**Learning in the world:**

**Van Musschenbroek's lessons for the philosophy of experimentation**

**Paper Presented at the Society for Philosophy of Science in Practice (SPSP) Ninth Biennial Conference, July 4 2022**

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In this paper, I discuss Petrus van Musschenbroek’s (1692-1761) philosophy and practice of experimentation. In the current literature, van Musschenbroek is mostly mentioned for his “discovery” of the Leiden jar or in the context of his role in the spread of Newton’s ideas on the Continent. In his own time, van Musschenbroek was a well-known natural philosopher and a celebrated experimentalist.

In an oration titled “On the method of performing physical experiments”, van Musschenbroek gave an overview of what we could call his philosophy of experimentation. In my discussion of this philosophy, I will show how the complexity of nature played in important role in his thinking on the method of performing experiments. Van Musschenbroek emphasised that there are always a lot of (unknown) variables at play in experimental research. One therefore needs to repeat and vary one’s experiments in order to identify as much relevant variables as possible and to remove hidden sources of disturbances. However, for van Musschenbroek, there were other reasons to vary and repeat an experiment. I show how van Musschenbroek also characterised the process of repeating experiments as a learning process. I argue that this learning process should be seen as a process of augmenting one’s practical grasp and understanding of the experimental set-up and the phenomena under investigation. To illustrate these views, I discuss two fields in which van Musschenbroek performed experimental research: the strength of materials and electricity. I show how many points made by van Musschenbroek in his methodological writings were instantiated in his experimental research practice. In both cases, his research was characterised by an emphasis on the variety and heterogeneity of the phenomena under investigation, the need to explore bodies in different ways by means of experiments, and attention for the details of the experimental set-up.

In the second part of this paper, I will build upon the discussion of van Musschenbroek’s theory and practice of experimentation to provide a more elaborate philosophical discussion of experimental learning as a process of learning in the world. More specifically, I show how the choice to speak about learning in the world, instead of learning about the world, reflects a non-representationalist view on science. It is also connected to a view on science as a practice, more specifically as a situated and dynamic collection of activities. The main aim of this is to provide a philosophical view on the role of experimentation and the nature of scientific learning which allows me to do justice to the experimental research performed by van Musschenbroek. However, I will also make some more general philosophical points. More specifically, I will argue that van Musschenbroek’s work and ideas provide an interesting starting point to build further upon Friedrich Steinle’s concept of “exploratory experimentation (EE)”. Whereas Steinle’s notion of EE is still (I would argue) mainly centered on propositional knowledge, my discussion of van Musschenbroek’s work will allow me to expand Steinle’s notion of EE to include other kinds of learning. As mentioned, I argue that we should understand scientific practice as a process of learning in the world. According to this view, experimental learning is a process of actively engaging with and reshaping the world. The results of this learning process are not limited to propositions, but are also embodied in instruments, processes, procedures, standardised objects, and the skills of practitioners.

The presentation will mainly deal with the topics discussed in the text below. This text is the final chapter of my doctoral dissertation. My aim is to rework this chapter into a standalone article, hence my motivation to present it at SPSP in order to get relevant input. Some parts of this chapter refer back to earlier parts of the dissertation. The material referred to will be discussed during the presentation. Those who want to read the parts referred to in this chapter can find the entire dissertation here: <https://www.academia.edu/40712274/Learning_in_the_world_Petrus_van_Musschenbroek_1692_1761_and_Newtonian_experimental_philosophy_>

The chapter is quite long, but the parts most relevant for the presentation are sections 4.2, 4.4.2 and 4.4.3.

*‘How can know-how be extended? Like radios that are made in Hong Kong, or multiplication tables! There must be buyers and sellers, teachers and commercial circuits, representatives and books that are held to be authorative. We say that the laws of Newton may be found in Gabon and that this is quite remarkable since that is a long way from England. But I have seen Lepetit camemberts in the supermarkets of California. This is also quite remarkable, since Lisieux is a long way from Los Angeles. Either there are two miracles that have to be admidered together in the same way, or there are none.’*

(Bruno Latour, *The Pasteurization of France*, 1993, 226-227)

**4.1 Introduction**

In this chapter, I discuss the philosophical ideas informing the choice for the title “Learning *in* the world”. More specifically, I show how the choice to speak about learning *in* the world, instead of learning *about* the world, reflects a non-representationalist view on science. It is also connected to a view on science as a practice, more specifically as a situated and dynamic collection of activities. In line with this dissertation’s focus on experiments, I will pay special attention to what these views entail for our understanding of experimentation.

In chapter 2, we have seen how van Musschenbroek thought that research should be pursued in different steps. The first step consisted in the collection of data in the form of an experimental history. Only after this first step should the search for laws and causes be performed, based strictly on the data found in this experimental history. In the same chapter, I have also discussed van Musschenbroek’s oration on the method of performing experiments and argued that this two-step procedure should not be seen as implying that the experimentalist approaches the experiment as a blank slate. To be a good experimentalist, a researcher should possess a certain amount of foreknowledge. I have also shown how van Musschenbroek emphasised the importance of repeating experiments and I argued that he saw this repetition as a learning process. By repeating an experiment, the experimenter would become more skilful and also acquire what I have called a practical understanding or “circumspection” of the experimental set-up.

In the same oration, van Musschenbroek further argued that experimentation should involve a systematic variation of relevant experimental parameters. In the previous chapter, we have seen that the systematic variation of parameters was indeed an important (if not the most important) characteristic of van Musschenbroek’s experimental research. Moreover, both his research on the strength of materials and his work on electricity could be seen as consisting in a construction of an experimental history. His research did not aim to test or refute a certain theory. With regard to electricity, van Musschenbroek explicitly stated that existing theoretical explanations of electrical phenomena were inadequate. In this chapter, I want to take the non-theory-driven nature of van Musschenbroek’s experimental research seriously. Therefore, I will present a view on experimental practice which allows for experimentation to be pursued in a systematic way without it being driven by or made subservient to theory.[[1]](#footnote-1)

This view on experimental practice will be developed in section 4.4. In the following two sections I will first provide a discussion of scientific practice as a process of learning *in* the world. In section 4.2, I discuss Rouse’s and Hacking’s criticism of representationalist, theory-centred philosophy of science. Both argue that we should move from seeing science as aiming at a passive representation of the world to seeing science as a practice, which involves active interventions in the world. In section 4.3, I discuss Rouse’s (re)interpretation of Kuhn. Rouse argues that there are two ways in which one can read the work of Kuhn. One could read him as a theory-centred philosopher of science, or one could see his work as pointing towards a philosophy of scientific practice. I contrast both readings, more specifically looking at their implications for our understanding of the process of education undergone by scientists. This will allow me to introduce the concept of “circumspection” or practical understanding which I already alluded to and which will play an important role in the remainder of the chapter.

**4.2 From Learning *about* to Learning *in* the World**

In his *Knowledge and Power*, Joseph Rouse describes the aim of his book as the development of a more adequate understanding of scientific practice. According to him, in order to arrive at such an understanding, philosophy of science needs to move away from a representationalist account of (scientific) knowledge. Such a view sees science as being essentially ‘a means, the most successful means we have devised to date, for constructing and improving representations of the world’.[[2]](#footnote-2) This view on (scientific) knowledge, according to Rouse, ‘presupposes the possibility that there is a gap between the way the world is and our representations of it’.[[3]](#footnote-3) Science is seen as a means to learn *about* the world. As knowing subjects, we are separated from the world as it is in itself, but by means of science we are able to learn things *about* the world. The aim of science in this view is to improve our representations and thus to bridge the gap between the world and our representations of it.[[4]](#footnote-4)

In traditional philosophy of science, Rouse continues, this representationalist view has often been connected to a theory-dominant or theory-centred view on science.[[5]](#footnote-5) The improvement of our representation of the world is conceptualised as consisting in the development and improvement of scientific theories. This theory-centred view on science also involves a certain view on the nature of scientific knowledge. Scientific knowledge *as* theoretical knowledge is abstract, disembodied and should be as systematic as possible. The latter is understood as referring to a system of theoretical propositions or statements which should be as complete, coherent, and consistent as possible. This system would then provide the best possible representation of the world.[[6]](#footnote-6) In this view, learning about the world means accumulating propositional or theoretical knowledge. The theory-centred view on science also has implications for what parts or aspects of scientific practice one finds philosophically relevant. Given that theory construction is considered as the main aim of scientific practice, the theory-centred view considers other aspects of scientific practice as at best subservient or at worst irrelevant for a philosophical understanding of science. An example of the first can be found in the treatment of experiments, which are regarded as subservient to theory.[[7]](#footnote-7)

Elements which are considered irrelevant include:

[T]he local site of investigation, the experimental construction, the particular networks of social relations within which the investigators are situated, and the practical difficulties of getting on with research[.][[8]](#footnote-8)

When it comes to understanding experiments, these elements are irrelevant because experiments ‘are taken to be generally interpretable instantiations of theoretical claims’.[[9]](#footnote-9) As we have seen, theoretical knowledge was taken to be abstract and disembodied. Making experiments subservient to theoretical knowledge and interpreting them as instantiations of theoretical claims, allows for them to be treated in an abstract and disembodied way as well and thus warrants abstracting from the local and material circumstances in which they are made.

As mentioned at the beginning of this section, Rouse wants to move away from a representationalist view on scientific knowledge and arrive at a proper understanding of science as a *practice*.[[10]](#footnote-10) Rouse was not the first to criticise representationalism. Before him, Ian Hacking had already criticised the representationalism inherent in traditional philosophy of science in his 1983 *Representing and intervening*.[[11]](#footnote-11) According to Hacking, a representationalist view on (scientific) knowledge will inevitably lead to an idealist position according to which we are stuck in our representations, forever separated from the world itself.[[12]](#footnote-12) He refers to John Dewey’s earlier criticism of the so-called “spectator theory of knowledge” that had held Western philosophy in its grip.[[13]](#footnote-13) Like Dewey, Hacking beliefs that the representationalist view ultimately leads to an idealist position because it assumes a ‘false dichotomy between acting and thinking’.[[14]](#footnote-14) For Hacking, it is not the representationalism as such that is problematic, but rather the ‘single-minded obsession with representation and thinking and theory, at the expense of intervention and action and experiment’.[[15]](#footnote-15) As the title of his book suggests, he then argues that philosophy of science should take a turn from a focus on *representation* to a focus on *intervention*. In the second part of the book, Hacking sets out his own views on the role and nature of scientific experimentation.[[16]](#footnote-16) I will return to these in section 4.4, when I discuss Rouse’s notion of “microworlds”.

Rouse takes over Hacking’s emphasis on the importance of manipulation and active intervention to develop his non-representationalist view on scientific practice. Because of the practice-based and non-representational character of his views on scientific knowledge, in Rouse’s account we always find ourselves *in* the world, not trapped in mere representations *outside* of it.

In working on the world, we find out what it is like. The world is not something inaccessible on the far side of our theories and observations. It is what shows up in our practices, what resists or accommodates us as we try to act upon it. Scientific research, along with the other things we do, transforms the world and the ways it can make itself known. We know it not as subjects representing to ourselves the objects before us, but as agents grasping and seizing upon the possibilities among which we find ourselves. The turn from representation to manipulation, from knowing that to knowing how, does not reject the commonsense view that science helps disclose the world around us.[[17]](#footnote-17)

In this account, science should not be seen as providing a theoretical representation *of* the world, but as a practice involving an active engagement *in* the world. Other philosophical accounts of science only consider scientific activities as activities leading to a stable end-product, i.e. a theoretical representation. These “preliminary” activities are thus either ignored or only considered of secondary importance in relation to the “essential” scientific knowledge, the theoretical representation. Rouse in contrast puts these activities central: ‘science should be philosophically reconceptualized as something that scientists do rather than as a body of representations which results from the activity of research but which afterwards stands on its own’.[[18]](#footnote-18)

But what does it mean to see science as a practice of learning *in* the world? In the following sections, I will further explore this view on scientific learning. Special attention will be given to experimental practice as a learning practice, which will be the topic of section 4.4. Before turning to experimental practice as a learning practice, I will first discuss the process of learning (how to do) science. More specifically, I will discuss Rouse’s (re-)interpretation of Kuhn as a philosopher of scientific practice and what this interpretation implies for our views on what it means to learn science. Kuhn has recently been rediscovered as a philosopher of science pedagogy.[[19]](#footnote-19) Rouse argues that Kuhn could be read in two ways: either as a theory-centred philosopher of science, or as someone pointing towards a philosophy of science as a practice. I will compare these two ways of reading Kuhn, with a special emphasis on how the process of learning (how to do) science is conceptualised. This will allow me to provide a first example of a shift from thinking of learning in terms of learning *about* the world to thinking about it as a process of learning *in* the world. The discussion will also allow me to introduce the concept of “circumspection”. I already referred to this concept in chapter 2, when I discussed van Musschenbroek’s views on the process of repeating experiments as a learning process. This concept will also play a role in section 4.4, where I provide a discussion of experimental practice as a learning practice.

**4.3 Learning (how to do) Science**

Rouse refers to a remark made by Kuhn himself that in reading criticisms of his work he often had the feeling that there were two Kuhns, Kuhn1 and Kuhn2, who have different concerns and sometimes contradict each other.[[20]](#footnote-20) Taking inspiration from this remark, Rouse presents two different readings of Kuhn’s work. On the one hand, there is Kuhn2 or ‘the philosophers’ Kuhn’, i.e. ‘Kuhn as he has been read by many of his philosophical critics’.[[21]](#footnote-21) On the other hand, there is Kuhn1, ‘the radical Kuhn’, who points towards a philosophy of science as practice.[[22]](#footnote-22) The contrast between the two is described by Rouse as ‘the contrast between a representationalist, theory-dominant account of science and one in which the practices of scientific research take precedence’.[[23]](#footnote-23) The latter considers science as ‘a field of practices’, the former sees science as ‘a network of statements’.[[24]](#footnote-24) The two readings of Kuhn exemplify these two different ways of looking at science. In what follows, I will discuss the aspects of these readings that will be relevant for spelling out the implications of Rouse’s view on science as a practice for an understanding of the process through which people learn to do science.

Let us begin with the philosophers’ Kuhn. His account of science begins with the notion of “normal science”, which is characterised as a period in which scientists uncritically accept a certain paradigm. Paradigms are then in turn understood as ‘a set of theoretical doctrines constituting a worldview, which was postulated in the work that originally established the field of research (or reestablished it on a revolutionary basis)’.[[25]](#footnote-25) The function of the paradigm is to prescribe and proscribe certain beliefs, to determine the important facts to be known and to provide expectations of what facts will be encountered. Scientific communities are then in turn defined by the common acceptance of a shared paradigm, i.e. by their sharing of certain beliefs and theoretical doctrines.[[26]](#footnote-26) Research activities during normal science are described as an activity of puzzle solving, again operating at the level of beliefs and representations:

Normal science is essentially puzzle solving, attempting to reduce the discrepancies between a paradigmatic worldview and the world and to fill in the many blanks left open by the original sketchy development of the worldview.[[27]](#footnote-27)

At this point, it should be clear why in this reading, Kuhn’s views are described by Rouse as a representationalist and theory-dominant account of science. Scientific communities are characterised as having certain shared beliefs, paradigms are considered as ‘a set of theoretical doctrines constituting a worldview’, and scientific research is concerned with the fit between this worldview and observational data. The important provision added by Kuhn with regard to these observational data is that they are always theory-laden. This notion itself further underlines the heavily theory-dominant view on science in this reading of Kuhn. As Rouse puts it, in this view ‘theories constitute the way we observe and describe the facts’.[[28]](#footnote-28) This led Kuhn to the notion of the incommensurability of different paradigms and the statement that ‘the proponents of competing paradigms practice their trades in different worlds’.[[29]](#footnote-29) In the theory-dominant reading of Kuhn, this means that the theory one accepts structures one’s observation of the world. In Kuhn’s terms, proponents of different paradigms ‘see different things, and they see them in different relations one to the other’.[[30]](#footnote-30)

When we apply this to scientific education, a specific view on the nature of this education arises. Kuhn himself made explicit remarks about the way scientists learn to do science. Scientific communities are characterised by a shared paradigm. New practitioners acquire these paradigms through the process of education and thus processes of education are what form and keep scientific communities in existence:

A scientific community consists, on this view, of the practitioners of a scientific speciality. To an extent unparalleled in most other fields, they have undergone similar educations and professional initiations; in the process they have absorbed the same technical literature and drawn many of the same lessons from it. [...] There are schools in the sciences, communities, that is, which approach the same subject from incompatible viewpoints.[[31]](#footnote-31)

Education thus forms and reproduces communities by having new members accept a shared set of theoretical presuppositions. New practitioners are trained to have a certain viewpoint on things. Once the scientist has acquired the necessary viewpoint, she can begin to perform normal science which can thus also be defined as the ‘attempt to force nature into the conceptual boxes supplied by professional education’.[[32]](#footnote-32) On this view then, education is essentially the process in which students learn (and learn to accept) certain theoretical propositions and concepts and thus develop a worldview that is characteristic of the scientific community to which they belong.

