rules was constantly reinvented by a German family and their Italian forced laborers so that they could communicate:



δ) Deutsch + Italienisch: besonders interessant dadurch, daſs beide, der ital. Kriegsgefangene wie seine deutschen Gastgeber, denselben Mischjargon anwenden, sich also auf eine Mittellinie, ein Volapük geeinigt zu haben scheinen[1]:

Kgf. in Unter-Bautzen an Familie G. in Knocking, Post Erlauf:

Nubil familie —

Heute, sí Karte erhalten. Ich. grosse spasso, lessene. dies. familie, tenge imma a me — Ich. ho. grosse tenge-Fatto 10. *giorni lavorare fabriche e dopo baracche. Io fuort. subito scrivere, e si scrivere comandante —. undí sperare dies. familie. Miener muttar. scrivere. lettere. obs. famiglia gut. tu a me scrivere to Schnee. to. ha grosse. Schnee — Antonio cretino scrivere varumo? Saluti ales. prisonieri. baci dies. familie R. V. B. bacio. fest. Engle. Carlo Servo ha? ridere!*

Die Antwort lautet:

Buono Biagio!!!

Oggi tuoi due Karten ricéwere. Du Pako nicht ricévere? Do tutti gut? — du sempre singa, ridere. Immer, immer du morosa, du grosser Gauner. Tutti grande Freude, du scrivere. Baci Familie G. e Mariuza piccolo.

Fig. 6: Spitzer, *Die Umschreibung des Begriffs 'Hunger' im Italienischen,* detail of p. 312.

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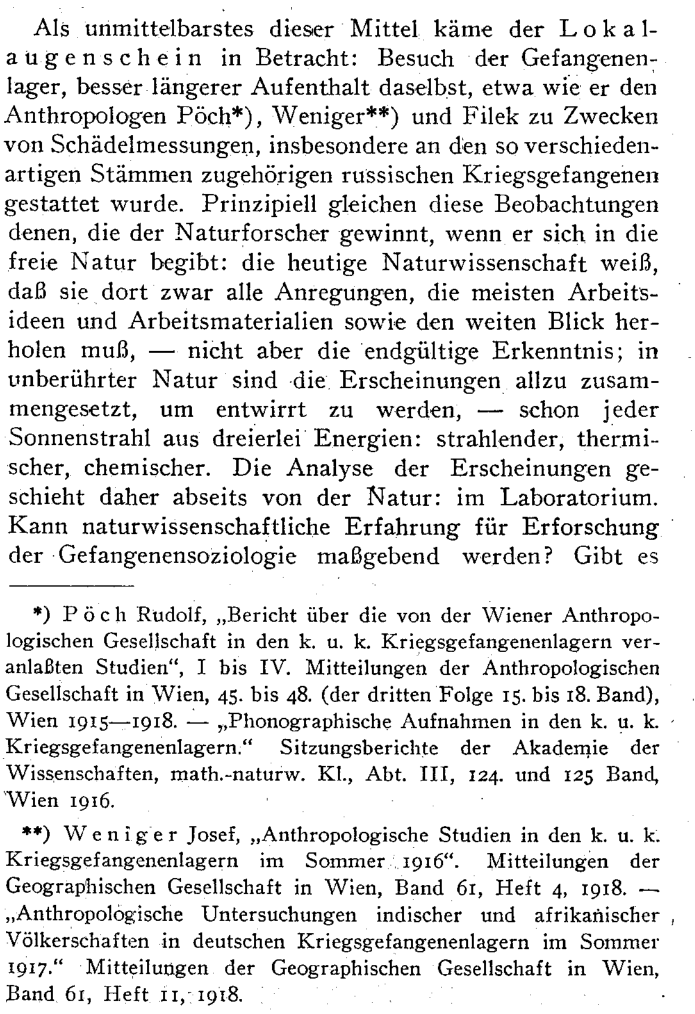
[5] See Leo Spitzer, "Organisationsmystik und Befehlswahn," *Die Wage* [!] (April 18, 1919), pp. 395-403.

Now I only have to mention a specific blindness for the first signs of a fundamental change that do not concern language, the object of philology, but rather philology itself. In the article written by Kammerer on sociological questions of war captivity that Spitzer cites in his introduction, there is a revealing footnote where the biologist illustrates with an example a totally different data-collecting procedure:



Als unmittelbarstes dieser Mittel käme der Lokalaugenschein in Betracht: Besuch der Gefangenenlager, besser längerer Aufenthalt daselbst, etwa wie er den Anthropologen Pöch*), Weniger**) und Filek zu Zwecken von Schädelmessungen, insbesondere an den so verschiedenartigen Stämmen zugehörigen russischen Kriegsgefangenen gestattet wurde. Prinzipiell gleichen diese Beobachtungen denen, die der Naturforscher gewinnt, wenn er sich in die freie Natur begibt: die heutige Naturwissenschaft weiß, daß sie dort zwar alle Anregungen, die meisten Arbeitsideen und Arbeitsmaterialien sowie den weiten Blick herholen muß, — nicht aber die endgültige Erkenntnis; in unberührter Natur sind die Erscheinungen allzu zusammengesetzt, um entwirrt zu werden, — schon jeder Sonnenstrahl aus dreierlei Energien: strahlender, thermischer, chemischer. Die Analyse der Erscheinungen geschieht daher abseits von der Natur: im Laboratorium. Kann naturwissenschaftliche Erfahrung für Erforschung der Gefangenensoziologie maßgebend werden? Gibt es

*) Pöch Rudolf, „Bericht über die von der Wiener Anthropologischen Gesellschaft in den k. u. k. Kriegsgefangenenlagern veranlaßten Studien", I bis IV. Mitteilungen der Anthropologischen Gesellschaft in Wien, 45. bis 48. (der dritten Folge 15. bis 18. Band), Wien 1915—1918. — „Phonographische Aufnahmen in den k. u. k. Kriegsgefangenenlagern." Sitzungsberichte der Akademie der Wissenschaften, math.-naturw. Kl., Abt. III, 124. und 125 Band, Wien 1916.

**) Weniger Josef, „Anthropologische Studien in den k. u. k. Kriegsgefangenenlagern im Sommer 1916". Mitteilungen der Geographischen Gesellschaft in Wien, Band 61, Heft 4, 1918. — „Anthropologische Untersuchungen indischer und afrikanischer Völkerschaften in deutschen Kriegsgefangenenlagern im Sommer 1917." Mitteilungen der Geographischen Gesellschaft in Wien, Band 61, Heft 11, 1918.

Fig. 7: Kammerer, "Soziologische Fragen der Kriegsgefangenschaft," p. 76.

"Lokalaugenschein," local evidence, "visitation of the POW camp, or rather an extended stay in that very place."[6] Yet the anthropologist Rudolf Pöch who is featured in this footnote not only inspected the POWs, he also recorded their voices and then reported on his "phonographic recordings" at the Academy of Sciences in Vienna in 1916. But his reports did not take place in the philological class; rather they were presented in mathematics and natural science classes. The kind of fieldwork that characterizes modern sociolinguistics was invented in the POW camps of the

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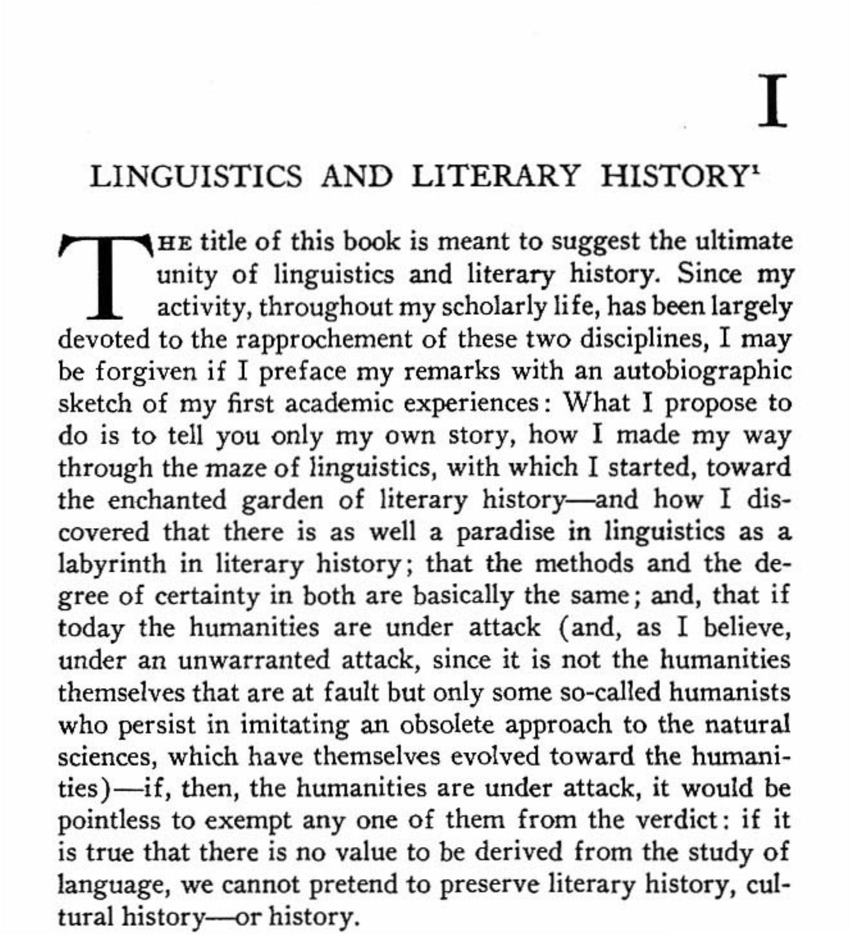
[6] Paul Kammerer, "Soziologische Fragen der Kriegsgefangenschaft," *Menschheitswende. Wanderungen im Grenzgebiet von Politik und Wissenschaft*, Wien: Verlag "Der Friede," 1919, pp. 74-85, quotation on p.76 (First published Dec. 6, 1918).

First World War, but the terminological armament including the crucial term of variation was developed in Vienna by philological censors. At least the great sociolinguist William Labov who greatly inspired Deleuze and Guattari in *Thousand Plateaus* owes more to the camp and censorship than we would care to admit.

Fig. 8: Leo Spitzer (1887-1960). See Hans Ulrich Gumbrecht, *Vom Leben und Sterben der großen Romanisten. Carl Vossler, Ernst Robert Curtius, Leo Spitzer, Erich Auerbach, Werner Krauss*, München: Carl Hanser Verlag, 2002, pp. 72-151.

While we can have a look at Spitzer's office at Johns Hopkins University in Baltimore I will briefly give some biographical dates concerning Leo Spitzer. He was born 1887 in Vienna. In 1910 he was awarded his doctoral degree with a dissertation on Rabelais. Three years later he completed his habilitation. In 1925 he was given his first professorship in Marburg, and in 1930 he relocated to Cologne. In October 1933 the Spitzer family left Germany to move to Istanbul where Spitzer taught for three years before being offered a professorship at Johns Hopkins. Spitzer's method, which he calls stilistics, is the hybrid compound of linguistics, literary history and close reading. But he also works on historical semantics and on the phenomena of creativity in ordinary language. He even used his domestic conversation with his wife to write a book on lovers' talk.

In the essay “Linguistics and Literary History”[7] (incidentally, in Foucault’s original translation the title reads “Art du langage et linguistique”) which opens a collection of essays with the same title, Spitzer explains his concept of, or to be more precise, his experience with philology:



I

LINGUISTICS AND LITERARY HISTORY[1]

THE title of this book is meant to suggest the ultimate unity of linguistics and literary history. Since my activity, throughout my scholarly life, has been largely devoted to the rapprochement of these two disciplines, I may be forgiven if I preface my remarks with an autobiographic sketch of my first academic experiences: What I propose to do is to tell you only my own story, how I made my way through the maze of linguistics, with which I started, toward the enchanted garden of literary history—and how I discovered that there is as well a paradise in linguistics as a labyrinth in literary history; that the methods and the degree of certainty in both are basically the same; and, that if today the humanities are under attack (and, as I believe, under an unwarranted attack, since it is not the humanities themselves that are at fault but only some so-called humanists who persist in imitating an obsolete approach to the natural sciences, which have themselves evolved toward the humanities)—if, then, the humanities are under attack, it would be pointless to exempt any one of them from the verdict: if it is true that there is no value to be derived from the study of language, we cannot pretend to preserve literary history, cultural history—or history.

Fig. 9: Spitzer, “Linguistics and Literary History,” detail of p. 1.

Whether Spitzer is right or wrong with his assertion that cultural history – or history would not survive philology, remains undecided. But what we can witness is a new attitude in the history of science. The great historical narratives of the development of single sciences are increasingly being replaced by detailed descriptions of concrete laboratory situations. At least this science has disengaged itself from the obsolete model of literary history. Maybe these accurate studies that strongly recall Leo Spitzer’s stylistic essays are in a certain sense philological.

Before ending I would like to leave the last word to Jean Starobinski. In his introduction to a French collection of essays by Spitzer, *Etude de style*, which contains Foucault’s translation mentioned before, Starobinski writes:

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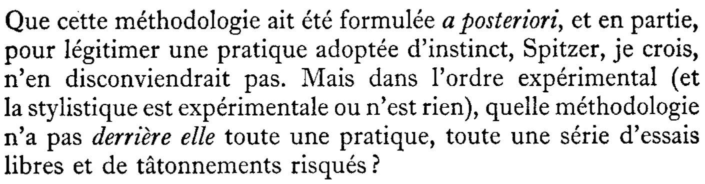
[7] Leo Spitzer, “Linguistics and Literary History,” in: idem, *Linguistics and Literary History. Essays in Stylistics*, Princeton: Princeton University Press, 1948, pp. 1-39.



Que cette méthodologie ait été formulée *a posteriori*, et en partie, pour légitimer une pratique adoptée d'instinct, Spitzer, je crois, n'en disconviendrait pas. Mais dans l'ordre expérimental (et la stylistique est expérimentale ou n'est rien), quelle méthodologie n'a pas *derrière elle* toute une pratique, toute une série d'essais libres et de tâtonnements risqués ?

Fig. 10: Starobinski, "Leo Spitzer et la lecture stilistique."

That this methodology had been formulated a posteriori, and partly to legitimize an instinctively adopted practice, did not cause Spitzer any discomfort. But in the realm of experiment (and stylistics are experimental or they are nothing), what methodology does not have *behind it* a whole practice, an entire series of deliberate attempts and risky gropings in the dark?[8]

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[8] Jean Starobinski, "Leo Spitzer et la lecture stilistique," in: Leo Spitzer, *Études de style*, Paris: Gallimard, 1970, pp. 7-39, quotation on p. 30.

The Garden as a Laboratory
Nineteenth-Century Scientific Gardening

Björn Brüsch

With the publication of Mathias Jacob Schleiden's *Grundzüge der wissenschaftlichen Botanik* in 1842 and the establishment of cell theory, the science of botany rose like a phoenix. In the eyes of the modern protagonists of the 1840s, botany finally became a rigorous science of its own, after withering decades of trifling "away time in plant-collecting in wood and meadow and in rummaging in herbaria."[1] Having grounded the botanical sciences in new, mechanistic foundations, the times "where plant-describing was comfourtly flourishing" leading to "frivolous dilettanteism"[2] were eventually overcome.

I don't wish to make an example either of the trifling away in plant collecting nor the degeneration of the science of botany. Rather I would like to take this starting point to investigate and analyse the science of botany in the first half of the nineteenth century. In the following I want to argue that while most botanists paid a great deal of attention to gradual improvements in classification, they also undertook further and more detailed research into plant anatomy and physiological morphology. Their investigation into the nature of plants included manifold *Versuche* or experimental trials. In order to shed some light on the work of botanists and gardeners within the Berlin context, I want first to take a closer look at the work of Heinrich Friedrich Link and Alexander von Humboldt, as both have contributed to the field of plant physiology and also share the local Berlin connection. Furthermore both scientists were members of a society aimed at advancing Prussian horticulture. With a specific focus on the garden, the second part of my paper will try to highlight some of the trials or experiments carried out with plants at the time, thereby providing some understanding of the ways in which one can actually speak of experiments or the shape those experiments had.

It seems to me a well-established fact that at some point botanists who were engaged in taxonomy had to take a closer and microscopic look at plant structures in order to distinguish between species. According to their affinities, those species were then assigned a place within the plant kingdom.

This microscopical research naturally led botanists to questions concerning the living nature of plants and their parts. As early as 1804, Heinrich Friedrich Link, director of the botanical garden at Schöneberg and holder of the chair for natural history at the Friedrich-Wilhelm University starting in 1815, had contributed a work to the Academy of Göttingen which had offered a prize for the best essay describing the nature of vessels in plants. The background for this problem was the much-discussed issue of whether plant vessels originated from single cells or had to be considered as a homogeneous mass into which cells were incorporated. In his work, which was published in a book of the prize-winning papers three years later, Link brought forward many ideas that were, strictly speaking, of physiological nature. Much of the paper's content was

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[1] Julius Sachs, *History of Botany 1530-1860*, New York: Russell & Russell, 1967, p. 187.

[2] *Ibid.*, p. 125 and 188.

stimulated by general physiology, which at that time and later, was conceived as a specialized physics and was accordingly considered a primary or exclusive field of medicine and functional anatomy. Link, himself a trained physician, adhered to this conviction arguing that "[t]he physiology of plants can only take its foundations from the general physiology of organic bodies. Hence, it is only an applied science, based especially on what anatomy has demonstrated, and cannot be sharply separated from anatomy."[3]

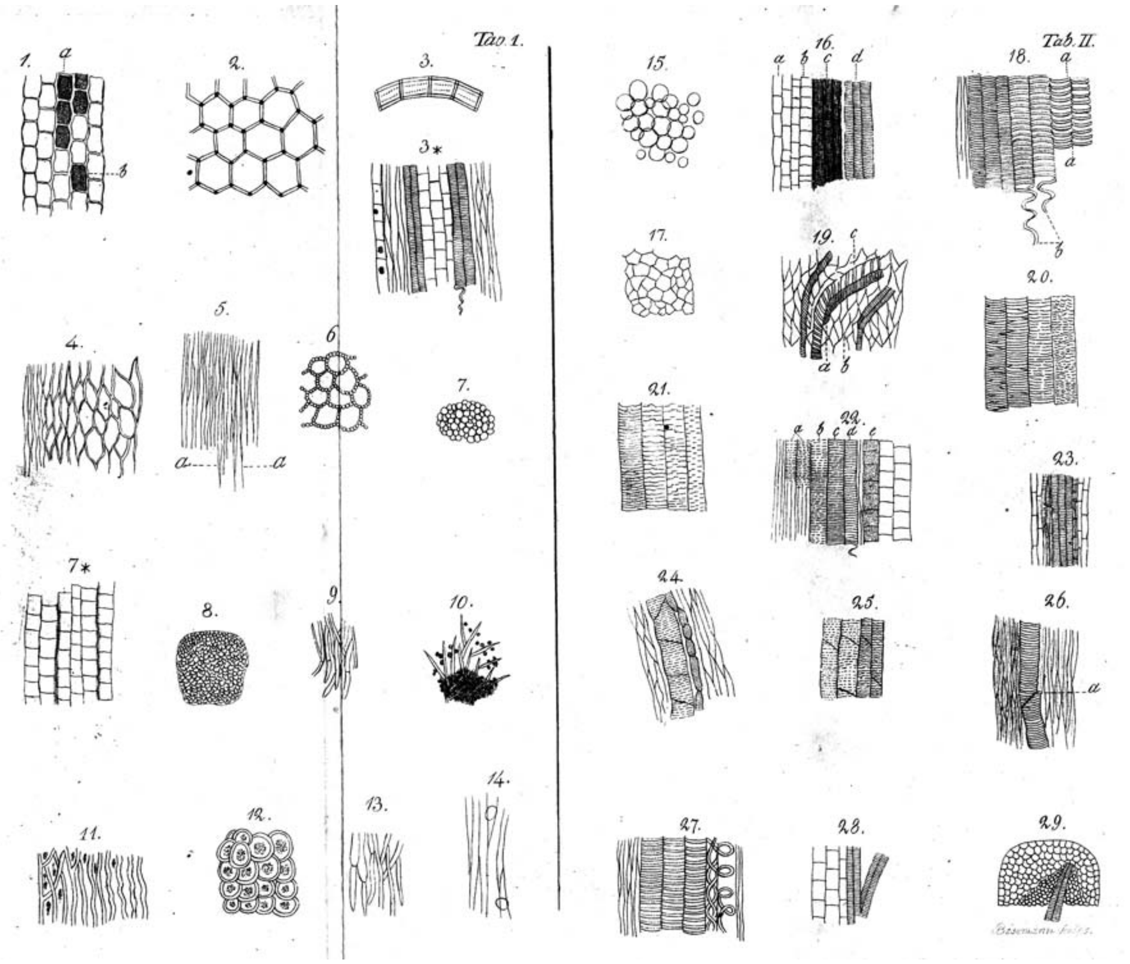

Fig. 1: Plate 1 of H. F. Link (1807) Grundlehren der Anatomie und Physiologie der Pflanzen.

Equipped with a microscope, which he especially praised, and a sharp anatomical knife, Link with this essay ventured into anatomical research as a field that explored the subtler parts of plants – the parts whose structure defined the way higher anatomical ranks were organized. (fig. 1). In doing so, he gave a rich account of how closely associated comparative anatomical and morphological, and also developmental, studies were with physiological enquiries. While the first

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[3] "Die Physiologie der Pflanzen kann ihre Grundsätze nur von einer allgemeinen Physiologie der organischen Körper hernehmen. Sie ist daher nur eine angewendete, besonders auf das, was die Anatomie dargestellt hat, und kann also von ihr nicht rein und schneidend getrennt werden." Heinrich Friedrich Link, *Grundlehren der Anatomie und Physiologie der Pflanzen*, Göttingen: Danckwerts, 1807, p. 4f.

two sections of the work deal with plant structures at different organisational and morphological levels, the last section, *Von der Pflanze überhaupt*, highlights the life of plants – as the primary concern of physiology – and the internal modifications and both the time and circumstances required for these alterations to come about.[4]

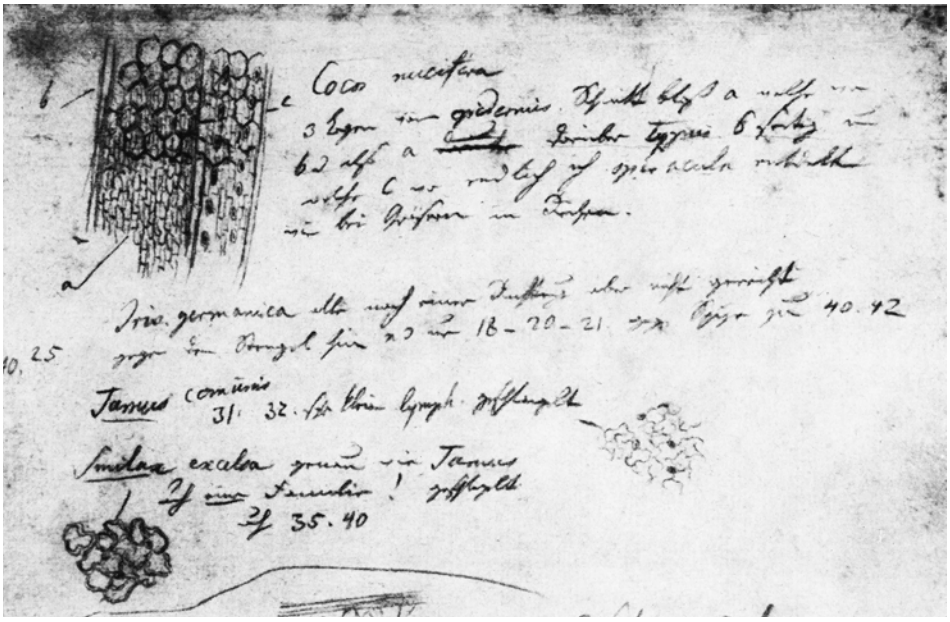

Fig. 2: Detail of Humboldt's study and observation journal with anatomical details.
In: Jahn (1969).

