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Autor: Herring, Peter / Sichau, Christian
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Instruments and experiments between the laboratory and the museum

Peter HEERING[a] and Christian SICHAU[b]

Abstract

Following the recent developments in the historiography of science, experiments as well as experimental practice have moved into the focus of many historical studies. Experimenting is no longer seen as being fully determined by theoretical imperatives. Many new questions arose and in order to find answers the whole process of experimenting has been put under scrutiny. This paper discusses how the "replication method" has recently contributed to get closer to the scientific practice, and how its requirements can help science museums to better find a new role for the future.

Keywords: *experimental practice, history of science, replication method, science museums*

Introduction, Part I: Entering a Science Museum

In the early 1920s, the Deutsches Museum was making plans to include Einstein's Theory of Relativity into its new exhibitions[1]. On various levels preparations were made: To test whether the theory could be explained to a wide audience, the curator of physics, Franz Fuchs, gave a talk on what he considered to be the essentials of the theory to the staff of the museum: mechanics, joiners, sculptors and so on from the various Museum's workshops[2]. Further, Einstein was asked directly for advice, but Einstein was hesitant and referred Fuchs instead to Hans Thirring, Professor of Physics at Vienna who had just written a book on the Theory of Relativity (Thirring 1921). So, Fuchs wrote a letter to Thirring – and all he got was a table, taken out of the book[3]. In the accompanying letter Thirring simply wrote that this table might not be of much use – but explaining the table would mean to bring the whole text of the book (Fig. 1)[4].

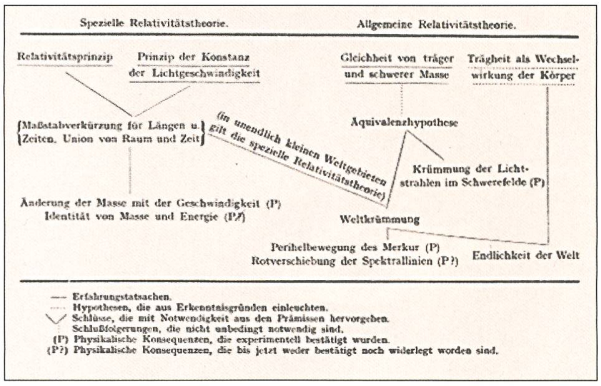


Fig. 1. Table from Thirring's book. Photo: Deutsches Museum.

1 A short description of the events described in the following was given by Franz Fuchs, curator of physics at the Deutsches Museum from 1906 to 1951 (Fuchs 1957). Fuchs included some quotations from his official correspondence which has been (in part) preserved in the Archives of the Deutsches Museum. All references are to these original sources unless otherwise specified.

2 A manuscript of his talk is kept in the Library of the Deutsches Museum: F. Fuchs, "Lichtbild-Vorträge, 1918-1936".

3 The table is at the very end of the book.

4 Letter from H. Thirring to the Deutsches Museum, 24.03. 1924. Archive of the Deutsches Museum. Thirring wrote: "Ich verhehle mir aber durchaus nicht, daß diese Tabelle, aus dem Zusammenhang meines Buches gerissen, dem Laien vollständig spanisch vorkommen muß. Wollte man nun eine erläuternde Legende dazu schreiben, so müßte man, um alles zu erklären, wiederum fast den ganzen Text des Buches bringen."

a Research Group on Higher Education and History of Science, Physics Department, Carl-von-Ossietzky-Universität, Oldenburg, Germany.

b Deutsches Museum, Munich, Germany.

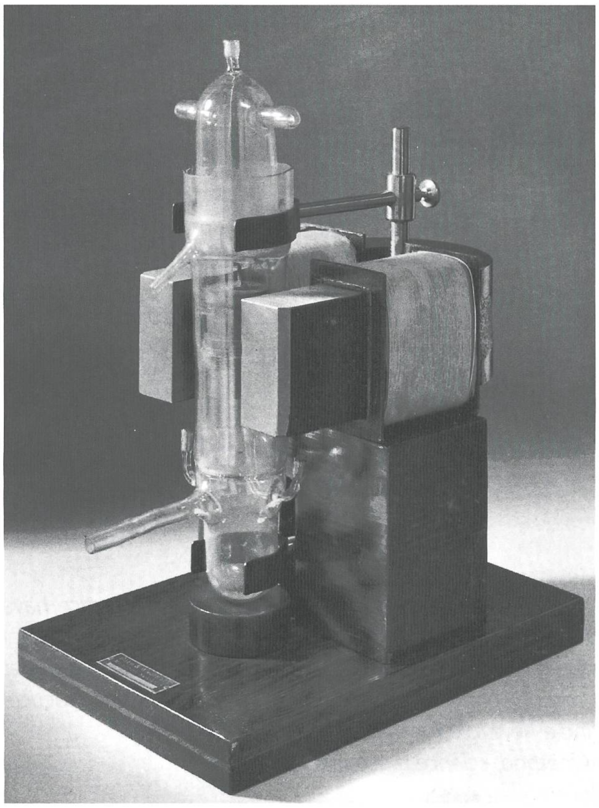


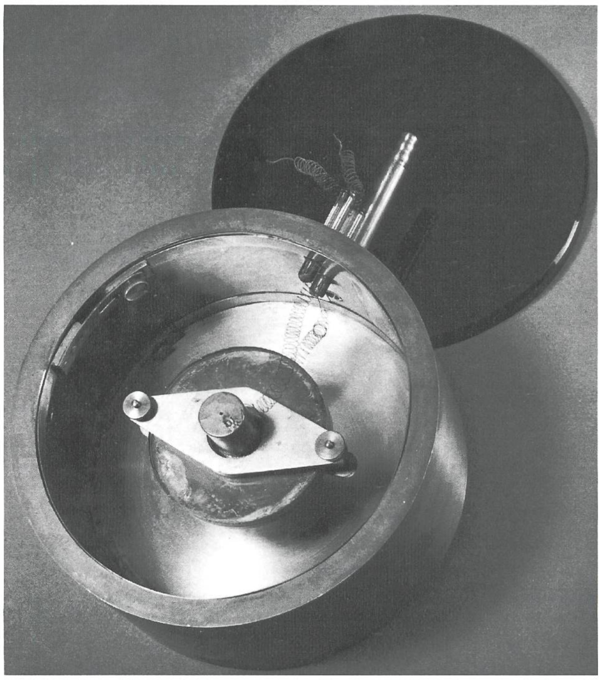
Fig. 2. Kaufmann's apparatus. Photo: Deutsches Museum.

At the very same time, the very same person, Franz Fuchs, was organising another new exhibition on "electrical rays" ("Elektrische Strahlen"); he wrote letters to various institutions and scientists asking for instruments which could be exhibited at the Museum. From Walter Kaufmann, Physics Professor at the University of Königsberg, the museum only got the apparatus he had used in 1906 to measure the increase of mass of very rapid electrons (Kaufmann 1906, Fig. 2), since all of his other instruments had been taken apart, were destroyed or lost[5]. Alfred Bucherer, Professor of Physics at the University of Bonn, offered the apparatus he had used in 1906 to measure the charge to mass ratio of the electron (Fig. 3).

Both experiments, Kaufmann's and Bucherer's, had played a very important role in the early history of the Special Theory of Relativity; Bucherer had published his experiments even under the title: "The experimental confirmation of the Principle of Relativity" (Bucherer 1909)[6]. Nevertheless, in Fuchs' talk to the museum personnel neither of these experiments nor any other was mentioned at all, except the notorious Michelson-Morley-Experiment, and they also did not form part in the planned exhibition on the Theory of Relativity. They were – at least in this context – considered to be of no importance.