Let us now turn to the other Kuhn presented by Rouse. If the Kuhn presented above is the Kuhn of a theory-dominant view on science, the other Kuhn is that of the practice-based view on science: ‘Kuhn2 describes scientific groups as communities of believers. For Kuhn1 they are communities of fellow practitioners’.[[33]](#footnote-33) Given the entanglement of the concepts of a paradigm and a scientific community in Kuhn’s thinking, this reconceptualisation of what a scientific community is, is accompanied by a reconceptualisation of the concept of a paradigm:

[Paradigms are] concrete scientific achievements that disclose a field of possible research activities. Paradigms are not primarily agreed-upon theoretical commitments but exemplary ways of conceptualizing and intervening in particular empirical contexts. Accepting a paradigm is more like acquiring and applying a skill than like understanding and believing a statement. Actually, it involves multiple skills simultaneously: applying concepts, employing mathematical techniques (not just calculating, but choosing the right mathematics, applying it correctly to an empirical situation, knowing its limitations and approximations, etc.), using instrumentation and other apparatus, and recognizing opportunities for varying or intervening in particular theoretical or experimental situations.[[34]](#footnote-34)

In contrast to the theory-dominant view, which characterises scientific communities as having a shared set of beliefs and theoretical presuppositions, the latter view allows for disagreement between different practitioners in the same community.[[35]](#footnote-35) It also involves another understanding of what it means to learn to do science. Rather than conceptualising a scientist’s education as the process through which she learns to accept a certain theoretical worldview, it is seen as a process through which scientists acquire ‘a reliable sense of what they are dealing with, what can affect it, how it can make itself known, and what they can do with it’.[[36]](#footnote-36) In the discussion of the theory-centred reading of Kuhn, we have seen how Kuhn thought that proponents of different paradigms ‘see different things, and they see them in different relations one to the other’.[[37]](#footnote-37) I have linked this to Dewey’s notion of a spectator theory of knowledge: knowers are conceived as passive observers of the world, constructing representations on the basis of their observations, from a standpoint outside of the world. According to Rouse, a practice-based reading of Kuhn leads to a different assessment of the nature of “learning to see”:

Observation in the philosophical sense of registering what one sees is not central to science. Being attentive to what is going on in the context of one’s activities *is* important, but this attentiveness is influenced as much by one’s practical concerns and craft skills as by one’s theories. What is being criticized here is not an overemphasis on vision, but rather the philosophical account of vision as “observation.” Scientific observation is much more like what Heidegger has called “circumspection” (*Umsicht*) than like reporting what one sees, however much one acknowledges the theory ladenness of the reports.[[38]](#footnote-38)

This view on the process of learning science as an acquisition of practical understanding is linked to a different conceptualisation of the role and nature of paradigms in the education of a scientist. As we have seen, in the practice-oriented reading of Kuhn, paradigms are characterised as concrete scientific achievements and the acceptance of them is not to be seen as the acceptance of certain theoretical propositions, but as acquiring and applying a skill. These ideas can also be found in Kuhn’s work itself. Regarding the notion of a paradigm as a concrete scientific achievement, it is worthwhile to look at Kuhn’s explication of the concept in the postscript added to the second edition. There, Kuhn states that the term “paradigm” is actually used in two ways in his work. These two meanings are in a way parallel to the two readings of Kuhn’s work discussed above:

On the one hand, [the term “paradigm”] stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science.[[39]](#footnote-39)

The second description refers to the use of the term paradigm in the sense of an exemplar. Exemplars are concrete scientific achievements in the past that can function as a guide for further research by functioning as a model for the further continuation of the practice. An exemplar is thus not so much a theory. Although it does have theoretical and conceptual aspects, it is more broad and heterogeneous and embodies an ‘exemplary way of conceptualizing and intervening in particular empirical contexts’.[[40]](#footnote-40) Kuhn uses this notion of an exemplar to provide an alternative view on how one learns to do science. In the beginning of *The Structure of Scientific Revolution*, Kuhn had famously criticised philosophers for producing an image of science based on the reading of textbooks.[[41]](#footnote-41) In the postscript, he now criticises philosophers of science for not taking seriously enough the material encountered by science students:

Philosophers of science have not ordinarily discussed the problems encountered by a student in laboratories or in science texts, for these are thought to supply only practice in the application of what the student already knows. He cannot, it is said, solve problems at all unless he has first learned the theory and some rules for applying it.[[42]](#footnote-42)

The view criticised by Kuhn here is clearly an example of the theory-centred philosophy of science discussed above. It makes the practical exercises encountered by the student subservient to the theory, and presupposes that theoretical understanding must surely precede practical application. We will re-encounter this presupposition in section 4.4.1 (on “microworlds”). That the theory-centred view is the real target is made explicit by Kuhn himself. Whereas traditional philosophy of science saw scientific knowledge as ‘embedded in theory and rules’ and the problems which students encountered during their education as being ‘supplied to gain facility in [the] application [of the theory and rules]’,[[43]](#footnote-43) Kuhn thinks that this theory-centred view is misguided:

I have tried to argue, however, that this localization of the cognitive content of science is wrong. After the student has done many problems, he may gain only added facility by solving more. But at the start and for some time after, doing problems is learning consequential things about nature. In the absence of such exemplars, the laws and theories he has previously learned would have little empirical content.[[44]](#footnote-44)

Kuhn gives the example of the complexities involved in learning to use an abstract symbolic generalisation such as *f = ma*.[[45]](#footnote-45) This expression itself is just a ‘law-sketch or a law-schema’ and depending on the problem situation (free fall, the simple pendulum, interacting harmonic oscillators, the gyroscope) ‘the symbolic generalization to which [the logical and mathematical] manipulations apply changes’.[[46]](#footnote-46) The student does not learn an abstract theory, devoid of any practical bearing, which is then applied to practical examples in order to further exercise the grasp of the theory. Rather, the theory is grasped through an acquisition of similarity relations by working through concrete examples and learning to see the similarities between them. This also allows us to understand how this account moves away from a theory-dominant view of science, while still allowing that theoretical aspects of exemplars play a role. Kuhn himself in his discussion of paradigms as exemplars describes theories as tools which are only grasped through their use and their connection with the concrete aspects of the exemplars.[[47]](#footnote-47)

In this view, scientific education does not work by letting the student learn and understand certain theoretical propositions *in abstracto*. Rather, students learn to use and understand theories only by working through exemplary problems. As Rouse notes, this also involves a different understanding of what a theory is: ‘Instead of being seen as a network of interconnected sentences or conceptual schemes, theories are taken to be extendable models’.[[48]](#footnote-48) To learn a theory in this view is ‘learning to understand [exemplary] problem solutions in such a way that what was done in the model case can be extended and transformed to deal with a range of more or less similar cases’.[[49]](#footnote-49) This ability to extend and transform the model case, developed during education, is a skill. Put in another way: as with other tools, one only learns to use theories by concretely using them. Already in *The Structure of Scientific Revolutions*, Kuhn himself described this process as a process of skill acquisition through use:

[T]he process of learning a theory depends upon the study of applications, including practice problem-solving both with a pencil and paper and with instruments in the laboratory. If, for example, the student of Newtonian dynamics ever discovers the meaning of terms like ‘force’, ‘mass’, ‘space,’ and ‘time,’ he does so less from the incomplete though sometimes helpful definitions in his text than by observing and participating in the application of these concepts to problem solution. That process of learning by finger exercise or by doing continues throughout the process of professional initiation.[[50]](#footnote-50)

As we have seen, this view on scientific education was contrasted by Kuhn with the assumed view that ‘[s]cientific knowledge is embedded in theory and rules’.[[51]](#footnote-51) According to this view one first learns the theory, along with some general rules which govern its application. Kuhn emphasises this difference with his own account, because according to him there are no such explicit rules governing the use and extension of a theory. The ability to use and extend the theory is a skill that cannot be fully articulated.[[52]](#footnote-52) This also aligns with Kuhn’s criticism of the (philosophical) notion of an abstract scientific method (consisting of certain rules) which is supposed to be followed (implicitly or explicitly) by scientists in their research. One of the lessons provided by the history of science, according to Kuhn, was ‘the insufficiency of methodological directives, by themselves to dictate a unique substantive conclusion to many sorts of scientific questions’.[[53]](#footnote-53) In chapter 2, we have seen how van Musschenbroek made similar remarks on the limitations of a general methodology of science and also emphasised the necessity of learning by doing.

In the previous chapters, I have shown how van Musschenbroek emphasises the importance of repeating experiments and performing variations on the experiment. I have argued that one of the reasons that van Musschenbroek emphasises the importance of repeating experiments is linked to his view on learning by doing. By repeating an experiment, one can gain and improve one’s practical understanding. One can understand this process in terms of an acquisition of “circumspection”. We have encountered this term already when I discussed Rouse’s reconceptualisation of what it means to learn to see. This kind of seeing was indeed described as a practical grasp of the experiment. Although not using the term “circumspection”, Hacking makes a very similar remark in relation on the difference between “observation” and “being observant” and their role in experimental practice:

Observation, in the philosophers’ sense of producing and recording data, is only one aspect to experimental work. It is in another sense that the experimenter must be observant – sensitive and alert. Only the observant can make an experiment go, detecting the problems that are making it foul up, debugging it, noticing if something unusual is a clue to nature or an artefact of the machine.[[54]](#footnote-54)

Hacking likewise states that the role of performing experiments in a scientist’s education is exactly to enable her to develop this ability to be observant.[[55]](#footnote-55) For Rouse, this circumspection is not only something which one performs in one experiment, but rather something which guides the research as a whole: ‘It is a practical assessment of what it makes sense to do, given the resources available and the aims and standards that govern scientific practice within a given field’.[[56]](#footnote-56) Rouse uses this notion, and its role in guiding research, to further underscore the non-representational and non-theory-centred nature of his views on scientific practice:

We are accustomed to crediting theoretical imagination with [a leading role in directing experimental work]; philosophers repeatedly emphasize that experimentation is pointless without some idea of what the experiment is supposed to reveal and why it would be important. But the error is in confining such ideas to theoretical representations. A good experimenter must have a practical grasp of the workings of her apparatus and its possibilities and limitations. This “feel” for the instruments, more a practical craft knowledge than a theoretical representation, does not just tell scientists when their equipment is working properly. It also suggests possible directions for investigation – to take advantage of the capabilities of one’s tools – as well as constraints on the scope and precision of the results obtainable.[[57]](#footnote-57)

In chapter 2, we have seen how van Musschenbroek emphasised the importance of knowing one’s equipment. We have also seen how he conceptualised the repetition of experiments as a learning process through which one acquires a feel for the instrument. The view presented in the citation contrasts with a theory-centred view in which experiments are made subservient to theory and their role is reduced to the function of testing theories or providing data for theory construction. Although van Musschenbroek at several instances described the role of experiments as providing data on which theories can be built, we have seen that his views on experimentation were more nuanced than these remarks would suggest. In the previous chapter, we have seen how in van Musschenbroek’s research practice, experiments do more than just providing raw data for theory construction. A big part of his research practice consisted in the exploration of the workings of his apparatus and its possibilities and limitations.

The aforementioned passage from Rouse also makes clear how scientific research practices can be understood as having a certain directedness without necessarily taking this directedness as theory-driven. Research is not necessarily driven by the desire to develop, test, or refine theories, but can also take the form of an exploration of the experimental set-up, its components, and their behaviour and properties. In the previous chapter, I have shown how the research on electricity leading to the experimental set-up used by van Musschenbroek could be seen as an example of this.[[58]](#footnote-58) I will come back to the notion of exploratory experimentation in section 4.4.2.

According to Rouse, a full account of the role of experiments in scientific practice and the centrality of practical understanding in it, requires a reconceptualisation of ‘the place of laboratories and their apparatus within our understanding of science’.[[59]](#footnote-59) I will discuss this reconceptualisation in the next section. I will more generally look at experimental practice as a learning practice. In other words: if we step away from a theory-centred view on experimental practice, how can we conceptualise experimental research as a learning process?

**4.4 Experimental Practice as a Learning Practice**

**4.4.1 The Construction and Spread of “Microworlds”**

In section 2.2, I have discussed Hacking’s criticism of traditional philosophy of science’s ‘single-minded obsession with representation and thinking and theory, at the expense of intervention and action and experiment’.[[60]](#footnote-60) He himself argued for a shift from representing to intervening and emphasised the importance of active intervention and experimentation in scientific practice. Hacking also wanted to move away from a theory-centred view on experimentation. Rather than seeing experimentation as always being informed by theory, Hacking argued that ‘[e]xperimentation has a life of its own’.[[61]](#footnote-61)

This is of course not to deny that experimenters perform an experiment with a certain idea of what they are dealing with or what they want to investigate. To clarify what he means by emphasising that experimentation has a life independent from theory, Hacking distinguishes a strong and a weak version of the claim that ‘an experiment must be preceded by a theory, that is, an idea’.[[62]](#footnote-62) The weak version states that ‘you must have some ideas about nature and your apparatus before you conduct an experiment’.[[63]](#footnote-63) Hacking agrees with this and takes it to be uncontroversial. A certain understanding of the experiment is necessary in order to learn something from its result.[[64]](#footnote-64) The strong version of the claim states that ‘your experiment is significant only if you are testing a theory about the phenomena under scrutiny’.[[65]](#footnote-65) It is this view that Hacking criticises by referring to several historical examples in which experimental research was performed without being preceded or informed by a theoretical understanding of the phenomena under investigation. We have seen that van Musschenbroek emphasised that the phenomena related to electricity were not understood, but that this did not prevent him from pursuing experimental research on electricity. I have also shown how developments in the study of electricity before van Musschenbroek could be understood independently from theoretical considerations of the phenomena related to electricity. I will come back to this in section 4.4.2.

Hacking also shows that the relation between theory and experimentation is complex and can take different forms, so that experiments can play different roles in scientific practice.[[66]](#footnote-66) One of the (neglected) roles that experiments play in scientific practice is what he calls ‘the creation of phenomena’.[[67]](#footnote-67) This function of experiments provides further arguments for the claim that experimentation has a life of its own, and also plays an important role in Rouse’s notion of the construction of “microworlds”, which I will discuss shortly. Hacking remarks that the term “phenomenon” has a long (philosophical) history.[[68]](#footnote-68) In chapter 2, we have indeed seen that van Musschenbroek provided a definition of the term in his textbooks and uses it in his definition of a law of nature. Hacking himself uses the term in the following way:

A phenomenon is *noteworthy*. A phenomenon is *discernible*. A phenomenon is commonly an event or process of a certain type that occurs regularly under definite circumstances. The word can also denote a unique event that we single out as particularly important. When we know the regularity exhibited in a phenomenon we express it in a law-like generalization. The very *fact* of such a regularity is sometimes called the phenomenon.[[69]](#footnote-69)

Phenomena can in some cases be expressed in a law-like generalization, but this does not have to be the case. Hacking further clarifies that he regards a phenomenon as ‘something public, regular, possibly law-like, but perhaps exceptional’.[[70]](#footnote-70) In line with his argument that philosophy of science should move away from an emphasis on passive observation and instead focus on active intervention, Hacking states that there are only a limited number of phenomena which can be observed in nature. As examples he refers to the movements of the stars and planets, and the tides.[[71]](#footnote-71) Nature however abounds in complexity and variety, so phenomena are very rare to be found by mere observation. Sounding remarkably like van Musschenbroek’s comments on so-called special laws of nature, Hacking says: ‘Each species of plant and animal has its habits; I suppose each of those is a phenomenon. Perhaps natural history is as full of phenomena as the skies of night’.[[72]](#footnote-72) This does not mean that we only know a limited number of phenomena. But rather than saying that scientists have by now discovered many phenomena, Hacking argues that these phenomena have been “anufactured. That is, their occurrence depends on a specific material set-up and active intervention and manipulation. They are not given, but made.[[73]](#footnote-73) As an example, Hacking refers to the Hall-effect, first manufactured by Edwin Herbert Hall (1855-1938) in 1879.[[74]](#footnote-74) To have a clear view on what Hacking does (and does not) mean by saying that the Hall-effect is manufactured, it is worthwhile to cite his own comments at length:

I suggest […] that the Hall effect does not exist outside of certain kinds of apparatus. Its modern equivalent has become technology, reliable and routinely produced. The effect, at least in a pure state, can only be embodied by such devices.

That sounds paradoxical. Does not a current passing through a conductor, at right angles to a magnetic field, produce a potential, anywhere in nature? Yes and no. If anywhere in nature there is such a pure arrangement, with no intervening causes, then the Hall effect occurs. But nowhere outside the laboratory is there such a pure arrangement. There are events in nature that are the resultant of the Hall effect and lots of other effects. […] We should not have the picture of God putting in the Hall effect with his left hand and another law with his right hand, and then determining the result. In nature there is just complexity, which we are remarkably able to analyse. We do so by distinguishing, in the mind, numerous different laws. We also do so, by presenting, in the laboratory, pure, isolated, phenomena.[[75]](#footnote-75)

In chapter 2, I have shown how van Musschenbroek’s reflections on the role and nature of experiments was informed by his views on the complexity of nature. Van Musschenbroek emphasised the necessity of repeating an experiment and varying the experimental set-up in order to identify all the variables influencing the outcome of the experiment and in the end (if possible) arrive at an identification of a stable relationship between a determinate cause and a determinate effect. In order to do this, the relevant variable had to be shielded from disturbing factors. The process of repeating and varying an experiment could thus be seen as a learning process in which one acquires knowledge about a particular experimental set-up, and the (causal) properties of the materials, substances, and processes at play. To further analyse this learning process, I will now turn to Rouse’s notion of a “microworld”.