While studying at Freiberg, and later at the Royal Chemical Laboratory of Sigismund Friedrich Hermbstaedt in Berlin, Alexander von Humboldt also pursued both chemical and physiological experiments with plants (fig. 2). Much of his research was collected in his publication, *Aphorismen aus der chemischen Physiologie der Pflanzen*, in 1793, which was a part of his first major botanical work, *Florae fribergensis specimen*. Within this work, which must be regarded as a continuation of his earliest physiological and experimental works, Humboldt gives a rich picture of his experiments, in addition to a detailed bibliography. The experiments amounted to some 4,000 in the years between 1792 and 1797 alone.[5] Among other studies, Humboldt experimented on the germination of seeds that had been treated with various chemical substances, e.g. hydrochloric acid, and on the influence of sunlight, oxygen and electricity on growth, excitability and the luminescence and greening of plant materials.[6] All of this focussed on major questions – on the

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[4] *Ibid.*, p. 245ff.

[5] Alexander von Humboldt, *Versuche über die gereizte Muskel- und Nervenfaser, nebst Vermutungen in der Physiologie über den chemischen Process des Lebens in der Thier- und Pflanzenwelt*, vol. 2, Berlin: Decker, 1799, p. 173. See also Ilse Jahn, *Grundzüge der Biologiegeschichte*, Jena: Fischer, 1990, p. 291.

[6] Ilse Jahn, "Der Einfluß experimentell-botanischer Forschungen auf die Wandlungen in der Physiologie von Alexander von Humboldt bis Emil Du Bois-Reymond," *NTM – Internationale Zeitschrift für Geschichte und Ethik der Naturwissenschaften, Technik und Medizin*, 4/9 (1967), pp. 66-83, p. 68. – Chlorine, or as Humboldt uses it, oxidised chlorine acid, was found to accelerate the germination of seeds remarkably. According to the principles of the chemistry of Lavoisier this enhancement was due to the provision of oxygen.

origins of plant physiology – and was to be undertaken by experiments, trials and observations in order to distinguish between animate and inanimate spheres of nature. It was a physiology of plants that took up many of the improvements of contemporary chemistry and was accordingly so perceived.

Years later, while highlighting the relevance of comparative anatomical and microscopical works, Link also hinted at the importance of chemical analysis:

> Apart from breaking down and analysis, chemistry provides the means to identify objects. It shows us inner differences where no, or only indistinct, outer ones exist and one must not disdain this help. In short, all means of achieving finer distinctions are to be applied in plant anatomy.[7]

It also becomes clear that Link initially had different things in mind than Humboldt when advising the use of chemically inspired observations and research, namely taxonomical research. While many, if not all, of Humboldt's early experiments were stimulated by the concept of vital force or *vis vitalis*, which essentially addressed the question of the connection or difference between inorganic and organic, of the organism as a whole, of galvanism and irritability, something that Humboldt himself termed "vital chemistry" or "chemical physiology of plants,"[8] Link was mainly aiming for an additional tool to distinguish between plant species taxonomically.

Years before the publication of Link's book, Humboldt, in a introduction to the translated work of the Dutch physician Jan Ingenhousz, *An essay on the food of plants and renovation of the soil*, published in German in 1798, did raise some questions about the general topics and the future purpose of plant physiology. Already in 1795, in a contribution to Schiller's journal *Die Horen*, Humboldt was arguing that chemistry was able to reliably shed the brightest light on the physical life of natural bodies[9] – suggesting a research program that was very much encouraged by the quantitative methods and work of the chemists of the time.[10] According to Humboldt, Ingenhousz's work featured the "application of physical-chemical knowledge to the physiology of organic bodies,"[11] which in consequence offered many advantages for the cultivation of plants.

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[7] "Ausser der Zerschneidung und Darlegung liefert die Chemie noch manche Mittel, die Gegenstände zu erkennen. Sie zeigt uns innere Verschiedenheiten, wo keine äussere, oder diese nur undeutlich vorhanden sind, und man darf ihre Hülfe nicht verschmähen. Kurz alle Mittel, zu feinen Unterscheidungen zu gelangen, sind in der Anatomie der Pflanzen anzuwenden." Link, *Grundlehren der Anatomie und Physiologie der Pflanzen*, p. 4. – Link, years later, set aside a laboratory within the greenhouses of the botanical garden to further pursue plant chemistry or, as he then termed it, "*vegetabilische Chemie*."

[8] Ilse Jahn, "Der Einfluß experimentell-botanischer Forschungen auf die Wandlungen in der Physiologie von Alexander von Humboldt bis Emil Du Bois-Reymond," p. 68 and 70. See also *idem.*, *Dem Leben auf der Spur. Die biologischen Forschungen Alexander von Humboldts*, Leipzig, Jena and Berlin: Urania, 1969, p. 40, and "The transformation of plant and animal physiology in the programmatic work of A. v. Humboldt (1797)," in: Brigitte Hoppe (ed.), *Biology Integrating Scientific Fundamentals*, München: Institut für Geschichte der Naturwissenschaften, 1997, pp. 101-113, p. 103.

[9] Alexander von Humboldt, "Die Lebenskraft oder der Rhodische Genius," *Die Horen (*1795), pp. 90-96. See also *idem.*, *Friedrich Alexander von Humboldt's Aphorismen aus der chemischen Physiologie der Pflanzen*, Leipzig: Voss, 1794, VIII.

[10] Alexander von Humboldt, "Versuche und Beobachtungen über die grüne Farbe unterirdischer Vegetation," *Journal der Physik* 5/2 (1792), pp. 195-204, p. 195. See also Ursula Klein's contribution in this volume.

> The deeper we penetrate into the dark of organic forces, [and] the more we guess of the capacious process of life, through which all vital phenomena in the bodies of plants and animals are affected, the sooner we may hope to find the means to promote a faster development of the organs and the refinement of the saps of plants."[12] "Hence, scientific knowledge of the cultivation of plants (*Pflanzenkultur*) cannot exist without plant physiology, and this cannot exist without general meteorology and chemistry.[13]

Fig. 3: State nursery at Wildpark – Detail of "Verschoenerungs-Plan der Umgegend von Potsdam entworfen von Lenné" (1833) – Stiftung Preußische Schlösser und Gärten Berlin-Brandenburg/ Fotograf.

In subsequent passages Humboldt argued in favour of a strenuous application of quantitative chemical principles within agricultural and horticultural domains. This research shed light on the material basis of plant growth in relation to the nutrition of plants and with respect to the proportions and ratios of inorganic components of the soil and organic constituents of plant material. Though in his view, the physiology of plants was more praised than actually studied, Humboldt, as a trained *Kameralist*, was convinced of the ultimate benefit and profit of a mutual conjunction of chemistry and plant physiology.

With this view Humboldt was in accordance with many of his contemporaries. I mention here only the works of Theodore de Saussure and Albrecht Daniel Thaer, both of whom established the fundamentals of the physiology of plant nutrition. Especially Thaer, who had been recruited by Hardenberg in 1804, pursued an applied botany in combination with nutrition physiology that was perceived to facilitate utilitarian purposes such as the generation of useful natural products. This, in return, would provide for an increase in the wealth of the nation. It was a progressive combination of science, economy and agriculture that ultimately was to be of the highest benefit to all residents. By aiming to chemically characterize and understand the processes of plant life on both an anatomical-morphological and physiological level through experimental analysis, plant growth could be enhanced by actively managing the environment or *Lebensumwelt* of plant species.

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[11] Alexander von Humboldt, "Einleitung über einige Gegenstände der Pflanzenphysiologie," in: Jan Ingenhousz, *Über Ernährung der Pflanzen und Fruchtbarkeit des Bodens*, Leipzig: Schäfersche Buchhandlung, pp. 3-44, p. 4.

[12] "Je tiefer wir in das Dunkel der organischen Kräfte eindringen, je mehr wir von dem großen Lebensprocesse errathen, durch den alle vitalen Erscheinungen im Thier- und Pflanzenkörper bewirkt werden, desto eher dürfen wir hoffen, die Mittel aufzufinden, durch welche die schnellere Entwicklung der Organe, und die Veredlung ihrer Säfte befördert wird." *Ibid.*, p. 5.

[13] "Wissenschaftliche Kenntniß der Pflanzenkultur kann daher nicht ohne Pflanzenphysiologie, diese nicht ohne allgemeine Meteorologie und Chemie bestehen." *Ibid.*, p. 9.

In 1822, the more-or-less practically oriented *Society for the Advancement of Horticulture in the Royal Prussian States* was founded in Berlin.[14] The society had a piece of land within the garden adjacent to the Gardeners' Institute in Schöneberg and was also institutionally connected to a state-administered nursery at Wildpark (fig. 3 and 4). The latter in particular provided the space necessary to grow plants that had been sent to the society or that the society had acquired by exchange or other means. The experimental fields connected to the nursery were to be used for the propagation of scientific knowledge and the enrichment of both art and sciences by means of authoritative and reliable trials and observations. The location of the so-called *observatorio* – which assembled, in a bounded forest, the many plant species grown in trials – suited the overall utilitarian aims connected to it. It took advantage of the favourable climatic conditions and the various types of soil and substituted for the insufficient provincial institutions at different venues within the Prussian state.[15]

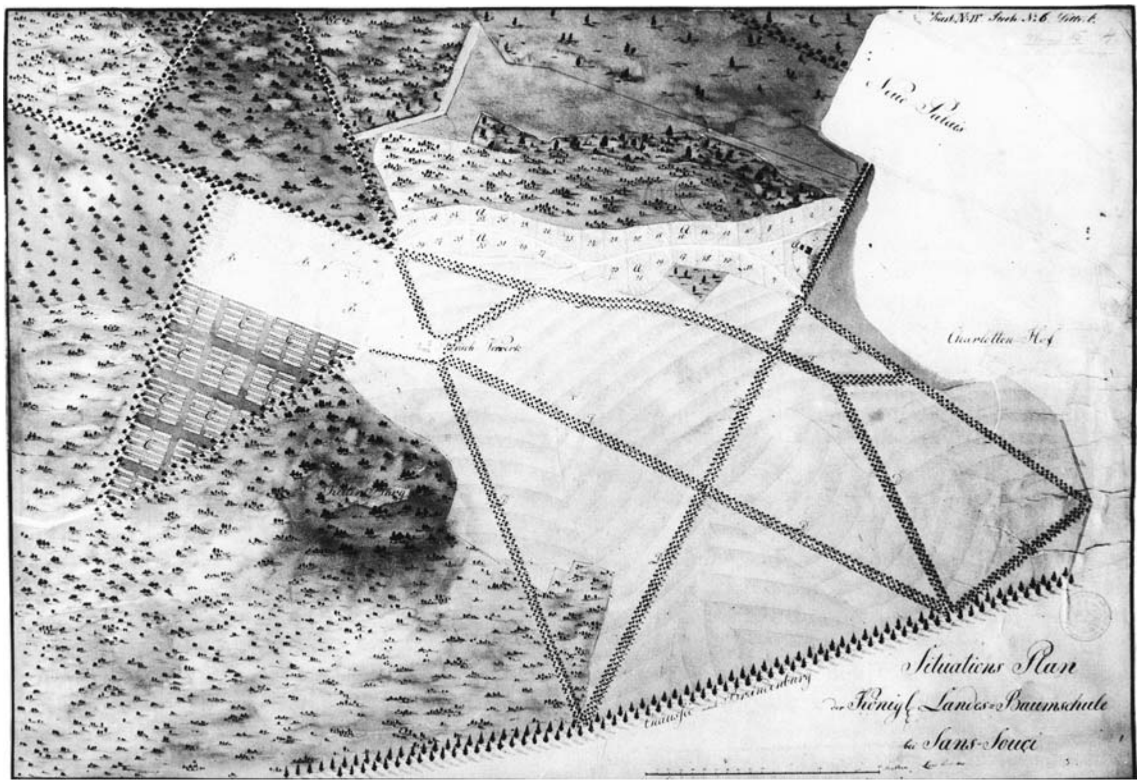

Fig. 4: Plan for the arrangement and establishment of a state nursery in the Pirschheide, Lenné (1821), Original lost. In: Dreger (1992), 155.[16]

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[14] For a general overview of the society's history see, e.g., Gert Gröning, "Peter Joseph Linné in der 'Verein zur Beförderung des Gartenbaus in den Königlich Preußischen Staaten'," in: Florian von Buttlar (ed.), *Peter Joseph Lenné. Volkspark und Arkadien*, Berlin: Nicolaische Verlagsbuchhandlung, 1989, pp. 82-90.

[15] This instance was later used by Ferdinand Jühlke, director of the Royal Prussian gardens starting in 1866, to propose a physiological research station in connection with the Royal Gardeners' Institute at Wildpark. Although similar research stations had already been established at the universities in Berlin and Breslau and in Proskau and Geisenheim, Jühlke, in an official request, explicitly argued for an additional practical research station, as this would help to analyse the physiology of plants scientifically in diverse geographical settings.

In connection with the botanical garden, many species were planted and scrutinizingly observed. Moreover, its head botanical gardeners Link and Friedrich Christoph Otto contributed tremendously to the exchange at the society's meetings. Foremost, it was the efficiency and benefit of novel species compared to known fruit and vegetable species that was of interest and that was accordingly reported at the society's monthly gatherings. Some of the more interesting species were also studied on an anatomical and morphological level. The results again fed into the society's meetings and were sometimes published in the society's journal.

Much of the society's work focussed on *Pflanzungs-Versuche* and *Kulturversuche*. These cultivation trials included the experimental cultivation of new species and the accurate observation of local environmental factors (e.g. climate, soil, temperature and precipitation). Some fields were assigned to the testing of grafted, improved or foreign species. The society further initiated trials on artificial pollination and fertilisation, experimental tests to answer questions concerning the nutrition of plants and the evaluation of the effectiveness of fertilizers, both organic, such as bonemeal or clay, and artificial, such as acidic lime. Notably the research on fertilisers included procedures which were not, strictly speaking, experimental but were rather carried out through observation of actual growth and which nevertheless included some basic experimental proceedings. In this context, the initiated experimental tests were an application of the Humboldtian vital chemistry, a stimulation of the life of plants that emphasised the vital functions of plants and focussed on the material outcome.

At the society's meeting on May 25th 1825, it was communicated that Otto, Peter Joseph Lenné and Albrecht Daniel Thaer from Möglin had each received two balloons of acidic lime (*salzsaurer Kalk*)[17] and had been asked to undertake tests to analyse the efficiency of the substance. After the acquisition costs were reported, it was mentioned that the trials, which had already been initiated, would eventually provide evidence of the success of the substance.[18] On September 12th, a comprehensive list of relevant literature was presented at the society's meeting.

Only a little while later, at a meeting on October 9th, Link himself presented an abstract and overview of the material that had been collected so far.[19] While many, according to Link, had been

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[16] Dreger, H.-J. (1992) Die Königliche Landesbaumschule zu Potsdam und Alt-Geltow. In: Peter Joseph Lenné. Gartenkunst im 19. Jahrhundert. Beiträge zur Lenné-Forschung. D. Karg (Ed.) Verlag für Bauwesen: Berlin, 147-170.

[17] The precise translation of the German term *salzsaurer Kalk* is complicated. According to the new chemical nomenclature introduced by Lavoisier, Guyton de Morveau, Berthollet and Fourcroy in the late eighteenth century (*Method of chymical nomenclature : a new system of chemical charcters*, London: Kearsly, 1788) lime belonged to the five earths, being termed a calcareous earth (p. 46). Its acidic character was ascribed to muriatic acid (pp. 32-34), its salt then being termed muriat of lime (*Muriat calcarcus*) (p. 138). The German translation of the above-mentioned work gives *kochsalzgesäuerte Kalkerde* as the term (*Methode der chemischen Nomenklatur für das antiphlogistische System von Morveau, Lavoisier, Berthollet und de Fourcroy*, Hildesheim: Olms, 1978 [1793], pp. 225, 229). This seems to be in accordance with the meaning of *salzsauer*, which described dry chlorine metals (*Handwörterbuch der reinen und angewandten Chemie*, Braunschweig: Vieweg, 1859, vol. VII, p. 218). However, the term muriatic acid is no longer in use, today being called hydrochloric acid. As pure lime does not contain noteworthy amounts of chlorine, the substance designated as *salzsaurer Kalk* was probably a mixture of calcareous earth or lime and calcium chloride ($CaCl_2$), which is still used as a common fertiliser. I will refer to the substance as acidic lime subsequently.

[18] *Verhandlungen des Vereins zur Beförderung des Gartenbaues in den Königlich Preußischen Staaten*, vol. 2, 1826, p. 238.

[19] *Ibid.*, p. 419f.

writing comprehensively on the beneficial use of acidic lime as fertiliser, he did not know of any detailed trials that would shed some light on the question. He reported that many members of the society had been using the substance but some disastrously, as the recommended dilution of the lime powder had proven to be inexact if not completely false. Link continued to report on experimental trials concerning the influence of soil on the components of plants carried out by *Ober-Medizinal-Assessor* Schrader, who later gave a lengthy account of his results in the society's journal.[20] By way of comparative trials Schrader had first made use of various known fertilisers. Then he had varied the amount of lime powder applied in order to be able to make statements about the benefit to observable plant growth, which he could easily measure in terms of height, foliage and time of blossoming, fruiting and withering. The experimental set-up also included various plant species and different types of soils, gardens and pots for planting.

According to Schrader's findings, lime powder enhanced the growth of both seeds and plants most effectively. Link discussed these results in the light of suggested explanations taken from the literature. In Link's opinion, the lime powder only acted as a sort of sensation or stimulus on the germination and growth of plants, "because it should not to assumed that acidic lime actually nourishes the plant."[21] The chemical substance stimulated some common plant character, and in doing so, activated growth by increasing the potential of plants to absorb nutrients in a given time span, therefore also providing for a better circulation within the plant. While rejecting other theories, such as that lime primarily enhances soil moisture as it generally attracts moisture, Link was feeding experimental results back into physiological discussions of the time, linking them both to known theories of plant nutrition and microscopically established structures of plant anatomy and morphology. Although the physiological principles of the lime-induced enhancement of plant growth were not fully understood, the observed and registered increase in biomass was reason enough to discuss the results physiologically.

Even though many of the experimental trials and tests that the society carried out in its garden at the state nursery and in privately owned gardens of its members did not specifically aim at a deeper understanding of the physical life of natural objects, contemporary developments within the science of botany were incorporated. Much was discussed at the society's meetings, and many of the experimental trials contributed to an understanding of the physiological nature of plants and the correlations between plants and their environments.

In the opinion of Lenné and other founding members of the society, this was gardening at its peak. By incorporating all relevant and useful aspects of the natural sciences, especially botany, chemistry, physics and plant physiology, gardening itself became a science, with a thorough theoretical basis and means of establishing observations and carrying out scientific analysis and examination.

Scientific gardening thus was regarded as a practical domain of botany, closely linking it to the botanical sciences. It did not concentrate on the careful determination and framing of experimental set-ups, but, above all, on the practical application of scientific fundamentals and general principles that seemed important.

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[20] Schrader, "Bemerkungen über die Anwendung des salzsauren Kalkes als Düngmittel," in: *Verhandlungen*, vol. 2., p. 425-431.

[21] "Also möchte wohl die Wirkung ganz ..., auf die reitzende, zurückkommen, denn daß der salzsaure Kalk geradezu nähre, ist nicht anzunehmen." *Verhandlungen*, vol. 2., p. 419.

Having said this, one could argue that it was an applied physiological botany that ventured outside thereby following up an experimental procedure which was known as other botanists and gardeners had done so in earlier times. But as much of the discussion of contemporary physiology centered around the applicability or utility of the research, it seemed natural to study plants in their environments. It was even more natural to observe how plant species grew within the restrictions imposed on them by both natural and artificially altered environmental factors – an aim that was also of relevance to the many acclimatisation projects of the time. Understanding the life of plants would eventually contribute to the prosperous life of the country.

The garden as a bounded, albeit open, cultivated area provided all the control necessary to carry out experimental trials. As a place mimicking nature the area of the garden was as close to nature as to its imposed culture. The garden therefore helped to establish the means to question, observe and describe nature physiologically. It was a space for *Erfahrungswissenschaft* – a bridge or an intermediate station somewhere between amateur and natural science. By visually demonstrating the living processes of plants, it also allowed for a public sphere where anyone – scholars and amateurs of science – could follow the trials that were being carried out, thus providing both hands-on experience and an understanding of stimulated and perfected *Bildung*. It had as much to do with the popularisation and visualisation of scientific foundations as it had to do with the utilisation of the many species grown. From this perspective the *observatorio* also highlighted its innermost dual function: it was designed both for research and illustration.

Within the trials the microscope and the anatomical knife did not play too much of a role. It was the visibility of structural details that counted. Because the plant as the object of study displayed its complete developmental history, the structural details were almost as easily comprehensible as the object of study – the plant – itself.[22] It was both the visibility of the structural organisation of plants and also the numerous plant species that allowed for the rather extensive time frame of the experimental trials, the extended time, in return, enabling almost every gardener to become an observer and experimenter.

Observation, as the name *observatorio* at the state nursery suggests, was the most prominent part of an experimental set-up, as plants were accessible for the research and view of the many observers.[23] As both a tool and a method of visualisation, the scientific "art of observation" was an experiment on its own, which almost everybody could follow, as the field for observation was, as also Humboldt was convinced, overwhelming and immeasurable.[24]

As much as one has to understand Julius Sachs's comment, considering the many developments in plant physiology of the nineteenth century, that the protagonists of the time only offered "lifeless phrases" to later generations of botanists and plant physiologists, it becomes clear that they did not merely "trifle away time in plant collecting."[25] Although many of the activities of the *Society for the Advancement of Horticulture* and the botanical garden were indeed concerned

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[22] Heinrich Friedrich Link, "Ueber das Anwachsen von Theilen in den Pflanzen," *Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin* (1836), pp. 179-186, p. 179.

[23] For the history and use of the term *observatorio* in nineteenth-century physiological research see Richard L. Kremer, "Building institutes for physiology in Prussia, 1836-1846," in: Andrew Cunningham & Perry Williams (ed.), *The Laboratory Revolution in Medicine*, Cambridge: Cambridge University Press, 1992, p. 72-109.

[24] Alexander von Humboldt, "Einleitung über einige Gegenstände der Pflanzenphysiologie," p. 7.

[25] Julius Sachs, *History of Botany 1530-1860*, New York: Russell & Russell, 1967, p. 187.

with the collection of plants, the members also applied their knowledge of the established fundamentals, laws, causes and foundations of the life of plants, derived from chemical and physical investigations, in the field. The garden, though not a fully equipped laboratory, was the most useful and cultivated botanical laboratory of the time. Within this laboratory, a physiological praxis could be established and easily followed. As a location or stage for experiences it allowed for an almost immediate *Anschauung* of the successive processes of life, and thus for an organic study of nature and for a scientific progress shaped by seeing.

Within the comparative approach, the art of observation was the most prominent part of the experimental set-up. This practice in particular would be incorporated into the various doings of botanists and gardeners of the time, as they always had a fraction of land under cultivation set aside for experimental trials. By engaging themselves with plants in the garden they contributed massively to contemporary botanical sciences.

An-Aesthetic Revelations: On Some Introspective Self-Experiments in the History of Inebriation

Katrin Solhdju

Vital functions and animate organisms are unstable research objects: they change from moment to moment, they move in space and they transform in time. It is due to their very vitality that they resist clear-cut objectification. The task of scientific disciplines such as physiology and psychology, which deal with the operating modes of the living body, is therefore to isolate certain functions and investigate them over a period of time. In the following paper, I want to focus on two experimental arrangements that dealt with functions at two opposite borders of vitality: pain and anaesthesia – both are states of an imbalanced vitality and, pushed to their limits, result in death.