This episode from the beginning of the 20th century serves to highlight some of the problems we are confronted with in the long history of exhibiting science and its history. Although at least on a theoretical level we have learned a lot and a discussion has been going on in recent years of presenting science not as a set of ideas but as a process, we have not been as successful as we might wish[7]. The traditional views as expressed by Fuchs are still present today: Science museums are mainly about scientific theories, concepts or ideas; thus, in many exhibitions, instruments and experimental set-ups are at best only manifestations of these abstract theories, of "ideas" which otherwise could not be "exhibited" in museums. Instruments and experimental set-ups are

Fig. 3. Apparatus donated by Bucherer. Photo: Deutsches Museum.



[5] The letter to Kaufmann, signed by Fuchs, dates from 15. October 1921, the answer by Kaufmann is from the 27. October 1921; here Kaufmann explained the situation to the Museum and offered the apparatus from 1906. All letters are in the Archive of the Deutsches Museum. According to a museum-guidebook the apparatus (as the one by Bucherer, see below) was indeed on display at least in the 1920s; see: "Amtlicher Führer durch die Sammlungen", 2nd edition, 1928.

[6] For a detailed discussion of the experiments by Kaufmann and by Bucherer see (Miller 1981), (Hon 1995). See also the contribution of Lacki and Karim in the present volume.

[7] See for example (Butler 1992), (Durant 1992), (Hochreiter 1994), (Perace 1996), and (Arnold 1996).

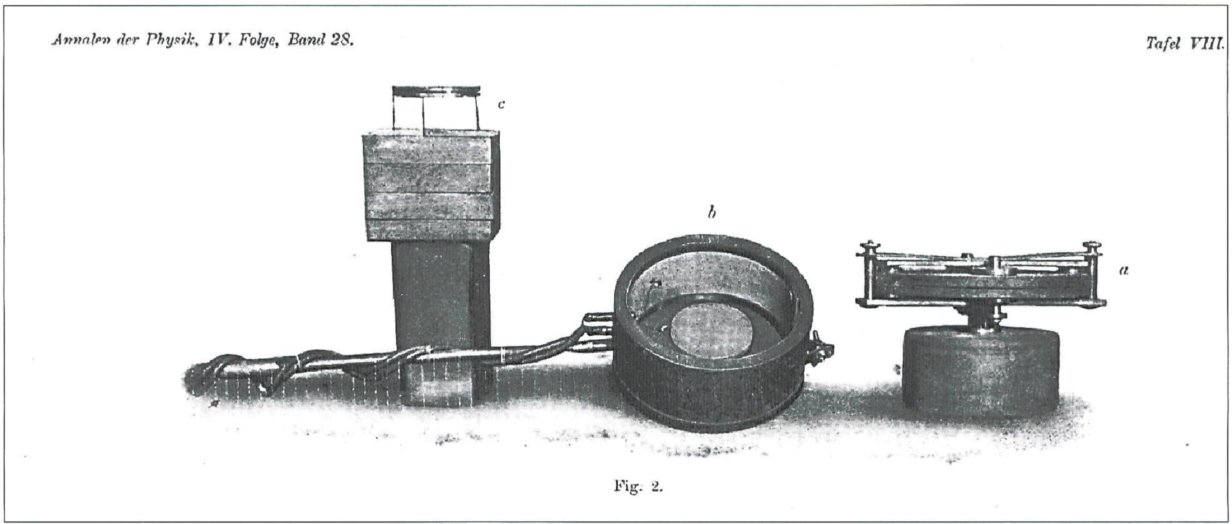


Fig. 4. Bucherer's apparatus as shown in the publication. Photo: Deutsches Museum.

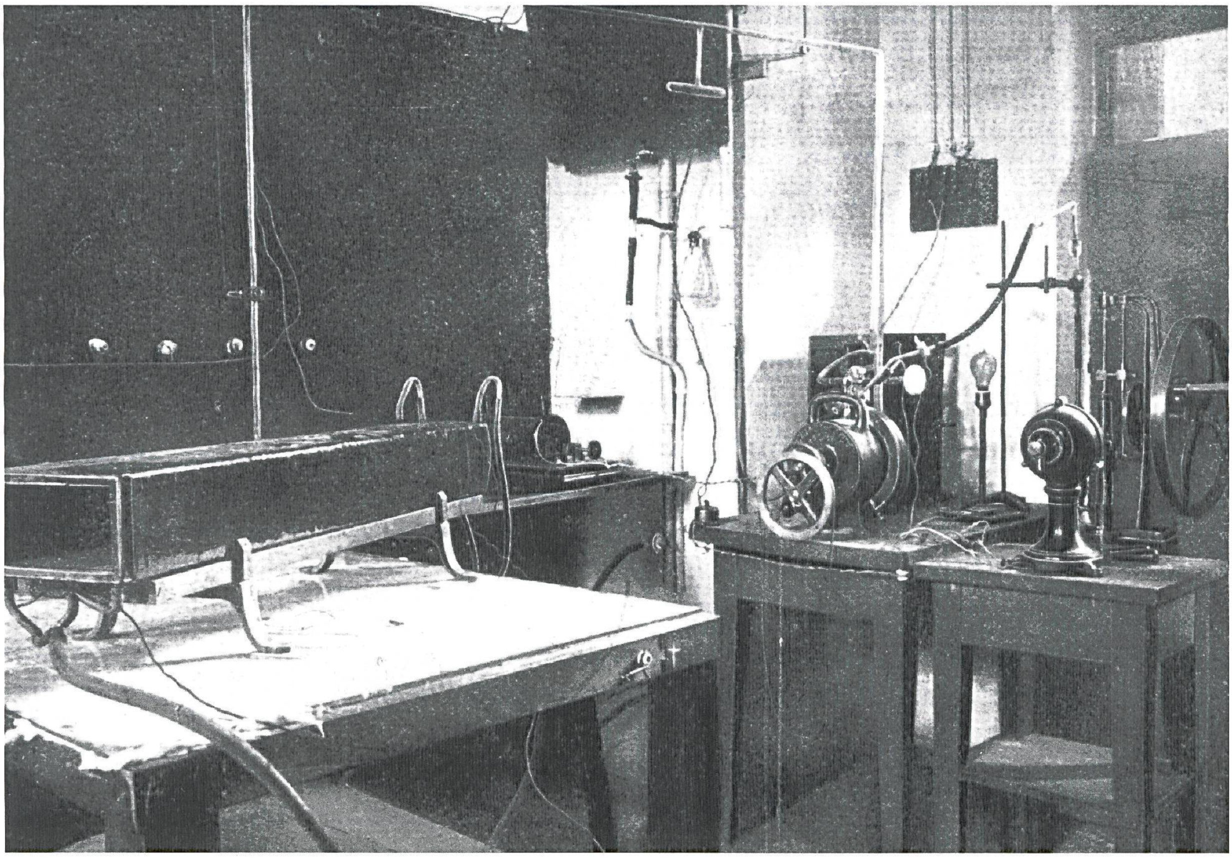


Fig. 5. Picture of Bucherer's laboratory. Photo: Deutsches Museum.

not real objects possessing their own identity and history – histories which quite often are different from those traditionally told[8].

Further, instruments like Kaufmann's apparatus whose experiment could not be subsumed under the heading of "successful confirmation" of a theory, have

[8] An apparatus of the Hallensian physicist E. Dorn offered to the Museum for its exhibition on "Electrical Rays" came even to be regarded as almost useless for the museum's purposes since Dorn had changed it in order to do new experiments. As his colleague Gustav Mie wrote: "*Diese neue Apparatur hängt also mit den Dingen, die Sie für das Deutsche Museum interessieren, eigentlich nicht zusammen*"; Archive of the Deutsches Museum.