The concept of a microworld is part of Rouse’s development of a non-theory-centred account of scientific practice. The notion of a microworld builds upon Hacking’s work on the experimental manufacture of phenomena, but also incorporates the analysis of circumspection and the nature of learning science as discussed in section 4.3. The emphasis on circumspection already reflected a move from a theory-centred view, to a practice-centred view: ‘scientific research is a circumspective activity, taking place against a practical background of skills, practices, and equipment (including theoretical models) rather than a systematic background of theory’.[[76]](#footnote-76) In section 4.3, we also saw how in a practice-oriented reading, Kuhn conceptualised the education of a scientist as the development of this kind of circumspection. His views on the process of education were also non-theory-centred. Kuhn argued against the view that a student first learns to understand the theory (and some rules guiding its applications), and then exercises this understanding by means of concrete applications. Rather, the understanding of the theory was developed by, and embedded in the exercises and experiments that exemplified the theory. Rouse develops the concept of a microworld in order to make an analogous argument for scientific research practices as practices of learning in the world.

In traditional philosophy of science, scientific knowledge had been construed as universally valid knowledge of general laws. These are then applied to particular situations, if necessary with the additional use of bridge principles and the determination of factors relevant for the application of the law. According to this view, technical and practical experimental abilities ‘[derive] from knowledge (or hypothetical assumption) of universal laws and theories’.[[77]](#footnote-77) Rouse’s decentralised account puts this image on its head:

In scientific research, we obtain a practical mastery of locally situated phenomena. The problem is how to standardize and generalize that achievement so that it is replicable in different social contexts. We must try to understand how scientists get from one local knowledge to another rather than from universal knowledge to its local instantiation.[[78]](#footnote-78)

This process should be understood as a practical and material process. Rouse thus invites us ‘to take seriously the *labor*atory’.[[79]](#footnote-79) Following Hacking, Rouse observes that many of the objects and phenomena investigated by scientists do not occur in nature but only in the highly contrived and controlled environment of the laboratory. Examples include electrical currents, the objects of particle physics, substances used in chemistry, and special strains of bacteria and animals cultivated with the specific aim of studying them in laboratory settings.[[80]](#footnote-80) The list could be expanded with numerous examples. Rouse concludes that ‘[t]he study of artificially constructed or manipulated phenomena has become characteristic of most of the modern sciences’.[[81]](#footnote-81)

This brings Rouse to introduce the notion of an “(experimental) microworld”, which he uses to conceptualise the central role of the laboratories and the activities performed there for scientific practices. Experimental microworlds are

settings that allow interactions among selected objects to be brought about and closely monitored under conditions of causal isolation or randomization (other objects or events that might affect the outcome of the intended interactions are either excluded from the setting of the microworld, or their influence is deliberately randomized to prevent any systematic effect).[[82]](#footnote-82)

The process of isolation has two components. Rouse distinguishes internal isolation from external isolation of the microworld. The internal isolation refers to the aforementioned use of artificially produced objects, which allows the researcher to have a precise idea of what the internal components of the microworld are and how they will behave in certain settings. The external isolation refers to the shielding of the experimental set-up from external causal factors.[[83]](#footnote-83) This isolation is made in order for the manipulations performed by the experimentalist to be informative. It allows the experimentalist to have a good grasp of the nature and behaviour of the system that is being manipulated. By manipulating the system, the experimentalist can learn new things. Or she can learn something about a new object by introducing it in the ‘entirely characterized system’ of the microworld.[[84]](#footnote-84)

By emphasising the role of manipulation, Rouse follows Hacking in making the move from representation to intervention. In section 4.3, we have seen how Rouse de-emphasised the role of passive observation in science in favour of what he called “circumspection”. Rather than saying that the experimentalist observes an outcome of her experiment, Rouse uses the term “tracking”:

Tracking an experiment […] involves monitoring the entire progress of the experiment beginning with its construction. It is a matter of seeing that things are working right rather than just seeing what the outcome is. […] The actual activity of monitoring is less a thematic perceptual act than it is a circumspective attentiveness to the entire course of events.[[85]](#footnote-85)

This tracking is on the one hand made possible by the feel for the experimental design that the experimenter has acquired through experience, and on the other hand facilitated by the design of the experiment.[[86]](#footnote-86) In chapter 2, we have seen that van Musschenbroek emphasised both in his oration on the method of performing experiments.

The manipulation of the microworld again points towards the importance of intervening in the world (as opposed to merely representing). In section 4.2, I have argued that the shift away from viewing science as a representation of the world should not be seen as a sceptical attack on the possibility of scientific learning. It does involve a shift in understanding what it means to learn things in science: rather than learning things *about* a world that is supposed to lie hidden behind the phenomena or which awaits us at the end of our inquiries, we are already and always *in* the world. Scientific learning is an activity by which we learn *in* this world and (re)shape it and our connections with it. For Rouse, his account of the construction and manipulation of microworlds leads to ‘a robust view of causation’ in contrast to the ‘Humean suspicion of causes’ which is implied by a representationalist, empiricist epistemology.[[87]](#footnote-87) An epistemology which sees knowers as passive observers indeed leads to a sceptical worry of how we might ever observe a causal connection between phenomena. A view that takes into account the role of intervention and manipulation in scientific practice, however, acknowledges that ‘scientific research proceeds with the agent’s presumption of causal efficacy rather than the observer’s concern to discern it’.[[88]](#footnote-88) In chapter 2, I have discussed van Musschenbroek’s notion of a “true cause (*vera causa*)”, in which the ability to manipulate the cause and (experimentally) demonstrate the link between the cause and the effect played an important role.

As I mentioned, Rouse developed the notion of a “microworld” to provide an alternative for the traditional philosophical view on scientific knowledge. Rather than discovering general, theoretical laws, which can then be applied in specific, local context, Rouse saw the spread of results obtained in the laboratory as involving a move ‘from one local knowledge to another rather than from universal knowledge to its local instantiation’.[[89]](#footnote-89) With the concept of a microworld in place, we are now able to explain what Rouse means by this move from one local knowledge to another. In our discussion of Kuhn’s views on education we have seen that he wanted to turn away from conceiving of scientific education as the student learning a certain abstract theory, which is afterwards applied to concrete situations. Rather, what one studies are concrete examples, which function as (what Rouse calls) extendable models. To learn a theory is ‘learning to understand [exemplary] problem solutions in such a way that what was done in the model case can be extended and transformed to deal with a range of more or less similar cases’.[[90]](#footnote-90) In the case of microworlds, the knowledge produced in these microworlds should not be seen as universal knowledge which is then afterwards applied in concrete settings.[[91]](#footnote-91) This transfer of knowledge on the one hand takes place in the replication of experimental results in other laboratories, and on the other in the transfer of the knowledge produced in the laboratory to society at large. With regard to the replication of results in other laboratories, Rouse refers to sociological and ethnographical studies of scientific practices that show how this process involves the necessity of persons working in other laboratories to develop the relevant skills and often also involves ‘transferring the people involved and even relevant features of the physical setting itself’[[92]](#footnote-92). With regard to the transfer of knowledge produced in the laboratory to society at large, following Bruno Latour, Rouse conceives the transfer of knowledge produced in the microworld to other settings as a process involving ‘transferring the conditions of the laboratory itself out into the world’.[[93]](#footnote-93) The transfer of knowledge thus involves work, on the one hand in adapting the products of the laboratory to make them more suitable for use in different contexts, on the other in adapting these contexts to make them into suitable locations for the use of the laboratory products.

The process by which laboratory products are adapted to make them suitable for contexts outside of the laboratory is referred to by Rouse as a process of standardisation, i.e. ‘the transformation of a tool originally designed for a highly specific task within a particular context into a more general-purpose item of equipment’.[[94]](#footnote-94) He refers to examples from science studies which illustrate and clarify this concept of standardisation, such as Ludwik Fleck’s discussion of the development of the Wassermann reaction, the evolution of gene cloning, and Latour and Woolgar’s discussion of the investigation of thyrotropin releasing hormone (TRH).[[95]](#footnote-95) This process of standardisation is not limited to tools and procedures, but also takes place with theories and concepts.[[96]](#footnote-96)

David Gooding’s analysis of Michael Faraday’s work leading to the electromagnetic motor serves as a good example of the importance of skill in the production of a phenomenon and as a clear example of standardisation. Gooding shows how the series of experiments performed by Faraday can be seen as a learning process.[[97]](#footnote-97) This learning process involved the acquisition of certain skills, the continuous (re)design of the experimental set-up, and a continuous reinterpretation of what was happening in the experiment. In the end, Faraday arrived at a somewhat stable configuration that allowed him to produce a phenomenon, namely a ‘continuous motion of a current-carrying wire about a magnet’.[[98]](#footnote-98)

Gooding also discusses the specific way in which Faraday made it possible for this phenomenon to be reproduced in different contexts. To do this, he constructed a special device that he sent to a number of European scientists along with instructions for its use. The only thing the user had to do, was to fill it with a bit of mercury and connect the two ends to a battery. Once this was done, the device would display the phenomenon. The fact that the receiver did not have to do much was made possible by the design of the device, which, as Gooding puts it, ‘packaged the resources and skills [Faraday] had brought together’.[[99]](#footnote-99) Users did not have to go through the same process as Faraday in learning to work with and understand the behaviour of wires and electric needles. The device made by Faraday is thus a good example of what Rouse calls standardisation, in that it allows for the phenomenon to be produced reliably in contexts outside of the laboratory. This use of standardised equipment and products is now very widespread in scientific practice. In the discussion of the notion of a microworld we have seen how Rouse conceives of the laboratory as a local reconfiguration of the world in which specially prepared objects and instruments were used. Scientists typically no longer make these themselves, but rely on technicians or industrial processes to manufacture them. I will come back to this in section 4.4.3.

The local knowledge obtained in the laboratory is not only being spread by adapting the products of the laboratory to allow them to work outside the laboratory context, but also by adapting the outside world to make it resemble the circumstances of the microworld. To illustrate this, Rouse refers to the effort put in establishing and maintaining standards of measurement. Other examples include:

[T]he development of the chemical industry to manufacture the pure substances that chemical know-how presupposes, the vast transformation in farming practices that has permitted the extensive application of agricultural science, or the development of machine tools capable of fabricating metals to exact tolerance.[[100]](#footnote-100)

The last example of course reminds us of the varying results in van Musschenbroek’s research on the strength of metals and alloys, due to the lack of purified samples and standardised procedures. In the discussion of Hacking’s view of phenomena, we have seen that he believes that only a limited amount of phenomena are there to be discovered by means of passive observation. Given the causal complexity of the world, stable phenomena have to be manufactured by means of active intervention. Rouse further developed this line of thought by showing how this manufacture involved the construction of a microworld. These microworlds are thus constructed to circumvent the (causal) complexity of the natural world, by manufacturing a simplified microworld containing a limited amount of objects whose properties and behaviour are well-known and stabilised, and which are shielded from outside causal influences.[[101]](#footnote-101) The transfer of products and results from the laboratory to the outside world requires similar interventions and thus involves reconstructing the outside world to make it resemble the laboratory. This reconstruction includes the aforementioned use of standardised products, but also involves practices of isolating, controlling, and artificially simplifying the environment in which the products are to be used.[[102]](#footnote-102)

Rather than reducing this learning process to the accumulation of abstract, propositional knowledge, Hacking and Rouse invite us to see this learning process as an embodied process. On the one hand, this means that it invites us to look at the embodied nature of scientific practice and the importance of skills. On the other hand, it also invites us to consider the ways in which the results of this learning process are embodied in instruments, standardised objects and processes, and structured environments.

**4.4.2 Exploratory Experimentation**

In the previous section, I have shown how Hacking and Rouse wanted to move away from a theory-centred view on scientific practices, and argued that experimentation has a life of its own. Friedrich Steinle likewise presents his notion of “exploratory experiments” as ‘a systematic account of a specific type of experimentation – an experimentation which is not, as in the “standard view”, driven by specific theories’.[[103]](#footnote-103)

In order to illustrate what he means by exploratory experimentation, Steinle compares two (types of) experiments made by Ampère at the beginning of his career. The first experiment involved a so-called “astatic magnetic needle”. This needle was set-up in such a way that it could only move perpendicularly to the plane of the magnetic dip, thus shielding the needle from the effects of terrestrial magnetism. Ampère then tested the effects of an electric wire on the needle. On the basis of these experiments he ‘formulated the general rule that the needle always swings into a position perpendicular to the direction of the wire’.[[104]](#footnote-104) In the series of experiments leading to the general rule, there was no theory of electricity or magnetism guiding the investigations. The research consisted in a systematic variation of the position of the needle vis à vis the wire.[[105]](#footnote-105) The other experiment involved two spiral-shaped electric wires. One of the wires was static, being fixed on a stand, the other was suspended and could move freely towards and away from the static wire like a pendulum. Unlike the other experiment, this experiment was theory-driven. The speculation at play was that ‘all magnetism might be made up of (hypothetical) circular electric currents within the mass of magnetic bodies’.[[106]](#footnote-106) Ampère thus expected that electric spiral wires would behave like magnets and attract or repel one another. The research thus took the shape of a search for the set-up which would most clearly exhibit the effect that would confirm Ampère’s theoretical speculation.[[107]](#footnote-107) The difference between these two kinds of experiment(al series) was reflected in the experimental set-up itself. The set-up of the first experiment was flexible and allowed for various kinds of variations and outcomes. The set-up of the second experiment was highly specific, ‘at the price of a considerable loss of flexibility and openness to unexpected experimental outcomes’.[[108]](#footnote-108)

To distinguish these two types of experiments, Steinle introduces the term “theory-driven” versus “exploratory” experimentation.[[109]](#footnote-109) The latter is typically undertaken in periods where ‘no well-formed theory or even no conceptual framework is available or regarded as reliable’.[[110]](#footnote-110) In the previous chapter, we have seen that this is exactly the way van Musschenbroek characterised the state of the art in the research on electricity. The aim of exploratory experimentation, according to Steinle, is ‘to obtain empirical regularities and to find out proper concepts and classifications by means of which those regularities can be formulated’.[[111]](#footnote-111) In the example of the exploratory experiments with the astatic magnetic needle, the empirical regularity obtained by Ampère was: “the needle always swings into a position perpendicular to the direction of the wire”. As an example of a case in which new concepts and classifications had to be developed in order for the formulation of an empirical regularity to be possible, Steinle refers to the distinction between two kinds of electricity made by du Fay, which we have encountered in the previous chapter. As one might recall, du Fay distinguished between two types of electricity (and two corresponding classes of materials which produce that specific type), namely “resinous” and “vitreous”. According to Steinle, du Fay’s research can be understood as an example of exploratory experimentation and the concepts of “resinous” and “vitreous” electricity were introduced in order to enable him to attain the aforementioned epistemic goal of formulating a regularity. It was only after this conceptual distinction was introduced that du Fay was able to ‘account for hundreds of experiments’ by subsuming them under the regularity that ‘similarly electrified bodies [repel] each other, dissimilarly electrified ones [attract] each other’.[[112]](#footnote-112)

Although the notion of “exploration” might make one expect that exploratory experimentation encompasses a wide variety of experiments, Steinle emphasises that his notion of exploratory experimentation is quite specific. That is, it is a type of experimentation characterised by a specific *epistemic goal* , namely ‘the goal of finding empirical rules and systems of those rules’.[[113]](#footnote-113) Although in one passage, Steinle identifies the specific goal of exploratory experimentation as being ‘directed at ordering and categorizing, at the level of categories of concepts, as elements of languages’[[114]](#footnote-114), other passages make clear that the aim of formulating laws and regularities is the central and characteristic aim of exploratory experimentation. The introduction of new categories is subservient to this main goal. That is, exploratory experimentation ‘typically aims at the level of laws, and sometimes that of concepts, but not of theories’.[[115]](#footnote-115) Conceptual revision or innovation is only undertaken when it is necessary for the formulation of regularities, and even then often only reluctantly.[[116]](#footnote-116)

Steinle’s main aim is to argue that exploratory experimentation has its own systematicity or directedness, despite the fact that it is not theory-driven. He provides the following characterisation of the procedures characteristic of exploratory experimentation:

The most prominent characteristic of the experimental procedure is the systematic variation of experimental parameters. The first aim here is to find out which of the various parameters affect the effect in question, and which of them are essential. Closely connected, there is the central goal of formulating empirical regularities about these dependencies and correlations. Typically they have the form of “if – then” propositions, where both the if- and the then-clauses refer to the empirical level. In many cases, however, the attempt to formulate regularities requires the revision of existing concepts and categories, and the formation of new ones, which allow a stable and general formulation of the experimental results. It is here, in the realm of concept-formation, where exploratory experimentation has its most unique power and importance. There is, finally, often the attempt to develop experimental arrangements that involve only the necessary conditions for the effect in question and thus represent the general regularity or law in a most obvious way. Those experiments are attributed a particular status in that they serve as core effects to which all other phenomena of the field can be “reduced.”[[117]](#footnote-117)

In a later publication, Steinle further clarifies what he meant by “if-then” conditions. The empirical rules that are the goal of exploratory experimentation according to him, typically take the following form: ‘If particular conditions obtain, then this or that effect may be observed’.[[118]](#footnote-118) He later refers to these “if-then” conditions as ‘“laws” or “empirical laws” that specify the conditions under which a particular phenomenon occurs’.[[119]](#footnote-119) He emphasises that both the conditions mentioned in the if-clause, and the phenomenon dependent on these conditions are all ‘at the level of phenomena’.[[120]](#footnote-120) This of course resonates with van Musschenbroek’s thinking about laws, which, as we have seen in chapter 2, he characterised as empirical regularities. We have also seen how van Musschenbroek thought that the search for laws should be the central aim in natural philosophical inquiry.