The first experiment I will talk about was carried out by Henry Head who, starting in 1903, dealt with pain perception using external stimuli and introspective observation. Head began his study of medicine after his training in the natural sciences and some research on respiratory movement in the 1880s in Prague that was later widely applied in anaesthesia. In 1892 he received his PhD from Cambridge; his thesis dealt with hyperalgesia and hyperaesthesia of irritated body parts in visceral disease.[1] This work established the basis for his later self-experiments.

Benjamin Paul Blood, in contrast, was an author and philosopher who lived quite detached from the academic world in Amsterdam, New York, mostly writing for little-known newspapers and journals. Starting in 1860 he conducted a series of self-experiments with anaesthetics that resulted in a pamphlet with the title *The Anaesthetic Revelation and the Gist of Philosophy*.[2]

In Head's as well as in Blood's case, the experimenter's body and mind represented the means and ends of the accomplished experiments. Both were self-experiments that intervened in the body physically and/or chemically and at the same time required introspection and subjective utterances as a method. What they produced on very different levels was knowledge about subjectivity, in the first case generated through a combination of introspectively observed perceptions and physiological quantifications and in the second case resulting from philosophical reflections on the subjectivity as an epistemic entity. What both experiments had in common methodologically was their somewhat paradoxical approach: they investigated modes of perception by voluntarily destroying them in the first step.

Henry Head: From pain to insensibility and back again

"On April 25, 1903, the radial (ramus cutaneus n. radialis) and external cutaneous nerves were divided in the neighbourhood of my elbow, and after small portions had been excised, the ends were united with silk sutures,"[3] Henry Head wrote in 1905, two years after the beginning of his long and quite destructive self-experiment.

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[1] Henry Head, "On the Disturbances of Sensation, with Special Reference to the Pain of Visceral Disease. Thesis for M.D." *Brain* 16 (1893), pp. 1-133; 17 (1894), pp. 339-480; 19 (1896), pp. 153-276.

[2] Benjamin Paul Blood, *The Anaesthetic Revelation and the Gist of Philosophy*, Amsterdam/New York 1874.

[3] Henry Head, "The Afferent Nervous System from a New Aspect," *Brain* II (1905), pp. 99-115, p. 102.

After Head and his colleagues had experimented on the issue of pain perception by observing their patients' random accident-caused lesions in the London Hospital where Henry Head had been a medical registrar since 1896, they figured out that working with patients held two major disadvantages. As they put it: "It became obvious, that in order that we might examine more exhaustively the sensory condition of parts that had been robbed of their nerve supply, it was necessary that the patient should be a trained observer, and the injury determined beforehand."[4] These two thoughts, which motivated Head to do serious harm to his own body, describe two preconditions that make for a scientific experiment: First the object under investigation has to be defined theoretically and materially as precisely as possible. Second, the researcher should be a trained observer. In this case not only trained with regard to external scientific objects but with regard to his internal perceptions. The experimenter had to be an attentive virtuoso of disciplined self-observation and self-control.

What strikes one as unusual in the case of Head's experiments on pain perception is the fact that the first step of the experiment was to destroy the material objects of research and make them, at least to some extent, inoperative. In order to find out about the operating modes of the peripheral nervous system, one of Head's arms was anaesthetized in two steps. First chemically, before the operation; then the operation itself, in which the parts of the nervous system under investigation were materially cut, resulted in a second, less complete anaesthetic state of the arm, which had become insensible to most stimuli. As soon as the wound had healed and started to scar, Head spent weekend sessions at St. John's College in Cambridge where the psychologist William Halse Rivers[5] mapped the areas of sensory loss and their gradual recovery by correlating quantities and qualities of administered stimuli with Head's conscious perceptions of them. Sensations and their subjective perception were investigated by pinching and pressing different areas of the arm, which slowly regained certain kinds of sensation, and by comparing Head's introspectively gained perceptions of these sometimes cruel attacks to his body with what he perceived when his healthy arm was stimulated in the same way. One of the observations Head described about the abnormal perceptions caused when his wounded arm was stimulated at very precise spots was that the position of the stimulated point could not be recognized and each stimulus caused a widespread, radiating sensation. He proposed to give the name "protopathic" to this form of sensation.

The second kind of sensation that generated information concerning more precise localization and quantitative discrimination of the administered stimuli, Head proposed to call "epicritic." What led him to discriminate between different parts that together build the peripheral nervous system was the fact that, in the injured arm, the thresholds of just perceptible differences had shifted in comparison to the healthy arm. It appeared to him that if epicritic sensation, responsible for the discrimination of slight differences, was absent, there appeared voids, discontinuities in perception. From the skin in the protopathic condition, pain was evoked suddenly and perceived

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[4] Ibid.

[5] William Halse Rivers had accompanied Haddon to the Torres Straits in 1898 for one of the first ethnographic fieldworks (experimenting on the islanders' sensitivity and perception). He was an experimental neurologist and psychologist who had studied with Binswanger in Jena and carried out research on fatigue with Kraepelin in Heidelberg. From 1897 on he "was in charge of both of England's experimental psychology courses: at University College London and in Cambridge." For further information, see: Simon Schaffer, *From Physics to Anthropology – and Back Again*. Cambridge: Prickley Pear Press, 1994.

vehemently, without the preliminary painless sensation of the point. The conclusion Head drew from these observations was that the usual psychophysical conception of intensity had to be readjusted, at any rate as far as painful sensation was concerned. Head claimed that there were other factors such as extensity that had to be taken into account when aiming for a precise map of nervous functions and their correlation with external stimuli.

The examination of perception thresholds could not be successfully conducted with purely quantitative means, however, as the pain intensity and the stimuli administered did not show a stable algorithmic relation. Head, using temperature as an example, put it as follows: "It would even seem as if sometimes a less cold object applied to a larger surface will cause a sensation more intensely cold than the stimulation of a single spot by an iced rod. [...] From these facts it follows that Weber's law or other expressions of exact quantitative relations between stimulus and sensation must undergo revision."[6]

In this first case of the work of Head, what was under investigation were materially traceable borders of the body and thresholds of intensities and extensities of perception. The objects thus generated were different types of sensation, pain perception and correlating nervous structures that had not been as precisely discriminated before. Head gained knowledge about *aisthesis* by inducing an anaesthetic state in one of his limbs. The just-perceptible differences were experimentalized through the destruction of the peripheral and cutaneous nervous system and the physiological and introspective observation of its gradual reconstruction. One could say that Head's experiment was a deconstruction of nervous structures that resulted in a more differentiated picture of the nervous system's concrete materiality and operating modes. At the same time, the acknowledgement that pain thresholds are unstable with regard to various individual factors led to a reformulation of psychophysical laws, which had presupposed a stable algorithm for determining the correlation between the magnitude of a stimulus and the perceived intensity.

The correlation between physiological sensation and its perception was not only of interest within physiology, but as mentioned above, it had been a topic dealt with by psychophysicists throughout the nineteenth century – the Weber-Fechner law was a famous equation that aimed at quantitatively defining this relationship. A precise definition of the nature of reciprocal actions between psychic states of consciousness and physiological processes also lay at the basis of William James's experimental psychology. He soon realized, however, that even if it was possible to measure the interactions between the two exactly, psychology would always fail to explain consciousness itself and its idiosyncrasies such as the ability to spontaneously intend something based on free will. As a result, James turned towards a radically empirical metaphysics that he hoped would enable him to tackle questions and find answers at a different level.

Within his pluralistic philosophy, James then defined consciousness as a non-entity, a function that could only be grasped diachronically and through a process of experience. For him, consciousness and experience only existed in time and change, as streams. A clear-cut dichotomy between subject and object therefore appeared to him as an artificial construction. It is this view that makes James's fascination with Benjamin Paul Blood's philosophical project understandable.

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[6] Henry Head, "A Human Experiment in Nerve Division," *Brain* III (1908), pp. 323-450, p. 428.

Benjamin Paul Blood: From an aching tooth to philosophy

For Benjamin Paul Blood, who inhaled excessive amounts of laughing gas and ether over a long period of time, the psychophysical parallelism never played a role; from the beginning, what he experienced under the influence of laughing gas transcended the realm of quantitative correlations and aimed rather at transcendental ones.

Blood's daughter wrote in an undated letter about her father's first encounter with laughing gas: "I think his first experience with nitrous oxyde was an accident. He was having some dental work done; although in those days one generally had to grin and bear things of that kind. After that he tried it several times."[7] In Blood's case the medical context only served as an initiation, as it was responsible for his first encounter with the anaesthetic substance in 1860. Impressed by the revealing qualities he experienced during the process of recovering from the substance's effects, Blood started a long series of self-experiments with ether and laughing gas.

It was fourteen years later, in 1874, that Blood first wrote about his extremely aesthetic anaesthetic experiences and distributed a pamphlet with the title: *The Anaesthetic Revelation and The Gist of Philosophy*.[8] One of the lucky recipients, and at the same time one of the few who seems to have appreciated Blood's text, was William James, who not only reviewed Blood's book but was inspired when reading it to conduct some experiments with laughing gas on himself, which culminated in his claim that "Hegel was right after all and all my intellectual convictions hitherto were wrong."[9]

Robert Walt Marks who wrote the only monograph on Blood's philosophy in the 1950s argued that "it might be their common concern about the relation of appearance and reality which drew Blood and James so closely together."[10]

Similarly to Head, Blood investigated *aisthesis* through its contrary. Blood, like Head, created extraordinary states of sensibility, or better, insensibility in order to find out inductively something about the regular operating modes of conscious perception. While Head's experiments required disciplined self-control, Blood's experiments required its temporary loss. In the case of Head's pain experiments, however, a subjective perception only played a role as a supplement to physiological measurements, whereas in Blood's case the experiment's object turned out to be nothing but the subject itself; consciousness thus was destroyed and reconstructed in his experiments.

The major obstacle to Blood's illuminative explosions of knowledge or anaesthetic revelations, however, turned out to be the fact that they were lost constituently: the narcotic substances administered by inhalation not only caused asphyxia and lowered sensibility, but they also deactivated the memory. The contents of the revelations could only be remembered when back under the influence, whereas in a sober state there remained nothing but a vague idea of an alien knowledge. The anaesthetic experience was an instrument that should have enabled an

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[7] Robert Walt Marks, *The Philosophic Faith of Benjamin Paul Blood: A Study of the Thought and Times of an American Mystic,* Ann Arbor: Umi Dissertation Services, 1953, p. 106.

[8] See footnote 2.

[9] William James, "On Some Hegelism," in: *The Will to Believe and Other Essays in Popular Philosophy*, New York: Dover Publications, 1956 (1897), p. 294f.

[10] Marks, *The Philosophic Faith of Benjamin Paul Blood: A Study of the Thought and Times of an American Mystic*, p. 76.

undisguised look at the production process of consciousness through an unfocused floating and inebriated perception and thus answered questions about the human condition. The problem remained that what was clearly revealed at the thresholds of inebriation was concealed as soon as conscious awareness reappeared.

The revelations, Blood concluded, belonged to a symbolic system incompatible with the system of language organized by dichotomies. As it was impossible to delineate the experiences the way it was possible introspectively to discriminate a certain type of pain perception from another one, Blood had to refer to philosophical traditions and by discussing them define his own position and find a language that would enable him to communicate the abstractions of what he had experienced in an adequate way. He wrote: "But the substance here alleged, although accessible to even vulgar empiricism, can hardly be either critically entertained or thankfully received without some appreciation of philosophy."[11] At the basis of philosophical thought for Blood lay the question of how to designate the relation between being and knowing, the self and the other, referring mainly to Fichte's theory of consciousness and Hegel's dialectics. One could thus describe Blood's experiences of "the instant of recall from anaesthetic stupor to sensible observation, or 'coming to' in which the genius of being"[12] was revealed to him as a chemically induced and empirically perceived process of Hegelian dialectics in which knowledge about consciousness as its synthesis resulted from a double negation: self-negation and the negation of the exterior objective world in favor of an experienceable metaphysical one.

The object of Blood's research was not only gained subjectively, but the subject and the method of introspection were themselves at stake for him as they were for James. The radical empiricism of the two men made them question relations that were otherwise taken for granted, such as the clear-cut discrimination of subject and object. Blood concluded that all objects claimed as external are at least partly as we are, and "we are at least partly as they are. Empirically each of us as one is a subject-object, although rationally this is impossible."[13]

The idea of overcoming the subject-object dichotomy is a genuinely aesthetic one as it reflects upon the possibilities of gaining a kind of access to things that transcends a relation of being opposed to them and objectifying them. Blood's book can therefore be read as an aesthetic theory more than anything else.

Conclusion

In the history of philosophy, pain has often been referred to as a paradigmatically subjective state that could reveal knowledge about self-relation and consciousness. Hegel claimed that vital objects, in contrast to inanimate objects, had the privilege of pain and that the principles of pain were negativity and subjectivity.[14] As a component in the dialectics of life, pain for Hegel was thus constitutive of the "nature of mind" (*die Natur des Geistes*). For Schopenhauer,[15] the extreme

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[11] Blood, *The Anaesthetic Revelation and the Gist of Philosophy*, p. 3.

[12] William James, "On Some Hegelism," p. 294f.

[13] Ibid., p. 24.

[14] See G.W.F. Hegel, *Wissenschaft Der Logik II, Teil Zwei: Die Subjektive Logik*, Frankfurt am Main: Suhrkamp, 1978.

[15] See Arthur Schopenhauer, *Die Welt Als Wille Und Vorstellung*, Vol. I-IV, Köln: Könemann, 1997.

sensibility to pain was the privilege of genius and the precondition of any creative action as the anaesthetic experience was for Blood.

Analogous to pain, anaesthesia raised interesting issues concerning the accessibility of feelings. The difficulty of deciding whether ether produced true anaesthesia or merely amnesia stimulated a revival of interest in the question of whether it is ever possible to tell what someone else is “really” feeling, the question of intersubjectivity. It was due to this virtual inaccessibility of pain, as well as of anaesthetic states, that both pain and anaesthesia required introspective experimentation.

In the case of Henry Head, introspection served as an additional device that made it possible to correlate quantifiable, material information with its conscious perception. Self-consciousness in this case was presupposed and not itself under investigation. Blood’s case was somehow the reverse: Here the anaesthetic drug as a scientific instrument had a severe influence on consciousness itself, which was at stake in his solipsistic experiments as well as in his contribution to philosophy. What was gained at the thresholds of the anaesthetic states, Blood suggested to be the illumination “that sanity is not the basic quality of intelligence, but is a mere condition which is variable.” Both experimenters explored in-between states in which subject and object as an indivisible unity processed flexible knowledge.

What I have not mentioned until now is that Head also drew an evolutionary conclusion from his experiment, claiming that the protopathic nervous system belonged to an earlier stage than the epicritic one. The process of recovery in his arm he saw as a model for evolution. Referring to the discussions we have had during this conference about the significance of time organization in experimental systems, one could speak of an experiment that speeded up time, while functioning as a real-time experiment with respect to the individual under investigation. Blood’s philosophical account, in contrast, was based on *a posteriori* reflections upon timeless experiences.

Both Head and Blood pleaded on different levels for a pluralistic approach to their respective animate objects of research. Blood put it as follows: “Life is an unending process; and if it yields to a logic, this logic must necessarily be exceeding and unfinished. It must be a ‘science of the fleeting’”[16] and one could add: the logic of life and various experimental approaches to grasp some of it’s operating modes invented fleeting experimental shapes that crossed the borders between scientific, artistic and philosophical practices and theories.

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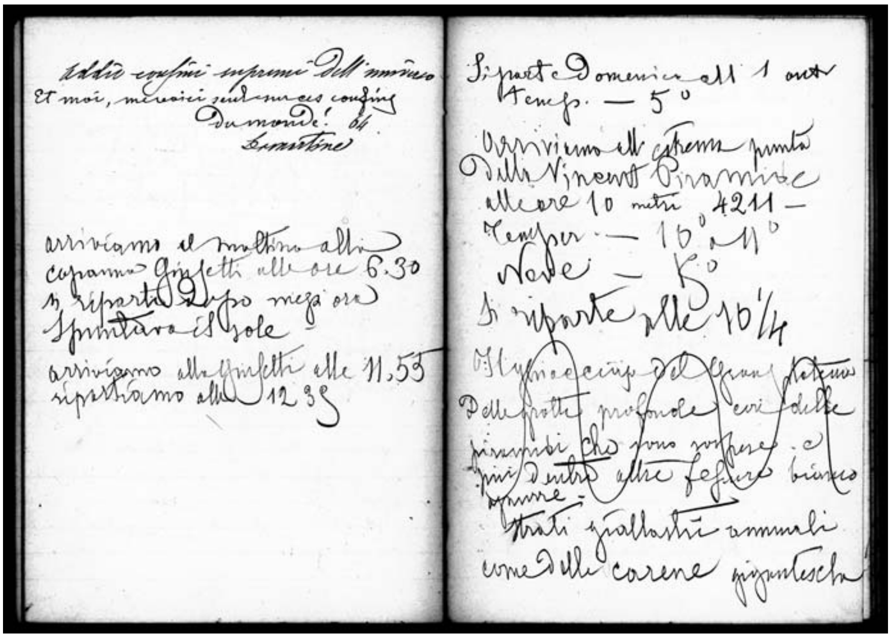
[16] Blood, *The Anaesthetic Revelation and the Gist of Philosophy*, p. 26.

Cultures of Speechlessness
Scrambles amongst the Alps, 1800-1900[1]

Philipp Felsch

I would like to call your attention to a diary page (fig. 1). The author is the Italian physiologist Angelo Mosso, who acquired international fame at the end of the 19th century for his pioneering studies on human fatigue. After climbing Monte Rosa, the highest peak of the Italian Alps, in February 1885, Mosso copied poetry into his notebook, a line of the famous French romantic Alphonse Lamartine that reads in English: "And I, here I am alone at the frontier of the world." The quotation reappears in the printed account of the tour that Mosso published in the same year: "I could *neither express* the emotion I felt, nor the magic of the view. One feels that we have reached the outer frontier of the world." On the summit itself though, the physiologist prosaically confined himself to note down time, height and temperature, before he left the spot after fifteen icy minutes. In a later publication he explained the conspicuous tremor of his handwriting: "I was very tired."[2]



Addio confini supremi dell'universo
Et moi, me voici seul sur ces confins
du monde! [illegible]
Lamartine

arriviamo al mattino alla
capanna Gnifetti alle ore 6.30
si riparte dopo mezz'ora
spuntava il sole
arriviamo alla Gnifetti alle 11.55
ripartiamo alle 12 35

Si parte Domenica all 1 ant
Temp. — 5°
Arriviamo all'estrema punta
della Vincent Piramide
alle ore 10 metri 4211 —
Temper. — 10° a 11°
Neve — 5°
Si riparte alle 10 1/4
[illegible] del Gran [illegible]
Delle grotte profonde cui delle
[illegible] che sono sospese e
più dentro altre fessure bianche
[illegible]
Strati giallastri annuali
come delle corone gigantesche

Fig. 1: Angelo Mosso's Diary, 1885. Lamartine on the upper left side, the summit scribblings on the right side (from: Biblioteca Angelo Mosso. Dipartimento di Neuroscienze dell'Università di Torino).

My aim is to display the two kinds of written speechlessness that concur on Mosso's pages: One – on the left – that links a sublime experience to poetry; the other – on the right – that links physical

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[1] An extended version with complete references has appeared in *Nach Feierabend. Zürcher Jahrbuch für Wissensgeschichte* 1 (2005), pp.15-32.

[2] All quotes from Angelo Mosso, *Una Ascensione d'inverno al Monte Rosa*, Milano 1885, 90f. and idem, *Der Mensch auf den Hochalpen*, Leipzig 1899, 34.

exhaustion to a shaky handwriting. I argue that they represent two different cultures, two different ways of traveling, representing and knowing about the Alps, that have followed each other in the 19th century: one, around 1800, that centered around the aesthetic category of the sublime and linked visual perception to the search for images and words; the other, beginning in the 1860s, that centered around the physiological category of fatigue and linked locomotion to indices in handwriting and mechanical curves. Eventually, the alpine studies of Angelo Mosso and contemporary colleagues led to an experimental physiology of fatigue and altitude, preparing the ground for the scientific study of work and for aviation medicine that both emerged after the turn of the 20th century.

Demarcating a zone of indistinction between sublimity and fatigue, Mosso's alpine expeditions offer key material to study the transition from aesthetics to physiology, from romantic symbols to analog tracings of the mountain experience, or from the "wordless subjectivity" of picturesque travelers to the "wordless objectivity" of *fin de siècle* life scientists.[3] The exhaustion of symbolic conventions and the boom of traces and indices seems to be a recurring motive in the modern history of signs. The American art critic Rosalind Krauss has observed how the 20th century experts of symbolic signification, avant-gardes and structuralists, turned to traces and indices, when they felt the potential of conventional signs had been depleted: Marcel Duchamp invented the ready-made, Roland Barthes discovered the photographic message without a code.[4] The success of graphology around 1900 tells a similar story: Handwriting was stripped of its conventional power to signify and turned into an index of character. In the following, I will consider travel journals to see how the aesthetic conventions of sublimity were exhausted and how writing became a physiological trace.

1. Sublimity and its blind spot

Until well into the 18th century, Livius' ancient word of the *foeditas Alpium*, the ugliness of the Alps, was barely contested. When the German father of classicism Winckelmann had to cross the Saint Gotthard Pass in 1760 on his way to Italy, he shut the windows of his coach to avoid the ugly sight of rocks and snow. Fifty years later, the romantic Shelley wrote poetry beneath the slopes of Mont Blanc. In the meantime, the aesthetics of sublimity had conventionalized how dazzled beholders of mountains could overcome their confusion symbolically, i.e. by transforming their initial speechlessness into words and pictures. "Delightful terror" became the most prolific sentiment for works of art. Ever since, the sublime genre has produced paradoxical descriptions of speechlessness. Again, Lamartine may serve as a prominent example: "Oh! If I only had words, pictures, symbols to depict what I feel!" read the opening lines of his poem *Desire*. Following is exactly the assumed ineffable: a lengthy description of the alpine landscape that the poet contemplates.[5]

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[3] Cf. Lorraine Daston, *Wordless Objectivity*, Max-Planck-Institut für Wissenschaftsgeschichte, Preprint 1, Berlin 1994.

[4] Cf. Rosalind Krauss, "Notes on the Index, Part 1 & 2," in: idem: *The Originality of the Avant-Garde and Other Modernist Myths*, Cambridge, Mass. 1985, pp. 196-209, 210-219.

[5] Alphonse de Lamartine, *Oeuvres poétiques completes*, Paris 1963, pp. 385f.

It was Horace Bénédict de Saussure, the Genevan professor of natural history, who transcended the romantic mountain genre. Upon seeing the Mont Blanc for the first time, he promised a reward for anyone who would guide him to the summit. In 1887, 25 years later, when his dream was finally realized, Saussure could not help but stamp around the mountain top in anger. Far above the tree line, the sublime shudder had turned into a physical calamity. Staggering, short of breath, and heavily exhausted, the professor could hardly observe his instruments let alone enjoy the prospect from the summit.