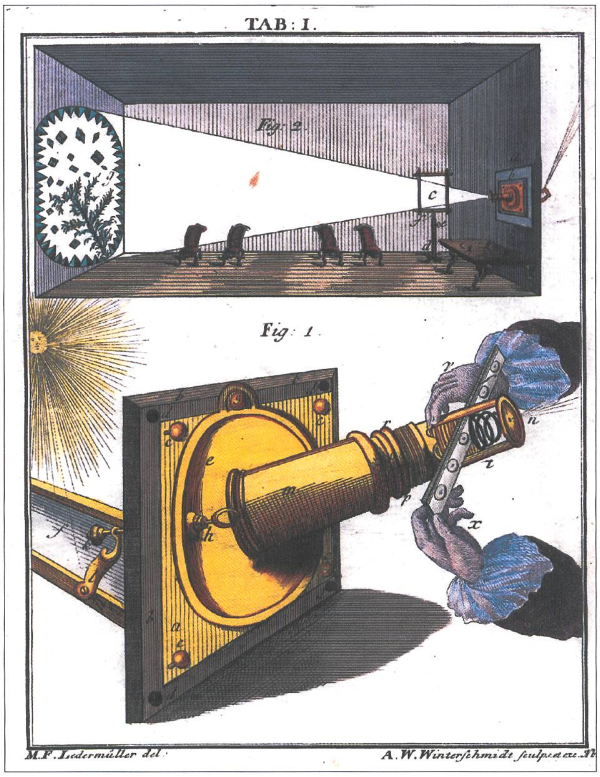


Fig. 6. Solar microscope and its use for demonstrations. Photo: Deutsches Museum.

difficulties of finding a place in a Museum. Of course, there were a lot of instruments in the exhibition on "Electrical Rays", but they were only used to illustrate curious effects of nature or technological progress (in this case of X-rays) in form of a genealogy ("Entwicklungsreihe")[9].

But it is not only a lack of "good will". Another difficulty we face in Museums can also be illustrated by this episode just told. What we are left with in many cases are just small parts of the apparatus. Take for example the apparatus of Bucherer: Fig. 3 shows the apparatus which had been given to the Deutsches Museum; it is only a part of the apparatus used by Bucherer which can be seen in Fig. 4; and within Bucherer's publication you will even find a photography of the whole set-up of the experiment in the laboratory (Fig. 5). Obviously, it is very difficult to even identify the apparatus given to the Museum within this experimental set-up![10]. Consequently, it would be very difficult to use these remaining parts for an illustration of "scientific practice" without any further research or much work on the presentation of the apparatus.

Introduction, Part II: Entering a Library

However, taking publications and the material held in archives as our starting point for the analysis of scientific practice makes the task no less easy. Let us try to illustrate this with a standard apparatus from the late eighteenth century that can be found in most museums: The solar microscope, a device that served for the projection and demonstration of microscopic specimens (Fig. 6). It consisted of a mirror that reflected sunlight onto a condensing lens, this lens focuses the light onto the sample under examination and a Wilson pocket microscope was used to project the image of the object. This apparatus had been modified by one of the natural philosophers who tried to establish himself as a public demonstrator. By removing the Wilson pocket microscope from the set-up, he transformed the apparatus into a research device he named helioscope. With the help of this device he attempted to visualise the 'fluide igné', an imponderable that was responsible for heat phenomena (Fig. 7).

Moreover, he used the helioscope also in experiments that were designed to demonstrate that white light is diffracted due to the attraction of the light particles by solid objects, thus attempting to overthrow the Newtonian theory of optics. He failed with his approach, however, his experiments and its difficulties remained obscure.

This researcher was born some 100 km north-east of Geneva and became well-known as a political journalist during the French Revolution: Jean Paul Marat. He published several huge monographs containing all in all more than 500 experiments[11], about one third of them were carried out with the helioscope. However, none of his instruments did survive: no science museum is displaying any helioscope. Besides the

9 This can be seen clearly in the museum guidebook on the exhibition "Electrical Rays, X-Rays" from 1928 (quoted above). In the introduction these two aspects were combined: The exhibition was justified by a reference to the enormous practical importance of X-rays for "the suffering people" ("die leidende Menschheit") and by stressing its importance for the research on the "nature of electricity" ("Wesen der Elektrizität"). The first section, in which the instruments of Kaufmann and of Bucherer were mentioned, was entitled: "The Development of Discharge Tubes" ("Entwicklung der Entladungsröhren").

10 It is inside of the 103 cm long solenoid cooled by water on the table.

11 Jean Paul Marat: Découvertes sur le Feu, l'Électricité et la Lumière, constatées par une Suite d'Expériences nouvelles qui viennent d'être vérifiées par MM. les Commissaires de l'Académie des Sciences. 2. ed., Paris: Clousier 1779; idem: Recherches physiques sur le feu. Paris: Jombert, 1780; idem: Découvertes sur la Lumière; constatées par une suite d'expériences nouvelles: qui ont été faites un très-grand nombre de fois sous les yeux de MM. les Commissaires de l'Académie des Sciences. London & Paris: Jombert, 1780; idem: Recherches Physiques sur l'Électricité. Paris: Clousier, 1782.