In his reflection on the nature of laws discovered by means of exploratory experimentation, Steinle adds:

In physics, laws of this kind are usually expressed in mathematical equations. Though this adds the additional dimension of quantitative precision in the relationship among conditions, it does not change the underlying notion of law. These laws demand no recourse to theoretical or hidden entities.[[121]](#footnote-121)

If we replace “equations” with “proportions” we can find an example of such a law in van Musschenbroek’s work, namely the law related to the load that can be carried by a beam.[[122]](#footnote-122) He arrived at this law after having performed tests on specimen made from different types of wood, and with different dimensions, i.e. after varying several parameters. We have seen how in his research on the strength of alloys, van Musschenbroek systematically varied the ratio between two (or several metals) in order to find out which ratio yielded the strongest alloy. Following Steinle, one could argue that this constitutes an example of exploratory experimentation with the aim of finding a regularity of the form “If an alloy is made, containing the metals X and Y in a ratio of A to B, then the resulting alloy will have the maximum strength (obtainable with that combination of metals)”. However, we have also seen how van Musschenbroek was sceptical towards the establishment of general laws. In the case of the law related to the load that can be carried by a beam, we have seen that van Musschenbroek emphasised that it should only be taken as applying to wooden beams, not to all materials in general. With regard to his research on alloys, we have seen how he emphasised the differences between the properties of different metals, even metals that were given the same name. Depending on the place where the metal originated, it could have different properties. Rather than being aimed at finding general regularities, van Musschenbroek’s research (especially the later research on the strength of alloys) seemed to aim more at exhibiting the variety of nature and learning about the *specific* properties of *specific* types of substances or materials, rather than at discovering general regularities.

In chapter 2, we have seen how this emphasis on variety and specificity was reflected in van Musschenbroek’s thinking on laws of nature. He distinguished between general laws and special laws. The latter only applied to certain species or substances. Van Musschenbroek emphasised that there were probably more special laws than general ones. In chapters 2 and 3, we encountered several instances where van Musschenbroek criticised hasty or premature generalisation. In itself, Steinle’s formulation of the kind of regularities that exploratory experimentation aims to find, seems to allow for van Musschenbroek’s emphasis on special laws. The “if-then” formulation provided by Steinle leaves open the level of generality inherent in the regularity. One can formulate very general regularities such as “If two bodies are similarly electrified, they will repel each other”. But it also leaves open the formulation of very specific conditional statements, taking into account all the relevant variables. In the case of van Musschenbroek’s research on alloys, this would result in “regularities” of the form “If an alloy of pure silver and Swedish red copper are cast in a mould of sand from Brussels, in a ratio of 5 units of silver to 1 unit of red copper, then the resulting alloy will have the maximum strength (for that type of alloy cast in that specific way)”. This statement contains all the variables that van Musschenbroek had found to have an effect on the strength of the material (the type of mould used, the ratio between the metals, and the origin of the metal) and states a relationship between a very specific set of variables, which one could perceive as a “regularity” observable given a very specific set of conditions. However, one gets the sense that trying to fit van Musschenbroek’s research on alloys in this specific framework results in a very artificial construal of his research goals. Moreover, Steinle seems to presuppose that exploratory experimentation is always aimed at the most broad generalisation possible.[[123]](#footnote-123) With its emphasis on the formulation of general empirical regularities as the main goal of exploratory experimentation, Steinle’s conception does not seem completely adequate to capture the specificities of van Musschenbroek’s experimental practice.[[124]](#footnote-124) This becomes especially clear when we look at van Musschenbroek’s research on electricity. As we have seen, this research was also characterised by a systematic variation of different experimental parameters. However, given the variety inherent in van Musschenbroek’s research on electricity, it is not clear what kind of empirical generalisation van Musschenbroek was hoping to find or even whether this was even (implicitly or explicitly) the goal of his series of experiments.

One could of course argue, given van Musschenbroek’s explicit comments on the search for laws as the most important task for natural philosophy, that it was probably also the goal informing his research on electricity. Even if one grants this, the point remains that his series of experiments does not seem to aim at finding a *specific* generality. As such, it is different from the examples of exploratory research provided by Steinle, where the experimenters already seem to have an idea of the kind of regularity they are looking for. Dufay wanted to find a regularity related to the attraction and repulsion of electrified bodies. Ampère wanted to find a regularity related to the behaviour of the magnetic needle relative to an electric wire. No such specific aim seems to have motivated van Musschenbroek’s research. In that sense, it is perhaps even more exploratory than the examples of exploratory experimentation provided by Steinle. I will come back to this when I discuss the limitations of Steinle’s conception of exploratory experimentation.[[125]](#footnote-125)

First, I will discuss another aspect of Steinle’s conception of exploratory experimentation which *will* turn out to be helpful to analyse van Musschenbroek’s experimental practice and which will also allow me to further delineate both the potential and limitations of Steinle’s specific take on exploratory experimentation.

One of the last characteristics of exploratory experimentation mentioned by Steinle, was that it involved ‘the attempt to develop experimental arrangements that involve only the necessary conditions for the effect in question and thus represent the general regularity or law in a most obvious way’.[[126]](#footnote-126) These “simple cases” can then be used to explain other phenomena in the field by reducing them to the simple case. Steinle distinguishes this type of explanation from what we typically refer to as explanations, namely the explanation of a phenomenon on the basis of ‘other phenomena of a different kind’ or ‘hypothetical (“theoretical”) entities not themselves on the phenomenal plane’.[[127]](#footnote-127) In the type of “reduction” Steinle is referring to (in the way the term was also used by Faraday and Ampère), the reduction ‘take[s] place entirely within one and the same domain of phenomena. Both *explanans* and *explanandum* are phenomena of the same kind’.[[128]](#footnote-128) These explanations are made by structuring the phenomena into a certain hierarchy, with “simple” phenomena being used to explain more “compound” ones. This is done by reducing the compound phenomena to the simple phenomena. The latter are defined as the phenomena which involve ‘only the minimal set of experimental conditions that are necessary to produce the defining effects of the relevant domain’.[[129]](#footnote-129) The reduction takes the form of showing that more compound phenomena can be understood as a modification of the simple case or a superposition of different simple ones.[[130]](#footnote-130) Given that all the phenomena are from the same (experimental) domain, this reduction can in principle take the form of an experimental demonstration, by actually building the compound phenomenon oneself on the basis of the simple ones.[[131]](#footnote-131)

As mentioned, according to Steinle, Faraday and Ampère themselves used this notion of reduction and put it into practice in their experimental research. In van Musschenbroek’s oration on the method of experimental philosophy, there is a passage that contains a similar message. I have discussed this oration in chapter 2, where I discussed van Musschenbroek’s views on mathematics and idealisation as discussed in this oration. At the end of the oration, however, van Musschenbroek added a small passage on the role of experiments:

But there remains time, so that we will briefly show, how physics should be made to advance through experiments, and could remain a certain knowledge. This can be done, if the effects of bodies should be explained through other effects. Because every time several [effects] occur in a very entangled way, either due to the concurrence of several causes at the same time, either due to [their] subtlety, or either due to the fact that they elude the senses with [their] speed. In these cases, they are known [only] in an obscure way, and more often not at all. These [effects] are however made easy for the intellect, when experiments are made with other, more simple bodies, which exhibit effects of the same kind […] because these will elucidate the more subtle operations through a perfect similitude. Thus experiments are explained by means of experiments.[[132]](#footnote-132)

This passage in van Musschenbroek’s oration is however not the only reason why I included a more elaborate discussion of this specific aspect of Steinle’s characterisation of exploratory experimentation. For Steinle, this points to the importance of making many different experiments in the course of exploratory experimentation. To begin with, an assessment of ‘the simplicity or complexity of a given effect can be made only in the context of a larger field of phenomena’.[[133]](#footnote-133) This assessment is based on a “systematic phenomenology” which has been established by means of exploratory experimentation. By means of experimental research, one can arrive at a collection of phenomena and organise them in a hierarchical structure. A “systematic phenomenology” thus refers to ‘a network of phenomena in which multiple regularities are knit together’.[[134]](#footnote-134) This led Steinle to add a further characteristic to exploratory experimentation, namely that ‘individual experiments carry little weight in exploratory experimentation, it is chains, series, or networks of experiments that lead to conclusions’.[[135]](#footnote-135)

I emphasise this notion of a systematic phenomenology because it provides an entry point to develop a more broad characterisation of exploratory experimentation which can do justice do the kind of research performed by van Musschenbroek. I have argued that Steinle’s conception of exploratory experimentation seems to require that the researcher already has a certain idea of the kind of regularity she is looking for. However, some passages in his work (implicitly) seem to allow for a more indirect search for regularities. In one passage, Steinle describes exploratory experimentation as ‘striv[ing] toward a preliminary orientation and organisation, removed from any theory’.[[136]](#footnote-136) In a list of literature providing examples of exploratory experimentation, he refers to Klaus Hentschel’s work on early twentieth-century spectroscopy, which discusses how the researchers in the field focussed on phenomenological classification.[[137]](#footnote-137) In the article, Hentschel provides a list of “strategies” used in this research. He compares these strategies with the characteristics provided by Steinle for exploratory experimentation. He argues that Steinle’s ‘establishment of empirical rules’ can be ‘unravelled’ into the following strategies:

4. Extension and amplification of the empirical basis[,] 4a[.] through systematic variation of empirically adjustable parameters[.] 5. Development of a fine-grained classification scheme, 5a[.] partially connected with new means of representation of data (graphs)[,] 5b[.] and exclusion of data that resist this classification.[[138]](#footnote-138)

That is, rather than starting with a clear idea of the kind of regularity that is sought for, exploratory experimentation can also take place without such a preconceived regularity in mind. By collecting and classifying phenomena, salient regularities can suggest itself to the researcher. In chapter 2, I have discussed how van Musschenbroek emphasised the need to collect an experimental history. In the oration on the method of performing experiments, this was explicitly linked to the practice of classification and the search for regularities:

All the phenomena that are collected in this way, will have to be ordered in their classes, those that are common to all, are to be put separately, distinct from singular [properties]: If however in these or others a magnitude should hold, so that they could in turn be compared with one another, they become an object for a mathematician, who by acquiring new data, can enlarge them with his demonstrations, and elucidate [them], and he will come closer to the determination of the causes, whether general or singular, and their magnitudes and proportions.[[139]](#footnote-139)

This passage also illustrates the aforementioned point that van Musschenbroek’s research was not only aimed at finding generalities, but also aimed at discovering properties that were specific to certain kinds of objects or classes of objects.

Despite his very specific characterisation of exploratory experimentation, Steinle is aware of the fact that the concept might require revision, or that new concepts might have to be introduced.[[140]](#footnote-140) He refers to several studies which have either revised the concept, or which have proposed new ones.[[141]](#footnote-141) Kevin Elliott has proposed to broaden the notion of exploratory experimentation, and provides a (tentative) taxonomy of different kinds of exploratory experimentation. In general, exploratory experimentation has ‘two fundamental characteristics: (1) it does not aim to test theories, and (2) it involves extensive variation of experimental parameters’.[[142]](#footnote-142) As we have seen, van Musschenbroek’s experimental research clearly has these characteristics. However, given the general nature of these characteristics, we now do not seem to gain much by labelling van Musschenbroek’s research as exploratory experimentation. To allow for more specificity, Elliott proposes a taxonomy of different kinds of exploratory experimentation. The taxonomy is based on the following three dimensions:

(1) The positive aim of the experimental activity;

(2) The role that theory plays in the experimental activity;

(3) The methods or strategies used for varying parameters.[[143]](#footnote-143)

Although I do not think that we will gain much further insight in van Musschenbroek’s experimental research through an exercise in box-ticking, Elliott’s discussion of these three dimensions provide a clear indication of the limitations of Steinle’s conception of exploratory experimentation and suggests ways to broaden it.

With regard to the aim of the experimental activity, I have already argued that Steinle’s conception is too limited because it requires that the researcher already has an idea of the type of regularity she is looking for.[[144]](#footnote-144) Elliott mentions other aims that might drive exploratory experimentation. The first one is ‘[i]solating or manipulating particular entities or phenomena’.[[145]](#footnote-145) Although Elliott does not refer to Hacking, this can be seen as corresponding to Hacking’s emphasis on the role of experiments in creating phenomena. The experiments with the Leiden jar provide a good example of this type of exploratory experimentation. As we have seen in the previous chapter, van Musschenbroek performed several experiments, varying different parameters, in order to find out which variables were relevant for the production of the phenomenon of the uncommonly strong spark. Van Musschenbroek thought that the type of glass used was the relevant variable. Further exploratory experimentation by Nollet demonstrated that this was not the case. In their article on the early history of the Leiden jar, Silva and Heering argue that the experiments with the Leiden jar made by several researchers after van Musschenbroek can be seen as examples of exploratory experimentation.[[146]](#footnote-146) They replicate the experiments reported by van Musschenbroek and again vary certain parameters in an attempt to ‘stabilize the experimental findings’.[[147]](#footnote-147) As with the experiments performed by van Musschenbroek and Nollet, their aim was to identify the variables which were relevant for the production of the phenomenon. William Watson, for example, after having performed several experiments with the jar, published a list of parameters which were relevant for the occurrence of the effect in the *Philosophical Transactions*.[[148]](#footnote-148) Silva and Heering show how a communal process of exploratory experimentation took place, which in the end led to a ‘stabilization of the standard experimental procedure with the Leiden jar’.[[149]](#footnote-149) Gray’s experiments on the communication of electricity, which I have discussed in the previous chapter, could also be seen as an example. The variations that were performed in these exploratory experiments were not made with the aim of formulating empirical regularities, but with the aim of discovering the variables relevant for the occurrence of a certain phenomenon and thus stabilising the production of that phenomenon.[[150]](#footnote-150)

A second possible aim driving exploratory experimentation that Elliott mentions is ‘to develop new experimental techniques, instrumentation, or simulations’.[[151]](#footnote-151) He adds that even when instruments are available, scientists often ‘engage in an extensive exploratory process of “pretesting” to determine what conditions are needed in order to get any experimental results at all’.[[152]](#footnote-152) Elliott calls this type of experimentation “preliminary explorative exploration” and argues that it is necessary to make the type of exploratory experimentation described by Steinle possible. In the previous chapter, we have seen how van Musschenbroek started his series of experiments on electricity with exactly this kind of “preliminary explorative explorations” by varying different parameters and trying to discern which set-up was most effective.[[153]](#footnote-153) Here again, the formulation of an empirical regularity is not the motivation guiding the experiments.

In his discussion of the different possible roles ‘of theory in guiding the experimental activity’, Elliott notes that ‘although Steinle’s preferred form of [exploratory experimentation] includes very little role for theory in the *design* or *guidance* of [exploratory experiments], the *goal* of the activity is to develop new theoretical ideas and concepts’.[[154]](#footnote-154) Given that Steinle himself explicitly distinguishes theories from empirical laws (which, as we have seen, he takes as phenomenological regularities), and given that concepts are at play in (and are entangled with) the process of finding these regularities, he would probably counter this criticism by referring to these distinctions.[[155]](#footnote-155) Despite these remarks, the fact that Steinle’s account limits exploratory experimentation to those experiments driven by the aim of finding regularities might be a remnant of a theory-centred philosophy of science. Although he clearly distinguishes empirical regularities and concepts from theories, Steinle still argues that the goal of exploratory experimentation is ‘to *formulate* stable and ever more general regularity’.[[156]](#footnote-156) As Rouse has argued, theory-centred philosophy of science thinks that ‘science can be understood as the production of a network of statements’, so that the main question for philosophers should be ‘how we infer one statement from others’.[[157]](#footnote-157) By limiting the application of the concept of exploratory experimentation to experimentation which has the aim of producing a statement (the *formulation* of an empirical regularity), Steinle seems seems to presuppose that experimentation is only philosophically interesting in so far as it leads to a propositional end-product.[[158]](#footnote-158)

This brings us back to the role of practical understanding or circumspection in experimental practice, which I have discussed in section 4.3. Rouse described it as ‘a “feel” for the instrument’ which also ‘suggests possible directions for investigation – to take advantage of the capabilities of one’s tools – as well as constraints on the scope and precision of the results obtainable’.[[159]](#footnote-159) In the same way as Steinle argued that experimentation does not necessarily need to be theory-driven in order to have a certain systematicity or directedness, I would add that it also does not need to be driven by propositional knowledge to have a certain systematicity or directedness. Steinle’s focus on the production of generalising *statements* involves the risk of occluding the role that circumspection can play in guiding experimental practice.

In chapter 2, I have argued that despite his strict separation of the collection of experimental data from the construction of theories, van Musschenbroek did not expect the experimenter to approach his research as a blank slate. I have argued that a good experimenter, according to van Musschenbroek, already had a certain understanding and practical grasp of the experimental set-up and its components, that is, to have circumspection. Despite Steinle’s emphasis on the production of statements (and although he does not frame it in terms of a practical understanding), he does seem to point towards the importance of this kind of foreknowledge in the practice of exploratory experimentation. It especially plays a role in the systematic variation of experimental parameters. Steinle points to the fact that ‘[i]n principle, the number of possible parameters to be varied is unlimited’.[[160]](#footnote-160) In practice however, a researcher will have some ideas about which parameters might turn out to be relevant, based on ‘previous experience in the field or in related ones’.[[161]](#footnote-161) As an example, he refers to the fact that researchers pursuing exploratory experiments in the field of electromagnetism did not vary the colour of the wires, since it was known from earlier experimental research that colour was not a relevant variable.[[162]](#footnote-162)

This kind of foreknowledge does not necessarily have to be possessed by the subject doing the experimentation, but can also be embodied in instruments and standardised procedures. In section 4.4.1, I have discussed the spread of results from the microworld of the laboratory. This was done on the one hand by standardising the products and procedures of the microworld, so that they could be employed in other locations, and on the other by reconstructing the outside in order to resemble the laboratory.