What the 18th century philosophers of the sublime had avowed, that only detached beholders could enjoy a terrible mountain scene, became a painful certainty for Saussure and his followers. In the snowy heights, any attempt to master the challenging landscape symbolically was baffled by vertigo and somnolence. Take the example of John Auldjo, who accomplished the 14th ascent of Mont Blanc in 1827. At the summit, his romantic ambitions had thoroughly left him: "The mind was as exhausted as the body, and I turned with indifference from the view, and throwing myself on the snow, in a few seconds I was soundly buried in sleep." The aesthetics of the picturesque voyage, one could say, was followed by the anaesthetics of the mountain tour.[6]

Having barely begun, alpine exploration suffered from a blind spot. The reason for all exertions, i.e. the crucial moment on the overlooking summit, could hardly be contemplated or described, since vision and speech, attention and memory all showed heavy disturbances. On less elevated vantage points, initial vertigo was soon overcome by the new visual habits of the time – think of Goethe's panoramatic self-therapy on the tower of Strasbourg's cathedral. In the high Alps however, things went differently: As the suffering body denied aesthetic pleasures and scientific observations, the mountaineers' afflicted attention shifted slowly from the mountains to themselves. When Frederic Clissold set out for the tenth Mont-Blanc-ascent in 1822, he renounced all scientific instruments, because instead of physical measurements he intended a "psychological self-experiment: observing the properties of the soul when it is forced to release such new and noble energies." A new esteem for adventurous self-experiments took over where classical natural philosophy had reigned.[7]

The collapse of the picturesque voyage overtook painting as well. The rich iconography of mountain landscapes, that had been established around 1800, dissolved, as the pioneers of mountaineering left the valleys for the peaks. For obvious reasons, their contemporaries refused to count the few attempts of high mountain drawing as art. In 1862, the famous French critic Théophile Gauthier declared: "Art, as we know it, does not reach higher than vegetation."[8]

2. Indices of apathy

At the same time that Gauthier declared the end of art, physical troubles disappeared from the Alpinist literature. The British and continental middle-classes that flocked to the Alps since the 1860s did not engage in romantic self-experimentation any longer; in the mountains they

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[6] John Auldjo, *Narrative of an Ascent to the Summit of Mont Blanc, on the 8th and 9th August, 1827*, London 1828, p. 47. I thank Katrin Sohldju for "anaesthetics."

[7] Frederic Clissold, "Détails d'une ascension au sommet du mont Blanc," *Bibliothèque universelle de Genève*, 23, 1823, pp. 138 and 140.

[8] Théophile Gauthier, "Vues de Savoie et de Suisse," *Le Moniteur Universel*, 16.6.1862.

demonstrated their national, class, and gender identities, and held their tongues on the embarrassing effects of thin air and fatigue.

Denied by the Victorians, the handicap of mountaineering found a new home in the life sciences. Physiologists visited archives and libraries to examine the older literature of the amateurs, criticizing what they considered to be dubious observations and alleged facts. Beyond professional boundary work, their skepticism indeed touched a serious problem: How was a collapsing observer, with failing senses and speech, to observe his very own collapse?

The precarious and fleeting character of travel impressions has been discussed as early as the 18th century. A treatise like Franz Posselt's *Apodemics or the Art of Traveling*, a successful book of its genre from 1795, thus put much emphasis on writing techniques. By use of fountain-pen and portfolio – the latest achievements of travel equipment – events should be noted down, if possible, on the spot itself to avoid the uncertainties of subsequent memory.[9] Saussure and his alpine followers took this apodemic standard to heart. But contrary to gentler surroundings, taking notes in the mountains was not particularly pleasant. Saussure's own scribblings reveal how difficult it was to commit readable words to paper with trembling hands: Some of their passages have not been deciphered to this day.

In the course of the 19th century, such failures of signification have gained a specific surplus. The shaky characters, the white pages, and the unfinished journals of either victorious or failing explorers turned into autographs, whose tremor seemed to point directly their heroic origins. Apart from mountaineering, such documents originated from aeronautics and polar exploration. Take Gaston Tissandier, who published writing samples of his unfortunate companions when reporting of the tragic balloon accident in Paris in 1875. Or take Robert Scott's ill-famed journal of his fatal south pole expedition in 1912. Its shocking last sentences moved the Western world: "It seems a pity, but I do not think I can write more. Send this diary to my wife!" With dying forces and never ending precision, Scott had subsequently replaced "wife" by "widow."[10]

Physiologists intervened at the very same point: where the writing hand had begun to tremble. They were familiar with simultaneous writing techniques since the era of romantic self-trials, i.e. since experiments on pharmacological inebriation, vertigo, and optical delusions. But above all, the *graphic method* had appeared. New self-registering instruments allowed to translate physiological functions into analog curves, seemingly representing the language of nature itself. The French doyen of the graphic method, Étienne-Jules Marey, argued that it allowed physiologists to overcome the two main obstacles of their science, i.e. the imperfection of the senses and the imperfection of language.[11]

Since Saussure, modern mountaineers had lost both sight and speech. It thus makes perfect sense that in 1866, Marey's colleague and coworker Auguste Chauveau climbed Mont Blanc to trace his pulse and respiration curves during the ascent. Chauveau's subsequent account shows, first, how the epistemic functions of both language and the senses were delegated to analogue

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[9] Cf. Andreas Hartmann, "Reisen und Aufschreiben," in: *Reisekultur. Von der Pilgerfahrt zum modernen Tourismus*, ed. by Hermann Bausinger et al., München 1999, pp. 152-159.

[10] Cf. Robert Falcon Scott, *Letzte Fahrt. Kapitän Scotts Tagebuch. Tragödie am Südpol, 1910-1912*, Darmstadt 1997, pp. 300 and 312; Gaston Tissandier, "Le voyage a grande hauteur du ballon 'Le Zénith'," *La Nature* 3 (1875), p. 340.

[11] Étienne Jules Marey, *La méthode graphique dans les sciences expérimentales*, Paris 1878, p. i.

curves, and second, how the expedition journal itself was subdued to the new regime of the analog. In order to "keep the range for arbitrary [...] estimates as small as possible," Chauveau refrained from considering any personal impressions and relied wholly on the material evidence that his instruments and his writing hand had produced. Their diagnosis was clear enough: The curves showed heavy disturbances of pulse and respiration. Of particular significance was Chauveau's diary: As he had approached the summit, his handwritten notes had turned into unreadable scribbling, and finally expired completely. Far from deploring this breakdown of signification, the physiologist appraised his tremor "as an index of apathy," carrying physiological knowledge that the expert could read. Useless in its denotations, the travel journal began to speak as a physiological trace.[12]

Chauveau's expedition marks a significant transition point for the history of speechlessness in the Alps: After a period of unsettling calamities, the "wordless subjectivity" of romanticism eventually gave way to the "wordless objectivity" of analog recording. The silent language of curves replaced the stammering language of poetry, the conventional signs of travel journals turned into symptoms, and sublimity gave way to the new reference object fatigue. Even in the valley, sublime feelings could no longer be experienced but as stereotypes: "The sight of Mont Blanc threw Mr. Pontifex into conventional ecstasy," we read in Samuel Butler's late 19th century novel *The Way of all Flesh*. The picturesque voyage was clearly decaying.[13]

Following Chauveau, we see the emergence of an experimental culture in the Alps that aimed at the physiological hot spots of the *fin de siècle*, i.e. the turnover of food into work, the relation between nerves and environment, and, above all, fatigue. Angelo Mosso claimed the mountains as a laboratory landscape for muscular thermodynamics. A shrewd politician, he was able to involve the *Italian Alpine Club*, mountain-loving Queen Margherita, and the Italian military as generous allies. A disciple of Marey and author of alpine travel journals, Mosso translated exhaustion, vertigo, and somnolence into an overflowing archive of mechanical curves and handwriting. He constructed new recording instruments and collected the shaky summit scribblings of his fellow climbers from the *Italian Alpine Club* for thorough physiological analysis.

The method reached far into the emerging new branches of physiology, i.e. into the science of work and aviation medicine: Mosso's coworker Zaccharia Treves extended writing analysis to manual labor. The Berlin physiologist Nathan Zuntz examined the journals of Prussian aeronauts.[14] In 1936, the Hamburg aviation physician Lottig proposed a new aptitude test for German air-force-pilots. In the "writing test," subjects had to undergo an artificial ascent in the pressure chamber while jotting down numbers and words (fig. 2). Lottig claimed that writing was best suited to reveal the psychic and physical changes during the ascent, allowing for a reliable physiological diagnosis. At the bottom of this page we see how the subject, at an artificial height of 7000 m, finally lost his words.[15] "If I only had words, pictures, symbols to depict what I feel!"

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[12] All quotes from Auguste Chauveau, "Le mal de montagne," *Revue scientifique* 12 (1894), pp. 353-362.

[13] Quoted from Marjorie Hope Nicolson, *Mountain gloom and mountain glory. The development of the aesthetics of the infinite*, Seattle & London 1997, p. 373.

[14] Cf. Zaccharia Treves, "Sur les lois du travail musculaire," *Laboratoire de Physiologie de l'Université de Turin. Travaux des années 1896-1899*, Turin 1899, pp. 131-164; Nathan Zuntz et al., *Höhenklima und Bergwanderungen in ihrer Wirkung auf den Menschen*, Berlin 1905, p. 449.

[15] H. Lottig, "Zur Vereinheitlichung des Schreibversuchs bei der Höhentauglichkeitsprüfung," *Luftfahrtmedizin*, 1, 1936, pp. 15f.

Lamartine, the romantic, had uttered a century earlier with reference to himself on a Swiss mountain top. The written speechlessness in the pressure chamber was altogether different. Instead of symbolizing sublimity, it indicated fatigue. Writing, the old medium of romantic impressions, had become a physiological trace.

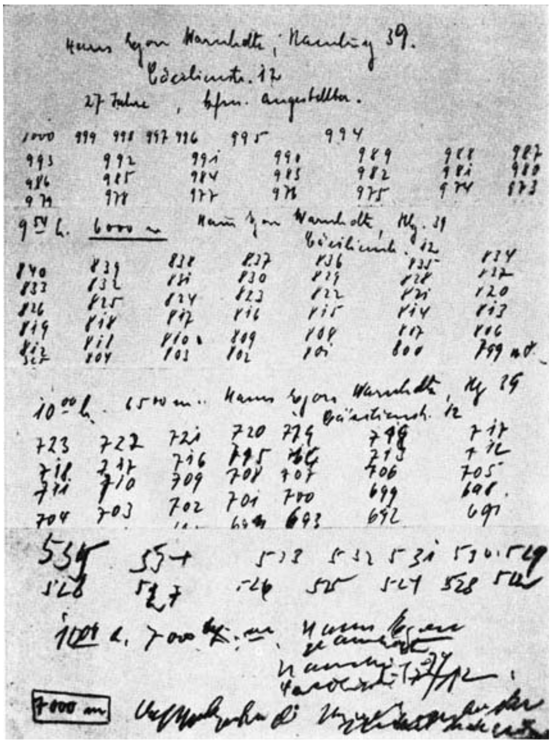

Fig. 2: Writing test, 1936 (from: Lottig, H.: "Zur Vereinheitlichung des Schreibversuchs bei der Höhentauglichkeitsprüfung," in: *Luftfahrtmedizin,* 1, 1936, 17.)

Experimental Life

Mark B. N. Hansen

In their recent book, *L'Art numérique*, Edmond Couchot and Norbert Hillaire make the effort to think new media within the larger history of technology. "Informatics and its formalized models," they argue, "truly mark a new state of technics. A hybrid of hardware and of software, informatics is a technology in the literal sense of these terms, a fusion of *techne* and of *logos*."[1] Understood in this way – as a conjunction of hands-on know-how and discursive knowledge – 'informatics' can easily be situated within the trajectory of postwar cybernetics, a movement which, after all, was inspired by the dream of modelling the machine on the human being. The salient question in both cases is how to think the human and the technical in their commonality. Yet, informatics as here defined marks something of a reversal in the directionality of the initial cybernetic project. Arguing that the activity of simulation defines the digital image in all its forms, Couchot and Hillaire claim that the task of modeling the machine on the human now gives way to that of (re)introducing the human – which is to say, human embodiment – back into the simulational circuit.

Where a first-generation cybernetician like Norbert Wiener could only ward off the threat of technical contamination by dogmatically proclaiming the human to be inviolate, Wiener's contemporary legatees – a diverse group encompassing researchers in fields ranging from Artificial Life to robotics, cognitive psychology to neurobiology – celebrate and draw inspiration from the mutual interpenetration of the human and the technical. For these researchers, there is nothing to fear in the convergence of the human and the machinic: the human has always been technical, it has from the beginning evolved through means other than life, and we not only need not fear technical contamination, but we can and must invest it as a crucial dimension of our on-going evolution, of what must henceforth be called our 'human technogenesis.'

In light of this conclusion, the role of art undergoes a fundamental mutation: *new media art functions by stimulating the mutual interpenetration of the human and the technical*. New media art comprises a crucial domain for experimentation with the conditions of embodied human life in the contemporary technosphere. New media art parts company with its processural forebears of the 1960s and 70s to the precise extent that it sheds their narrowly aesthetic aim – to operate a critical deconstruction of the discourse of Western art – in favor of a broadly experiential one – to trigger new sensations and forms of life. This very inseparability of technology and life is what gives the lie to the all-too-popular notion of 'immateriality' as a way of describing new media art: digital technologies massively infiltrate materialized objects and spaces, and even the territorialized spaces of our environment. We literally live in technology, and no opposition between presence and telepresence can be rigorously maintained.

This dissolution of the distinction between technics and life informs Couchot and Hillaire's fruitful concept of second-order interactivity:

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[1] Edmond Couchot and Norbert Hillaire, *L'Art numérique: comment la technologie vient au monde de l'art*, Paris: Flammarion, 2003, p. 27.

> A single principle lies at the basis of neural networks and genetic algorithms: that of an interactivity of a high level of complexity between elements constitutive of life or artificial intelligence (genes and neurons) that, on account of their configuration, interact to produce emergent phenomena. Mirroring the evolution of cybernetics, interactivity has attained a higher stage of complexity and autonomy. Whereas the 'first cybernetics' focused its inquiry on notions of information, control and communication (in animals and in machines), the second cybernetics focuses rather on notions of self-organization, emergent structures, networks, adapation and evolution. In an analogous way, whereas the first interactivity understood human-computer interactions on a stimulus-response or action-reaction model, the second interactivity concerns itself with action insofar as it is guided by perception (what has been called *enaction*), with corporeity and sensorimotor processes, and with autonomy (or more precisely *autopoiesis*).[2]

What is most crucial in this shift to second-order interactivity is the prominence it accords human embodiment. Not only does the unidirectional simulational project of first-generation cybernetics give way to a functional program rooted in human-machine co-operation, but the salient principles informing the latter turn out to be the very ones that govern biological emergence. It is as if these principles were imported into human-machine interactivity in order to catalyze emergence on a dual basis, the emergence of new human behaviors *and* of new machinic processes.

Thus, while both human and machine find their respective cognitive-perceptual agency expanded through co-functioning with the other, neither yields up its constitutive autonomy, which is equally to say that neither allows the influence of the other to enter into its operationality. Coupled in second-order interactive systems, both human and machine respond to the other's influence by undergoing what can be loosely termed a 'self-(re)organization' on the basis of distinct operational rules internal to them. We can thus characterize second-order interactivity as a dynamic system comprised of two coupled, yet separately evolving agents. The human uses the machinic to destabilize its functioning, thereby opening itself to new emergent experiences, while the machinic does or is made to do something similar, opening itself in its turn to new emergent processes.

Bearing in mind this concept of second-order interactivity, we can now specify what is signified by the phrase 'information arts' and by the project of conceiving art as experiment in our contemporary technosphere: whereas both human and machine 'components' are essential for the continued dynamical evolution of any second-order interactive system, *information art distinguishes itself from informational technics per se because and insofar as it foregrounds and exploits the human side of second-order interactivity.* Interactive art systems focus on deploying the technical expansion of embodied enaction in order to destabilize habitual cognitive-perceptual patterns of human beings. In this process, the principles of embodied enaction remain primary, even as they undergo abstraction: in information art the margin of abstraction or of flexibility vis à vis biological emergence is ultimately in the service of the human and is never wholly detached from human embodiment. Within interactive systems created by new media artists, machinic emergence functions primarily to catalyze the abstraction of embodied enaction that serves as the trigger for human agents to tap into their constitutive, but latent, flexibility, their evolutionarily-

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[2] Ibid., p. 99.

generated, organismic potentiality. And if the open-ended, mutually-recursive interactivity of today's human-machine systems differs markedly from early interactive systems, this is due primarily to the crucial role played by the computer, and more precisely, to the capacity the computer opens for machinic emergence, for the machinic dimension to evolve dynamically, in ways not preprogrammed, but rather generated through the computer's own 'creative' response to unexpected inputs. It is, therefore, only on account of machinic emergence that information art makes a contribution to human technogenesis: specifically, it deploys the new dynamic processes of machinic emergence in order to stimulate the evolution – understood as the actualizing of potentiality – of embodied human beings.

My recent work has focused on the role of hetero-affection as a necessary corollary of self-affection. Put schematically, my argument is that new media technologies do not simply expand our external memories – as most conceptualizations of the archive would have it. Far more profoundly, they intervene in and modify our primary capacity to produce the present (and the future) and, in so doing, they assert the irreducible role of hetero-affection, of affection by alterity – which is above all to say, by the body lived as a first source of delay, of distance from self, of temporal alienation.

For the current discussion, what is most pertinent in this understanding is the crucial role played by *indirection*. Understood as an alternative to causal coupling, indirection defines the coupling of two (or more) systems that do in fact co-evolve, but in a way that maintains their respective operational closure or autonomy. As I see it, indirection in this sense forms a necessary correlate of hetero-affection: for if technics functions in today's informational art systems to open embodied life to alien rhythms of an increasingly technologized environment, it can only do so in a non-reductive way if the operational specifity of embodied enaction can be preserved. Indirection is, simply, the means for such preservation, and as a governing principle for my understanding of contemporary human-machinic coupling, indirection forms a distinct alternative to the concept of digital convergence theorized by Friedrich Kittler.

To flesh out the crucial and complex operation of indirection, I shall focus on one key site of contemporary information art practice, a recent work by Dutch architect Lars Spuybroek entitled *Son-O-House*. In order to set the context for an appreciation of this work, let me first briefly present an example of contemporary informational arts – the photographic work of German artist Thomas Demand – where indirection is deployed as a materially consequential practice or technique. What is crucial in this example is that material transformation is built in to the *process* through which a work of art – in this case, a photograph – gets produced. This intrinsic material transformation introduces an input that is 'exterior' to the intentional artistic process and, to a greater of lesser extent, outside the control of the artist, even though it has been set up by the artist.

Like other photographers of his generation, Demand seeks to situate photography within the social and cultural contexts in which it appears in our world today. Accordingly, Demand's work typically begins with his discovery or selection of an image that either appeared in the mass media or that has otherwise become archetypal for our collective cultural memory. Demand then proceeds to construct a life-size model out of paper that reproduces the image. He photographs the model and then destroys it, such that the photograph forms the sole evidence of the 'reality' it documents, whether we understand this to be the paper model itself or the "photographic referent" of which it is a representation, to deploy Roland Barthes's famous term.[3] Clearly, Demand's procedure is calculated to challenge and obscure the traditional indexical vocation of photography. As Roxana Marcoci puts it in her catalog essay, "Paper Moon," Demand's "handcrafted facsimiles of architectural spaces, exteriors, and natural environments are built in the image of other images. Thus his photographs are triply removed from the scenes or objects they depict."[4] Indeed, his photographs pose the quandary of just what their referent is: is it the paper model that is the object sending itself to the future through the material inscription of light, or is it the "original" object that is mediated by the paper model?; or is it somehow both, ... or neither? That such a quandary remains undecideable goes far toward accounting for the appeal of Demand's photography: here we have a truly digital-age-inspired interrogation of photographic indexicality whose vehicle is, however, decidedly low-tech – the construction of a model out of paper. Yet, the mediation performed by the paper model does not simply reproduce the original photographic referent. Rather, something important happens in the conversion to paper that is imposed as a necessary stage in Demand's vision of what photography is today.

Though it is difficult to say just what this something is, it certainly involves an abstraction that functions simultaneously to deprivilege indexicality and to liberate some less image-centered form of collective memory. Marcoci speaks to this point by contrasting Demand's practice with that of Vic Muniz, a photographer who deploys materials ranging form chocolate to ash as the content of his iconic photographs, of a sort of indexicality by other means. Discussing Demand's *Barn* (fig. 1, see appendix), a sanitized rendering of a famous Hans Namuth photograph of Jackson Pollack's studio, Marcoci notes that,

> in contrast [to Muniz], by blotting out any traces of paint cans, brushes, sticks, paint spatters, and cigarette butts on the floor, Demand's image has just the opposite effect [from that of conveying the ethos of Pollack's creative process]. It estranges the space of Pollack's presence, allowing it to retain a stripped-down, bare-bones aspect, evidenced only by light streaming in through the windows and in between the barn's wooden slats. Deliberately understated, *Barn* alludes to all the images that turned Pollack into an American cult figure while deemphasizing their iconic quality.[5]

A similar effect is achieved in countless other works by Demand, including *Zimmer* (1996), a photograph of a paper model of the New York hotel room where L. Ron Hubbard wrote *Dianetics* (fig. 2); *Archive* (1995), a photograph of a paper model of a room storing film canisters of Leni Riefenstahl movies (fig. 3); and, perhaps most strikingly, *Room*, a photograph of a paper model depicting Hitler's headquarters in Rastenburg, East Prussia after it was bombed by a member of the German resistance (fig. 4). In all of these cases, Demand's interest is clearly directed toward probing the "sociological function of specific architectures that shape individuals,"[6] and indeed, the abstraction achieved through the conversion of the iconic photographic referent into a paper

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[3] Roland Barthes, *Camera Lucida: Reflections on Photography*, translated by Richard Howard, New York: Hill & Wang, 1981.

[4] Roxana Marcoci, "Paper Moon," in idem (ed.): *Thomas Demand*, New York: Museum of Modern Art, 2005, pp. 9-10.

[5] Ibid., p. 19.

[6] Ibid., p. 17.

model serves precisely to uncover this dimension *beneath* the surface of the image. Such an uncovering is necessary in an age where "things enter reality through photographs" rather than firsthand, as Demand himself puts it;[7] the superficial lure of the visual icon must somehow be disturbed so that the collective resonance of the event can be grasped and mediated.

Indeed, one of the key effects of Demand's technique of abstraction is to reorient the temporal valence of photography. Thus, rather than focus on storage and questions concerning fidelity to historical truth, Demand's photographs seek to integrate photography into a form of historically-impacted thinking of the future. Marcoci sees this inversion of the vocation of photography as a response to the media saturation of our contemporary culture:

> In an era when even the most sensationalist stories have only a brief shelf life, Demand's photographs [...] offer a paradigm for memory premised on a new way of looking at what is presented to us as the 'truth.' His images of paper constructions set out not so much to determine what truth was in a historical sense (since that context is irretrievably lost) but to put it to work again.[8]

A perfect example of this temporal reorientation comes by way of *Staircase* (1995) a model of an image of the stairwell from Demand's secondary art school which, by way of juxtaposing the Nazi vilification of the Bauhaus and the rehabilitation of its modernist principles during the postwar reconstruction, poses questions to us concerning how we've gotten to our present and where we're going from here (fig. 5). Interestingly, the image likewise subordinates the personal dimension of memory – i.e., Demand's memory of his school, the distortions of which are preserved in 'errors' of the paper model (rectilinear form of the staircase rather than curvature, etc.) – to the collective dimension which it channels. In this respect, the image testifies to the way that Demand's work bypasses or voids the indexical function of photography in order to go straight to its content – namely the transmission and reactivation of memory. Moreover, the image exposes Demand's conception of photographic individuation as a deeply materially-impacted process. In direct contrast to the serialist practice favored by many (if not most) of his artist compatriots (the generation trained by Bernd and Hilla Becher, a generation that includes Andreas Gursky, Thomas Struth, Thomas Ruff and Candida Höfer), where the technical individuation linked to the simple act of taking a picture is, in effect, dissolved, Demand reasserts the importance of individuation, suggesting through his elaborate compositional practice that it cannot be separated from some process of material transformation.

More than just a new stage or medium in the history of art, new media art is, ultimately, the medium for experimentation – properly philosophical experimentation – with the contemporary conditions of human subjectivity. Nowhere is the convergence of aesthetic and super-aesthetic dimensions of human embodiment more compellingly set into play than in (Dutch architecture firm) NOX's project *Son-O-House* (fig. 6). Situated in a large industrial park along a highway that is home to new media and IT industries in the Netherlands, the project is specifically intended as a vehicle for 'strengthening the identity of the area.' As the project description explains, *Son-O-House* expressly eschews the narrow industrial aim of making a 'technological statement' in favor

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[7] Cited in: Ibid., p. 9.