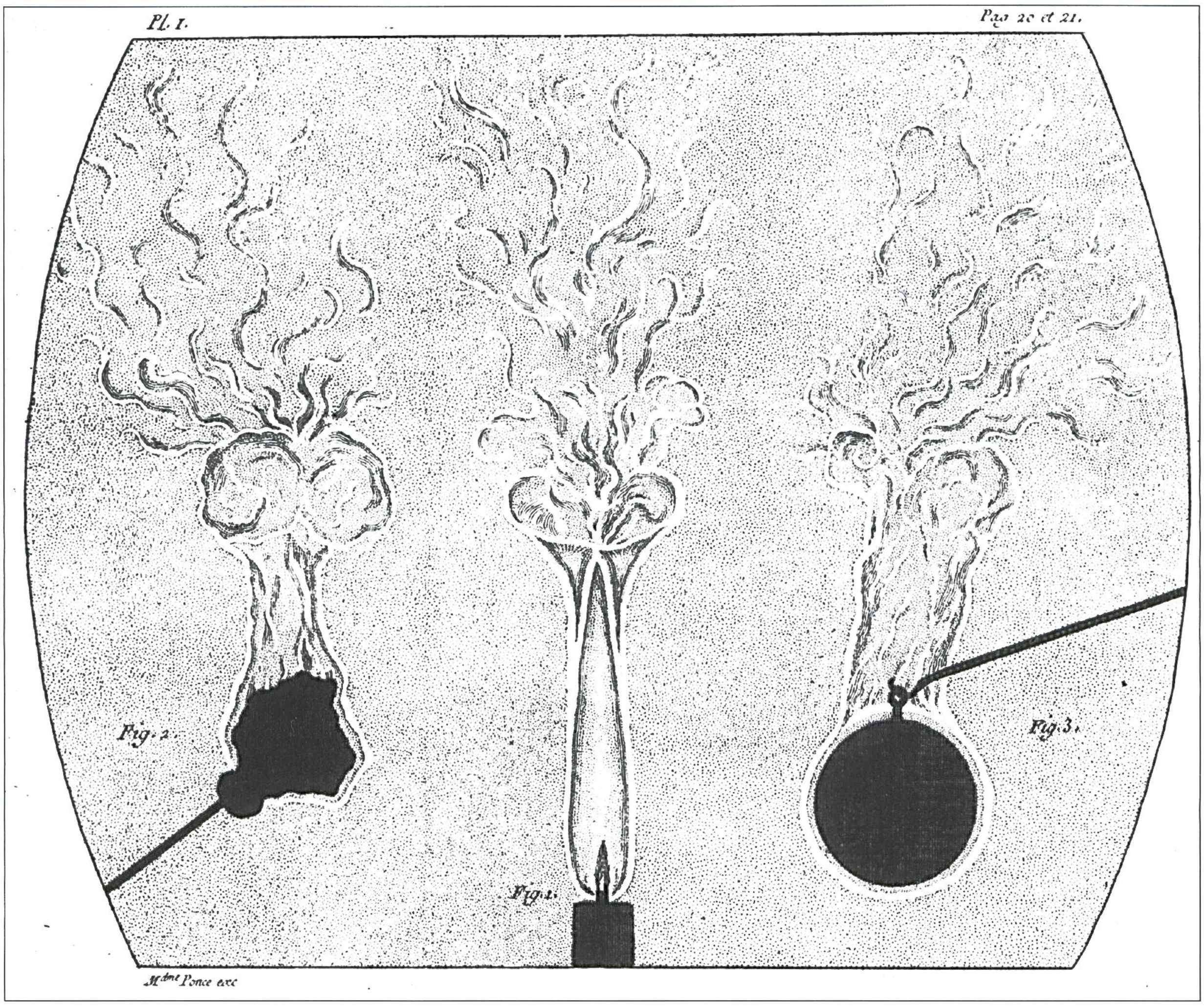


Fig. 7. Shadow projection of the 'fluide igné'. Photo: Bakken Library.

rejection of his scientific approach, his political role may also be responsible for the loss of his instruments[12]. Being not represented in museums is one thing, however, there are also problems for the historiography in respect to Marat's work: In his publications, Marat did not describe the helioscope in detail but just mentioned the retailer and his address, sufficient for his contemporaries but not for modern historians of science. Moreover, unless Marat is supposed to be either ignorant or a charlatan, it is not clear how he could publish his experimental findings – and why the committee of the Academy was not able to verify them. To make things more difficult, Marat had designed his experiments in order to be able to demonstrate them to an audience, thus it is not clear what the intention of his written publication might have been. These few remarks shall suffice to illustrate the difficulties of giving an adequate account of Marat and his scientific practice. At this point it can be questioned why we should be interested in Marat's scientific practice, however, we hope to make clear during our discussion in this contribution that developments of experimental practice can be particularly understood if not only the retrospectively successful experiments are analysed but also those who were rejected for various reasons. Such a symmetrical approach may be helpful to understand the development and nature of science more deeply.[13]

This claim can be taken as a result of recent developments in the historiography of science: Experiments as well as experimental practice have moved into the focus of many historical studies[14]. Of course, we can-

12 It had been argued very convincingly that discrediting Marat's scientific reputation should serve as an indication of his political unreliability, see (Conner 1997).

13 For a case study based on such an approach see (Hochadel 2003).

14 See for example (Galison 1987), (Gooding et al 1989), (Heidelberger & Steinle 1998).

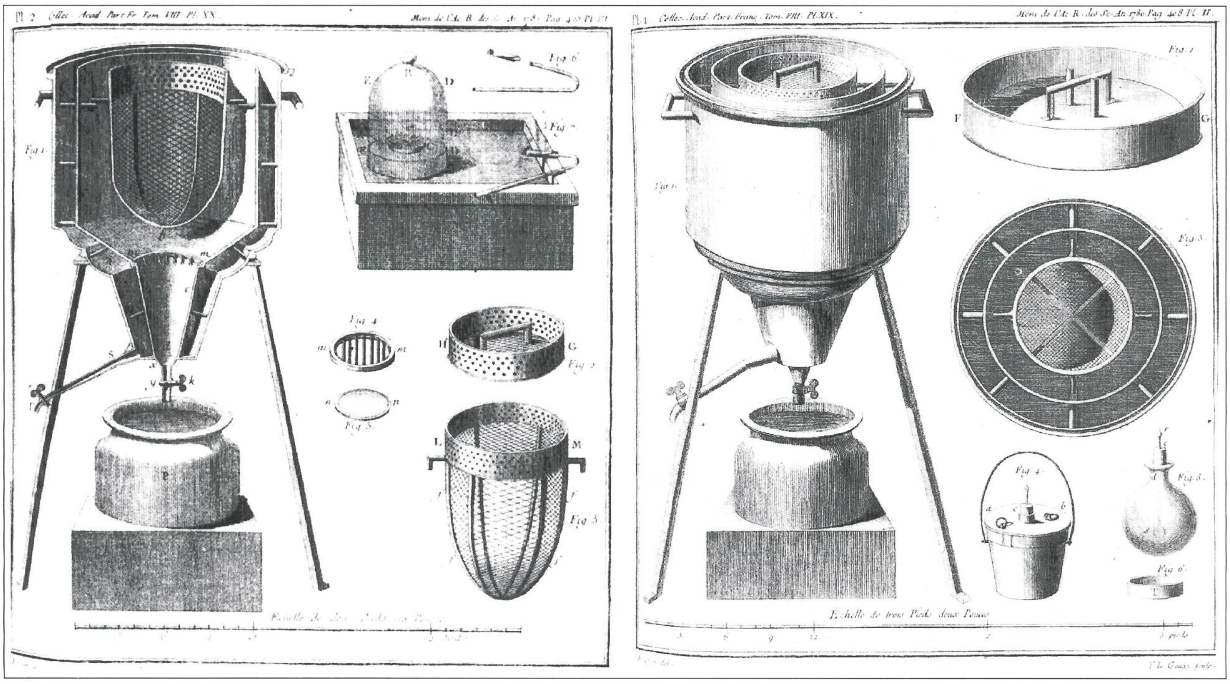


Fig 8. Plates of the ice-calorimeter. Landesbibliothek Oldenburg.

not do justice here to all the work done in this field in the last 10 to 15 years. For our purposes here, we can focus on two moves: First, classical experiments which have formerly been used only as illustrations or confirmations of theoretical ideas are now considered in a different light[15]. Secondly, the historical interest is no longer limited to these classical experiments but extends far beyond to forgotten and even unsuccessful ones[16].

If we take the now already twenty-year-old dictum of Ian Hacking that "Experimentation has a life of its own" (Hacking 1983, p. 150) as a starting point, we can discern some details of this change. Experimenting is no longer seen as being fully determined by theoretical imperatives. Goals and specific pathways chosen by the experimenter cannot always be explained by reference to theory. The same is true for experimental tests and justifications of empirical data. Many new questions arose and in order to find answers the whole process of experimenting has been put under scrutiny – from the first plans, the construction of the apparatus, maybe its modification during the course of the experiment, to the data evaluation and finally the publication and justification. Simple terms like "discovery" have been criticised and the creation, refinement and stabilisation of a phenomena are now considered to require complex moves and a lot of work by experimenters. Within this micro-perspective it has been realised that the skills of instrument-makers constructing the apparatus and of the scientists doing the experiment were in many cases crucial for its success. Further, as some studies have shown within this micro-perspective we have found more in the laboratory than we expected: Sometimes there were other "forgotten" pieces of apparatus which have, for example, influenced the design of the experimental set-up or were needed to make it work, sometimes there were hitherto unknown laboratory assistants who contributed significantly to the experiment (Shapin 1989)[17]. Extending this micro-perspective step by step we have achieved a much better understanding of the experimental work of a particular scientist or a small group of scientists. Due to this extended view on experimenting we look now more carefully at the work within a laboratory; it is no longer only the "highlights", the "famous discoveries" we are interested in, since in our attempt to understand and explain experimenting at a certain place and time we need to explore other possible factors which shaped an experiment.