Steinle remarks that whether a certain experiment is easy or difficult to reproduce is context-dependent. The experimental results of Ørsted’s discovery of electromagnetism were accepted quickly throughout Europe. This was due to the fact that given the context, the experiment was easy to reproduce as it ‘involved only experimental procedures and techniques that had been well known for decades’.[[163]](#footnote-163) The operations necessary to perform Ørsted’s experiment had become ‘common knowledge and procedure in every laboratory’, and therefore ‘no novel laboratory procedures were required for their implementation’[[164]](#footnote-164) As an example of an experiment which was difficult to replicate, he refers to Harry Collins’ study of gravitational wave detectors, which led Collins to develop his notion of “experimental regress”. The difficulty of replicating the experiment, according to Steinle, can be explained by the fact that ‘almost no standard equipment’ was involved:

Most instruments had to be developed specifically for a particular experiment, and procedures for their proper handling had to be learned and practiced for that specific equipment. This holds especially true for procedures to test the equipment’s correct operation.[[165]](#footnote-165)

This brings us back to the process by which results from the laboratory spread, discussed in section 4.4.1.[[166]](#footnote-166) Ampère’s experiments were easy to reproduce because other locations and researchers had undergone the same processes and thus resembled each other. Other laboratories possessed the necessary objects and instruments, other researchers possessed the relevant skill and circumspection to handle these objects and instruments. These skills had to be learnt by the researchers.[[167]](#footnote-167) From a communal perspective, the spread of the relevant skills and objects can also be seen as a learning process. Rather than seeing the learning process involved in the development of the sciences as essentially consisting in the accumulation of propositional knowledge, the views discussed in section 4.4.1 invite us to see this learning process as also involving the development and spread of skills, procedures, instruments, and other kinds of material objects.

**4.4.3 Experimental Learning as a Distributed Learning Process**

The emphasis on the embodied, situated, and performative nature of scientific practices has affinity with recent trends in cognitive science and philosophy of mind that want to provide an alternative for a disembodied and representationalist view on cognition.[[168]](#footnote-168) Recently, these new approaches to cognition have also been applied to the study of scientific practices.[[169]](#footnote-169) Together with a group of researchers, Nancy Nersessian has for example investigated the practices of several laboratories working in the field of biomedical engineering.[[170]](#footnote-170) According to Nersessian, the laboratory can be seen as a site of learning on different levels. On the one hand, it is a site where new practitioners learn to participate in laboratory practices. On the other hand, it is also a site of research, which is a process of continuous learning.[[171]](#footnote-171) The process of learning is described as a “distributed”[[172]](#footnote-172) process:

The complexity and interdisciplinarity of the laboratory problems means that knowledge or understanding does not reside inside single individuals but is rather distributed or stretched across people, devices, texts, and other lab instruments and artifacts. For these reasons, we have come to characterize the learning in these settings as *agentive*. The notion of *agent* emphasizes how these learning cultures afford and sustain the formation of relationships between agents. Agentive in this sense implies the person/learning, who is characterized by her relationship to other agents (in distinction to the individual who would be characterized essentially by her separation from other participants.) Its relation to the notion of *agency* emphasizes that these learning cultures depend on human agents who are authorized to enlist other entities, human and nonhuman, as agents in their work and understanding.[[173]](#footnote-173)

An example of such a relationship is the one that practitioners were seen to develop with “their” cells in ‘the process of learning to cell-culture in the context of tissue engineering’.[[174]](#footnote-174) This process involves several steps, but if successful, the practitioner in the end develops a “working relationship” with the cells, namely an understanding of the cells ‘as systems that have capabilities that can potentially be enlisted for design purposes’.[[175]](#footnote-175) This working relationship is what I have described above as circumspection, i.e. a practical grasp of the material she is working with, which allows the researcher to move forward in her research practice.

Above, I have said that the views presented in 4.4.1 entail that learning *in* the world also involves the development of instruments. The results of this learning process are thus also embodied in these instruments. This point can also be described as pointing towards a non-subjectivist view on the process of learning *in* the world. In the “standard/traditional” philosophy of science criticised by Rouse and Hacking, the learning process involved in scientific research was construed as a learning process of an (idealised) epistemic subject, a process which was in turn construed as an accumulation of propositional knowledge. By making the connection with science studies that use the notion of distributed cognition, such as those performed by Nersessian and others, I want to make this non-subjectivist view more explicit. The characterisation of distributed cognition provided by Kurz-Milcke, Nersessian, and Newstetter can thus be used to characterise learning *in* the world as a distributed process, the results of which ‘[do] not reside inside single individuals but [are] rather distributed or stretched across people, devices, texts, and other lab instruments and artifacts’[[176]](#footnote-176)

As I said above, Nersessian sees the laboratory as a site of learning on different levels. As a site of research, it is a site of experimental learning. Given the distributed nature of experimental learning, this learning process is also reflected in the fact that the material artefacts and devices used in the laboratory go through continuous ‘cycles of redesign’.[[177]](#footnote-177) The laboratory is also a site of individual learning. In an analysis of the process of learning to work with certain devices within the laboratory (*in casu* the so-called *flow loop* which models artery blood flow), Nersessian shows how this learning process also involves learning the historical trajectory of the device. The device has been ‘modified, constructed, and reconstructed in the course of research with respect to problems encountered and changes in understanding’.[[178]](#footnote-178) During one’s apprenticeship, one learns about this history of the device, and thus also ‘how and for wat purposes [it] was built, why it was modified […], and what worked and did not work for various purposes, so that time and resources are not wasted going down an old path’.[[179]](#footnote-179) According to Nersessian, this kind of history should not be seen as part of the “heritage” of a lab, but as ‘part of the problem space’ of the lab and thus ‘a cognitive-cultural resource for moving problem-solving forward’.[[180]](#footnote-180) This can again be seen as an example of what Rouse called circumspection, which, to recall, he at one point described as ‘a practical assessment of what it makes sense to do, given the resources available and the aims and standards that govern scientific practice within a given field’.[[181]](#footnote-181) Learning the history of a device in the context of learning to pursue research within a laboratory is thus entangled with getting a sense of “what it makes sense to do” in that specific laboratory. The history of the device and the adaptations it has undergone inform the new practitioner about the capacities (and limitations) of the device and the ways it has been put to use in research. This practical grasp of the capacities of the device allows her to further pursue research with it, which will in turn result in adaptations being made to the device.

In the previous chapter, I have discussed the development of the experimental set-up involved in electrical research. The set-up used by van Musschenbroek was the result of various exploratory investigations by his predecessors, in which they had learnt the properties and behaviours of certain (combinations of) materials.[[182]](#footnote-182) Although the series of explorations were not confined to one laboratory, the development of the experimental set-up can be seen as analogous to the cycles of redesign undergone by the devices in the laboratories described by Nersessian et al. The set-up thus also has a history, and learning this history is thus also related to learning the capacities (and limitations) of the set-up. In the first part of van Musschenbroek’s manuscript on electricity (which, as we have seen, contained a kind of “state of the art”), there is a section that provides a list of references related to researches who have ‘invented several machines to produce electricity’.[[183]](#footnote-183) More specifically, the list contains references to the works in which a description of these instruments and their components is provided. More than a “who’s who”, the list can be seen as part of learning about the history of the set-up, and thus developing one’s circumspection to be able to pursue further research. The process of repeating the experiments of one’s predecessors can also be seen as a way to learn the history of the device/set-up and its capacities.[[184]](#footnote-184) Johann Heinrich Winkler started his research practice by repeating all the experiments found in van Musschenbroek’s textbook.[[185]](#footnote-185) Georg Matthias Bose had learnt about Hauksbee’s machine and experiments from ’s Gravesande’s textbooks. He constructed his own version of the machine and repeated the experiments. Later, he also learnt about and repeated du Fay’s experiments. Finding du Fay’s procedure tedious, he made some adjustments to the set-up which would allow him to repeat du Fay’s experiments by means of the Hauksbee machine.[[186]](#footnote-186) His redesign of the set-up is thus linked to this process of learning the history of current devices in use, their history, and their capacities.

In section 4.4.1, we have seen how artefacts and instruments are not only subjected to cycles of redesign, but also to processes of standardisation. In the long run, this also means that the production and maintenance of certain instruments and artefacts used in the laboratory is no longer internal to the lab, but is delegated to outside industries. People in the lab still need to acquire the skills to work with the lab, but no longer the skills and understanding necessary to repair or adapt the equipment. In some cases, instruments can be seen as embodying or black-boxing certain skills which were previously necessary to perform research or produce phenomena. In the case of the electromagnetic motor, we have seen how Faraday “packaged” the skills necessary to produce the phenomenon in the instrument he sent to his colleagues. In his discussion of the development of scanning tunnelling microscopes in the United States, Cyrus Mody shows how this process in the end led to ‘a [routinized] division of knowledge and labor between instrument “builders” and instrument “runners”’.[[187]](#footnote-187) This was done by companies ‘blackboxing many of the embodied skills needed in home built-instruments’.[[188]](#footnote-188) The development and use of blackboxes is not limited to the natural sciences. In an article on the impact of the SPSS (Statistical Package for the Social Sciences) on research and teaching practices in sociology, Uprichard, Burrows and Byrne describe how the spread of the software not only impacted the skills that a practicing sociologists was expected to have, but also led to a blackbox approach to the software.[[189]](#footnote-189) The skill and knowledge related to the design and functioning of the software now resides with the mathematicians and computer scientists responsible for the software. The spread of the results of experimental learning thus often takes the form of a distribution of skills and knowledge, across both people and artefacts.[[190]](#footnote-190)

In the same way as deeply entrenched skills over time become self-evident (and are thus often performed without being explicitly aware of them), the standardisation of processes and artefacts, and the blackboxing of skills in instruments and devices tends to make the work that had been necessary to develop them, and the effort necessary to keep them functioning stably, become invisible. Here, historical work can help to make this work and the learning processes involved in them “visible” again. It can show the amount of work that went into constructing such a simple device as a reliable thermometer, or the complexities involved in setting up an electric power system.[[191]](#footnote-191) In this way, van Musschenbroek’s struggles with impure batches of metal, secretive craftsmen, and capricious electrical instruments also reveal something about the complexities involved in experimental learning.

**4.5. Conclusion: Van Musschenbroek’s Learning in the World**

As I made clear at the beginning, my main aim was to provide a philosophical view on the role of experimentation and the nature of scientific learning in the world which would allow me to do justice to the experimental research performed by van Musschenbroek.

To do this, I tried to provide an alternative for a theory-centred view on experiments. Such a view makes experimentation subservient to and dependent on theory. Kuhn’s discussion of the Leiden jar, provided in one of his more theory-centred moments, can be seen as an example of this. He discusses early research on electricity as an example of the fact that scientific practice always involves certain “schools” characterised by ‘at least some implicit body of intertwined theoretical and methodological belief’.[[192]](#footnote-192) The discovery of the Leiden jar is then also explained by recourse to these theoretical beliefs:

Those electricians who thought electricity a fluid and therefore gave particular emphasis to conduction provide an excellent case in point. Led by this belief, which could scarcely cope with the known multiplicity of attractive and repulsive effects, several of them conceived the idea of bottling the electrical fluid. The immediate fruit of their efforts was the Leyden jar, a device which might never have been discovered by a man exploring nature casually or at random, but which was in fact independently developed by at least two investigators in the early 1740’s.[[193]](#footnote-193)

This interpretation of the early days of electrical research is historiographically problematic on many levels. As we have seen, the Leiden jar was not developed by experimentalists who were “led by a belief” or based on “the idea of bottling the electrical fluid”. Although, as we have seen in the previous chapter, there were many different attempts at providing a theoretical explanation of known electrical phenomena, there was not such a thing as a “school” of electricians whose research was guided by the idea of an electrical fluid. Nor were beliefs of this kind necessary to guide research. In the case of van Musschenbroek, we have seen how he explicitly stated that existing theoretical treatments of electrical phenomena were all inadequate. This did not prevent him from pursuing research on the phenomena related to electricity. As we have seen, despite the fact that it was not guided by certain theoretical preconceptions, van Musschenbroek’s research did have a certain directedness. On a philosophical level, this passage thus betrays the theory-centred view that we encountered at several places throughout this chapter: experimental research can only be pursued in a systematic way if it is guided by theory. Kuhn clearly only sees two options: either researchers like van Musschenbroek were “led by a belief”, or their research is just a case of “exploring nature casually or at random”.

In section 4.2, I have contrasted a theory-centred representationalist view on science with a non-representationalist view on science as a practice. This entailed a shift from seeing science as a matter of learning *about* the world to seeing science as a process of learning *in* the world. In section 4.3, I have discussed Rouse’s (re)interpretation of Kuhn. Despite theory-centred passages such as the one on the Leiden jar, Kuhn’s work was shown to also contain passages that pointed towards an understanding of science as a practice. I have contrasted both ways of reading Kuhn by looking at the different ways the process of learning to become a scientist was conceptualised. From a practice-perspective, this process consisted in the acquisition of circumspection, a practical understanding which was acquired by working through certain exemplars.

In section 4.4, I have presented a view on experimental practice as a form of learning *in* the world. I have discussed Hacking’s views on “experimentation having a life of its own”. Against a theory-centred view on experimentation, Hacking argued that experimental practice could (and often does) proceed without being guided by theory. He further argued for a shift from representing to intervening. Experimental interventions played an important role in the production and stabilisation of phenomena. I have shown how Rouse’s notion of microworlds elaborated upon Hacking’s views on experimentation. Rouse likewise emphasised the importance of active intervention in scientific practice and argued that the production and stabilisation of phenomena involved the construction of microworlds. These microworlds are simplified reconstructions of the world, which are causally isolated from the outside world. The objects in these microworlds are purified and standardised to allow for reliable manipulation. Research in these settings heavily depended on the skills of the experimentalists. The spread of results obtained in these microworlds depended on the one hand on the adaptation of the outside world to resemble the controlled environment of the microworld in important respects and was on the other hand made possible by the standardisation of these results, making them applicable in other contexts.

In section 4.4.2, I discussed Steinle’s notion of exploratory experimentation. With this notion, Steinle tried to argue for the existence of a type of experimentation which was not theory-driven, but which did have a specific kind of systematicity and directedness. Kuhn was thus wrong to think that experimentation is either “led by a belief”, or is just a case of “exploring nature casually or at random”. Exploratory experimentation takes the form of a systematic variation of relevant experimental parameters. For Steinle, this systematic variation of parameters had a very specific goal: formulating empirical regularities. In order to be able for these regularities to be formulated, conceptual revision was sometimes required. In these cases, exploratory experimentation was closely intertwined with conceptual innovation. I have argued that Steinle’s conceptualisation of exploratory experimentation was still too limited. It still betrayed a limited focus on propositional knowledge by only taking into account research which aimed at *formulating* regularities. Based on the historical research presented in the previous chapters and using the notion of circumspection, I argued that exploratory experimental research could also be pursued in a systematic manner on the basis of the practical understanding of the experimentalist.

In section 4.4.3 I completed my discussion of experimental learning as a process of learning *in* the world by conceptualising it as a distributed learning process. I referred to studies which used the notion of distributed cognition in order to understand learning processes in scientific laboratories. According to these studies, cognition should not be seen as a process distributed among people and artefacts. In the case of the laboratory, this implied that research, as a learning process, involved the continuous redesign of the devices used in the laboratory. For the researchers themselves, learning to work in the laboratory involved developing a certain relationship with the equipment, which I again described as the acquisition of circumspection. This learning process also involved learning the history of the device, the knowledge of which allowed the practitioner to have a practical grasp of the limitations and capacities of the device and thus to pursue further research with it.

In general, this chapter has argued for the importance of skill, practical understanding or circumspection, and instrumentation in experimental practice. A view on science as a process of learning *about* the world reduces learning to an accumulation of propositional knowledge. It thus neglects the role of non-propositional aspects of science such as tacit knowledge, instrumentation, and other material aspects of scientific practice. I have therefore argued to understand scientific practice as a process of learning *in* the world. According to this view, experimental learning is a process of actively engaging in and reshaping the world. The results of this learning process are not limited to propositions, but are also embodied in instruments, processes, procedures, standardised objects, and the skills of practitioners.