[8] Ibid., p. 26.

of a cultural and aesthetic aim, creating a 'social space' for informal gatherings and relaxation. It seeks to facilitate an interruption of the technical rhythm driving the IT industry together with a correlative deepening of the aesthetic dimensions of temporal experience. To this end, it combines architectural and sound elements, in a way that allows visitors to hear sound within a specific structure but also to participate in the composition of that sound.

Less a "real house" than a "house where sounds live," "a structure that refers to living and the bodily movements that accompany habit and habitation," *Son-O-House* attains its aesthetic dimension – its status as a work-as-process in the above sense – as a function of the challenge it poses to the structure of information flow germane to the new media industry.[9] Far from mirroring the direct realtime immediacy and one-directional movement of information exchange that comprises the core of IT, the project introduces a margin of indirection that, as we shall see, refocuses information flow on and around human embodiment. More precisely still, *Son-O-House* transfers the site of media transmission and reception from the technical circuit to embodied movement: rather than facilitating an informational exchange between machine and human, the project privileges as selectional criteria for the creation of information the very operational rules that assure the autonomy of embodied enaction.

While this transfer occurs at a literal, surface level – the space is expressly intended to be a retreat from work – far more important are the ways in which both the project's construction and its effects tap into and stimulate the further development of the principles underlying embodied enaction. We could sum up this investment by saying that the project everywhere deploys as its organizing principle human indirection (the transformational processing of stimuli through the autonomous system of embodied enaction) rather than digital convergence (the fusion of separate media, potentially including the body).

Son-O-House is literally constructed on the basis of indirection. Taking as an initial dataset recordings of movements of actual human bodies in a domestic space (fig. 7), the architects painstakingly transcribe movement, broken down into three categories referencing scale (whole body movements, movements of limbs, movements of hands and feet), into a paper structure (fig. 8). As the project description explains, the structure of *Son-O-House* is "derived from typical action-landscapes that develop in a house: a fabric of larger-scale bodily movements in a corridor or room, together with smaller-scale movements around a sink or a drawer."[10] Bodily movements correspond to uncut areas of paper; limb movements to first cuts of the paper; and hand/feet movements to finer cuts. Following cutting, the preformed paper bands are stapled together "at the point where they have the most connective potential."[11] Curvature emerges, yielding "an arabesque of complex intertwining lines that is both a reading of movements on various bodily scales and a material structure."[12] In a final step, the lines of this paper model are swept sideways such that their open structure is joined to the closed surface of the ground. The result is a three-dimensional porous structure comprising what the architects refer to as an analog-computing model.

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[9] Lars Spuybroek, "Project Description" for *Son-O-House*, unpublished document, courtesy of the V_2 Institute.

[10] Ibid.

[11] Ibid.

[12] Ibid.

This analog computer becomes the preformed element for the sound installation and, in this process, both of its constitutive elements – its transformation of the initial human bodily movements and its unique materiality as a paper model – play a crucial role. To create the installation, the paper model is digitized and remodelled, such that the potentiality overlayed in the intersections of cuts registering the three scales of movement in the paper model undergoes an actualization that will subsequently serve (in conjunction with the movement of the visitor and the interference of sound elements) as a new source of potentiality. At the same time as it actualizes the potentiality of the paper model, the shift to the digital carries out an abstraction of the constitutive organization of the analog computer. Itself a recapitulation – with crucial material differences – of the abstraction of human movement realized through its transformation into the paper model, this abstraction allows the transformation of the latter's organization into a new material domain (the digital) and, at the same time, introduces into the digital an important source of constraint. Precisely because of its recapitulated origin, the constraint hereby transferred to the digital marks, at the remove of one layer of abstraction, the constraint associated with the principles governing embodied enaction.

Most significant here is the role played by the intermediary of the paper analog-computing model. It is only because this intermediary retains the constraints of embodied movement in an abstracted, but nonetheless materially-specific form (paper) that the subsequent cofunctioning of human and machinic in the sound installation can be truly creative. We can grasp why if we contrast *Son-O-House* with a scenario in which the human and the digital are directly coupled with one another. In such a scenario, the sole source of interaction would be the mapping of movements and processes from one onto the other, which means that it could only mobilize both human and digital as static, entirely actualized or preformed elements.

Now it is precisely to break with this paradigm – the paradigm that prevails throughout the information industry and on the global scale in industrialized consciousness – that NOX invests in analog computing as a machine for generating indirection. Analog computing, explains Lars Spuybroek, finds an exemplary instance in the 'optimized path systems' or 'material machines' developed in the 1990s by Frei Otto at the Institute for Lightweight Structures in Stuttgart, Germany. Material machines invest in the specificity of material as an agent of autonomy, such that "through numerous interactions among its elements over a certain time span, the machine restructures or 'finds (a) form.' Most material machines consist of materials that process forces by transformation."[13] The capacity of such materials to function as 'agents' depends on the two correlatives of autonomy, interactional flexibility and constraint, as Spuybroek recognizes: it is essential, he insists, that material machines have "a certain amount of freedom to act" and also that this freedom be "limited to a certain degree set by the structure of the machine itself."[14]

In deploying analog computing as a machine for generating indirection in *Son-O-House*, Spuybroek and his colleagues retool the concept of the material machine, expanding the process of self-organization via material specificity to encompass a dynamic, materially-heterogeneous environment. In the project, the material machine constituted by the paper model undergoes a

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[13] Lars Spuybroek, "The Structure of Vagueness," in idem: *NOX. Machining Architecture*, London: Thames & Hudson, 2004.

[14] Ibid.

double de-specification. On the one hand, its proper materiality is made to bear the traces of another materiality: it abstracts but preserves, preserves by abstracting, the initial set of recorded bodily movements. On the other hand, its potentiality as a concrete material – the force it overlays at points of intersection – is made dependent on an external trigger, namely, actualization through digital transformation.[15]

The payoff of this double de-specification of the material machine/analog computer/paper model comes by way of the recapitulation of the process of construction in the interactivity afforded by the sound-space installation. As I suggested above, the interactive component of *Son-O-House* introduces a second source of indirection which is, not surprisingly, correlated with the indirection afforded by the analog computing model. At strategic spots within the built structure are positioned 24 sensors that relay information concerning visitor movement to a computer responsible for generating sound in the environment (fig. 9). The sound generated is, in turn, projected into the space via 20 loudspeakers grouped into five spatial groups each possessing its own range of frequencies (fig. 10). Each group or 'sound field' is programmed through a set of rules encouraging interference with the others, and the resulting effect of movement and transformation – the sense that the sound source is itself moving – has the further effect of triggering bodily movement on the part of the visitors. Captured by the sensors, the latter, in turn, becomes the trigger for further transformation of the sounds.

The interactive component of *Son-O-House* thus brings together body, sound, and space into a positive feedback system that creates two kinds of emergence: of new bodily movements and of new frequency interferences. And while both emergences – human and machinic respectively – are only possible through the perturbation introduced by the other, both occur solely through a reorganization that respects the principle of operational closure. While both follow the same basic rule – let movement create space – each does so in a manner entirely particular to it. This is equally to say that both retain the crucial investment in indirection, a point brought home in the description of the sound-generating component of the installation: "As a visitor one does not influence the sound directly, which is so often the case with interactive art. One influences the real-time composition itself that generates the sounds."[16] Just as the sounds themselves do not directly cause changes in bodily movement, but influence the internal processing that yields such changes, visitor movement impacts the composition of the sound, which is to say, the event of frequency interference itself. In this way, the 'autonomy' of the digital sound-generating (compositional)

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[15] In his essay on *Son-O-House*, "Notes on the Surfacing of Walls: NOX, Kiesler, Semper," in: Spuybroek, *NOX: Machining Architecture*, Andrew Benjamin explains, with specific reference to the architectural elements of the project, how this digital transformation releases the creative potential held by the paper model: "As the informed strips [of paper] are stapled together, they begin to form a complex arabesque, which has the potential to yield wall, floor, and corner relations. Those relations emerge out of the interconnection of the vaults implicit in the analog-computer model but which are only truly actualized once the model is digitized. In addition, the digitization of the models gives rise to further developments – ones with their own important consequences. Digitization allows, via a movement from surface to line, each of the vaulted sections its own discreet termination. In other words, surfaces reach their own termination in a line. This occurs because of the move from one form of modelling to another. The potential of paper is actualized through digital transformation."

[16] Lars Spuybroek, "Project Description" for *Son-O-House*, unpublished document, courtesy of the V_2 Institute.

system combines with the (distinct) autonomy of embodied enaction to support the complex interactivity produced by this work.

As I have suggested, the interactive indirection recapitulates the indirection that structures the construction process. In so doing, however, it brings the force of the latter – the double despecification of 'paper' – to bear on dynamic, real-time human-machine interactivity. This, finally, is how *Son-O-House* intervenes, critically and productively, in the contemporary media system: *it exposes the complex pre-programming that produces the industrialized paradigm for real-time interaction* (and that must be effaced to insure its smooth functioning). And, by way of the analog-computing paper model, it introduces a source of 'deferral' and 'delay' that facilitates *the reprogramming of interactivity* (by restoring its bidirectionality) and *the creation of new human sensations and new machinic processes.*

What makes this recapitulation most intriguing is the way it reverts to the project's initial engagement with bodily movement, only now in a manner that taps into the intensity of movement, rather than simply registering its recorded extensivity. Surveying the project as a whole, we can see how the potentiality of movement that is accumulated in the paper model qua analog computer only comes to fruition in the interactive sound installation, which is to say, at the point when movement becomes active and expressive. This means that analog computing is here accorded a distinctly provisional status: far from producing a definitive material transformation (as it does in Frei Otto's material machines), the paper model liberates a potentiality that emerges from the relations between distinct materialities. Despite its complex preparation in the construction process of the project, this potentiality becomes actualizable – and gets actualized concretely – only through the real-time interaction involving visitors' bodily movements within the sonic environment. The entire transformational process set into motion by the paper model qua analog computer only takes place by way of a return to embodied movement in real-time.

Son-O-House encompasses a complex process whose utmost aim is to convert external, passive bodily movement into intense, active, and productive movement. What specifically catalyzes such conversion is the substitution of realtime rendering for recording as the machinic correlate of embodiment. By bringing the body into a dynamic, evolving coupling with a dynamic and evolving machinic element, realtime interactivity facilitates a delinearization of the realtime flux that holds sway in the contemporary global media system. This is precisely what Spuybroek means when he describes van der Heide's score – which is to say, the entire sound component of *Son-O-House* – as an "evolutionary memoryscape that develops with the traced behavior of actual bodies in the space."[17] Not only does the visitor become both 'listener' and 'interpreter,' in a way that fuses or confounds the functions of primary and secondary retention from the get-go, but the results of the visitor's auditory acts sacrifice their autonomy to the greater productive function of the installation: "the results [of the complex feedback system linking sound, architecture, and visitors] are stored in a growing database. Previously generated sounds are re-used in the future for new combinations."[18]

Far from furnishing a surrogate 'temporal object' that would simply mirror the flux of an autonomous consciousness, the sound component of the installation – its machinic element –

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[17] Ibid.
[18] Ibid.

functions, as part of a work in the sense introduced by Couchot and Hillaire, to stimulate the actualization of bodily potentiality. If this function lends a certain privilege to embodiment – as the source for the principles of emergence operative in the work – it also foregrounds the crucial role of indirection: for in the end, what is most notable about *Son-O-House* is the way it places the motile body into correlation with a machinic source of creativity whose function remains autonomous (fig. 11). While both the constructive and the interactive components of the work are generated on the basis of the same simple rule – let movement create space – it is the autonomy of their respective operations that facilitates the shift from extensive, passive, and preprogrammed movement to intensive, active, and creative movement. In this respect, *Son-O-House* perfectly exemplifies the indirect path toward bodily creativity: the necessity for a detour through machinic deterritorialization. It teaches us that, as Spuybroek puts it, "extensive, bodily locomotion is only possible when it is intensive first, both in the body and in the system."[19]

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[19] Ibid.

Figures:

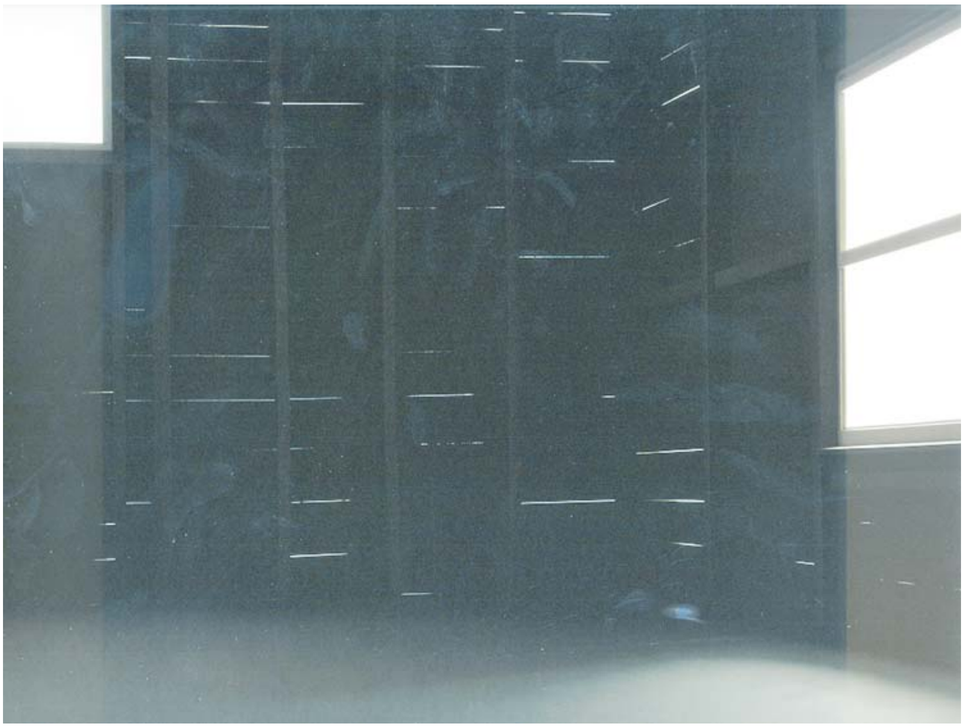

Fig. 1

Fig. 2

Fig. 3

Fig. 4

Fig. 5

Fig.6

Fig. 7

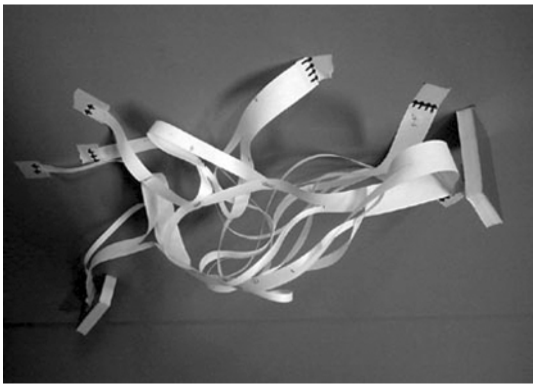

Fig. 8

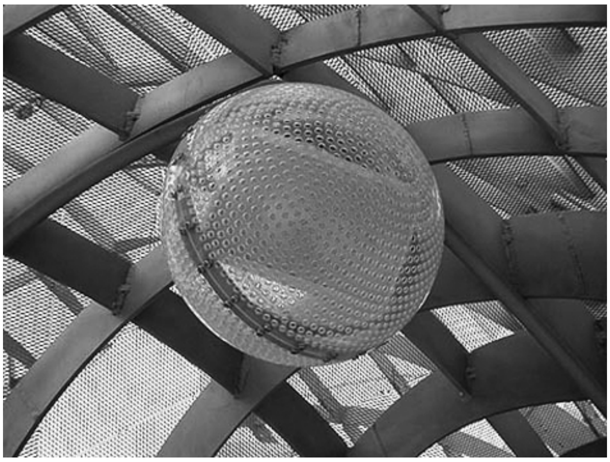

Fig.9

Fig.10

Fig. 11

Design of Physical Reality
Cognitive Strategies in STM-Picturing

Gernot Grube

In the discussion on so-called nano images there is one central question: What is it that these images show? More specifically: Do the images actually reveal atoms; do they make the invisible visible? Or do they show the creative power of certain physicists and others, such as advocates of nanotechnology, who work in the wider field of nano picturing? Or do they simply summarise data in a convenient and convincing way? All of these more specific versions of the main question reflect another general problem: How do these images function in our understanding or creating of physical reality?

Before I begin, let us recall the principle of scanning tunneling microscopy (fig. 1, see appendix). My colleague Jochen Hennig gave a nice short description:

> A tip is placed over the conductive surface that is to be examined. When a voltage with the intensity of several volts is applied, a current flows between the tip and the surface, overcoming the vacuum, that is, the non-conductive gap between the tip and the surface. [...] It is only in accordance with quantum mechanical interpretations of probability that they are able to *tunnel* through this potential barrier with the certain degree of probability.[1]

Because the current flow depends on the distance between the tip and the probe, the current flow changes according to the structure of the surface of the probe. Therefore the control unit causes vertical movements of the tip to correct such deviations. "These displacements of the metal tip," wrote Binnig and others, "given by the voltages applied to the piezodrives then yield a topographic picture of the surface."[2]

Of course most scientific projects involve a theoretical and complex technical apparatus, an experimental setting that is a precise puzzle of highly interdependent components, including the abilities of the researchers as well as particular concepts, instruments, and materials. All of these components merit investigation into their role in the research process and in our understanding of reality (Ursula Klein, for example, conducted such an investigation into the role of so-called paper tools[3]). Nano images are a relatively new component of the technical apparatus; they were first published in 1982 when the scanning tunneling microscope was invented by Gerd Binnig and Heinrich Rohrer. There is one important characteristic that distinguishes nano images from many other scientific images, a characteristic shared with images produced by particle accelerators and detectors to illustrate particle physics. It is that these images are understood to refer to a realm of

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[1] Jochen Hennig, "Changes in the Design of Scanning Tunneling Microscopic Images from 1980 to 1990," *Techné: Research in Philosophy and Technology* 8/2 (2004), http://scholar.lib.vt.edu/ejuornals/ SPT/v8n2/hennig.html.

[2] G. Binnig, Ch. Gerber, H. Rohrer & E. Weibel, "Surface Studies by Scanning Tunneling Microscopy," *Physical Review Letters* 49/1 (1982), p. 58.

[3] See Ursula Klein, *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century*, Stanford: Stanford University Press, 2003.

entities that is, in principle, not visible, and to which optical categories such as *visual shape*, *foreground and background* or *colour* cannot, in a literal sense, be applied. At the same time, these images do not refer to an abstract formula or theoretical model as do, for instance, images of a simulation, since the former claim to concern invisible physical reality itself. While particle physics and nano images function in a very similar way in terms of reference, they nevertheless differ significantly in their manner of representing physical reality.

To capture this difference, let us have a look at an image produced by the ALEPH detector at CERN (fig. 2), which measures the physical events created by collisions between electrons and positrons. We can see a schematic representation of the components – the layers – of the detector and some particles as lines and dots. Let us compare this image with a small gallery of nano images (fig. 3-5). The ALEPH image seems more like a diagram and the nano images look more like pictures. While the ALEPH diagram shows data of physical events, the nano pictures show actual physical objects. For example, in one nano image (fig. 3), we see a rhombus of twelve adatoms of the surface of 7 x 7 silicon. We see the shape of atoms and a specific pattern that looks like a new, alien landscape. On another (fig. 4), we see spheres of silicon oxide sputtered with gold, and on a third one (fig. 5) a copper surface with three ammonia molecules. All the images show experimental results of an invisible physical reality, but only the latter ones picture this reality as a world consisting of entities similar to everyday objects. These kinds of nano images therefore give rise to a specific understanding of physical reality. They seem to work as a key strategy for grasping a new world of physics.

I want to turn now to this specific understanding: How does this key strategy function? What is the value and the scientific role of these images? To give one provisional answer I will discuss the images as a factor within a kind of thought experiment and analyse them by means of a system of symbols developed by Nelson Goodman in 1968. His system promises to be quite useful in clarifying the status of these images and their role within scanning tunneling microscopy.

Obviously there is an important difference between nano images as they are produced during an experiment and used within the research process, and images produced and designed for publishing: in short it is the difference between an image in progress and an image standing on its own as a result of the experimental process. In terms of this differentiation, I am not very original and refer to nano images as results, in accordance with the contemporary discussion. On the other hand, there are reasons to believe that researchers themselves are looking at these images as results, as perfect representations of their experiments. Thus Binnig and Rohrer wrote in their 1986 Nobel lecture: "[...] we observed the 7 x 7 wherever the surface was flat. We were absolutely enchanted by the beauty of the pattern."[4] The use of the word *beauty* is significant, as the beauty of the pattern is derivative of the beauty of the pictured physical reality. Or, to give another example, let us listen to part of a transcription of laboratory shoptalk recorded by the French researcher Catherine Allamel-Raffin during a session using a transmission electron microscope. In an excerpt from the transcription, "A," a researcher and "M," the microscope operator, are watching the same screen, observing the same images:

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[4] Gerd Binnig & Heinrich Rohrer, "Scanning Tunneling Microscopy – From Birth to Adolescence" (Nobel lecture 1986), in: Tore Frängsmyr & Gösta Ekspång (eds.), *Nobel Lectures in Physics 1981-1990*, Singapore: World Scientific, 1993, p. 399.

> A: I don't understand, copper has never done that. You know what that is? It is the layer of oxidized silicon. Can you magnify there? (*A indicates a precise point on the screen.*)
> M: Yes, alright, I had seen it.
> A: There, I see the zone. (*He shows a thin white line.*)...
> A: It is magnificent.
> M: Magnificent that's something else...
> A: OK, it is there.
> M: Yes I saw the layer.
> A: You give me information, I must avoid growing on iron. Can you not do high resolution on Friday? ...
> M: Wait, I will try. It looks very fine.
> A: Make an effort! You will have a cappuccino! Ah, it is beautiful! It is there.[5]

The researcher and the microscope operator are not talking about data produced by the microscope and transformed by imaging software, but about an image representing the beauty of some newly discovered physical reality.

Let's return to scanning tunneling microscopy and explain in what way nano images are parts of a kind of thought experiment. Following the conference "The Place of Thought Experiments in Science and Philosophy," organised by Tamara Horowitz and Gerald J. Massey in 1986 at the University of Pittsburgh, an intensive philosophical discourse has developed around the notion of thought experiments. I do not want to enter into this debate, but I will draw on work by Elke Brendel that is situated in this discourse. Brendel characterises thought experiments in a way that is very fruitful for my purposes here. I will proceed in two steps: First, I will make a general argument for viewing scanning tunneling microscopy as thought experimentation, and second, I will connect this perspective to the characterisation of thought experiments given by Brendel.

Step one: One way to describe thought experiments is to say that the results of the experimental process are achieved without performing "real" experiments. The Platonist perspective holds that a thought experiment does not involve empirical input. Perhaps it is possible to maintain a Platonist view concerning nano images, since the empirical input – voltage measurements – is translated into corresponding conceptual input – digital data. The opposing view derives from an empiricist philosophy of science, according to which every thought experiment can be reconstructed as an argument based on empirical data. Despite this controversy, we can say that an experiment counts as a thought experiment when its execution does not involve a "real" experiment, or in other words, when the medium of the experiment is the imagination of a researcher rather than laboratory instruments. Nano images are in some sense *imagined*. They are symbolically mediated objects of the imagination that are not controlled by empirical data. This is because the given data could result in an infinite number of different images, or instead of an image, some other medium entirely, such as sound.[6]

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[5] Handout by Catherine Allamel-Raffin at the conference Imaging NanoSpace May 11-14, 2005, in Bielefeld.

[6] An extensive discussion with Jutta Schickore after my talk convinced me for the moment that it could be unwise to analyse nano images as part of a thought experiment. But the central argument here still holds, namely, that a classification of pictorial symbols shows that nano images are more then a simple interpretation of data, because they fabricate a visual scene where no such scene can exist.