In other words: The history of science is now really historical – in the sense that the actual scientific practice we want to analyse is firmly rooted within

15 See for example (Heering 1994), (Sichau 2000a), (Sichau 2002).

16 See for example (Frercks 2001).

17 Similar experiences we made by replicating experiments are discussed below.

Fig. 9. Reconstruction of the ice-calorimeter. Photo: W. Golletz, University Oldenburg.

the broader history, be it in its social, economic, political or cultural variants; and its results have also started to change the philosophy of science. (Radder 2003)

From the museum in the laboratory

But how can one get closer to historical scientific practice? We do not want to claim that the approach we are going to describe here is the only possibility; nevertheless, we are convinced of its usefulness – not only for the "academic world" but also for museums. It is called "Replication method" and in short its central aspect is to bring the historical apparatus back into the laboratory, mostly in form of a reconstructed apparatus[18]. Apart from publications and manuscripts, instruments kept in museums are therefore a highly important and valuable source for the making of a replica of an apparatus that is to be used for the analysis of an experiment. In some cases it might even be possible to use parts of the original set-up for the experiments[19]. However, if the original apparatus still exists it can at least serve as a basis for this reconstruction.

Take for example our reconstruction of the ice-calorimeter, an apparatus originally used by Lavoisier and Laplace to measure quantitatively the "calorique", an imponderable responsible for the phenomena of heat (Laplace & Lavoisier 1783, Fig. 8)[20]. This device consists of a double wall container in which a basket of iron wire is inserted. The object whose heat is to be determined is placed into the basket, the space between the basket and the inner container is filled with crushed ice with a temperature of 0°C. Each transfer of heat from the object would result in ice being mel-

[18] For a detailed discussion of this approach see (Heering 1995), (Sichau 2000b), (Sichau 2002).

[19] This had been done in two studies of the Oldenburg group, see Michael Friedrich: "Vom Problem zum Instrument. Die Entwicklung des Bolometers", Oldenburg: Universität (Diplomarbeit), 2002; Andreas Makus: "Felix Ehrenhaft und der Streit um das Elektron". Blätter für Technikgeschichte 64, 2002, pp. 25-45. One of the authors has been working on a research project in which he worked with 18th century solar microscopes kept at the 'Deutsches Museum Munich'.

[20] For an analysis of this experiment see in particular (Roberts 1991).

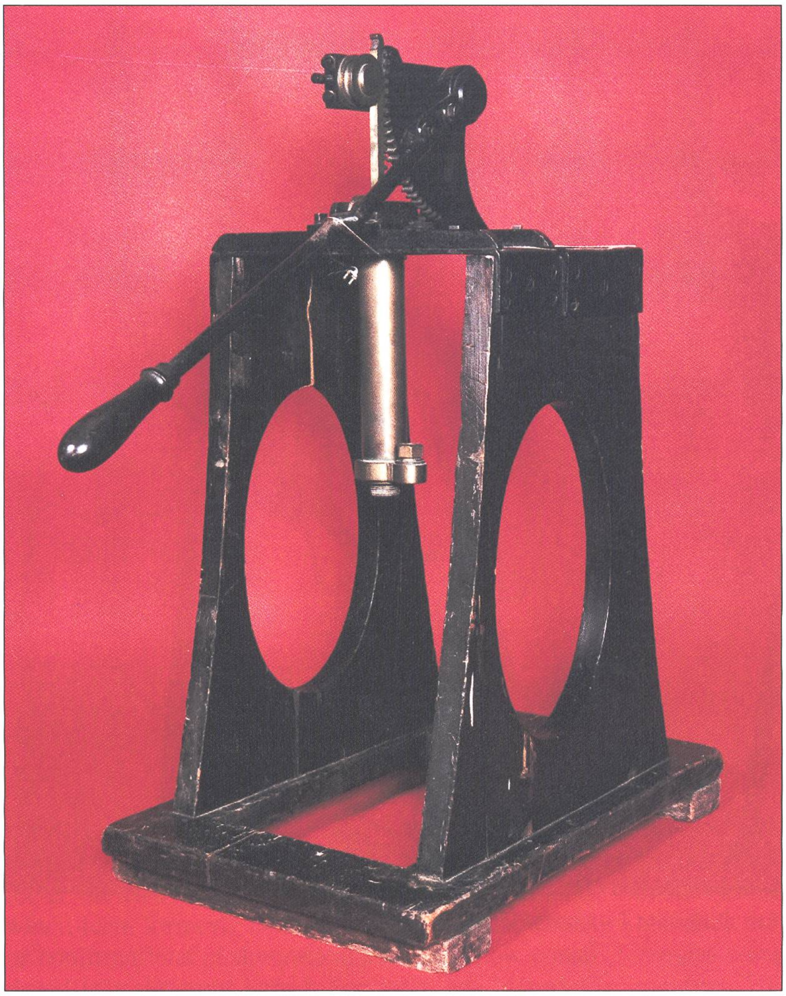


Fig. 10. Joule's pump in Manchester

ted, the amount of melted ice serves thus as a measure for the amount of heat the object contained. The space between the two metal containers is also filled with crushed ice that serves as an insulation, thus making the machine less dependant on outside temperatures. The machine still exists and is on display at the Musée des Arts et Métiers in Paris[21]. Due to the support of this institution, and in particular Élisabeth Drye and Thierry Lalande, it was possible to take all the measures necessary for its reconstruction (Heering 2005a). However, even though I had the opportunity to examine the apparatus several times, some aspects remained unclear, others posed technical problems in the realisation as some craft skills of the 18th century are lost. For example, the paint of the apparatus turned out to be very difficult; it serves also as a protection and we decided not to use a modern anticorrosion paint but to use substances that had been used for these purposes in the late 18th century (Fig. 9)[22].

Another case study began in the store rooms of the Manchester Museum for Science & Industry[23]. Thanks to the curator, Jenny Wetton, it was possible to examine some items of the fairly large collection belonging to James Prescott Joule[24]. The apparatus we were particularly interested in was an apparatus which in the past most would not have been considered a "scientific instrument": It was a hand-pump (Fig. 10). In his biography on Joule D. Cardwell (Cardwell 1989) called it "simple" – however, it wasn't as I will show here. This pump was to be rebuild at the university workshops at Oldenburg in order to study the experiments Joule did jointly with William Thomson in 1852. Their research project lasted for several years and one of its results was the "discovery" of the so-called "Joule-Thomson-Effect". The pump had not been made especially for these experiments, but had been used by Joule before; its very existence was a decisive impetus for the Joule-Thomson-experiment. Later, the pump was used by Joule on a different occasion. So, at the beginning of the study it was not entirely clear whether the pump in the store room of the Museum had not been altered substantively – only experimenting with a replica could tell.

There is lot to say about this pump – about the material it is made of or about the form of the wooden stand. A careful examination, together with a careful analysis of the correspondence of Joule, would reveal how much it was indebted to the industrial prowess of Manchester.

21 Actually there are two ice-calorimeters kept in Paris which had been used by Lavoisier and Laplace for different purposes.

22 Likewise, other set-ups such as the helioscope of Marat has been reconstructed based on a solar microscope kept at the university museum in Utrecht and made available to us by Jan Deiman.

23 The study on the history of the Joule-Thomson-Effect began in 1995. The details are described in (Sichau 1995); for further information on the following account of this history see (Sichau 1998), (Sichau 2000a), (Sichau 2000c), (Sichau 2000d).