To conclude, I would like to argue that this view allows us to have a better appreciation of van Musschenbroek’s experimental practice. From a theory-centred perspective, van Musschenbroek is a minor figure. He introduced no great theoretical or conceptual innovations. His experimental work did not lead to the development of a new theory, nor did he provide a striking test or refutation of an important theory. Although his work on the strength of materials and his “discovery” of the Leiden jar is mentioned in the literature, one often finds that it is only deemed important in so far as it is connected to later theoretical developments.[[194]](#footnote-194) If we move beyond this theory-centred view and appreciate the role of skills, objects, and instruments, we are able to arrive at a much better and richer appreciation of van Musschenbroek’s experimental practice. Coming from a family of instrument makers, van Musschenbroek put a lot of effort in the development and refinement of scientific instruments and the material aspects of his experimental set-ups. In chapter 2, we have encountered the tribometer that he had developed to measure friction. In chapter 3, we have seen how he improved Mariotte’s procedure to measure the absolute coherence of materials by developing a new measuring device. Van Musschenbroek also paid close attention to the preparation of his specimens. In my discussion of his research on electricity, I have shown how the experimental set-up van Musschenbroek had designed to measure the strength of magnets was used by Kratzenstein in an attempt to measure and quantify the strength of electrical attraction. Van Musschenbroek repeated these experiments, but with an attention to detail much exceeding that of Kratzenstein. A further example of van Musschenbroek’s work in instrumentation is provided by de Pater, who discusses van Musschenbroek’s work on the “pyrometer”, a device developed to measure the heat-induced expansion of metals.[[195]](#footnote-195)

In his reflections on the method of experimental philosophy, which I discussed in chapter 2, van Musschenbroek made many points which resonate with the view on scientific practice presented in this chapter. By this, I do not want to make van Musschenbroek into a proponent of a philosophy of scientific practice. To begin with, in chapter 2 I have also shown how van Musschenbroek adhered to a representationalist view on knowledge, according to which the truth of our ideas depended on their correspondence with the attribute they represented. However, I argued that in his oration on the method of performing experiments, van Musschenbroek can be seen to emphasise the importance of skills and practical understanding in the process of experimental research. The repetition of experiments was seen as a learning process, which allowed the experimentalist to acquire the necessary skills and circumspection. Van Musschenbroek further advised that repeating experiments should also involve the variation of relevant parameters. This would allow the experimentalist to identify all the variables affecting the experimental outcome and identify possible sources of disturbances. In turn, this would allow the experimentalist to redesign the experimental set-up and shield it from these sources of disturbance. Not only would this lead to a more reliable production of the phenomenon under investigation, it also made it possible to experimentally establish a causal relation between two parameters. Despite his scepticism towards a theoretical search for causes, we have seen that as an experimentalist, van Musschenbroek (to use Hacking’s words) ‘proceed[ed] with the agent’s presumption of causal efficacy’.[[196]](#footnote-196) Van Musschenbroek’s emphasis on the experimental work necessary to establish causal relationships (including isolating the experimental set-up from sources of disturbance) resonates with Rouse’s discussion of microworlds. Van Musschenbroek’s lamentations on the impurity of his batches of metals, and the secretive and non-standardised practices of the craftsmen providing him with materials foreshadow the importance that purification and standardisation would later come to play in the development and spread of experimental results.

Lastly, van Musschenbroek’s work as a university professor, and especially the textbooks that he wrote in this context, should not be forgotten. In chapter 3, we have seen how Desaguliers recommended the French edition of van Musschenbroek’s textbook as the best introduction to the state of the art in electricity. In this chapter, we have seen how Winkler started his research on electricity by repeating the experiments that were described in van Musschenbroek’s textbook. Van Musschenbroek’s textbooks thus allowed new researchers to start their own experimental practice, not by telling them what to *believe*, but by showing them what to *do*.