Step two: Once Brendel had argued against a Platonist and for an empirical description of thought experiments, she had to face the problem of the legitimacy of these kinds of experiments. "We should try," she wrote, "to differentiate between classes of thought experiments which are legitimate and those in which misleading intuitions are involved."[7] Because thought experiments can be treated as arguments, she continues:

> Thought experiments can be dismissed because they are based on implausible, incoherent or inconsistent premises or because they involve inconclusive judgements, illogical inferences or other kinds of argumentative shortcomings like a *petitio principii.* [...] They often employ highly suggestive imaginary scenarios that appeal to intuitions and coerce them in certain directions.[8]

Although most of the aspects she mentions relate to articulated expressions like sentences of a language, the problem of misleading intuitions in thought experiments concerns nano images as well. The question is whether the images are legitimate scientific descriptions or rather *intuition pumps*, to use Daniel Dennett's phrase. "A popular strategy in philosophy," Dennett wrote,

> is to construct a certain sort of thought experiment I call an *intuition pump* [...]. Such thought experiments ... are *not* supposed to clothe strict arguments that prove conclusions from premises. Rather, their point is to entrain a family of imaginative reflections in the reader that ultimately yields not a formal conclusion but a dictate of 'intuition'.[9]

A perfect example of such an intuition pump is Hilary Putnam's well-known "twin earth" thought experiment.

Thus the following question has to be addressed: Are nano images unqualified intuition pumps? Recently, at a conference concerning nano images, the philosopher Davis Baird responded to a question that seemed to point out the problem of the legitimacy of nano images. In order to make his point, Baird compared still images to dynamic images and suggested that the latter are closer to the dynamic aspect of atomic reality. The physicist Pieter Vermaas made a similar point, arguing that the reality of quantum physics is represented in a more adequate way if the images depict waves, or dynamic instead of static states. But both arguments presuppose that there is a criterion of appropriateness to decide whether one highly suggestive nano image is more adequate than another, just as highly suggestive, one.

To give a more well-founded answer to the question above, I will now turn to Goodman's concept of a notational system and his analysis of pictorial representation. Some might take the view that Goodman's concept of pictorial representation is strongly oriented towards the symbols of a spoken or written language. This is indeed why Goodman's *Languages of Art* has inspired attempts to create an alphabet or grammar of pictures. But this characterisation unnecessarily reduces Goodman's view. It is a remarkable achievement of his theory of symbols that he provided a conceptual demarcation of the two incomparable types of symbols: (1) language and notational

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[7] Elke Brendel, "Intuition Pumps and the Proper Use of Thought Experiments," *Dialectica* 58/1 (2004), p. 97.

[8] Ibid.

[9] Cited ibid.

expressions like scores and (2) pictures, according to certain precise characteristics. All other symbols (sketches, diagrams, clocks, measuring devices etc.) could then be classified somewhere between these two very different poles.

As is well known, the core of Goodman's theory is the concept of a notation of symbols that function in a notational symbol system. An interesting and important aspect of this concept is that symbols can be analysed, differentiated and classified only by their syntactical conditions. Therefore we can split the question about nano images into two sub-questions: First, what kind of symbols *are* nano images? Second, what is their *reference*? To answer the first question it is necessary to investigate the syntactical structure of the symbols. Thus digital and analog symbols or symbol schemes must be distinguished from each other: a symbol scheme is digital when its characters are disjointed and finitely differentiated, like the letters of our alphabet. A symbol scheme is analog when it is dense, meaning that for any two characters there is a third one in between. Looking at an analog symbol it can hardly be decided to what character it belongs, because even the most minor aspect, like colour or thickness, counts. Just as the Latin alphabet is typical of a digital scheme, a painting is typical of a dense or analog scheme. So, what about diagrams? "Diagrams," says Goodman,

> whether they occur as outputs of recording instruments or as adjuncts to expository texts or as operational guides, are often thought – because of their somewhat pictorial look and their contrast with their mathematical or verbal accompaniments – to be purely analog in type."[10]

But, Goodman continues:

> The mere presence or absence of letters or figures does not make the difference. What matters with a diagram, as with the face of an instrument, is how we are to read it. [...] Many diagrams in topology, for example, need only have the right number of dots or junctures connected by lines in the right pattern, the size and location of the dots and the length and shape of the lines being irrelevant. Plainly, the dots and lines here function as characters in a notational language; and these diagrams, as well as most diagrams for electrical circuits, are purely digital.[11]

To figure out the syntactical structure of a symbol system it is important to know how to read it, to determine what counts. Although that question may already imply all sorts of problems with nano images, we can say that, concerning one of the first nano images from 1982 (fig. 6) constructed by Binnig, Rohrer and colleagues, every line is a connection of disjointed and finitely differentiated points corresponding to numbers that encode voltage control. The dots below the lines are to be read as symbols of atomic steps. So the thickness of the dots, for example, plays no role; the dots too are symbols in a digital scheme. Therefore the diagram is more an articulated then a pictorial symbol. With the other picture from 1986 (fig. 3) things become more complicated. It does not look like a diagram, and the luminous intensity seems to be relevant, telling us how deep a corner hole is. But at the same time, the characters behind the marks we see

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[10] Nelson Goodman, *Languages of Art. An Approach to a Theory of Symbols*, 2nd edition, Indianapolis: Hakkett, 1976, p. 170.

[11] Ibid., p. 170f.

combined to a complex image are more or less the same in both cases. The characters, which serve to mirror voltage control, are depicted by a shape that appears like a specific piece of scenery and that leads us to read more information than is encoded. Although the level of information is the same, fig. 3 forces us to look at a *picture*, a dense and ambiguous symbol. From there, it and other images as pictures (fig. 4-5) mislead our perception and open up an unfounded space of interpretation. On the other hand, as Goodman suggests, this ambiguity could stimulate cognitive creativity.

I now turn to the problem of reference, the trickier of the two questions. Images, as far as they are results of experimental configurations, should refer more-or-less directly to facts. But how can we be sure that experiments reveal facts? Answering this question concerns the whole experimental setting, the construction of instruments, the development of software, the preparation of samples etc. The facts can get lost in all of these areas. Supposing that it is even possible to catch hold of the fact, or rather the sought-after object, how do we know in what way the image represents the object? How do pictures represent, in contrast to diagrams, which transform an abstract articulated database into an articulated but more iconic symbol? Goodman's work is famous for its radical critique of the so-called theory of representation as resemblance. "The copy theory of representation then," he writes, "is stopped at the start by the inability to specify what is to be copied. Not an object the way it is, nor all the way it is, nor the way it looks to the mindless eye. [...] it is the object as we look upon or conceive it, a version or a construal of the object. In representing an object, we do not copy such a construal or interpretation – we *achieve* it."[12] And, as Goodman adds in a footnote, this is "no less true when the instrument we use is a camera rather than a pen or brush."[13] Goodman's view here is perhaps hypersensitive; he later admitted that the concept of resemblance probably couldn't be excluded from a theory of representation. In the case of nano images, however, the question of a copy of an atomic surface is meaningless.

We have to resort to another concept used by Goodman, the concept of "representation-as." Consider as an example, the following: We can show the president as a child, but, and here's where it gets interesting, we can show the *adult* president as a child. Three cases have to be distinguished. "A picture," writes Goodman, "that represents a man denotes him; a picture that represents a fictional man is a man-picture; and a picture that represents a man as a man is a man-picture denoting him. Thus while the first case concerns only what the picture denotes, and the second only what kind of picture it is, the third concerns both the denotation and the classification."[14] Some nano pictures are of the third type, concerning both denotation and classification. They are "atomic-structure-of-surface-pictures" and they denote current control, which depends on the topography of the surface. It is obvious, to keep things as simple as possible, that when the voltage – as fact – is represented as a surface object, this object is as fictional as a unicorn. The data flow is not represented as an iconic translation of data flow, but is represented as an imaginary object; it is, in any case, an object-picture. So the – somewhat paradoxical – situation is that we have the picture of a fictional object that at the same time denotes physical facts.

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[12] Ibid., p. 9.

[13] Ibid., p. 9, note 8.

[14] Ibid., p. 27f.

When we look at a unicorn-picture we don't say that what we are seeing is a picture of a representation of a unicorn. In this case we cannot distinguish a represented object from the representing object. A unicorn-picture as a fictional representation has no denotation or, in Goodman's terminology, it has null-denotation. When we look at an atomic-structure-of-surface-picture, like the second image, we don't see a representation of the atomic structure of a silicon surface. We can differentiate between the image and the data. There is a picture of data that turns into an atomic-surface-picture without any corresponding object. It is not possible to say in any ordinary sense that we see a picture of such and such an object. But we are still looking at some sort of object. In some sense there is nothing other than a pictorial sign, and at the same time there is no longer a sign but an object. This complex situation is reminiscent of the early years of photography when the first photos were used in court and lawyers warned that some members of the jury could take the photos for the objects themselves. This problem in a legal context now reappears in a scientific one, thanks to digital or computer-manufactured images that have unleashed a world of new objects, or as Binnig wrote: "I could not stop looking at the images. It was like entering a new world."[15] Another striking example that can be used to illustrate this phenomenon is the computer-generated simulation. The object we see is the only object we have. The depicted objects also show some characteristics of ordinary entities when they come in the form of so-called three-dimensional images that can be turned around or opened up for a look at their insides.

Now we should be prepared to answer the question of whether nano images as pictures are unqualified intuition pumps or not. Using Goodman's analytical view, we have to classify all nano images as diagrams, as iconic but articulated and digital symbols. But some representations of data such as diagrams are, by means of imaging software, actually transformed into representations of objects, such as atomic-surface-pictures. Consequently these nano images qualify as functioning intuition pumps that produce an absolutely unfounded and probably misleading understanding of physical reality at the nano scale.

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[15] Gerd Binnig & Heinrich Rohrer, "Scanning Tunneling Microscopy," p. 399.

Figures:

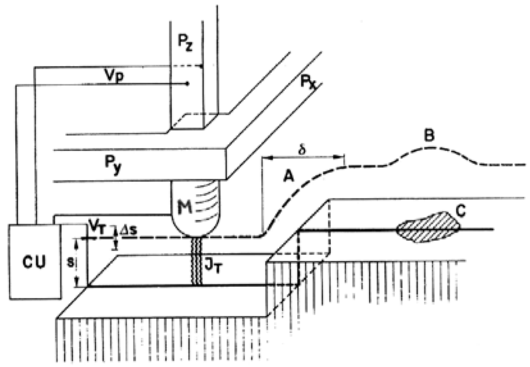

Fig. 1: Principle of operation of the scanning tunneling microscope, G. Binnig, Ch. Gerber, H. Rohrer & E. Weibel, “Surface Studies by Scanning Tunneling Microscopy,” in: *Physical Review Letters*, Vol. 49, Nr. 1, 1982, p. 58.

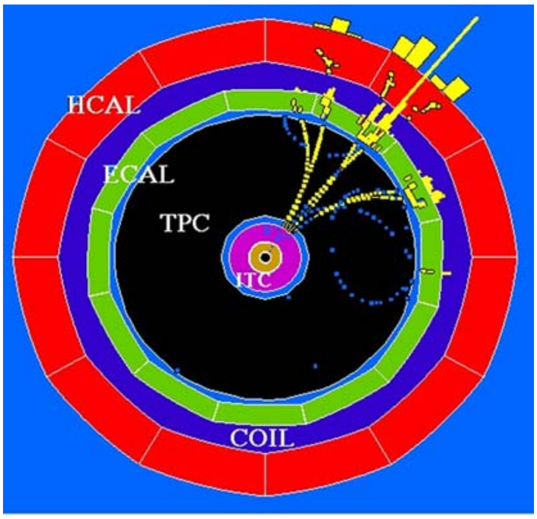

Fig. 2: ALEPH detector at CERN, a schematic representation, http://aleph.web.cern.ch/aleph/aleph/newpub/intro.html.

Fig. 3: Processed image of the 7 x 7 reconstruction of Si(111), Gerd Binnig & Heinrich Rohrer, “Scanning Tunneling Microscopy – From Birth to Adolescence” (Nobel lecture, 1986), in: Tore Frängsmyr & Gösta Ekspång (eds.), *Nobel Lectures, Physics 1981-1990*, Singapore: World Scientific, 1993, p. 400.

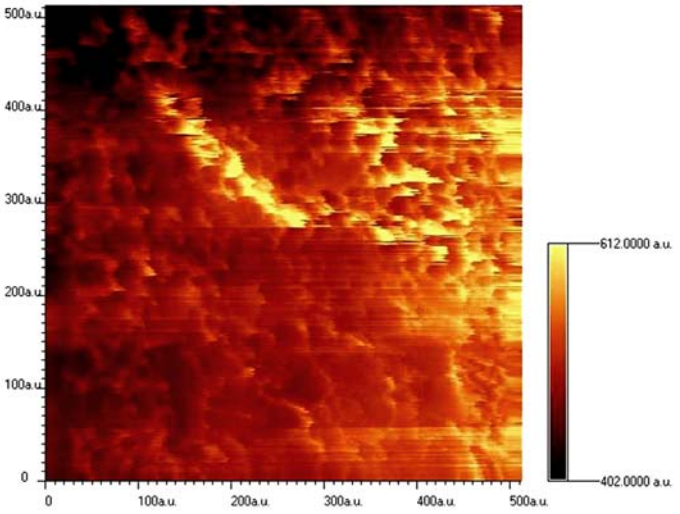

Fig. 4: Spheres of silicon oxide sputtered with gold, Dirk Hausmann, http://sxm4.uni-muenster.de/stm-en/STMPictures.html.

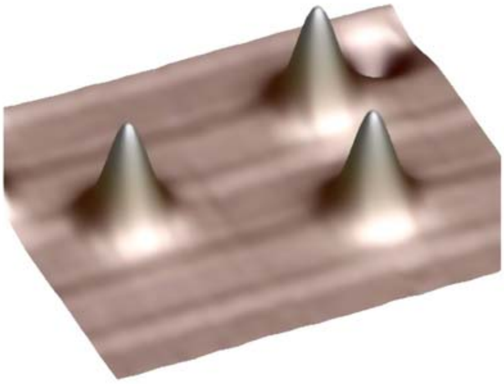

Fig. 5: Copper surface with three ammonia molecules, http://www.mpg.de/bilderBerichteDokumente/multimedial/galerie/bilderWissenschaft/2003/06/ammoniak1/index.html.

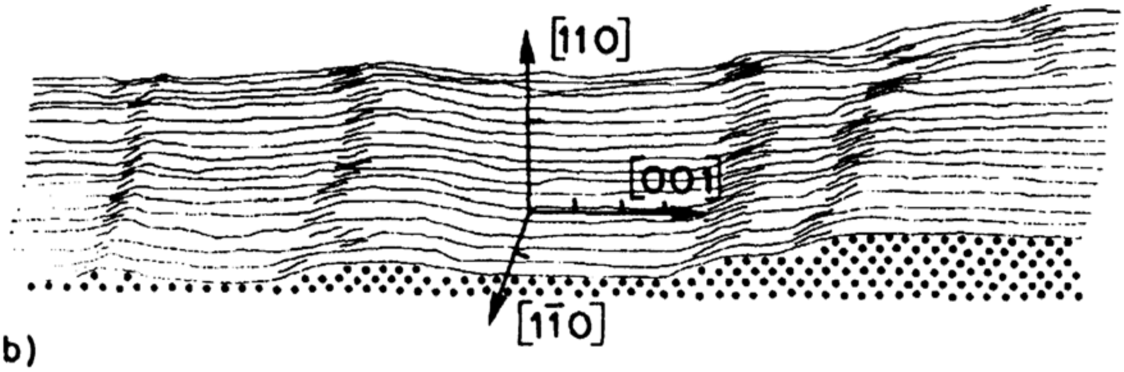

Fig. 6: A scanning tunneling micrograph of an Au(110) surface, G. Binnig, Ch. Gerber, H. Rohrer & E. Weibel, “Surface Studies by Scanning Tunneling Microscopy,” *Physical Review Letters* 49/1 (1982), p. 59.

Shaping Differences: Hermann von Helmholtz's Experiments on Tone Colour

Julia Kursell

In the first half of the nineteenth century, musical encyclopaedias typically referred to *Klangfarbe* in an entry like the following: "*Timbre*, French word for the German *Klangfarbe*, accidental properties of a voice."[1] As opposed to pitch, *Klangfarbe* was not considered to be a constitutive part of a composition, hence its definition as "accidental." This changed with the appearance of Hermann von Helmholtz's book *On the Sensations of Tone as a Physiological Basis for the Theory of Music* in 1863.[2] "Tone colour" – to use the term Alexander Ellis coined in his translation of the book – is a central topic in Helmholtz's work on the physiology of hearing. Helmholtz not only provided a new definition of tone colour, he also assigned this notion a central place in his resonance theory of hearing, and he applied the notion to the sounds of language and of musical instruments as well.

In the following, Helmholtz's work on tone colour will be recapitulated through a discussion of three different experimental situations. The first is the description in Helmholtz's book *On the Sensations of Tone* of a situation in everyday life in which hearing is involved. As I argue, this description stands as an experimental justification of his definition of tone colour. The second is Helmholtz's main experiment on tone colour, the famous synthesising of vowel sounds he worked on in Bonn and Heidelberg. The third experiment was carried out in 1910 by Carl Stumpf in his Berlin laboratory, where he experimentally verified the hypothesis Helmholtz inferred from his description of hearing in everyday life. I will comment finally on some of the aesthetic implications of the concept of tone colour as developed by Helmholtz.

In his work on the physiology of hearing, Helmholtz revealed a specific correlation between acoustics, physiology, and the cultural use of sound. I will argue that the concept of difference inherent in the notion of tone colour is crucial for this correlation. In his laboratory Helmholtz recreated the cultural phenomena of music and language sounds. By doing so he also recreated their organising principles. Music helped him to define a manageable object of investigation, and language provided a set of objects for investigation. The sounds of language are in constant use, they are perceived, and they can be discriminated. In the following, I want to show how Helmholtz opened up the study of the field of differences that allowed for a systematic description of the various sounds of language and musical instruments. Tone colour, a property of sound formerly inaccessible to systematisation both in music and in physical acoustics, became a manageable

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[1] Cf. Lemma "Timbre," in: *Encyclopädie der gesammten musikalischen Wissenschaften oder Universal-Lexikon der Tonkunst*, edited by Gustav Schilling, vol. 6, Stuttgart: Franz Heinrich Köhler, 1938, S. 647: "Timbre (franz.), in der Musik – Klangfarbe. Man versteht hierunter vornehmlich die zufälligen Eigenschaften einer Stimme."

[2] Hermann v. Helmholtz, *Die Lehre von den Tonempfindungen als physiologische Grundlage für die Theorie der Musik*, Braunschweig: Friedr. Vieweg & Sohn, 1913; idem, *On the Sensations of Tone as a Physiological Basis for the Theory of Music*, 2nd English edition, translated, thoroughly revised and corrected, rendered to conform to the fourth German edition of 1877 by Alexander J. Ellis, London: Longman & Co, 1885 (repr. New York: Dover, 1954).

object of investigation. In the long run, this research would be expanded to any sound and would encompass all kinds of sound systems.

The emergence of tone colour as a theoretical problem in the 1840s

In the beginning of the 1840s, August Seebeck and Georg Simon Ohm discussed the nature of tone.[3] Seebeck carried out a series of experiments on the shape of sound waves. He conjectured from the results that the impression of a tone depended on the periodicity of impulses rather than on the sinusoidal shape of the sound wave. Ohm reacted to this hypothesis by arguing that tones were composed of sine waves, and he proposed to analyse them by means of Fourier analysis. Ohm thereby postulated the composed nature of tone in accordance with the Fourier series. What was new in this discussion was the role both Ohm and Seebeck attributed to hearing. They both asked why tones were perceived as entities, although they consisted of a number of components, and why only one frequency was attributed to this entity.[4] To answer this question their arguments followed different lines: Ohm searched for the natural law that would account for an acoustic dominance of the fundamental frequency in a tone. In contrast, Seebeck did not require a physical reason for the dominance of pitch; he even asserted that a fundamental frequency could be perceived where there was no physical equivalent to such a frequency. While Ohm explained what was heard by explaining physical phenomena, Seebeck relied on hearing to challenge mechanistic explanations.

Helmholtz took up this thread, transforming the question into an issue of the physiology of hearing. He reformulated the question of how a multitude of components can be perceived as one entity, introducing the notion of tone colour: How can we, he asked, discern different tone colours? Thus he shifted the question from a discussion of the essence of tone to an investigation into the differences between tone colours. Summing up the debate on the nature of tone, Helmholtz pointed out that tone colour had so far emerged only as a "negative quality." While loudness and pitch could be related to the amplitude and frequency of sound waves, only the shape of the sound wave was left to specify tone colour. So, there was a positive correlation between two properties of tone, namely loudness and pitch, and the quantifiable parameters of amplitude and frequency, but there was no measure for the shape of the waves. In music, the notion of tone colour, or "timbre," comprised the unspecified sum of all properties differentiating two tones of the same pitch and loudness played by two different instruments. Helmholtz's own reformulation

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[3] See R. Steven Turner, "The Ohm-Seebeck Dispute, Hermann von Helmholtz, and the Origins of Physiological Acoustics," *The British Journal for the History of Science* 10 (1977), pp. 1-24; Johannes Barkowsky, *Das Fourier-Theorem in musikalischer Akustik und Tonpsychologie*, Frankfurt/M. et al.: Lang, 1996 (Schriften zur Musikpsychologie und Musikästhetik 8, edited by Helga de la Motte-Haber), pp. 212-230.

[4] Cf. Turner, "The Ohm-Seebeck Dispute," p. 7: "Ohm was not only reasserting that the ear decomposes complex vibrations into a harmonic series of pendular components; he was also asserting by implication that the ear carries out this decomposition even on complex waves that cannot be shown to have been originally compounded of pendular vibrations. Logically Ohm's definition of tone was not, as most previous analyses had been, an assertion about vibrating or sounding bodies or about the mathematics of waves. It was really an assertion about the physiology of auditory perception." See also Dieter Ullmann, *Chladni und die Entwicklung der Akustik von 1750-1860*, Basel, Boston, Berlin: Birkhäuser, 1996 (Science Networks. Historical Studies 19, edited by Erwin Hiebert and Hans Wußing), pp. 173-179, "Das Problem der Klangfarbe."

of the problem – expressed once again against the background of the physical and musical notion of tone colour and the dispute between Ohm and Seebeck – was the following: How can we distinguish two tones of the same pitch and loudness played by two different instruments even though we have no knowledge about the components of the tones and do not experience them as composed?

Instead of discussing the physical nature of sound or its impression on the listener he thus asked about the function of the ear as evidenced by the ability to discriminate tone colour. Although the ear somehow coped with the composed nature of tones, this discrimination did not happen consciously. This showed, for Helmholtz, that the discrimination of tone colour was not a matter of cognition, but rather had to be explained as a physiological function of the ear. His argument demanded a new concept of discrimination: the sounds Helmholtz investigated could not be ordered under a given parameter, and it would not make sense to ask for the smallest noticeable difference between tone colours. Instead, the study of these sounds would eventually bring about a recognition of other differences operable in language and music.

Distant philology

In his own discussion of tone colour, Helmholtz followed a twofold strategy. First, he carefully defined a set of elements necessary for the very narrow definition of tone colour he would eventually investigate.[5] This set of elements described tone and sound (*Ton und Klang*) in terms of physical and musical understandings. Second, he listed elements that could possibly differentiate one sound from another, but that did not fit into his own definition of tone colour. In this way, many properties of sound, including those he did not plan to investigate, were mentioned in the text. Helmholtz thus not only isolated a specific object for experiment but at the same time located this object in a broader field of sound properties. Two of these sound properties he considered to be very important: first, that every sound has a distinctive temporal structure, with its beginning and ending being especially characteristic; second, that sounds are composed of periodic components as well as non-periodic noises, the latter being, again, very helpful in the discrimination of sounds. This was true especially for the sounds of speech. Speech, as Helmholtz pointed out, provides the best examples of the relation between tone and noise, since written language even provides a notational system for this relation, with the beginning or ending of a sound being indicated by different consonants.[6]

Helmholtz eventually restricted the object of his investigation to what he called “musical tone colour,” i.e. stationary sound without accompanying noise, though he himself acknowledged the narrowness of this restriction to only those sounds in which no changes occur. However, to make his decision plausible, he described a situation in which sounds must be discriminated only according to their “musical tone colour:”

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[5] It has been pointed out repeatedly that Helmholtz carefully restricted his investigation to objects that could be treated by physiological or mechanistic analyses. Cf. Turner, “The Ohm-Seebeck Dispute,” p. 21; see also Ullmann, *Chladni und die Entwicklung der Akustik*, p. 196, who emphasises the role of Helmholtz as a physicist.