24 A first summary of the collection was published by Ashworth: "List of Apparatus now in Manchester which belonged to Dr. J.P. Joule", in: Manchester Memoirs 75 (1930-31), pp. 105-117.

In the Laboratory

However, we would like to move forward – and into the laboratory. After a replica of the Joule apparatus had been made in the university workshop (Fig. 11) – with the usual complications – the experimental work could begin (Fig. 12). Of course, the pump alone did not constitute the experimental set-up. Some copper-tubes and a copper vessel were needed – and most importantly a small item which has not survived: A porous plug made of leather through which the air streamed when the pump was worked (Fig. 13). At first, one might think any odd piece of leather would do. But that is not true. The aim of the experiment was to compare the temperature of the rushing air before and right behind the porous plug. This temperature difference was examined with respect to the pressure difference of the rushing air. This meant that the porous plug had to fulfil two conditions: It must be sufficiently thick so a fairly high pressure in the tube could be produced, on the other hand, it must be sufficiently thin to provide a strong stream of air behind the plug, so that the temperature is neither effected by small fluctuations of the air-stream nor by the temperature of the room in which the experiment is done. The experiences made while experimenting with various different pieces of leather raise doubts about the simple story told by Joule and Thomson in their publication[25]. This was just one of several aspects where experiences made in the laboratory together with a careful new reading of manuscript material led to a much more fine-grained analysis of the dynamics of the experiment than the original publication would allow[26].

Fig. 11. The reconstructed pump. Photo: W. Knust, University Oldenburg.

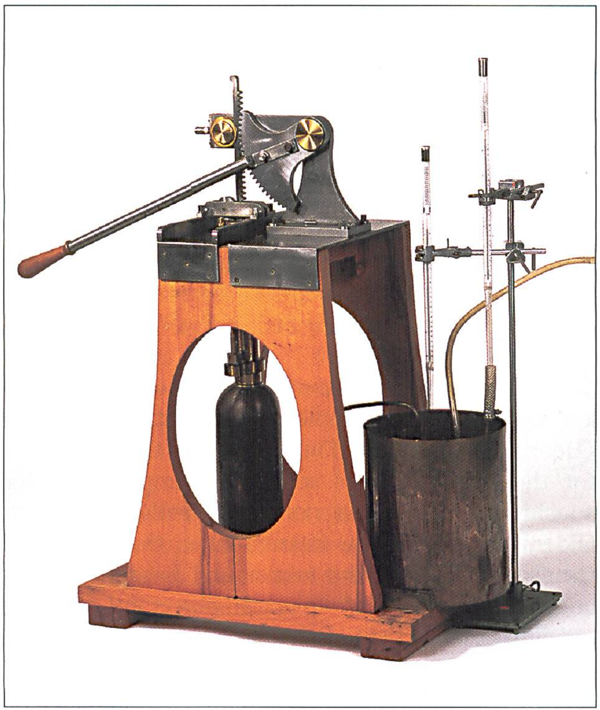


Fig. 12. Experimenting with the reconstructed pump. Photo: W. Knust, Museum für Mensch & Natur.

Since the temperature-differences to be measured in he experiment are around some tenths of a degree Celsius, the temperature in the room needs to be controlled – Joule did these experiments in a cellar of his private house. Who was working in this cellar? As we know, Joule thought himself an expert of temperature measurements; it's safe to assume that he inserted the thermometer at its appropriate position (which is a tricky affair) and did the reading of the thermometer, maybe also of the pressure gauge. But several tasks had to be done: Someone had to stir the water in the vessel to keep its temperature constant and thereby the temperature of the air before it reached the plug, and, of course, someone had to work the pump. This had to be done with a constant rhythm – and it had to be kept up for about an hour. Since Thomson wasn't present for most of the experiments it is probable that an hitherto unknown assistant helped Joule during the experiment – contrary to what is stated in the standard biography on Joule by

25 The importance of the many changes and improvements of the porous plug made by Joule and Thomson is indicated by the various sketches of these items in their publication and private correspondence; see for example (Cardwell 1989, p. 138).

26 For details on the relationship between various written descriptions of the experiment and the work in the laboratory see especially (Sichau 2000c).

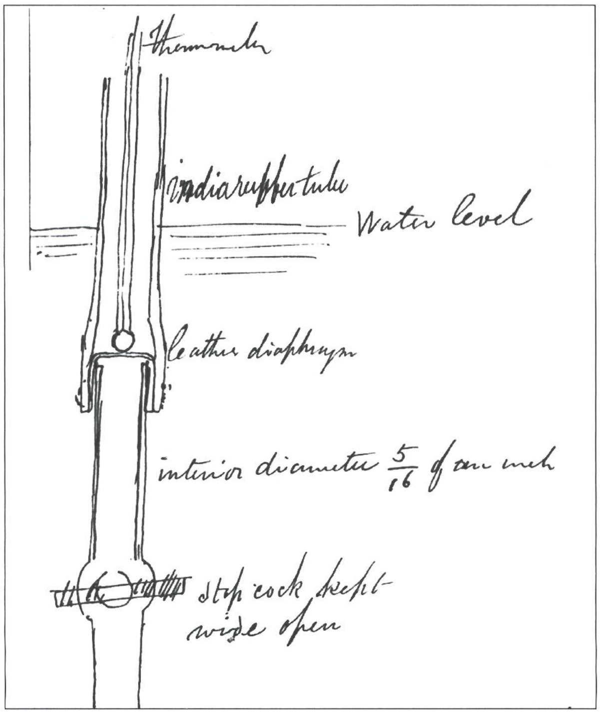


Fig. 13. Joule's drawing of the porous plug.

the late Donald Cardwell. Joule never mentioned such a person, and in a letter to Thomson he even wrote that "*it would not be worth while at present at any rate to get an assistant*". However, having made the experiences in the laboratory we are led to a different reading of Joule's remark. For Thomson an assistant was a student who helped in his laboratory at Glasgow University, sometimes Thomson would pay his assistants. Joule rejected such a "paid" assistant – with a fairly high social standing. Like in many other technical treatises at the time people of the working classes, in Joule's experiment maybe a servant or a worker from the family's brewery, were not mentioned[27].

The room and its temperature control turned also out to be one of the crucial aspects in the experiments with the ice calorimeter of Lavoisier and Laplace. To some extent it was already embodied in the set-up, to be more accurate, it is a result of the technical layout of the insulation. It consisted of an ice layer some five centimetres thick. As Lavoisier and Laplace had already mentioned in their publication, this would work only for room temperatures not below 0°C.

However, there was another limit: the room temperature was not to be above some 5°C, otherwise the insulation proved to be insufficient (Fig. 14). Contrary to the experiments of Joule and Thomson the operation of the machine turned out to be simple: Fill in the crushed ice in a specific manner, place the probe in the calorimeter, close the machine and wait for some 24 hours, then measure the amount of melted water. At least, the procedure seems to be simple, however, things are not that easy: Experimenting reveals that it is necessary to correct the result, as some ice melts even without any probe, its amount had to be determined in preliminary experiments. Moreover, the 'simple' instruction for the experiment is to some extent a result of working with the apparatus and thus developing a procedure that leads to meaningful results (and the criteria for labelling a result 'meaningful' are not self-evident but this discussion would require another ten pages). For example, the ice has to be squeezed into the machine, otherwise it is impossible to get a proper result. Moreover, particularly our first results seemed to be completely irregular and the amount of ice that was melted without any probe in the calorimeter seemed to be very high, thus indicating that the insulation wasn't good enough. From comparing our replica set-up with the textual description and the copper plates given by Lavoisier and Laplace it became obvious that something was missing: At the bottom of the inner cavity two filters were placed to prevent ice from gliding into the lower section of the set-up (Fig. 15). These filters are not in the instrument kept at the CNAM. In many cases it is difficult to decide whether the description of an experimental set-up is "wrong" or whether the relict in the museum had been altered; however, in this case, judging by the design of the inner container and taking into account our difficulties of producing meaningful results it became obvious that these filters had to be included in the ice-calorimeter – and later were simply lost. Adding these items to the machine made it possible to come up with results that did meet our expectations (Heering 2005a, Fig. 16).