1. My aim is of course not to deny any role to theory, nor to argue for a strict separation between experimental practice and theory. For a recent and informed treatment of the relationship between experiment and theory (or rather conceptualisation) on the basis of views close to those presented in this chapter, see Laura Georgescu, *Devising Magnetism: Concepts and Investigative Practices* (Gent: Doctoral dissertation, Universiteit Gent, 2017). In this work, Georgescu warns that the so-called “new experimentalists” in philosophy of science (like Hacking, whose views I will present in this chapter) in the end affirm the dualism between theory and practice that a philosophy of scientific practice should want to dismantle (Georgescu, ibid., 221-222). I do not wish to reinstall or reinforce such a dualism in this chapter. The following philosophical discussion should be seen as local in character, i.e. I emphasise and develop those points which will allow me to analyse and illuminate *van Musschenbroek’s* experimental practice. It should not be seen as an assessment of experimental practice *in general*. Where necessary, I will provide further disclaimers to clarify the scope of the philosophical points made at specific points in this chapter. [↑](#footnote-ref-1)
2. Joseph Rouse, *Knowledge and Power: Toward a Political Philosophy of Science* (Ithaca and London: Cornell University Press, 1987), 3. [↑](#footnote-ref-2)
3. Ibid., 3. [↑](#footnote-ref-3)
4. Ibid.*,* 3. The main problem for such a view is to provide an account of how we can know that our representations have improved, given the assumption that we do not have access to the world as it is in itself (ibid.*,* 3–5). Despite his criticism of correspondence realism, Rouse’s anti-representationalist stance should not be seen as a form of anti-realism. Rouse criticises both realism and anti-realism (including social constructivist versions of anti-realism) as being based on problematic assumptions (ibid.*,* 127–65). In his 1996 *Engaging Science*, he further shows how despite their differences, realists, social constructivists, and empiricists/instrumentalists share a commitment to a representationalist view on scientific knowledge (Joseph Rouse, *Engaging Science: How to Understand Its Practices Philosophically* (Ithaca/London: Cornell University Press, 1996), 17–20). At the end of the chapter, Rouse summarises his position as such: ‘my view no longer seems to put in doubt the truth or referential success of the best scientific theories, nor does it consign the pragmatic success of those theories to the realm of miracles. I happily endorse the pragmatists insight that the world *is* what shows up in our practices. But I need not accept the metaphysical extravagances of convergent realism to do so’ (Rouse, *Knowledge and Power*, 165). [↑](#footnote-ref-4)
5. Ibid., 69–70. Characterising traditional philosophy of science in this very general way of course involves abstracting from all the different positions and nuances within philosophy of science. As Laura Georgescu has remarked about Hacking’s similar use of the term, the label should not be seen as ‘apply[ing] to a well-determined theory about science, but to a cluster of views that aim to solve the problem of “the connection between theory and the world” (Hacking 1983: 130), by taking the theory side (and correlated concepts such as models, hypotheses, laws, etc.) as the starting point for inquiry’ (Georgescu, *Devising Magnetism*, 217). [↑](#footnote-ref-5)
6. Rouse, *Knowledge and Power,* 69–70. [↑](#footnote-ref-6)
7. As Rouse puts it: ‘Experiments and observations are [considered as] significant only within a theoretical context. Theory guides the construction and performance of experiments, supplies the categories within which observations are to be interpreted, and mediates the transmission and application of the results of research’ (Rouse, *Knowledge and Power,* 69). Rouse does not want to deny the role that theories and practices of representation play in science. What he does want to show is how ‘[s]cience’s technical capabilities (powers) are developed, communicated, and preserved without necessarily being mediated by theoretical representations’ (ibid.*,* 21). As an example: in a review of Allan Franklin’s book with the promising title *The Neglect of Experiment*, Robert Ackermann points out that Franklin still has a quite classical philosophical stance towards the role of experiments, ‘asking as his central philosophical questions what role experiment plays in theory selection or confirmation, and how experiment can be organised as to result in the rational separation of experimental fact from experimental artifact’ (Robert Ackermann, ‘The New Experimentalism’, *The British Journal for the Philosophy of Science* 40, no. 2 (1989): 186–87). In a very recent Stanford Enclycopedia of Philosophy entry on ‘Experiment in Physics’ (revised June 18, 2019), written together with Slobodan Perovic, Allan Franklin makes a polite nod to Hacking’s suggestion that experimentation might have a life of its own, but ends the short section on this topic by saying: ‘In all of these cases we may say that these were observations waiting for, or perhaps even calling for, a theory. The discovery of any unexpected phenomena calls for a theoretical explanation’. The next section then begins with the following statement: ‘Nevertheless several of the important roles of experiment involve its relation to theory. Experiment may confirm a theory, refute a theory, or give hints to the mathematical structure of a theory’ (Allan Franklin and Slobodan Perovic, ‘Experiment in Physics’, in *The Stanford Encyclopedia of Philosophy*, ed. Edward N. Zalta, Summer 2019 (Metaphysics Research Lab, Stanford University, 2019), https://plato.stanford.edu/archives/sum2019/entries/physics-experiment/, accessed 02/08/2019). [↑](#footnote-ref-7)
8. Rouse, *Knowledge and Power,* 71. [↑](#footnote-ref-8)
9. Ibid., 71. [↑](#footnote-ref-9)
10. In his *Engaging Science*, Rouse provides a further clarification and discussion of what he means by the concept of practice and what it implies to see science as a practice (Rouse, *Engaging Science*, 125–57). He also provides a more elaborate discussion of the representationalism underlying realism, instrumentalism, historical rationalism, and social constructivism, and develops an anti-representationalist view on the basis of Davidsonian semantics (ibid., 205-236). [↑](#footnote-ref-10)
11. Ian Hacking, *Representing and Intervening* (Cambridge: Cambridge University Press, 1983). [↑](#footnote-ref-11)
12. Hacking, *Representing and Intervening*, 130. [↑](#footnote-ref-12)
13. Although Hacking does not provide a reference, Dewey’s most extensive criticism of the spectator theory of knowledge can be found in John Dewey, *The Quest for Certainty: A Study of the Relation of Knowledge and Action* (New York: Minton, Balch & Company, 1929). Like Rouse, Dewey argues that despite all their differences and quarrels, (the then) traditional epistemological positions all shared this view on knowledge (ibid., 22-23, 47). Like Hacking, he proposes a shift from representing to intervening and argues that knowing and doing are interconnected (ibid., 196-197, 211-215). Dewey’s description of experimental procedures as involving a ‘systematic variation of conditions so as to produce a corresponding series of changes in the thing under consideration’ (ibid., 87) corresponds to the notion of “exploratory experimentation” which I will explore further in section 4.4.2. Dewey also provides a clear formulation of the contrast between a spectator theory of knowledge, and his own practice based view on knowledge, which can be taken as analogous to the contrast between “learning *about* the world” and “learning *in* the world” that I want to argue for in this section: ‘Mind is no longer a spectator beholding the world from without and finding its highest satisfaction in the joy of self-sufficing contemplation. The mind is within the world as part of the latter’s own on-going process. […] From knowing as an outside beholding to knowing as an active participant in the drama of an on-moving world is the historical transition whose record we have been following’ (ibid., 291). In *Knowledge and Power*, Rouse explicitly expresses his debt to the pragmatist tradition (Rouse, ibid., 7-9, 17-19). [↑](#footnote-ref-13)
14. Hacking, *Representing and Intervening,* 130. [↑](#footnote-ref-14)
15. Ibid., 131. [↑](#footnote-ref-15)
16. As we have seen, Rouse criticised both realism and anti-realism in philosophy of science, because both positions are based on problematic representationalist assumptions. For Hacking, the move to intervention and the focus on experimental science is part of his defence of ‘an uncontentious realism’ (Hacking, *Representing and Intervening*, 131). One of the main aims of his book is thus to move ‘away from realism about theories and towards realism about those entities we can use in experimental work’ (ibid., 29). This leads him to defend a so-called “entity realism”, according to which we can be realists about entities which we can reliably manipulate by means of experimental interventions (ibid., 262-275). [↑](#footnote-ref-16)
17. Rouse, *Knowledge and Power*, 25. This also implies that the philosophical problem of how our knowledge corresponds to the “real world” is a pseudo-problem: ‘The question is not how we get from a linguistic representation of the world to the world represented. We are already engaged with the world in practical activity, and the world simply *is* what we are involved with. The question of access to the world, to which the appeal to observation was a response, never arises’ (ibid., 143). [↑](#footnote-ref-17)
18. Rouse, *Engaging Science*, 161. [↑](#footnote-ref-18)
19. David Kaiser and Andrew Warwick, ‘Kuhn, Foucault, and the Power of Pedagogy’, in *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*, ed. David Kaiser (Cambridge, MA: MIT Press, 2005), 393–409. See also infra for further references to contemporary literature on science pedagogy. [↑](#footnote-ref-19)
20. Rouse, *Knowledge and Power*, 26–27. [↑](#footnote-ref-20)
21. Ibid.*,* 27. [↑](#footnote-ref-21)
22. Ibid., 30. It should be mentioned that Rouse does not aim to do engage in Kuhn-exegesis, but to develop Kuhn’s thought further in order to arrive at a philosophy of scientific practice. The point of this exercise is thus not to find out what Kuhn himself really meant, but to develop aspects of Kuhn’s thought which have remained unnoticed. Rouse’s take on the “radical Kuhn” should thus also not be seen as an exegesis of Kuhn. As Rouse himself states, in this radical interpretation ‘[he] undoubtedly take[s] [Kuhn] further in the direction of an account of science as practice than he himself would be happy with’ (ibid.*,* 27). [↑](#footnote-ref-22)
23. Ibid., 27. [↑](#footnote-ref-23)
24. Ibid.*,* 26. [↑](#footnote-ref-24)
25. Ibid.*,* 27. [↑](#footnote-ref-25)
26. Ibid.*,* 28. [↑](#footnote-ref-26)
27. Ibid.*,* 28. [↑](#footnote-ref-27)
28. Rouse, *Knowledge and Power,* 37. [↑](#footnote-ref-28)
29. Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 2012), 149. [↑](#footnote-ref-29)
30. Kuhn, ibid., 149. This emphasis on *seeing* is reminiscent of Dewey’s notion of a “spectator theory of knowledge” (cf. supra). [↑](#footnote-ref-30)
31. Ibid., 176. [↑](#footnote-ref-31)
32. Ibid.*,* 5. At his most theory-centred moments, Kuhn sees the presence of such an established theoretical viewpoint as the *conditio sine qua non* of research: ‘Effective research scarcely begins before a scientific community thinks it has acquired firm answers to questions like the following: What are the fundamental entities of which the universe is composed? How do these interact with each other and with the senses? What questions may legitimately be asked about such entities and what techniques employed in seeking solutions?’ (ibid.*,* 5). This also implies that experiments are subservient to theory. Only when a developed theoretical worldview is at place, can a practice of effective research be developed. In section 4.4, I will argue that a practice of effective (experimental) research can develop without the presence of such a theoretical framework. [↑](#footnote-ref-32)
33. Rouse, *Knowledge and Power*, 32. [↑](#footnote-ref-33)
34. Ibid.*,* 30. [↑](#footnote-ref-34)
35. Ibid.*,* 31–32. [↑](#footnote-ref-35)
36. Ibid.*,* 30. Andrew Warwick and David Kaiser similarly remark that for Kuhn ‘the process of learning physics has more in common with the mastery of a craft skill than with simply coming to understand and to believe a collection of formal propositions concerning the nature of the physical world’ (Kaiser and Warwick, ‘Kuhn, Foucault, and the Power of Pedagogy’, 395). [↑](#footnote-ref-36)
37. Kuhn, *The Structure of Scientific Revolutions*, 149. [↑](#footnote-ref-37)
38. Rouse, *Knowledge and Power*, 38. In *Engaging science*, Rouse further analyses this practical understanding in terms of temporality and again underscores the non-representational nature of this understanding: ‘To participate in practices as an agent is among other considerations to be ahead of oneself, that is, to have some understanding of what it would be to have done the action in question. It is also to have some sense of how to initiate or continue the action now. This is in turn to have a grasp of the situation one is already in, to which the action is an intelligible response. These three aspects are held together in the agent’s understanding in the form not of an explicit representation but of a practical capability. Our projecting ourselves ahead, by taking over the situation we find ourselves already in, by presently acting, is an understanding both enacted and displayed in the action itself’ (Rouse, *Engaging Science*, 162–63). [↑](#footnote-ref-38)
39. Kuhn, *The Structure of Scientific Revolutions*, 176. [↑](#footnote-ref-39)
40. Rouse, *Knowledge and Power*, 30. [↑](#footnote-ref-40)
41. Kuhn, *The Structure of Scientific Revolutions*, 1. [↑](#footnote-ref-41)
42. Ibid.*,* 187. [↑](#footnote-ref-42)
43. Ibid.*,* 187. [↑](#footnote-ref-43)
44. Ibid., 187. [↑](#footnote-ref-44)
45. Ibid.*,* 187–88. [↑](#footnote-ref-45)
46. Ibid.*,* 188. [↑](#footnote-ref-46)
47. Kuhn, *The Structure of Scientific Revolutions,* 189. [↑](#footnote-ref-47)
48. Rouse, *Knowledge and Power*, 83. [↑](#footnote-ref-48)
49. Ibid.*,* 83. [↑](#footnote-ref-49)
50. Kuhn, *The Structure of Scientific Revolutions*, 47. [↑](#footnote-ref-50)
51. Ibid.*,* 187. [↑](#footnote-ref-51)
52. Ibid.*,* 189–90. Borrowing the term from Michael Polanyi, Kuhn describes this process as the acquisition of “tacit knowledge”, i.e. knowledge ‘which is learned by doing science rather than by acquiring rules for doing it’ (ibid.*,* 190). [↑](#footnote-ref-52)
53. Kuhn, *The Structure of Scientific Revolutions,* 4. [↑](#footnote-ref-53)
54. Hacking, *Representing and Intervening*, 185. This is very close to what van Musschenbroek described as the central skill of a good experimentalist, see chapter 2. [↑](#footnote-ref-54)
55. Ibid., 229–30. [↑](#footnote-ref-55)
56. Rouse, *Knowledge and Power*, 88. Rouse takes over this notion from Heidegger (*Umsicht*). Assessing the Heideggerian origin of this term and discussing Heidegger’s views lies beyond the scope of this chapter. It should be noted however that Rouse also criticises certain views of Heidegger (especially certain remnant theory-centred views) that stand in the way of an understanding of experimental practice (ibid.*,* 95–98). As such, my use of the term “circumspection” should be understood in a Rousean sense, rather than as a strict Heideggerian sense. [↑](#footnote-ref-56)
57. Rouse, *Knowledge and Power*, 100. [↑](#footnote-ref-57)
58. Don Ihde has made a similar analysis of the way instruments (and technology) shape the course of research by means of what he calls the “telic *inclinations*” inherent in certain instruments, see Don Ihde, *Technics and Praxis: A Philosophy of Technology* (Dordrecht: D. Reidel Publishing Company, 1979), 42–44; Don Ihde, *Instrumental Realism: The Interface Between Philosophy of Science and Philosophy of Technology* (Bloomington and Indianapolis: Indiana University Press, 1991), 123. [↑](#footnote-ref-58)
59. Rouse, *Knowledge and Power,* 101. [↑](#footnote-ref-59)
60. Hacking, *Representing and Intervening*, 131. [↑](#footnote-ref-60)
61. Ibid.*,* 150. [↑](#footnote-ref-61)
62. Ibid.*,* 153. [↑](#footnote-ref-62)
63. Ibid.*,* 153. A similar distinction is made by Friedrich Steinle in his discussion of the presumed theory-ladenness of experiments (Friedrich Steinle, *Exploratory Experiments: Ampère, Faraday, and the Origins of Electrodynamics*, trans. Alex Levine (Pittsburgh: University of Pittsburgh Press, 2016), 316–17). [↑](#footnote-ref-63)
64. Hacking, *Representing and Intervening,* 153. In chapter 2, I have shown how van Musschenbroek makes a similar statement in his oration on the method of performing experiments. In chapter 3, I have shown how this plays out in van Musschenbroek’s research practice. [↑](#footnote-ref-64)
65. Ibid.*,* 154. [↑](#footnote-ref-65)
66. Ibid.*,* 154–66. Later in the book, he also argues that “theory” itself is not a monolithic entity, but that it refers to a plurality of things (ibid.*,* 210–19). [↑](#footnote-ref-66)
67. Hacking, *Representing and Intervening*, 220. [↑](#footnote-ref-67)
68. Ibid.*,* 220–24. [↑](#footnote-ref-68)
69. Ibid.*,* 221, emphasis in original. [↑](#footnote-ref-69)
70. Ibid.*,* 222. [↑](#footnote-ref-70)
71. Ibid.*,* 227–28. [↑](#footnote-ref-71)
72. Ibid.*,* 227. [↑](#footnote-ref-72)
73. Ibid.*,* 224–29. [↑](#footnote-ref-73)
74. Ibid.*,* 224–25. [↑](#footnote-ref-74)
75. Hacking, *Representing and Intervening,* 226. [↑](#footnote-ref-75)
76. Rouse, *Knowledge and Power*, 95–96. [↑](#footnote-ref-76)
77. Rouse, *Knowledge and Power,* 21. This is analogous to the theory-centred view on learning science discussed in section 4.3, according to which one first learns a general theory (and perhaps some rules guiding its application), and then practices this theory by applying it. [↑](#footnote-ref-77)
78. Ibid.*,* 22. [↑](#footnote-ref-78)
79. Ibid.*,* 23, emphasis in original. [↑](#footnote-ref-79)
80. Ibid.*,* 23. [↑](#footnote-ref-80)
81. Ibid.*,* 23. Given the historical character of this dissertation, Rouse’s remark on artificiality having become characteristic of most of the modern science might cause some to doubt the relevance of the current discussion for the historical parts of this dissertation. There are two reasons why this discussion will turn out to be informative for our discussion of van Musschenbroek’s research practice and his views on the role and nature of experimental research. First, in van Musschenbroek’s time, the amount of artificially prepared objects or purified substances available (or current in experimental research) was indeed very limited. In the previous chapter we have seen how this posed a challenge for his research on electricity and especially in his research on the strength of alloys. However, we have also encountered several examples where van Musschenbroek explicitly reflects on the heterogeneity or impurity of the substances that he is working with and hints towards standardising the procedures involved in producing them. Second, we have also seen that van Musschenbroek both in theory and in practice emphasised the importance of constructing a controlled environment. By stating this, I of course do not want to make van Musschenbroek into a kind of Rousean *avant la letter*. As we have seen in chapter 2, van Musschenbroek still adhered to a representationalist view on knowledge. [↑](#footnote-ref-81)
82. Rouse, *Engaging Science*, 128. [↑](#footnote-ref-82)
83. Rouse, *Knowledge and Power*, 102. [↑](#footnote-ref-83)
84. Ibid., 102. [↑](#footnote-ref-84)
85. Ibid., 102–3. [↑](#footnote-ref-85)
86. Ibid., 103. [↑](#footnote-ref-86)
87. Rouse, *Knowledge and Power,* 102. [↑](#footnote-ref-87)
88. Ibid., 102. [↑](#footnote-ref-88)
89. Ibid., 22. [↑](#footnote-ref-89)
90. Ibid.*,* 83. [↑](#footnote-ref-90)
91. It should be noted that this does not mean that Rouse wants to deny the possibility that scientific knowledge is universalised, however, this universality should be seen as an achievement, not as something given in advance (Rouse, *Knowledge and Power,* 119). See Latour’s remarks on universality: Bruno Latour, *The Pasteurization of France*, trans. Alan Sheridan and John Law (Cambridge, MA: Harvard University Press, 1988), 220–221, 226-227. [↑](#footnote-ref-91)
92. Rouse, *Knowledge and Power*, 111. [↑](#footnote-ref-92)
93. Ibid.*,* 101. [↑](#footnote-ref-93)
94. Ibid.*,* 133. [↑](#footnote-ref-94)
95. Ibid.*,* 114–16. [↑](#footnote-ref-95)
96. Ibid.*,* 116–17. [↑](#footnote-ref-96)
97. David Gooding, ‘Mapping Experiment as a Learning Process: How the First Electromagnetic Motor Was Invented’, *Science, Technology, & Human Values* 15, no. 2 (1990): 165–201. [↑](#footnote-ref-97)
98. David Gooding, *Experiment and the Making of Meaning: Human Agency in Scientific Observation and Experiment* (Dordrecht: Kluwer Academic Publishers, 1990), 160. [↑](#footnote-ref-98)
99. Gooding, *Experiment and the Making of Meaning,* 155. [↑](#footnote-ref-99)
100. Rouse, *Knowledge and Power,* 118–19. [↑](#footnote-ref-100)
101. Ibid.*,* 101. [↑](#footnote-ref-101)
102. For an elaborate discussion and further examples, see Rouse, *Knowledge and Power,* 227–36. See also Rouse, *Engaging Science*, 131–32, where he also emphasises the importance of training and disciplining people. [↑](#footnote-ref-102)
103. Friedrich Steinle, ‘Entering New Fields: Exploratory Uses of Experimentation’, *Philosophy of Science* 64 (1997): S65. A further discussion of this “standard view” is given in Friedrich Steinle, ‘Experiments in History and Philosophy of Science’, *Perspectives on Science* 10, no. 4 (2002): 408–9. [↑](#footnote-ref-103)
104. Steinle, ‘Entering New Fields’, S66. [↑](#footnote-ref-104)
105. Steinle, ‘Entering New Fields’, S66-67. In a later article, Steinle mentions further variations performed by Ampère: ‘the strength and polarity of the battery; the length and material of the needle’ but adds that the position of the needle is varied most extensively (Friedrich Steinle, ‘Experiments in History and Philosophy of Science’, *Perspectives on Science* 10, no. 4 (2002): 413). The article also contains a more elaborate discussion of Ampère’s experiment (ibid., 413-415). [↑](#footnote-ref-105)
106. Steinle, ‘Entering New Fields’, S67. [↑](#footnote-ref-106)
107. Steinle, ‘Experiments in History and Philosophy of Science’, 415. [↑](#footnote-ref-107)
108. Steinle, ‘Entering New Fields’, S67. Steinle provides two similar examples from the work of Faraday (ibid., S68-69). For a further discussion of the contrast between theory-driven and exploratory experimentation, see Steinle, ‘Experiments in History and Philosophy of Science’, 422. [↑](#footnote-ref-108)
109. Steinle, ‘Entering New Fields’, S69. [↑](#footnote-ref-109)
110. Ibid., S70. [↑](#footnote-ref-110)
111. Ibid., S70. [↑](#footnote-ref-111)
112. Steinle, ‘Experiments in History and Philosophy of Science’, 411–12. [↑](#footnote-ref-112)
113. Steinle, ‘Entering New Fields’, S71. [↑](#footnote-ref-113)
114. Steinle, ‘Experiments in History and Philosophy of Science’, 422. [↑](#footnote-ref-114)
115. Steinle, *Exploratory Experiments*, 319. [↑](#footnote-ref-115)
116. Ibid.*,* 314, 331–32. [↑](#footnote-ref-116)
117. Steinle, ‘Experiments in History and Philosophy of Science’, 419. [↑](#footnote-ref-117)
118. Steinle, *Exploratory Experiments*, 314. [↑](#footnote-ref-118)
119. Ibid.*,* 317. [↑](#footnote-ref-119)
120. Steinle, *Exploratory Experiments,* 317. [↑](#footnote-ref-120)
121. Ibid.*,* 317. [↑](#footnote-ref-121)
122. Cf. supra, chapter 3. [↑](#footnote-ref-122)
123. It aims at ‘ever more general empirical regularities’ (Steinle, ‘Experiments in History and Philosophy of Science’, 420). The establishment of laws is ‘pursued by the systematic variation of parameters, *under ceteris paribus conditions as broad as possible*’ (Steinle, *Exploratory Experiments*, 314, emphasis added). [↑](#footnote-ref-123)
124. This should not be taken as a criticism of Steinle’s specific take on exploratory experimentation as such. The fact that it does not capture the specificities of van Musschenbroek’s practice of experimentation does not mean that it cannot be used to adequately characterise other examples of experimental practice. I will come back to this later in this chapter. [↑](#footnote-ref-124)
125. We have also seen how van Musschenbroek still included the search for causes in his definition of physics. In chapter 2, I have shown how van Musschenbroek’s oration on the method of performing experiments can be seen to provide strategies that help in the search for stable cause-effect relations. The variation of experimental parameters played an important role in this. It is unclear how Steinle sees the relation between the empirical regularities which are sought for in exploratory experimentation (“if-then” statements) and causal relations. He does not address this issue explicitly in his discussion of the concept of exploratory experimentation. However, there are passages from his historical analyses which suggest that he sees the search for causes as a theoretical endeavour which thus has no place in exploratory experimentation: ‘The four remaining strands differ in characteristic ways from these exploratory domains. Instead of being undergirded by the broad-based search for empirical rules, they were guided by reflections on the underlying “cause” of the phenomena in question. In consequence, they typically involved positing entities inaccessible to observation. In the language of the time (which remains current in the contemporary philosophy of science), such reflections may be called “theoretical” in a more narrow sense in contrast with the “phenomenological” pursuits of the first three strands’ (Steinle, *Exploratory Experiments*, 131). It should be noted that, as we have also seen, van Musschenbroek likewise criticised such a theoretical search for causes and instead proposed a notion of a “true cause (*vera causa*)” which should be established by experimental means. There are passages to be found in Steinle’s historical account that provide an example of such “phenomenological” causal relationships which are discovered by means of exploratory experimentation, e.g. Faraday’s search ‘for the cause of the anomalous elevation of [a] wire’ (Steinle, ibid., 291). However, a bit further, Steinle uses a formulation which is closer to the “if-then” statement: ‘Further experiments allowed Faraday to determine the dependence of this effect on such conditions as battery strength’ (Steinle, ibid. 291). Steinle does refer to the importance that John Stuart Mill conferred to the variation of experimental parameters in the establishment of causes, and says that Mill’s point on establishing causal relations ‘plays an […] important role in [his] account of exploratory experimentation’ (Steinle, ibid., 320). He also refers to the work of Gerd Braßhoff on ‘the role of experimentation in establishing causal relations’ (Steinle, ibid., 321). But despite the remark that in both cases, his own work goes further because of its focus on conceptual change and revision, Steinle does not clarify how he sees the relationship between the search for causes and the search for regularities as described elsewhere in the book. [↑](#footnote-ref-125)