[6] Helmholtz, *Die Lehre von den Tonempfindungen*, p. 114; idem, *On the Sensations of Tone*, p. 66.

> The importance of these [noises and little inequalities] can be better appreciated by listening to musical instruments or human voices, from such a distance that the comparatively weaker noises are no longer audible. Notwithstanding the absence of these noises, it is generally possible to discriminate the different musical instruments, although it must be acknowledged that under such circumstances the tone of a French horn may be occasionally mistaken for that of a singing voice, or a violoncello may be confused with a harmonium. For the human voice, consonants first disappear at a distance, because they are characterised by noises, but *M*, *N*, and the vowels can be distinguished at a greater distance.[7]

When heard from a distance, noises contained in a sound as well as the beginning and end of the sound of a musical instrument may not be heard, or, to put it differently, the instrument's characteristic noises and temporal features are reduced in a way that enhances the perception of stationary sound. Distance turns sounds into musical tones. Still the source of the sound can, with a remarkable degree of reliability, be identified.

In this description of hearing distant speech and music, which I take to be *On the Sensations of Tone*'s first quasi-experimental observation on the discrimination of tone colour, sound is separated from its source. The various instruments or speech sounds are distorted, or "filtered," by the loss of energy in their transmission through space. To discern them and to understand how they were produced or what they mean becomes a challenge. The context to which these sounds belong is distorted. This description of sounds heard at a distance therefore can also render the recreation of cultural phenomena in the laboratory more plausible.

The electric synthesiser

Soon after beginning work on the physiology of hearing, Helmholtz conceived of his tuning-fork apparatus for synthesising tone colour.[8] The tuning-fork apparatus used an electromagnet to set the forks into steady motion. (fig. 1) As the sound was not very loud, it was amplified by resonators. (fig. 2) The resonators also removed the upper partials or overtones, i.e. those components of the tone that determine the shape of its wave, from the tones of the forks, since the resonators were supposed to amplify only the fundamental frequency of each fork. So what was heard when the apparatus was set into motion, was a tone without "overtones" or "upper partials,"[9] i.e., sound waves that could be described in terms of simple sinusoidal waves. The combination of forks and resonators also allowed phase and intensity to be manipulated by partly closing a resonator, and thus mistuning it, or by removing it from the resonating fork, thereby reducing its amplifying effect. Electricity, in turn, not only set the forks in steady motion, but it also enabled the coordination of the vibrations of several forks.

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[7] Helmholtz, *Die Lehre von den Tonempfindungen*, p. 117-8; idem, *On the Sensations of Tone*, p. 68.

[8] On the history of the apparatus and Helmholtz's experiments on the sound of vowels see Stephan Vogel, "Sensation of Tone, Perception of Sound, and Empiricism: Helmholtz's Physiological Acoustics," in: David Cahan (ed.), *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*, Los Angeles, London: University of California Press, 1994, pp. 259-287.

[9] A. Ellis, the translator of *On the Sensations of Tone* (cf. footnote 2), preferred "upper partials," see the index of the English edition, p. 569: "Overtones, used by Prof. Tyndall for upper partial tones, an error of translation, here avoided; the term should never be used for partials in general."

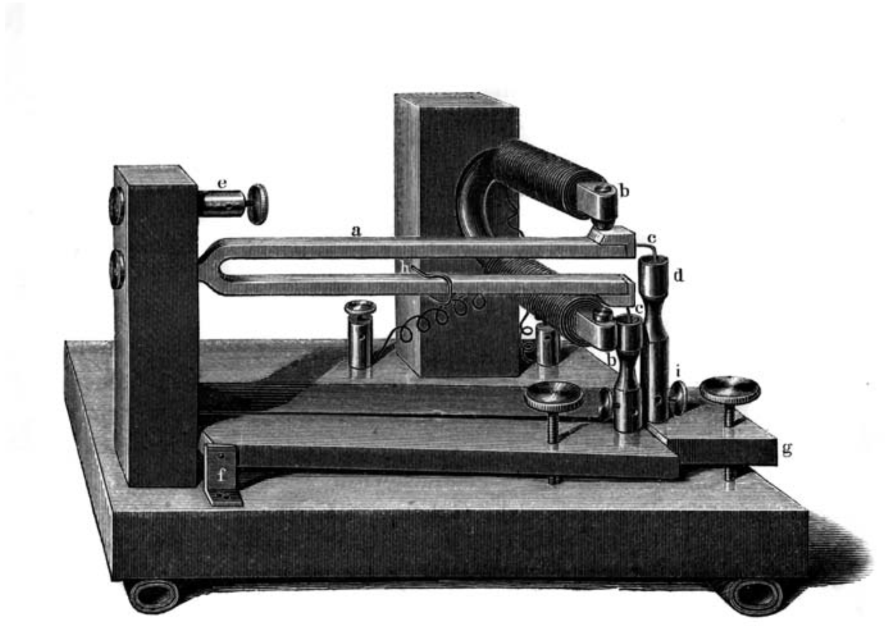

Fig. 1: Interruptor used in Helmholtz's tuning fork apparatus (from: Hermann Helmholtz, *Die Lehre von den Tonempfindungen als physiologische Grundlage für die Theorie der Musik*, vierte umgearbeitete Ausgabe, Braunschweig: Vieweg und Sohn 1877, p. 198)

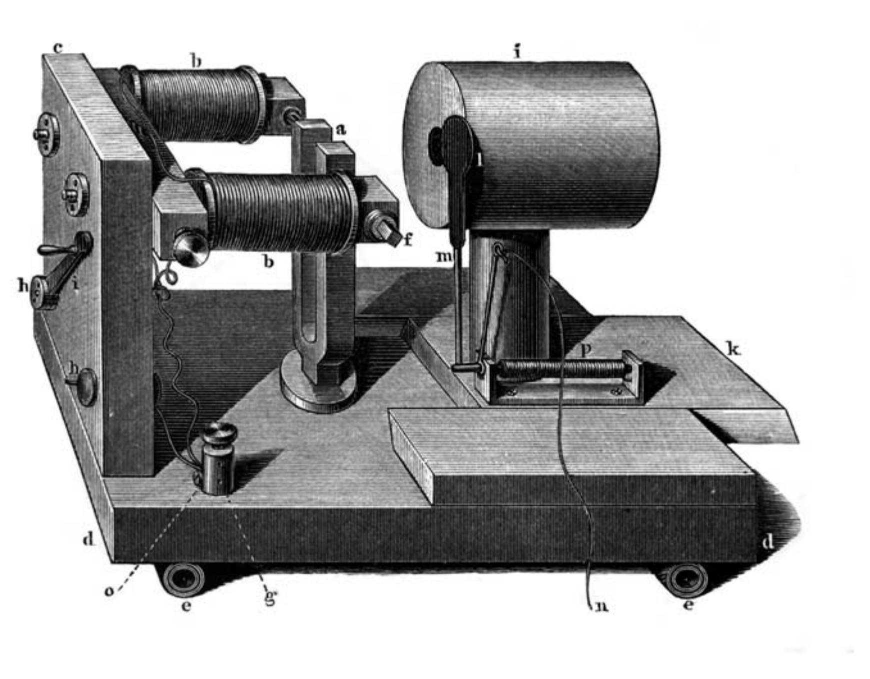

Fig. 2: Tuning fork (a) and resonator (i) (from: Helmholtz, *Die Lehre von den Tonempfindungen* 1877, p. 196)

The tuning-fork apparatus put sinusoidal sound waves at the disposal of the experimenter: each of the sound waves it produced could be heard alone, but the sound of one amplified fork could also be combined with those of the other forks. With a set of tuning forks and resonators tuned to the harmonic series, Helmholtz created sounds whose components or partials were thus proven to exist independently; and with a row of seven simple tones, he managed to synthesise sounds in which he could discern the vowels of the German language. Helmholtz noted, though, that in the first experiments with his electric tuning-fork apparatus he had overemphasised the darkness of the vowel *O* to make it more distinct from his *E*. The *E* he had been able to synthesise was barely recognisable, as he admitted. It was the difference between the two vowels that needed to be distinguished, not just the similarity of each single sound to the corresponding vowel.

This experiment isolated tone colour as an analytical category. The experiment on the sound of vowels had shown that it was possible to produce different sounds with a machine whose operating principle involved a mathematical assumption. To make this possible, the elements composing the synthesised sound had to be neutral in the sense that it did not impose its own ‘colour’ onto a sound.[10] Logically, Helmholtz therefore began his explanation of the tone colour of musical instruments in *On the Sensations of Tone* by commenting on sounds without upper partials: “These tones are uncommonly soft and free from all shrillness and roughness.”[11] Helmholtz had noted that the synthesised vowels sounded like sung, rather than spoken, vowels. This was due to their duration, but also to the lack of characteristic temporal features: the sinusoidal sound waves as heard from the tuning-fork apparatus appeared to be closest to the ideal of a purely stationary sound. This sound could not be ascribed to a specific mode of production: these simple tones always sounded the same, whether amplified by a string or by a resonator and independent of the material of the latter. As was demonstrated in the experiment, tone colour was not a necessary corollary of an instrument or a peculiar shape of the mouth cavity but could instead be reduced to a lawful relation between sinusoidal sound waves and their intensities. Certain tone colours could be produced independently of the specific bodily properties of a sound source. The incorporeal existence of the simple tone was an argument in favour of the description of tone colour as an analytic category.

The category of tone colour could not, however, be dealt with as pitch and loudness had been. The subject under discussion still concerned only periodic waves, as that was all the available mathematical methodology could analyse. The non-periodic, random movements of noise were left out of the investigation. By replacing “tone” with “tone colour,” however, Helmholtz was dealing with a difference rather than an entity. While Ohm’s approach to tone was essentialist, inquiring into the nature of *the* tone, Helmholtz took a *difference* as his point of departure and examined the ear’s capacity to discriminate sounds. The fact that the sounds of musical instruments and of language are discriminated, demonstrated that the ear is able to analyse sound.

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[10] On the qualities associated with sinusoidal sound waves see Wolfgang Auhagen, “Zur Klangästhetik des Sinustons,” in: Reinhard Kopiez (ed.), *Musikwissenschaft zwischen Kunst, Ästhetik und Experiment: Festschrift Helga de la Motte-Haber zum 60. Geburtstag*, Würzburg: Königshausen & Neumann, 1998, pp. 17-27.

[11] Helmholtz, *Die Lehre von den Tonempfindungen*, p. 119; idem, *On the Sensations of Tone*, p. 69.

For the plausibility of his argument he had to rely on the training listeners derived from their use of language; in other words, Helmholtz relied on the knowledge of what would later be called the phonologic system.

The sounds of language

In 1910 the question of sound quality was addressed once again, this time by the psychologist Carl Stumpf. He began what would become an extensive study on speech sounds, published under the title *Die Sprachlaute*, by taking Helmholtz's description of listening to sounds from a distance literally. In an experiment carried out in his laboratory he removed the sources of the sounds from the listeners by placing them in another room. The two rooms were separated by a corridor and connected only through openings in the walls about the size of a hand (20 square centimetres). (fig. 4) These openings could be closed by the experimenter.

In the experiment, only two seconds taken from a middle section of the sounds was presented to the experimental subjects. The subjects, who were all trained musicians, acousticians, or instrument makers, were asked to identify the tone colour of a flute, an oboe, a trombone, a tenor horn, a trumpet, a violin, a cello, and a tuning fork amplified by a resonator, i.e., a device to create sinusoidal tones.[12]

Never before had sounds been isolated from their characteristic features in this way. Accordingly, in some tests the correct identification was less than 50%, the best results being achieved by an instrument maker who had manufactured some of the instruments used in the test. Stumpf, however, declared that these results were good enough to prove Helmholtz's assumption that tone colour could be distinguished on the basis of stationary sound with only little accompanying noise. But Stumpf not only read Helmholtz's definition of tone colour as a temporal discrimination within a sound, he also understood Helmholtz's story of distant sounds as the prediction of a transmission rate. For Helmholtz, the temporality of sound had to be neglected, since it would have introduced changes that were uncontrollable; and while for Helmholtz, the purpose of his definition was to make sounds accessible to Fourier analysis, Stumpf's tests did not require previous analysis of the presented sounds. Introducing a group of experimental subjects who were to identify the sounds, he approached the question with statistical methods and thus took the question of the differentiation of the sounds one step further, investigating the conditions under which different sounds could be discerned. By creating an experimental set-up in which two rooms communicated in a controlled way, he transformed the sounds of musical instruments into signals.

Testing the fundamental hypothesis of Helmholtz's definition of tone colour, Stumpf relocated it within the broader field of sound properties. But while Helmholtz moved toward limiting the object of investigation in order to obtain something manageable, Stumpf reopened it for an investigation with different tools and new outcomes. He showed Helmholtz in a new light, namely, as the subject of his own experiences. And what had been the object under investigation in Helmholtz's experiment now became the stimulus for the investigation of a different object:

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[12] Carl Stumpf, *Die Sprachlaute. Experimentell-Phonetische Untersuchungen nebst einem Anhang über Instrumentalklänge*, Berlin: Julius Springer, 1926, pp. 374-412, "Über Instrumentalklänge."

communication. For the listeners in the remote room, there was no way back to a sound quality; the outcome of the experiments consisted of statistical data about sound perception and signal transmission.

Musical tone colour

In his textbook on harmony, first published in 1911, the composer Arnold Schoenberg gave an intuitive definition of "Klangfarbe" that rather closely corresponds to Helmholtz's:

> I find that the tone makes itself perceivable by its timbre, one of whose dimensions is pitch. Timbre is therefore the larger domain, and pitch one of its dimensions. Pitch is thus nothing but timbre, measured in one direction.[13]

In this definition of *Klangfarbe*, pitch functioned as a parameter. Yet it is important to note that for Schoenberg, the logical order of tone colour and its parameters had changed: tone colour was not considered to be an additional property of tones, rather, pitch was a property of sounds – not of tones – that could arbitrarily be singled out as a parameter. Schoenberg therefore postulated:

> If it is possible to let structures emerge from timbres with different pitch, which we call Melodies [...], then it should be possible as well to create such sequences from timbres in another dimension, i.e. from those entities we call simply "timbres" [Klangfarben] – sequences of elements, whose relation follow a kind of logic, which would be equivalent to that logic which is sufficient for us in a melody of pitches.[14]

If it was possible to single out one parameter, for example pitch, and construct a logical sequence out of it, i.e. melody, the same should be possible when singling out another parameter. Schoenberg called such sequences made of other elements than those defined by their pitch "Klangfarbenmelodien."[15]

The status of tone colour changed in the course of the nineteenth century. While Ohm and Seebeck had taken for granted that one tone had one frequency or pitch, this became less evident after Helmholtz defined "physical tone" as a simple, sinusoidal sound wave with one frequency as opposed to "musical tone," which had perceived unity with one pitch. But already in the 1840s, French composer Hector Berlioz had noted an unprecedented boom in the art of instrumentation.[16] While at the beginning of the eighteenth century instrumentation had not been an issue at all, as Berlioz remarked, in his own time it had become one of the most discussed topics among composers. Although tone colour as it was dealt with in instrumentation comprised many more characteristic features than those investigated by Helmholtz, his research profoundly affected the understanding of musical tone. As the example of Schoenberg's speculation about

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[13] Arnold Schönberg, *Harmonielehre*, Wien: Universal Edition 1986, p. 503 (author's translation).

[14] Ibid.

[15] Ibid., 504.

[16] Hector Berlioz, *Grand traité d'instrumentation et d'orchestration modernes*, edited by Peter Bloom, *Hector Berlioz. New Edition of the Complete Works*, issued by the Berlioz Centenary Commitee, London in Association with the Calouste Gulbenkian Foundation, Lisbon, vol. 24, Kassel, Basel, London, New York, Prag: Bärenreiter, 2003, p. 3.

"Klangfarbenmelodie" shows, it was as plausible to characterise musical entities by their "colour" as by their pitch.

Schoenberg indeed integrated other domains from the field of sound into his own compositions, namely spoken language. He did so, for instance, in his famous piece *Pierrot lunaire*, which he finished in 1912, one year after he published his textbook on harmonics. In *Pierrot lunaire,* speech sounds are aligned in a way to avoid "singing" with constant pitch. So, Schoenberg not only composed so-called atonal music, i.e. music that is not constructed according to the pitch relations given by the tonal system of occidental music, he also discovered the possibility of integrating the sounds of language into his compositions, treating them as tone colours. Although the voice of the "speaking" part in *Pierrot lunaire* does follow notated pitch – the part is notated in the usual five-line staves but with crosses instead of note heads[17] – it abolishes the principle of "tonality." The part is composed of the sounds of language. Sung and spoken vowels, the noises of consonants, and those of whispered vowels are treated equally. They all become sound colours, and their differentiation is achieved primarily by the organising principles of language. Schoenberg discovered speech to be a "Klangfarbenmelodie" in itself.

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[17] A detailed analysis of Schoenberg's use of notation for "Sprechgesang" can be found in Ulrich Krämer, "Zur Notation der Sprechstimme bei Arnold Schönberg," in: *Schönberg und der Sprechgesang*, ed by Heinz-Klaus Metzger and Rainer Riehn, (Musik-Konzepte 112/113), München: edition text+kritik, 2001, pp. 6-32.

Goethe's Colors

Joseph Vogl

Let me begin with an old and well-known story. It traces back to the theories of painting and visual aesthetics up until the end of the Eighteenth century – back to the reflections about the relationships between line and color, drawing and coloring. Let me recall these briefly.

Common to all the discussions on the role of sculpture, on the manners of representation in painting, and finally on the perception of the blind – discussions led by Locke and Molyneux, Rousseau, Diderot and Herder – common to all of these is a mutual subordination that assembles the visual code from optical and tactile components. The colorfulness of color is granted only insofar as it elevates itself to form, to contour or *disegno*. From the painters' academies to theories of vision and philosophical aesthetics, one worked on setting up an optical-tactile space in which what was fleeting and formless in color gained duration only through sculptural form. One could say that a blind, groping hand operated in the center of the visible image: a hand that, in the first place, produces those spatial, representational forms for which one liked to reserve the qualification of "beautiful." What was always described as a disqualification of the colorful and praise for the (colorless) statue found its most consequential formulation with Kant. While colors, like tones, were abandoned to the manifold in sensibility, where they supply only material for sensations, form and drawing alone provide the "essential" that can become object of the judgment of taste.[1] A theory of sensation and a theory of art converge only in the infinite, as the reflection of an object in the imagination.

It should be of no surprise if I wish to touch on one of the most controversial theories from the beginning of the nineteenth century as the hinge of this history, as the turning point in the conceptual history of color and space: that is, Goethe's *Theory of Colors*, which will be my subject now. Contemporary debates as well as the history of this text lay out a path that one might call a "path of color." Not only does this path lead from the eighteenth to the nineteenth century, and pass into an altered relation between color and space; it also characterizes the structure of Goethe's text, in particular the first, didactic part. For here color appears only insofar as it is led repeatedly along multiple paths: on a didactic path that promises to course from the simple to the complex; on a methodical path, a *methodos* or path in the rigorous sense, that completes an experimental purification of color and colorfulness; and finally on a theoretical path that doubles every empirical vision with a theory of this very vision. This is a parallelism of processes for which Goethe can claim a certain originality – and here we find a hidden narration, a scientific novel, which relates the destinies of its protagonists, of color itself. At stake is, in fact, the birth of the visible world, the becoming-world of the visible; at stake is the path that color takes at the founding of a world.

For it is hard not to notice the fact that Goethe's text is first of all organized around a kind of cosmogony, which traces the becoming of things and beeings from an original chaos. Like in Plato's *Timaeus*, in the beginning of the *Theory of Colors* was nothing but light and dark, and in

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[1] Immanuel Kant, *Kritik der Urteilskraft*, in: *Werke*, edited by W. Weischedel, Wiesbaden 1957, Vol. 5, pp. 303-306.

between a murkiness or turbidity that was the transition from light to dark. There was an unordered and, if you will, place- and shapeless space which, like Plato's *chora*, is best defined as the amorphous ground of all processes of becoming.[2] And just as in the *Timaeus*, in which the transition from becoming to being must issue in something that is "corporeal," "visible" and "palpable,"[3] what stands at the end of Goethe's path of color is not only the firm, visible, corporeal and palpable world of things, but much more: a chain of beings that ascends from minerals to plants, lower animals, birds and mammals all the way up to humans.[4] By the end of the *Theory of Colors* the rhythm of fleeting appearances has coagulated into objects, unordered space has become geometrical form. This process – and this would be a second aspect to this story – is simultaneously bound with a qualitative change of color itself.

This qualitative change is to be understood in a peculiar sense: for the path of color that Goethe follows from the "physiological" colors to the "physical" all the way to the "chemical" is equivalent to the process of a continual actualization of color, and to a realization of color. First, physiological colors, that is, fleeting and accidental appearances without representational place; then the physical colors, those reflexes in transparent media, which in Goethe's own words are characterized by a tiny degree more of reality ("einen geringen Grad mehr Realität" [70]); and finally the chemical colors, which in the end are nothing other than attributes of things and beings. What happens on this path is the transition from place- and objectless qualities to stable attributes, which designate enduring in the order of things. The emergence of the visible world and the gradual realization of color – that is the double narrative that Goethe's *Theory of Colors* follows.

Here, I want to underline the surprising turn in this narrative, a certain audacity in Geothe's *Theory of Colors*. For when Goethe addresses the so-called physiological colors as the "first" and "most important" section of his text (28), when he speaks of them as the foundation of the entire doctrine ("Fundament der ganzen Lehre"), and finally as the necessary condition of vision in general ("notwendige Bedingung des Sehens" [31]), he articulates a paradox that will have far-reaching consequences for the theoretical structure. In a hyperbolic manner Goethe cited the traditional names of physiological colors; he gathered together rejected figures from the optics of the recent past. Their names are: accidental colors or *couleurs accidentelles*, imaginary or fantastic colors, apparent colors, deception of vision, fleeting errors and specters of the eye ("couleurs accidentelles," "Scheinfarben," "Augentäuschungen," "Gesichtsbetrug," "vitia fugitiva," "ocular spekctra" [31]). The unreliable figures of an older theory return as key witnesses; more pointedly, this means that it is precisely deception that turns into the sure argument, the unfounded into solid ground, the accidental into necessity and the fleeting into the basis of the entire construct. With this logical drama and paradox, a project begins for which Goethe reclaims the title of Science. Here, perhaps, is one of the few auspicious cases in which polemical intent reveals in fact an alteration in the structure of knowledge, a transformation in the *episteme* of perception, aesthetics and science. In this regard, let me make a couple of remarks.

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[2] Platon, *Timaios*, 48a-53c.

[3] Ibid., 31b.

[4] Johann Wolfgang Goethe, *Zur Farbenlehre*, edited by M. Wenzel, in: *Sämtliche Werke* (Frankfurter Ausgabe), Part 1, Vol. 23/1, Frankfurt/M. 1991, pp. 32 ff., pp. 203 ff. (the quotations in the text refer to this edition).