Like with the ice-calorimeter it was also not simple to get results with a replica of Marat's helioscope (Heering 2002, 2005b, Fig. 17). Again, the procedure for the operation was seemingly simple: Wait for sunshine (which does not occur self-evidently in Oldenburg), place the instrument in the shutter of the window, adjust the mirror and produce a light cone into the room. Now place an object into the light cone: In cases of heated objects it should be possible to see the emanating 'fluide igné', in case of objects at room temperature, the resulting shadow should be

[27] The existence of such assistants in Joule's laboratory can also be inferred from two studies done at the University of Oldenburg, one by H.-O. Sibum ("Reworking the Mechanical Value of Heat: Instruments of Precision and Gestures of Accuracy in Early Victorian England", in: Stud. Hist. Phil. Sci. 26 (1995), pp. 73-106) and one by Jan Witte ("J.P. Joules Bestimmung des Mechanischen Wärmeäquivalent durch Kompression und Expansion von Luft", Schriftliche Hausarbeit an der Universität Oldenburg 1996).

Fig. 14. The ice calorimeter in the cooling chamber. Photo: U. Drüding.

bordered by coloured areas, their appearance depending on the position of the object in the light cone. Again things are not that easy or obvious. It turned out that effects were easily observed in case of the experiments on heat (Fig. 18). Things were different, however, when we turned to the experiments on the diffraction of white light. Here, it was not possible to get results that corresponded to Marat's description. Initially, it was not clear how this should be interpreted; however, after some experimenting we found that the weather conditions were the crucial factor. Only on days with bright sunlight and no clouds, it was possible to obtain results that were in accordance with Marat's description. Reproducing his results does of course not mean that we also accepted his theory; actually the appearances are a result of the physical properties of the lens which is not achromatic.

However, there is more to our experiences made in the laboratory with Marat's optical experiments as well as those on heat and their relation to Marat's written description. It turned out that the result as depicted in Marat's publication is somewhat misleading: the image gives a somewhat static expression, but this is not what appears on the screen. The dominant impression produced by the image on the screen lies in its dynamic, the silhouette is unstable and changing all the time. We need to remember that Marat's experiments were designed in order to be performed in front of an audience; and the publication cannot convey the emphasis Marat put on the dynamical effects and their rhetorical power.

Both experiments discussed above were designed to analyse imponderable fluids: both approaches are, however, entirely different. To broaden the analysis, it would be necessary to add some more experiments to the analysis, i.e. Coulomb's experiments on electricity and magnetism and Marat's experiments on electricity. However, this would go beyond the scope of this paper, therefore we think it to be justified if we are relating the general findings only to the experiments that have been discussed:

It is clear that Marat's experiments were designed to be demonstrated in front of an audience, and that the dominant impression is the dynamics on the screen. Moreover, a central aspect of Marat's analysis is to observe the imponderables whilst in motion. This is even more striking when Marat's electrical experiments are considered, here discharges form some sort of 'Leitmotiv'. The analysis Lavoisier and

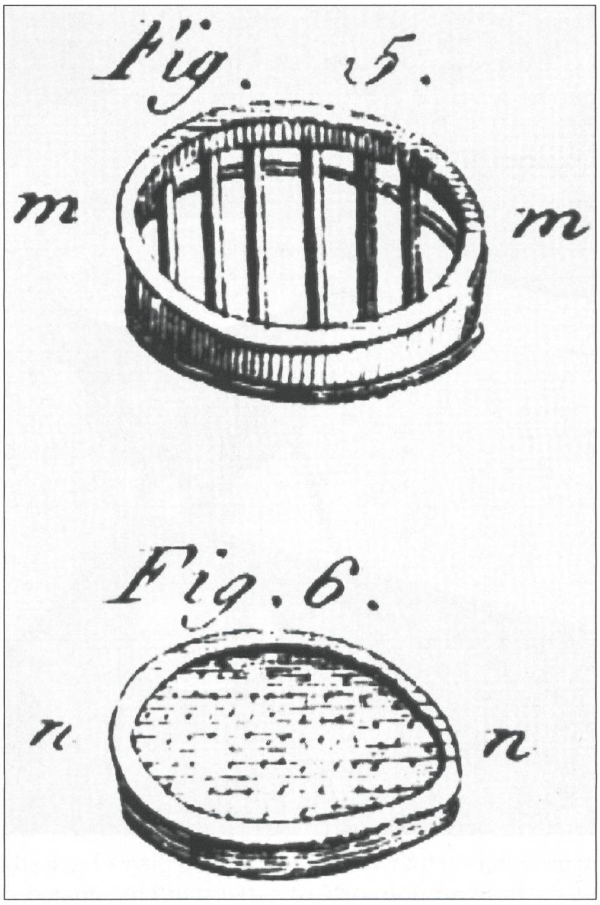
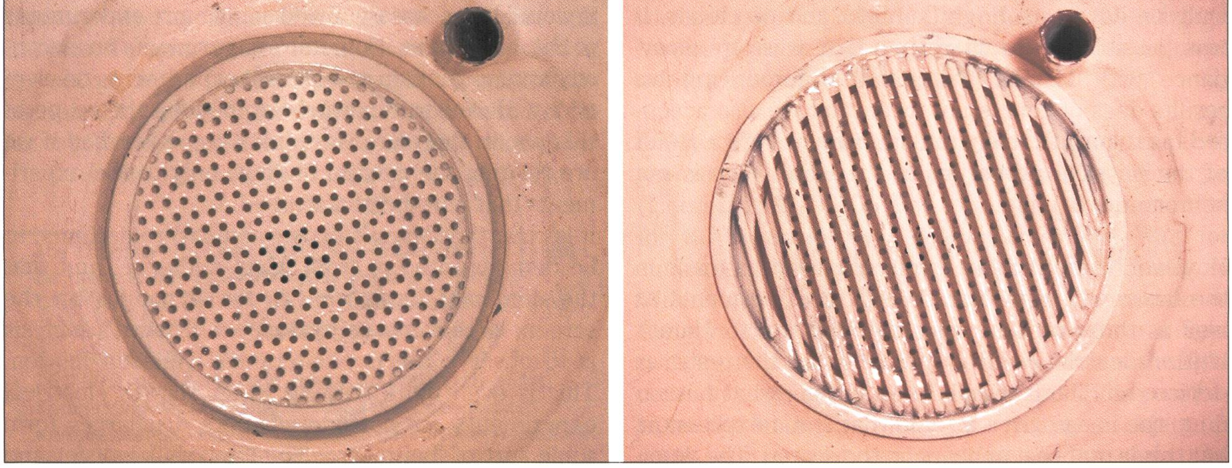


Fig. 15. Detail of the copper plate from the publication. Landesbibliothek Oldenburg.