126. Steinle, *Exploratory Experiments*, 419. [↑](#footnote-ref-126)
127. Ibid.*,* 323. Steinle provides the following examples: ‘[W]e speak of optical phenomena as reducible to electromagnetic waves, of heat, to the movement of tiny particles, or of chemical phenomena, to physical effects’ (ibid.*,* 323). [↑](#footnote-ref-127)
128. Steinle, *Exploratory Experiments*, 323. [↑](#footnote-ref-128)
129. Ibid.*,* 323. [↑](#footnote-ref-129)
130. Ibid.*,* 324. [↑](#footnote-ref-130)
131. Ibid.*,* 325. [↑](#footnote-ref-131)
132. ‘Sed tempus monet, ut quoque brevi ostendamus, quomodo physica promoveatur experimentis, atque certa scientia maneat: Fieri hoc potest, si effectus corporum, per alios effectus explicentur: quotiescunque enim occurrunt nonnuli valde intricati, sive ex concursu plurimarum causarum simul, sive ex subtilitate, sive ex celeritate sensus fere eludentibus, tum intelliguntur obscure, saepius nequaquam: hi tamen redduntur intellectu faciles, quum simpliciora instituuntur cum aliis corporibus experimenta, quae ejusdem generis effectus praestant; vel quum crassiora redduntur corpora; vel quando cum his tentamina fiunt, quae facile sensoriis tractari & observari possunt; haec enim subtiliorum operationes perfecta similitudine illustrabunt: ita experimenta explicantur experimentis’ (van Musschenbroek, *Oratio de certa methodo*, 32). It is in this context, that van Musschenbroek made the comment (a bit further in the oration) that ‘Whatever we have said until now only considers the things we have observed of effects, about their causes I am brought to silence (*Quaecunque hactenus diximus spectant tantum observata effectuum, de causis eorum silentium protraxi*)’ (van Musschenbroek, ibid., 33). After this sentence, van Musschenbroek starts a criticism of *a priori* philosophy which explains effects by assuming causes (van Musschenbroek, I bid., 33-34). This relates to the observation discussed above that Steinle excludes the search for (underlying) causes from exploratory experimentation, but in his historical treatment (implicitly) seems to allow for the search for what I called “phenomenological causal relations” to be considered as a part of exploratory experimentation. [↑](#footnote-ref-132)
133. Steinle, *Exploratory Experiments*, 325. [↑](#footnote-ref-133)
134. Ibid.*,* 325. [↑](#footnote-ref-134)
135. Ibid.*,* 331. [↑](#footnote-ref-135)
136. Ibid.*,* 330. [↑](#footnote-ref-136)
137. Ibid.*,* 328. [↑](#footnote-ref-137)
138. Klaus Hentschel, ‘The Interplay of Instrumentation, Experiment, and Theory: Patterns Emerging from Case Studies on Solar Redshift, 1890-1960’, *Philosophy of Science* 64 (1997): S58. Hentschel lists further strategies which can be found in both van Musschenbroek’s theory and practice of experimentation. In the citation from van Musschenbroek’s oration, we find a clear call for ‘Formation of analogies between the new effect and well-known phenomena’ (Hentschel, ibid., S58). Another strategy which I will deal with in more detail in what follows is the ‘[e]xtension and amplification of the empirical basis […] through instrument-guided heuristics for optimization of the apparatus’ (Hentschel, ibid., S58). [↑](#footnote-ref-138)
139. Van Musschenbroek, ‘Oratio de methodo instituendi experimenta physica’, XLIV. [↑](#footnote-ref-139)
140. At one point, he explicitly draws the analogy between the fact that in exploratory experimentation, conceptual revision is often necessary, and history and philosophy of science itself as a kind of exploratory experimentation, also requiring conceptual revisions from time to time (Steinle, ‘Experiments in History and Philosophy of Science’, 428). [↑](#footnote-ref-140)
141. Steinle, *Exploratory Experiments*, 331. [↑](#footnote-ref-141)
142. Kevin C. Elliott, ‘Varieties of Exploratory Experimentation in Nanotoxicology’, *History and Philosophy of the Life Sciences* 29, no. 3 (2007): 323. [↑](#footnote-ref-142)
143. Elliott, ‘Varieties of Exploratory Experimentation in Nanotoxicology’, 323. For a tabular overview of the different kinds of exploratory experimentation identified by Elliott, organised along these three dimensions, see Elliott, ibid., 324. [↑](#footnote-ref-143)
144. In the same way as Steinle does not want to deny that there are clear cases in which experimentation *is* theory-driven and where experiments are used to confirm theories or decide between competing theories (Steinle, *Exploratory Experiments*, 312), I do not want to deny that there are instances in which exploratory experimentation is driven by a certain idea of the kind of regularity one is looking for. What I do want to argue is that such a limited conception does not do justice to van Musschenbroek’s experimental research (which I think can be aptly characterised as an instance of exploratory experimentation), but also that (analogous to Steinle’s criticism of the “standard view” on experimentation) it poses the risk of certain historical instances of research being neglected. [↑](#footnote-ref-144)
145. Elliott, ‘Varieties of Exploratory Experimentation in Nanotoxicology’, 324. [↑](#footnote-ref-145)
146. Silva and Heering, ‘Re-Examining the Early History of the Leiden Jar’, 28. [↑](#footnote-ref-146)
147. Silva and Heering, ‘Re-Examining the Early History of the Leiden Jar’, 27. A helpful table containing an overview of the first replications of the Leiden jar by several actors can be found in Silva and Heering, ibid., 25-26. Such a communal form of exploratory experimentation is one of the different kinds of ‘methods or strategies for varying experimental parameters’ discussed by Eliott (Elliott, ‘Varieties of Exploratory Experimentation in Nanotoxicology’, 328–29). [↑](#footnote-ref-147)
148. Silva and Heering, ‘Re-Examining the Early History of the Leiden Jar’, 17. [↑](#footnote-ref-148)
149. Ibid., 27. This also led to the Leiden jar being “black-boxed”, i.e. used as a standardised piece of equipment for other types of research, including medical experiments and experiments on animal electricity (ibid.). [↑](#footnote-ref-149)
150. Again, I do not want to deny that there were experimenters who did perform experiments with this aim in mind. As Silva and Heering show, there were indeed experimenters who ‘analyzed the jar to develop an explanation of its behavior’ (ibid., 27). Benjamin Franklin would later make a conceptual innovation by introducing the notion of a deficit and surplus of electricity and by explaining electrical phenomena by means of this disequilibrium. This conceptual innovation allowed him to explain many phenomena in the field of electricity, including the Leiden Jar, and to formulate several regularities. For a discussion of Franklin’s work, see Heilbron, *Electricity in the 17th and 18th Centuries*, 324–43. [↑](#footnote-ref-150)
151. Elliott, ‘Varieties of Exploratory Experimentation in Nanotoxicology’, 325. [↑](#footnote-ref-151)
152. Ibid., 325. [↑](#footnote-ref-152)
153. One might also refer to certain experiments made on the Leiden Jar by researchers after van Musschenbroek, which went beyond attempts to stabilise the phenomenon as such and aimed at modifying the set-up in order to increase the strength of the effect even more (Silva and Heering, ‘Re-Examining the Early History of the Leiden Jar’, 27). [↑](#footnote-ref-153)
154. Elliott, ‘Varieties of Exploratory Experimentation in Nanotoxicology’, 326, emphasis in original. [↑](#footnote-ref-154)
155. For a discussion of these distinctions, see Steinle, *Exploratory Experiments*, 316–20. Steinle clearly distinguishes the conceptual from the theoretical: ‘In particular, the distinction between theories, laws, and concepts strikes me as highly significant. The ways in which theories and concepts are formed, stabilized, and used differ markedly’ (ibid., 319). [↑](#footnote-ref-155)
156. Steinle, ‘Experiments in History and Philosophy of Science’, 420, emphasis added. [↑](#footnote-ref-156)
157. Rouse, *Knowledge and Power*, 22–23. Rouse provides the following contrast with a practice-based approach: ‘The issue does not concern inferring a general claim from a number of claims about particulars: instead it concerns simplifying and generalising a problem formulation, replicating an achievement in different circumstances or for different purposes, or adapting a result to help understand a new situation or problem or to construct new possibilities for investigation. In short, it is a matter of transforming a concrete, local achievement in order to open up a field of possibilities for further scientific investigation’ (ibid.*,* 22). [↑](#footnote-ref-157)
158. Again, this is not to deny that there are examples of exploratory experimentation made with this aim, nor do I want to deny the role that the formulation of empirical generalities might play in science. [↑](#footnote-ref-158)
159. Rouse, *Knowledge and Power*, 100. [↑](#footnote-ref-159)
160. Steinle, 'Experiments in History and Philosophy of Science', 419. [↑](#footnote-ref-160)
161. Ibid., 419. [↑](#footnote-ref-161)
162. Ibid., 419–20. In chapter 3, we have seen how Gray first thought that the colour of the silk used to insulate the body to be electrified was a relevant variable and that blue silk provided the best results. Du Fay would later show that the colour was in fact not the relevant variable at play, but that the specific pigment used to dye the silk was the relevant factor (probably due to its moistness). [↑](#footnote-ref-162)
163. Ibid.*,* 308. [↑](#footnote-ref-163)
164. Ibid.*,* 308. [↑](#footnote-ref-164)
165. Steinle, *Exploratory Experiments,* 308. [↑](#footnote-ref-165)
166. In his contribution to *The Philosophy of Scientific Experimentation* (ed. Hans Radder), Rainer Lange discusses the role of technology in experimental practice. His discussion of the spread of laboratory results is similar to the view presented in section 4.4.1 in that he also emphasises the importance of skill acquisition and the spread of material objects (Rainer Lange, ‘Technology as Basis and Object of Experimental Practices’, in *The Philosophy of Scientific Experimentation* (Pittsburgh: University of Pittsburgh Press, 2003), 119–37). He also points towards the need for experimental practices to acquire ‘instrumental control of hitherto unknown processes’ (ibid., 133) and argues that this done in two steps. On the one hand, there is a “phase of exploration”, which he sees as closer to natural history, in which observations are collected ‘that might prove useful in learning more about those phenomena’ (ibid., 134). Then there is a phase of experimental “pretesting”. Although not using the term “exploratory experimentation”, the process he describes is very similar to the “preliminary exploratory experimentation” discussed above (ibid., 134-135). With regard to the topic at hand, what is interesting is that Lange describes the process aimed at stabilising the phenomena and experimental procedures as an attempt to make the experimental procedure “teachable” (and thus reproducible). This “teachability” is a necessary prerequisite for the process which I have discussed in section 4.4.1 as the spread of microworlds: ‘Does this mean that any experimental knowledge is only locally valid? It does not, because the claim that an experiment reproducibly leads to a certain result is not relativized to a community of researchers in the set of a finite set of real individuals. Rather, it is relativized to an experimental practice. An experimental practice is incomplete if it does not also provide the means to extend itself in time and space, including those needed to teach any skills necessary to perform its experiments and thus gain new members for the community of practitioners. This is whereby it transcends the local community of those who presently are members of that practice and ensures the transsubjective validity of its results. For the process of establishing a new experiment, this means that it is only finished when the skills necessary to perform it can reliably be taught to others’ (ibid., 135-136). He further notes that the extension of experimental practices should not only be seen as happening synchronously, by spreading to other places, but also diachronically, by being transmitted to new generations of practitioners: ‘Experimental practices are thus historical entities that make provision for their own indefinite extensibility both in space and time’ (ibid., 133). See also infra for Nancy Nersessian’s work on the diachronic nature of cognitive systems. [↑](#footnote-ref-166)
167. Scientific pedagogy has recently begun to attract the attention of historians and philosophers of science, see for example Alain Bernard and Christine Proust, eds., *Scientific Sources and Teaching Contexts Throughout History: Problems and Perspectives* (Dordrecht/Heidelberg/London/New York: Springer, 2014); Massimiliano Badino and James Navarro, ‘Pedagogy and Research. Notes for a Historical Epistemology of Science Education’, in *Research and Pedagogy: A History of Quantum Physics through Its Textbooks*, ed. Massimiliano Badino and James Navarro (Berlin: Neopubli GMbH, 2013), 7–25; David Kaiser, ed., *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives* (Cambridge, MA: MIT Press, 2005); Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago/London: University of Chicago Press, 2003). Warwick’s work is especially noteworthy, because it demonstrates the importance of skill and materiality in even such an abstract field as mathematical physics. His analysis of the introduction of analytical mathematics from the Continent at Cambridge further provides an example of the processes of standardisation and reconstruction that we have seen at play in the spread of microworlds. On the one hand, the introduction of the new developments called for a reconstruction of the place where it was being imported. It necessitated people to learn new mathematical skills in order to be able to teach them themselves. Learning practices also changed with the examination process being adjusted and the importance of private tutors increasing even more (Warwick, *Masters of Theory*, 77). On the other hand, it also involved processes of standardisation, where French textbooks had to be reworked in order to be usable in the new context. This led to the production of textbooks specifically suited for use in the Cambridge educational system (ibid., 145). [↑](#footnote-ref-167)
168. Several “approaches” or “traditions” have developed, each with their own specific emphasis. This has given rise to an umbrella term for these new approaches, “4E”, which refers to four ways in which cognition is approached, i.e. as “embodied”, “embedded”, “enactive”, and “extended” cognition (for a recent overview, see Albert Newen, Leon De Bruin, and Shaun Gallagher, eds., *The Oxford Handbook of 4E Cognition* (Oxford: Oxford University Press, 2018)). Nancy Nersessian uses the term “environmental perspectives” as an umbrella term referring to approaches which emphasise the embodied, distributed, enculturated, and situated nature of cognition (Nancy J. Nersessian, ‘The Cognitive-Cultural Systems of the Research Laboratory’, *Organization Studies* 27, no. 1 (2006): 126). [↑](#footnote-ref-168)
169. For a discussion, see Matthew J. Brown, ‘Science as Socially Distributed Cognition: Bridging Philosophy and Sociology of Science’, in *Foundations of the Formal Sciences VII, Studies in Logic: Bringing Together Philosophy and Sociology of Science*, ed. Karen François et al. (London: College Publications, 2011), 17–31. [↑](#footnote-ref-169)
170. Elke Kurz-Milcke, Nancy J. Nersessian, and Wendy C. Newstetter, ‘What Has History to Do with Cognition? Interactive Methods for Studying Research Laboratories’, *Journal of Cognition and Culture* 4, no. 3 (2004): 663–700. [↑](#footnote-ref-170)
171. Nersessian, ‘The Cognitive-Cultural Systems of the Research Laboratory’, 129–30. [↑](#footnote-ref-171)
172. In his *Cognition in the Wild*, Edwin Hutchins has used the notion of “distributed cognition” to perform an ethnographic study of navigational practices at a US Navy ship, analysing them as distributed cognitive processes. Hutchins also discusses both the learning process of individuals within this system, as the development of this system as a learning process. He provides a general definition of learning as ‘*adaptive reorganization in a complex system*’ (Edwin Hutchins, *Cognition in the Wild* (Cambridge, Massachusetts: MIT Press, 1996), 289). This definition can both be applied to individual learning as to ‘learning situated in the socio-material world’ (Hutchins, ibid., 289). [↑](#footnote-ref-172)
173. Kurz-Milcke, Nersessian, and Newstetter, ‘What Has History to Do With Cognition?’, 675. [↑](#footnote-ref-173)
174. Ibid., 677. [↑](#footnote-ref-174)
175. Ibid., 678. Cell-culturing also involves a lot of manual skill and coordination, which has to be acquired through practice (ibid., 679-680). One of the important reasons to coordinate’s one’s actions in a proper way is to avoid any kind of accidental contamination of the cell cultures (ibid., 679). This is another example of the importance of processes of isolation in the construction and maintenance of microworlds. [↑](#footnote-ref-175)
176. Kurz-Milcke, Nersessian, and Newstetter, ‘What has History to Do With Cognition?’, 675. In the original passage (which is also cited above), Kurz-Milcke et al. speak of “knowledge” being distributed. In line with my emphasis on and the use of the term “embodiment”, I could speak of knowledge as being embodied in artifacts and instruments. A similar point is made by Davis Baird in his account of “thing knowledge” (see Davis Baird, ‘Thing Knowledge: Outline of a Materialist Theory of Knowledge’, in *The Philosophy of Scientific Experimentation*, ed. Hans Radder (Pittsburgh: University of Pittsburgh Press, 2003), 39–67). The reason to avoid this particular way of describing the nature of artefacts and instruments as results of learning *in* the world, is that it implicitly implies a reified account of knowledge which I think sits uneasily with the notion of scientific practice as learning *in* the world (for a detailed criticism of a reification of (scientific) knowledge and a development of an alternative, dynamic account, see Rouse, *Engaging Science*, 179–204). Although I cannot develop this point further here, Baird’s account provides a good example of the risk involved. His account of “thing knowledge” is modelled on a very traditional epistemological understanding of knowledge as “justified, true belief”. To argue that there is such a thing as “thing knowledge”, Baird thus sets out to demonstrate that there is also such a way as material “truth” and material “justification” (Baird, ‘Thing Knowledge’, 54-56). Rather than revising or criticising traditional epistemology for its inability to account for the embodied and material character of scientific knowledge, Baird’s account thus ends up fitting these latter in the mould provided by an epistemology which was built on the presupposition of the immaterial and propositional character of knowledge. This leads Baird to resurrect Popper’s notion of “Objective Knowledge” in order to develop a “Metaphysics of Thing Knowledge” in which Popper’s “Third World” of objective knowledge is expanded with “Objective Thing Knowledge” (ibid., 60-66). Baird’s reason to develop this argument was his earlier insistence on the limitations of “subjective epistemology” (i.e. limiting knowledge to subjective beliefs) when it comes to understanding scientific and technological knowledge (ibid., 44). Given my point on providing a non-subjectivist account of learning *in* the world, I am of course sympathetic to Baird’s point. However, by using Popper’s notion of a “third world” he arrives at a somewhat strange marriage of materiality and Platonism. [↑](#footnote-ref-176)
177. Kurz-Milcke, Nersessian, and Newstetter, ‘What Has History to Do with Cognition?’, 672. [↑](#footnote-ref-177)
178. Nersessian, ‘The Cognitive-Cultural Systems of the Research Laboratory’, 133. [↑](#footnote-ref-178)
179. Ibid., 133. [↑](#footnote-ref-179)
180. Ibid., 133. [↑](#footnote-ref-180)
181. Rouse, *Knowledge and Power*, 88. [↑](#footnote-ref-181)
182. And which, as we have seen, van Musschenbroek himself subjected to further exploratory experimentation. [↑](#footnote-ref-182)
183. ‘Quinam varias invenerunt machinas electricitatem excitandi’ (LUL BPL 240.18, fol. 16v). [↑](#footnote-ref-183)
184. In chapter 2, I have shown how for van Musschenbroek, repeating experiments not only had the function of confirming experimental results, but also (and perhaps, most importantly) had the function of enabling the researcher to develop her circumspection. [↑](#footnote-ref-184)
185. Heilbron, *Electricity in the 17th and 18th Centuries*, 271. [↑](#footnote-ref-185)
186. Cf. supra chapter 3. [↑](#footnote-ref-186)
187. Cyrus C. M. Mody, ‘Instruments in Training: The Growth of American Probe Microscopy in the 1980s’, in *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*, ed. David Kaiser (Cambridge, MA: MIT Press, 2005), 206. [↑](#footnote-ref-187)
188. Mody, 'Instruments in Training', 206. The development of these blackboxes also provided commercial benefits for companies, because the instruments could be sold to laboratories working in many different disciplines (ibid., 207). [↑](#footnote-ref-188)
189. Emma Uprichard, Roger Burrows, and David Byrne, ‘SPSS as an “Inscription Device”: From Causality to Description?’, *The Sociological Review* 56, no. 4 (2008): 612–13. [↑](#footnote-ref-189)
190. Davis Baird refers to an illuminating example provided by Larry Bucciarelli in his *Designing Engineers*. Bucciarelli mentions how he was present at a conference on technological literacy where a speaker lamented the fact that very few people knew how their phone worked. Bucciarelli however wonders whether the speaker himself (and by extension *anybody*) really “knows” how his or her phone works, given the complexity of the systems allowing the phone to function properly: ‘Does [the speaker] know about the heuristics used to achieve optimum routing for long-distance calls? Does he know about the intricacies of the algorithms used for echo and noise suppression? Does he know how a signal is transmitted to and retrieved from a satellite in orbit? Does he know how AT&T, MCI, and the local phone companies are able to use the same network simultaneously? Does he know how many operators are needed to keep the system working, or what these repair people actually do when they climb a telephone pole? Does he know about corporate financing, capital investment strategies, or the role of regulation in the functioning of this expansive and sophisticated communication system?’ (Bucciarelli, as cited in Baird, ‘Thing Knowledge: Outline of a Materialist Theory of Knowledge’, 44). [↑](#footnote-ref-190)
191. Hasok Chang, *Inventing Temperature: Measurement and Scientific Progress* (Oxford: Oxford University Press, 2004); Thomas P. Hughes, *Networks of Power: Electrification in Western Society 1880-1930* (Baltimore and London: The Johns Hopkins University Press, 1983). [↑](#footnote-ref-191)
192. Kuhn, *The Structure of Scientific Revolutions*, 17. [↑](#footnote-ref-192)
193. Ibid.. [↑](#footnote-ref-193)
194. Truesdell discusses van Musschenbroek’s work on the strength of materials in so far as it fits in his teleologically oriented history culminating in the genius of Euler. It is telling that Truesdell added a prefatory note in which he emphasised that theories of elastic and flexible bodies were not influenced by technology (Truesdell, *The Rational Mechanics of Flexible or Elastic Bodies, 1638-1788*, 13–14). Benvenuto’s and Timoshenko’s discussion of van Musschenbroek’s work on the topic is more balanced (Benvenuto, *An Introduction to the History of Structural Mechanics*, 280–84; Timoshenko, *History of Strength of Materials*, 54–55). It is noteworthy that both Benvenuto and Timoshenko have a background in engineering. With regard to the Leiden jar, one can again point to the example of Kuhn, who reduces the importance of the “discovery” to the fact that it later allowed Franklin to provide a theoretical explanation of the jar and thus to provide the first paradigm in the research on electricity (Kuhn, *The Structure of Scientific Revolutions*, 17–22). Heilbron likewise sees the main importance of the jar as residing in the fact that it ‘shattered accepted theory’ (Heilbron, *Electricity in the 17th and 18th Centuries*, 315). Heilbron’s interpretation of the early history of the jar (especially his view that it was seen as a refutation of established theory) is criticised by Silva and Heering, ‘Re-Examining the Early History of the Leiden Jar’. [↑](#footnote-ref-194)
195. De Pater, *Petrus van Musschenbroek (1692-1761)*, 33–40. [↑](#footnote-ref-195)
196. Rouse, *Knowledge and Power,* 102. [↑](#footnote-ref-196)