First: one must recognize in this precarious turn the result of a shift from the physics of light to a physiology of the eye. This shift is quite well known, and can first of all be understood as a shift in the literal, topographical sense. In reference to Soemmering and in a short note on the eye, Goethe shifted the question of vision from refractive properties of light and lens to the activity of the retina and simply stated that the retina is the organ of vision in general as well as of the awareness of colors ("Organ des Sehens überhaupt sowie des Gewahrwerdens der Farben").[5] Here the acts and passions of light ("Taten und Leiden des Lichts" [12]) correspond to the acts and passions of an organ; and here the physiological colors become condition and grounds for the definition of vision. For it is all the phantoms of the eye and sensory illusions, the after-images, the colored shadows and subjective aureoles, and the electric and physical stimulations of the eye, which document the functioning of the senses and the process of being affected. What once was disregarded as deceptive imagination now demonstrates the manifest activity of the organ.[6] Precisely where the eye represents nothing, where nothing represents itself in the eye, there appears the function of a vision that is not a vision of something, but rather an internal and elementary proceeding. Jonathan Crary has pointed out this shift by which Goethe's theory of colors moves into the nascent life sciences and makes the living body, the organ itself, into the scene of optical experiences. What the eye does and what vision is reveal themselves with closed eyelids.[7] The logic of representation is subtracted from the work of the senses, and according to Goethe's definition, vision functions "outwards" only through a "living interaction in itself" (31). With that, Goethe's physiological underpinning not only distanced itself from a culture of optical-physical experimentation, it also subscribed to the autopoeisis of the new, Romantic conception of the organ, to a manner of functioning for which each affect is self-affectation, each relation is a self-relation and hence simultaneously active and passive. The eye thus distances itself from the older model of the *camera obscura*, becomes organ rather than mere apparatus. This means that seeing something is only a borderline case of vision in general, which gains its particular opacity precisely because an event of perception interferes with every perceived event.

Against this backdrop, Goethe's famous altercation with Newton is a misunderstanding and a precise reference at the same time. A misunderstanding, because color, light and vision do not mean the same thing in Newton, point to diverse fields of practices, and thereby indicate a change in the culture of experimentation, a change from *inquisitio naturae* of physics to the investigational practices of experimental physiology. At the same time, it was a precise reference, because the correctness of a physical theory of light could not coincide with the manifest correctness of a physiology of the senses. This brings me to my second point: the polemical play of truth in Goethe's *Theory of Colors* flared up around the geometric construction of images and copies, and aligned itself with the dismissal of the *camera obscura* as the place where optical truth was produced. If Goethe called Newton's light rays pure "fiction" (120), and if he rejected the

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[5] Johann Wolfgang von Goethe, "Das Auge," in: *Sämtliche Werke nach Epochen seines Schaffens* (Münchner Ausgabe), edited by Karl Richter, Vol. 6.2: Weimarer Klassik 1798-1806, edited by Viktor Lange et al., München et al. 1988, p. 814.

[6] Cf. Monika Renneberg, "Farbige Schatten – oder wie die subjektiven Farben in die Welt der Physiker kamen und was sie dort anrichteten," in: Gabriele Dürbeck et a. (ed.), *Wahrnehmung der Natur, Natur der Wahrnehmung: Studien zur Geschichte visueller Kultur um 1800*, Dresden 2001, p. 239.

[7] Jonathan Crary, *Techniken des Betrachters. Sehen und Moderne im 19. Jahrhundert*, Dresden 1996, pp. 77 f.

terms of “line drawings” and geometric representation (cf. 1143), then he addresses images and visibilities from which the structure of form and geometrical order has been extracted. Newton’s physics of light rays thus exemplifies the construction of an optical-tactile space in which the vision of the visible is measured as perspective construction. At one point of his polemic, Goethe describes this as a sort of trick in Newton’s method. “One can compare,” Goethe writes, “Newton’s presentation to stage set that has been painted in perspective, in which all lines converge and can be seen correctly at a single point. Newton and his followers cannot bear that one steps a little to the side in order to peek behind the scenes. All the while they assure the spectator, whom they hold to his seat, that there is a truly closed and impenetrable wall before them.”[8] To use a remark of Jacques Lacan, one could say that vision, as the perception of color, occurs for Goethe only where the optical and geometrical separates from the visual[9] – that is, where the visual image loses its plasticity, and tactile referent; and leaves behind nothing but a non-spatial modulation of color, and a trickling of light and dark. Already Joseph Priestley – in the notorious debate over the blind who can see again – mentions this as the other side of vision:

> One thought that [a young man who had long been blind] would soon learn to understand what a painting represented, but the opposite showed itself to be true. For, two months after the removal of his cataract, he suddenly made the discovery that paintings presented bodies, heights and depths; till then he had seen them only as colored surfaces.[10]

Vision, and the visual, the pure vision all fall out of the order of the gaze; the eye, Goethe writes explicitly, sees “no form” and finds itself a vessel containing only a mixture of “light, dark and color” from all the things it sees (24). In other words, “the eye sees no shapes, it only sees what differs in light, dark or color.”[11] In its proper sense, color is a visual function and therefore not an object; it is a-topical and removed from all spatial coordinates. The space that it occupies is dimensionless and fluid.

At this point – and this is the third aspect I want to mention – a particular type of empiricism comes into play, an empiricism whose concept Goethe grappled with awkwardly in various places. For if he followed Kantian guidelines in demanding a “Critique of the *Senses*”[12] in addition to a “Critique of Pure Reason,” it was not merely to topple Kant’s order of the faculties and to repatriate sensibility from its exile in the lower register of the faculties. And it was not merely to liberate color from the region of contingent pleasure and from the dictates of form and drawing,

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[8] “Man kann die Newtonsche Darstellung einer perspektivisch gemalten Theaterdekoration vergleichen, an der nur aus einem einzigen Standpunkte alle Linien zusammentreffend und passend gesehen werden. Aber Newton und seine Schüler leiden nicht, dass man ein wenig zur Seite trete, um in die offenen Kulissen zu sehen. Dabei versichern sie dem Zuschauer, den sie auf seinem Stuhle festhalten, es sei eine wirklich geschlossene und undurchdringliche Wand” (p. 325).

[9] Jacques Lacan, *Die vier Grundbegriffe der Psychoanalyse. Seminar XI*, Weinheim et. al. ^{4}1996, p. 91.

[10] “Man glaubte, [ein langer blind gewesener, junger Mensch] würde bald verstehen lernen, was Gemälde vorstelleten, es zeigete sich aber das Gegentheil. Denn zween Monate, nachdem ihm der Star gestochen war, machete er plötzlich die Entdeckung, dass sie Körper, Erhöhungen und Vertiefungen vorstelleten; bis dahin hatte er sie nur als buntscheckichte Flächen angesehen” (Joseph Priestley, *Geschichte und gegenwärtiger Zustand der Optik, vorzüglich in Absicht auf den physikalischen Theil dieser Wissenschaft*, Leipzig 1776, p. 513.

[11] Goethe, “Das Auge,” p. 814.

[12] Cf. Johann Peter Eckermann, *Gespräche mit Goethe in seinen letzten Jahren seines Lebens*, edited by R. Otto, München ^{2}1984, p. 274.

and to move it into the realm of aesthetic judgment.[13] The structure of the *Theory of Colors* reveals a procedure or rather an experimental practice that was oriented less towards subsumption than towards the "diversification" of color phenomena; "manifold," "diversity" and "diversification" ("mannigfaltig," "Mannigfaltigkeit," "Vermannigfaltigung") are, by the way, the most common keywords in Goethe's entire text. In any case, Goethe stated the facts of the matter and the problem very precisely: the *Theory of Colors* is less about the question of "what color is" than about *how* they appear (24). Accordingly, the series of experiments and observations, a series that does not lend itself to a clear overview, is laid down in such a way that it continually varies the situations, conditions and the manners in which colors take effect, thereby motivating a method that is improvisational and explorative.[14] With Gilles Deleuze one could say: while the question "what is?" has been burdened since Plato with an onto-theological weight that blocks the diversity of the empirical world through its relation to substance or essence, this very world requires a diversification of the form of the question itself. Not "what is?," but rather "where?," "when?," "in which case?," "under what circumstances?," and "in what manner?" etc.[15] This also seems to be Goethe's concern, for colors represent neither objects nor substances, but rather "activities" (244), and thus the *Theory of Colors* deals first and foremost with the registry of the contingent and accidental events that are the colors themselves.

This means, first of all, that these color-events appear in a field that turns out to be a zone of indistinction between subject and object, in a field in which the positions of subject and object have, in the best cases, developed only to a embryonic state. Goethe described this field again and again as a field of effects and relations between forces, in which color effects are brought forth through "pressure, breath, rotation, warmth and various types of movement and alteration" ("Druck, Hauch, Rotation, Wärme, durch mancherlei Art von Bewegung und Veränderung" [225]). The "foundation of the entire theory" are evidently sensations that take immediate effect upon the nervous system, vibrations, stimuli, percepts and affects, in which nature acts as diffuse pulse generator and erases the boundary between inner and outer, proper and alien. As the entire arc of the *Theory of Colors* traces a gradual transition from subjective to objective colors, this zone of diffusion becomes the actual place of their manifestation. Color is neither *on* a thing nor *in* a subject. The Czech physiologist Johann Evangelista Purkinje, an adherent of Goethe's theory, formulated this in his physiology of vision:

> One must abandon oneself totally to outer impressions, and take the visual field merely as a surface of lingering, fading or changing perceptions that find themselves beside and apart from one another with neither fore- nor background, just like a child of nature sees a painting as a mere surface of different colors. By means of this abstraction which is at the same time the most specific empiricism, one moves into the sphere of the vivid organic subject-object, in which each material process is simultaneously an ideal, subjective one, in which each movement therefore is a true movement, and in which even appearance becomes truth.[16]

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[13] Cf. Ursula Schuh, *'Die Sinne trügen nicht.' Goethes Kritik der Wahrnehmung als Antwort auf virtuelle Welten*, Stuttgart et al. 1999, pp. 13 f., p. 30 f.

[14] Cf. Friedrich Steinle, "'Das Nächste ans Nächste reihen': Goethe, Newton und das Experiment," *Philosophia naturalis* 39/1 (2002), pp. 141-172.

[15] Gilles Deleuze: *Differenz und Wiederholung*, München 1992, pp. 239-240.

The facts of the matter are here clearly outlined: color events play in a field that is neither inside nor outside, neither subjective nor objective, and thereby appear both empirical and ideal, deceptive and true at the same time. It is the field of effective simulacra.

If one takes such formulations as the signs posted along the path of colors, they clearly lead one into an empirical area that lies beyond all empiricity, or in Purkinje's words, "on the outermost borders of empiricity."[17] Let me briefly explain. According to the Kantian agenda of transcendental aesthetics, the faculty for empirical objects, that is, the faculty to differentiate between bodies, forms and shapes, arises only out of the synthesis of the manifold, in other words through a process in which not only a figural synthesis of intuition (Anschauung) is produced under the dominion of the imagination, but also an intellectual synthesis of cognition under the leadership of the understanding. By such means an empirical field of the corporeal is put in place, in which objects of possible experience constitute themselves. Disparate, diverse sense-data are linked to the unity of objects, to things whose identity remains guaranteed through the effective cooperation of the faculties: it is the self-same object that one sees, hears, feels or remembers. According to Kant, the empirical field is the milieu in which one ascends under the chairmanship of the forms of intuition from the manifold to the object of intuition, from the sensual to the intelligible. It is the field of recognition.

Given this background, one must take note of a double sense in Goethe's path of colors, a double sense in which "being colored" differs from color as the property of something. For on the one hand, colors are the epitome of the sensible, the last and utmost exponents of the visible; on the other hand, however, in the world of empirical things they are accredited only as properties among other properties and are therefore, to some extent, imperceptible, non-sensible and invisible. As placeless and objectless qualities, as mere color events, they are that in which vision and the visible manifest themselves as such, as "vision alone" and the "only-visible," as an unborn and non-referential world of color that realizes itself in the region of empirical things once again with nothing other than attributes of color. In color what is imperceptible in perception appears. And Goethe's colors are events in the concise sense, if one understands by "event" not a mere givenness, but rather the transition from a potentiality to its realization; the event preserves what remains in reserve behind every actualization.

In this tension Goethe's project used the guidance of colors to unlock the empirical anterior to all empiricity. Again with Gilles Deleuze one would have to recognize here a *videndum* in color and in the colorfulness of color: something which can only be seen but remains invisible in empirical usage; not visible existence, but rather an existence of the visible; not anything given, but rather something through which the given is given.[18] Goethe's efforts were thus led to cross

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[16] "Man muß sich ganz den äußern Eindrücken hingeben, und das Gesichtsfeld bloß als eine Fläche von bleibenden, vergehenden oder wechselnden Empfindungen, die nur neben und außereinander ohne Vor- und Hintergrund sich befinden, nehmen, so wie der Naturmensch ein Gemälde sieht, als eine bloße Fläche von verschiedenen Farben. Durch diese Abstraktion, die doch zugleich die speciellste Empirie ist, versetzt man sich rein in die Sphäre des organischen lebendigen Subject-Objects, in welchem jeder materielle Vorgang zugleich ein ideeller, subjektiver ist, also in diesem Sinne jede Bewegung eine wahre Bewegung, und wo auch der Schein zur Wahrheit wird" (Johann E. Purkinje, *Neue Beiträge zur Kenntnis des Sehens in subjectiver Hinsicht*, Berlin 1825, pp. 52 f.).

[17] Johann E. Purkinje, *Beiträge zur Kenntnis des Sehens in subjecktiver Hinsicht*, Berlin 1823, 6 ("an den äußersten Gränzen der Empirie").

[18] Deleuze, *Differenz und Wiederholung*, pp. 182 f.

beyond the landscape of empirical things into a horizon of pure visibility; they were led to release a sensibility in color that transcends the faculty for empirical things. Only in this way can it be understood how the modulation of light, dark and color allows for the emergence of a much more perfect visible world than the actual one could ever be ("eine weit vollkommner sichtbare Welt, als die wirkliche sein kann" [24]). And only in this way can it be understood that one experiences the visibility of things independently of the objects of experience, and that therefore painting is ultimately "truer to the eye" than "actuality" ("das wirkliche") itself.[19]

This pure visibility provokes an empiricism that one would have to call an extended or higher empiricism, in the very sense that Goethe spoke of "experiences of the higher sort"[20] and an increase or extension of empiricism to "unconditionality."[21] In this way, Goethe's difficult definition of experience, empiricity and empiricism is related to sensible effects in which the "conditioned is also the unconditional" and "the unconditional conditions itself and thereby makes the conditioned into its likeness."[22] Empiricism itself becomes transcendental. The world of empirical things is exceeded with the events and activities of color, where one encounters not the forms of intuition as the condition of possible experience, but rather something purely sensible, "only-visible," and a "pure phenomenon." Here the condition is not defined any more widely than the conditioned itself; one encounters an activity that in empirical sensibility is concealed by qualities and objective characteristics and which provokes a transcendental usage of the faculties.[23] This would be the place where – contrary to Kant – a physiology of vision coincides with an aesthetics of perception and ultimately with a theory of art.

Let me come to the end. In this turn of Goethe's towards a higher empiricism one has to conceive of the *Theory of Colors* as the beginning of an aesthetics that with the quality of the sensible aims at a life before or after the life of empirical things. The union of physiology and aesthetics leads into a region of pure intensities, in which contour fades, shape extinguishes and space disappears. In this realm of colors, figure and ground become indifferent and escort the observer to the verge of dizziness, which, if you will, marks the death of the empirical observer. That will be where the path of color leads in the nineteenth century.

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[19] Goethe, "Das Auge," p. 814.

[20] Johann Wolfgang Goethe, "Der Versuch als Vermittler von Subjekt und Objekt," in: Sämtliche Werke (Münchner Ausgabe), Vol. 4.2: *Wirkungen der Französischen Revolution 1791-1797*, edited by Klaus H. Kiefer et al., München 1986, p. 330 ("Erfahrungen der höheren Art").

[21] Johann Wolfgang Goethe, *Maximen und Reflexionen*, in: *Sämtliche Werke* (Münchner Ausgabe), Vol. 17: *Wilhelm Meisters Wanderjahre. Maximen und Reflexionen*, edited by Gonthier-Louis Fink et al., München 1991, p. 946 (1373).

[22] Ibid., p. 946 (1371, 1372): "Wie das Unbedingte sich selbst bedingen, Und so das Bedingte zu seines Gleichen machen kann;" "Daß das Bedingte zugleich Unbedingt sei."

[23] Cf. Johann Wolfgang Goethe, "Das reine Phänomen," in: *Sämtliche Werke* (Münchner Ausgabe), Vol. 6.2, p. 821: The "pure phenomenon" takes its place where one inquire "about conditions under which phenomena appear;" these conditions are, however, no forms of intuition, but rather demand "non-speculative" work that elevates the common understanding of mankind into a "higher sphere."

Commentary on the Shape of Experiments

Andrew Pickering

> We have to subvert the problematic of representation.
> – Hans-Jörg Rheinberger, this conference.
>
> We ought to have a child-like wonder.
> – Ian Hacking, this conference.
>
> Think a thought ... "unsupported by sights, sounds, smells, tastes, touchables, or any other objects of the mind." Can you do that?
> – Stafford Beer, quoting the *Diamond Sutra*.

The organizers of the conference suggested my closing commentary might include some thoughts on where we could go next in the history of experiment. I was very struck by Hans-Jörg Rheinberger's remark that "we have to subvert the problematic of representation," and that is the topic I want to pursue. The question is something like: what would one look at, and how would one write about it, if one wanted to get away from the canonical image of science as primarily a representational activity? I cannot actually see the future, so my strategy in what follows is to comment on the state of the art as exemplified in the papers presented at this meeting. One can detect several tactics at work in the subversion of what I call the representational idiom in science studies, and I can make a list.

1) Rheinberger's own approach is to subvert the problematic of representation by studying it, and showing us radioactive tracers in biological research as material-theoretical-semiotic objects, as hybrid *operators* that shunt between things and representations. This tactic subverts the problematic of representation by dissolving it. We do not need to worry about representation as some obscure correspondence between theory and things (as in traditional philosophy of science) because now we can see that they are glued together.[1]

Fine, but still, I want to say, we remain somehow in the space of representation. Instead of representations of matter, we have matter as representation, and I am left a bit unsatisfied. Rheinberger's history and philosophy are admirable, but I think there is something we need to dwell upon before we get to his analysis, and that something has to do with non-representational aspects of matter. The question that strikes me is: how can we get matter into focus in our historical writing as something prior to representation? How can we enrich our imagination of, and our discourse about, matter itself? How can we thicken up our appreciation of matter?

2) If I ever met a spokesman for brute matter in itself, it must be Ian Hacking. In his public address at this meeting, he told hundreds of people that he is, in effect, in love with Bose-Einstein

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[1] Gernot Grube's study of STM struck me as formally similar to Rheinberger's study of radioactive tracer techniques, but Grube drew a diametrically opposite conclusion: one cannot trust STM representations at all. I leave the authors to resolve this contradiction.

condensates, and that we should be too. This is a great maneuver for pulling us out of the space of representation, and, of course, we all have Hacking to thank for pushing and pulling us in this direction from his classic book, *Representing and Intervening*, onwards.

And yet ... somehow Hacking's examples don't quite get me to an appreciation of matter itself. Perhaps this is because of his enthusiasm for named and more or less understood phenomena, and perhaps it's also because I used to be a physicist. I know what a boson is and how lots of them can fall into the ground state at once, and I can't help hearing Hacking as speaking in the representational idiom, even if he isn't.

3) Instead of talking about the exotic phenomena of physics, therefore, we should perhaps focus instead on very familiar ones. In Bose-Einstein condensation, atoms fall down into the ground state, so perhaps we should focus on something as simple as falling down – which was the topic of Henning Schmidgen's paper. He offered us a very nice story about how this very familiar phenomenon, that we are all acquainted with in a pre-representional fashion, has historically been seduced into helping out in the sciences of experimental physiology and psychology – in the shape, we should note, of performative devices, machines (including guillotines).

Or perhaps we should think of something even closer to home: our own bodies as capable of strange performances, things that surprise us. I think here of Katrin Solhdju on self-experimentation and anaesthetic revelation and of Phillip Felsch's speechless Alpinists.

All of these papers help, I think, to conjure up an imagination of matter as prior to representation.

And, continuing this theme in another way, we could think of Mark Hansen's account of the information arts. One message of his paper is that we can build machines having their own obscure dynamics, which we can interact with but not control. We could take such machines as our model for matter more generally, as instances of ontological theatre, as I would call it.

These are all valuable examples of the kinds of things one might look at, and of ways one might write about them, if one wanted to bring matter to life.

4) Now for another strategy for breaking the spell of representation. The paper that I found most suggestive at the meeting was Falk Müller's on gas discharge research. I was struck by the sheer variety of the objects and phenomena he talked about – all those strangely shaped vacuum tubes, electrodes, patches of light glowing and moving in space. Somehow it is important that this was a failed research program. We do not have any sort of detailed explanation of all the things they found back in the 19th century, so, unlike the situation with Hacking's condensates, I can't possibly mistake what Müller is talking about for some set of equations that I learned at university. And because of that I personally am inclined to take the field of objects and phenomena that Müller told us about as a model for what the world in general is like prior to representation – as a kind of Ur-state – a very lively place full of shapes, colors, lights, endlessly variable. And if the world does not immediately strike us in that way, we should be curious about why that is.

Here I am attracted to a numinous concept from Deleuze and Guattari, the "body without organs," the BwO. Deleuze and Guattari are not easy to understand, but according to Henning Schmidgen one can think of an embryo as a model for the BwO – as unstructured yet manifesting the possibility of form, as pure possibility, pure becoming. The field of gas discharge research as a BwO is what I got from Müller's paper.

Where does it get us to say things like that? First, Müller offers us yet another way of writing the history of science outside the representational idiom, another possible model for the historiography of science. Second, his paper precipitates a further cascade of problematics. How do we induce a BwO to take form; how do we get to grips with such a thing? And then we can do historical studies to explore such questions.

Müller himself talked about the three phases that Hittorf's experiments moved through – playful, exploratory and then quantitative measurements. These are all strategies for domesticating a BwO – which did not quite come off in this instance. John Tresch talked a bit about Ampère's attempts to get to grips with electromagnetic phenomena (and I think here also of David Gooding's studies of Faraday). Ursula Klein's paper reminds us that even when we think we have got matter pinned down it can always surprise us – all those new organic compounds that suddenly popped out in Liebig's laboratory. We could take her paper, too, as drawing our attention to an aspect of matter prior to representation: this sense of a performative excess of matter, as the situation we are always in.

We could also remember at this point that there are all sorts of reasons we struggle with BwOs. John Tresch pointed to a spiritualised alchemical impulse in Ampère's research; Sven Dierig discussed Du Bois-Reymond as a classical aesthete; Helmar Schramm and Julia Kursell both examined research in which music and physics came together as part of a unitary assemblage; Andreas Hiepko offered us our own language as a BwO. There are many angles, then, on the struggle with BwOs, only some of which lead into science and representation.[2] One should surely think of production, consumption and capital here, a topic that Ursula Klein touched upon in her opening paper but that subsequently went under-represented, dare I say.[3]

What is the moral of all this for the future of the history of experiment. Is it that anyone should stop what they are currently doing? No. It is more a question of how we think and write about what we are studying. And this, in turn, is partly a question of balance. I do think we need more studies that come at science and matter outside the representational idiom, which is why I have run through a list of papers presented at the conference that are, for me, exemplary of ways to do this, models for future studies. And I think we should have such examples in mind even when representation is our topic, just because they serve to de-naturalise and make it strange again: why representation? If I were editing the Proceedings of the meeting, I would put Falk Müller's paper first (followed by Schmidgen, Solhdju, Felsch and Hansen, say) just to remind readers of what the material world is like, and of all the problems that have been wrestled with before Rheinberger's biologists can get to work with their radioactive tracers. That would be one way to subvert the problematic of representation.

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[2] One is reminded of Paul Feyerabend's question: "what's so great about science?" Historians of science often forget that scientific representation is not the necessary destination that matter is always seeking.

[3] See also her edited special issues of *Perspectives on Science* 13/2-3 (2005), on "technoscience."