Laplace were carrying out is completely different. Here, static situations are dominant, as has been observed already by Norton Wise (Wise 1993). Further, the experiments are no longer demonstrable, the time necessary for the experiment and the error-sensitivity are excluding this experiment from being executed in front of an audience. Thus, the result is not to be understood by inspection but the data of Lavoisier and Laplace are to be accepted. These differences – and other aspects could be mentioned – of the styles of experimentation had to do – apart from other reasons – with the political position of the protagonists: Marat, the revolutionary, is trying to produce dynamic situations that can be witnessed by an audience that should – under his guidance – make up their mind about the properties of the imponderables. Lavoisier and Laplace, both, more or less royalists, analyse static situations, and in their experiment it is only the sufficiently trained scientists who have access to the experimental space.

As we can see in this case the micro-perspective on experiments brings us directly into contact with broader historical developments. The case-study described earlier, the Joule-Thomson-Experiments, is another example in this respect. Joule and Thomson were not satisfied with their experimental findings; they argued that with the limited amount of gas which can be pumped with their small apparatus it was impossible to obtain proper results and built a much larger, steam-driven apparatus to continue their research (Fig. 19). However, at least in some of our "better" experiments, the linear relationship between pressure difference and temperature change that Joule and Thomson sought could be found, although no absolute value could be given. The crucial difference between our experiments and those of Joule and Thomson was the thermometer used, the one we had contained more mercury than Joule's thermometer; thus, it was less sensitive to small fluctuations. But to show that we can do sometimes better than the historical experimenters is not the point – what is important is that our experiences lead to new questions: The argument put forward by Joule

Fig. 16. Reconstructed filters. Photo: W. Golletz, University Oldenburg.



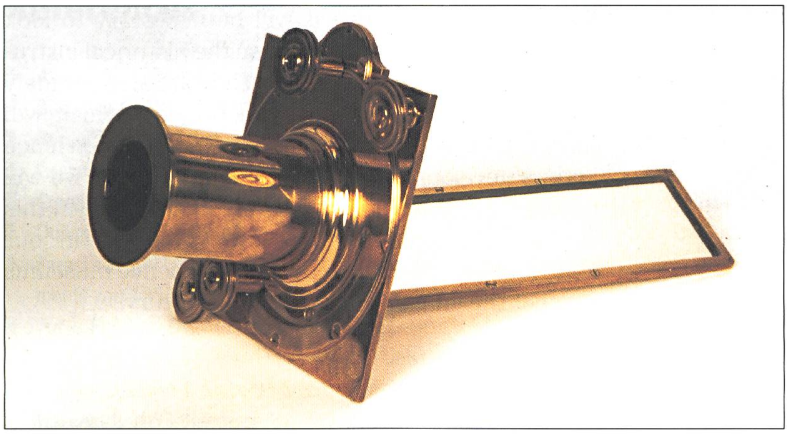


Fig. 17. The reconstructed helioscope. Photo: W. Knust, Museum für Mensch & Natur.

and Thomson for their move cannot any longer taken for granted, as, for example, Donald Cardwell did. It proved necessary to look for additional motives for the building of the new, expensive and big apparatus by Joule and Thomson – and within the available sources some documents could be found which then changed the perspective of the collaboration of Joule and Thomson considerably: Their project was to a great extent influenced by the political struggle to establish a new kind of professionalism within British science and a new relationship between science and the British State as their new apparatus was financed with the help of a so called Government Grant which had just been established (Sichau 1998).

Fig. 18. Shadow projection of a burning candle. Photo: Heering, University Oldenburg.

From the laboratory to the Museum

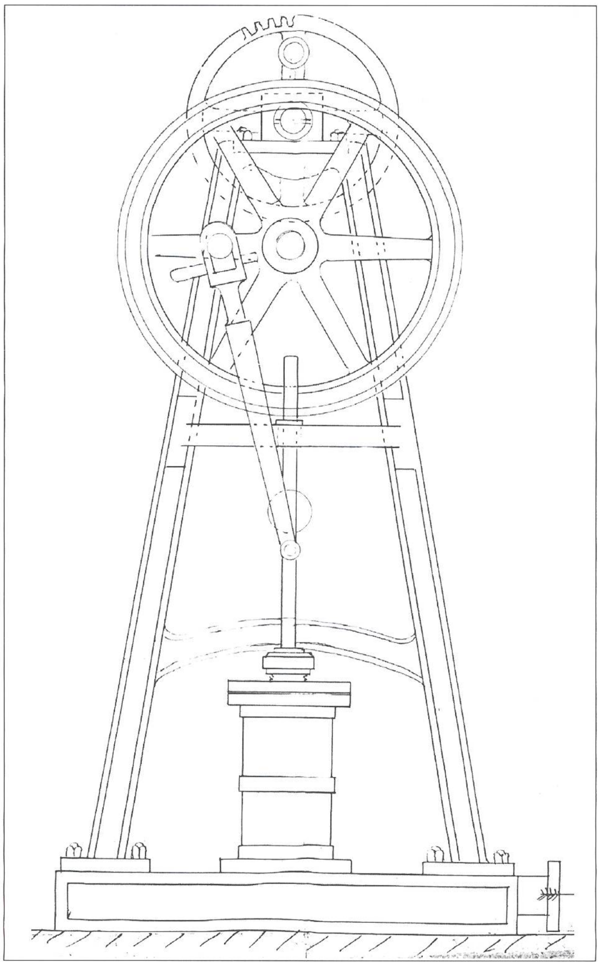
As mentioned earlier, this story ends in a museum. Indeed, in the Manchester Museum for Science & Industry a new science gallery in which Joule's experiment feature prominently has just recently been opened and some of our results have been considered useful for its construction[28].

Apart from this collaboration, which is aspired likewise with the CNAM in order to improve the information on the ice-calorimeter in the exposition, other possibilities exist: An exhibition on 'experimental culture' had been created in a local Oldenburg museum (Heering 1998; Heering & Müller 2002). It lasted for several weeks and was tremendously successful. Here, we tried to show several experiments, to give them a broader context and – when possible – to give the visitors the opportunity to use some of the set-ups and to make their own experiences (Fig. 20). In doing so, they could experiences some aspects of experimental cultures of the past, thus also creating an interest on modern science and the condition of the creation of scientific knowledge.

Approaches such as the latter might be helpful to museums in order to face new challenges: Science museums are currently under pressure to find a new role for the future. Although experiences in science-centres in the past decade have made the limitations of this approach clear, they are still "en vogue". We are faced with the dilemma that the importance of an historical approach can not to be considered to be self-evident any more. Museums will have to change – and these transformations will have to include several aspects. Within our exhibitions historical scientific instruments must find a place in new compelling stories in which the old one-sidedness – to take nature and natural phenomena as the only "subject" of science museums – is given up and a broader view on scientific practice as part of the broader history is embraced, be it in its social, economic, political or cultural variants. In future it will no longer suffice to portray "nature" in a science museum, but we will need to portray the "nature of science".[29]

[28] More details can also be found on the internet: http://www.msim.org.uk/joule/index.htm.

[29] Events such as the Geneva 'Nuit de la Science' or the Dresden 'Museums-Sommernacht' have already shown to some extent what might be possible in the future and they have been very important for improving the public recognition of science museums and their collection.



Within this perspective it will be necessary to look afresh at our collections because the historical instruments are our strongest asset. They are an extremely valuable resource for research projects which will form the basis for a better understanding of science and its development, and with these insights we will be able to construct new exciting exhibitions. In this respect, the approach we have sketched in our contribution might be an option for science museums to meet the challenges of the 21st century.

Fig. 19. The large steam driven apparatus.

Fig. 20. Demonstration during the exhibition 'Welt erforschen – Welten konstruieren'. Photo: W. Golletz, University Oldenburg.

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