Matter in Action

What is superconductivity?

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Introduction

What do we do if we would like to know what the fundamental constituents of our world are? In our modern, scientific materialist society,[1] chances are that we would turn to the discipline of physics for answers. At least, we would try to arrive at some answers using the scientific method, which would then be described as an attempt to produce knowledge by formulating hypotheses and doing experiments to see if these hypotheses hold up. Or, if we are not scientists, we would learn about this through science-communication. My interest in this text is not to carry out this same process of hypothesizing and testing, but to evaluate the fruitfulness of this strategy. Does science provide us with answers about the fundamental constituents of our world? Is there such a thing? I will try to evaluate this by considering the case of research into superconductivity, a phenomenon that is researched in materials at extremely low temperatures. In this text I aim to answer the question 'what is superconductivity?'. This seemingly simple question allows me to inquire into how such a question can be answered. I am not solely concerned with an answer that uses the content of the discipline of physics. Rather, to evaluate how scientific knowledge is made, I want to answer the question in a way that includes the ways in which physicists research superconductivity. While doing this, I also touch upon related philosophical questions such as 'where does the boundary between knowledge and what is known lie', 'what is the difference between making and observing in the laboratory sciences', 'do objects have essences', and 'what is objectivity'.

Before writing this text, I carried out fieldwork at a Dutch university-laboratory where superconductivity was one of the main research interests. By participating in the research of one group at that laboratory, I could observe the knowledge-making practices of the physicists first-hand. Every day, I logged my observations in a notebook. You will find excerpts from this notebook (also called vignettes) in the second chapter. I have replaced the names of the people in the laboratory with fictional ones, giving the first person to appear in this text a name starting with an A, and the fourth and last person a name that starts with a D.

In the first chapter, I outline how I went about this research. I first discuss a common practice, namely writing and reading, to illustrate in what way I approach my subject matter. I discuss that scientists and people who study science can be caught up in an epistemological anxiety that can be called 'representationalism'. This is the believe that we, as humans, have access to a space of reasons, images, or mental constructs that can accurately represent the world. However, whether we have privileged access to such a space, and whether it is separate from the world 'outside' has shown to be untenable in various strands of philosophy.[2] The first chapter shows that people who study science can circumvent representationalism by focusing on local practices. A study of practice shows that people who make knowledge rely on a lot more than reasons or mental images alone. Next, I discuss that anthropological methods are a good way to conduct research that avoids representationalism because of this same focus

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[1] Whether we live in a modern, scientific materialist society, what that means, and who 'we' are, is one of the greater debates in the history of science. See Cohen 2010 for a comprehensive review of whether, and why, there was a scientific revolution in Europe. See Cunningham and Williams 1993 for a discussion on how we can de-center the picture of modern science as a special and central form of knowledge.

[2] E.g. McDowell 1996 and Latour 1993.

on local practices. Following Mol 2002, I suggest that the name 'praxiography' might be used to stress this aspect.

In the second chapter, I answer the main question of this text: 'What is superconductivity?'. Using vignettes from my fieldwork in the laboratory I show that superconductivity is something different in every situation that it is researched in. I explore these different 'instantiations' of superconductivity in the laboratory and ask how they fit together. Are they separate versions or types? Or can they be combined into a coherent whole? I suggest that neither of these options is satisfactory. Instead, superconductivity in the laboratory should be regarded as a 'multiple': An object that is more than one but less than many. This notion comes from secondary literature (Mol 2002). In the second chapter, I deal with such literature in the footnotes. However, the footnotes can be read after each other without going back to the main text. Effectively, this divides the second chapter into a main- and a subtext.[3] They can be read separately, one of them can be skipped, or they can be read through each other like you would do with conventional footnotes. I have done this to enable you to read my discussion of superconductivity without having to read my genealogy of science studies, and vice versa. The main text can be read without being distracted by references, and the subtext without getting lost in physical details. However, the links between them can still be checked through the footnote system.

The third chapter considers the question 'what is objectivity' through the lens of feminist theory. I discuss a couple of key commentaries on objectivity that subsequently argue that objectivity is a male-centered virtue, that it can be improved by incorporating an evaluation of a researcher's subjectivity, and that it should be characterized by partiality. Furthermore, I discuss what the conclusions of the second chapter implicate for objectivity. If objects in practice are multiple, what would it mean to engage them in knowledge practices objectively? Finally, I come up with a reformulation of objectivity that does not rely on representationalism, but rather stresses openness in knowledge practices. Openness allows us to counter wrong representations and stereotypes by pointing at other possibilities of knowing and being.

The three chapters are written so that they can be read separately, or in any order you like. As I already indicated, the main- and subtext of the second chapter can also be read independently. If you are interested in the praxiography of superconductivity, you can read the main text of the second chapter. If you are also interested in how I carried out this praxiography, you could read the first chapter first. If you are mainly interested in a discussion of secondary literature and issues in science studies, the subtext of the second chapter, and the third chapter are most interesting.

Finally, I need to indicate that this text can be seen as an example of the type of thinking discussed in the cited literature. This text does not provide new physical answers to what superconductivity is. Instead, it explores new ways to think of that question. In the conclusion, I reflect on how I answered my main question, and how the question helped my philosophical discussion.

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[3] In *The Body Multiple,* Annemarie Mol splits every chapter into a main text and a subtext (2002). The main text deals with her ethnography of atherosclerosis, while the subtext discusses the literature that deals with the topic of each chapter. In my second chapter, I use the same division between literature and ethnography, but I also provide links between the two through numbered footnotes.

I have also added headings in between the footnotes.

This provides some structure for people who only read the subtext.

1. From words to practices

This chapter is a strange sort of methods section. In it, I introduce the type of thinking that I will use throughout the rest of the text. This method of thinking does not take the separations at the foundation of modern thought for granted, such as that between words and things or between nature and culture. I start by considering epistemological problems viewed from those modern separations and move gradually to a method of thinking in which those separations are not important or insurmountable anymore. I end with a discussion of 'praxiography', which is derived from ethnography, as a method that can reorder the modern separations by focusing on practices.

How to read

Reading a text, this one for example, seems to involve information that is somehow present on the paper or screen being transferred to your understanding. A host of philosophers have tried to solve the problems concerning knowers, such as a reader, getting access to the known, such as scholarly theories, for quite some time. They were questioning whether it is possible at all for *things in the world* to become known to *us*. How do we know that we have authentic access to the 'things themselves' and are not deceived in our perceptions? How do you know that what you are reading here is what I *actually* meant in this text? Aren't you missing any hidden or deeper meanings? How do I, as the writer, know that I am writing down what I *actually* mean to convey?[4]

This problem is discussed in the field of epistemology, the study of knowing, which traditionally had a contrasted position to ontology, the study of the nature of being. Traditional epistemology is concerned with knowers getting access to the world of the 'things in themselves' which are taken to be radically different from knowledge categories or representations. This field of study wants to know how to make the categories and representations that constitute knowledge accurate in that they represent the things 'out there' well. An assumption that underlies this whole schema of knower and known is that beings have some inherent or essential nature that makes them 'themselves'. This means the ontology necessary for the traditional epistemology is one of essences: This computer in front of me *is* something in the world with some inherent properties and I *am* a person with certain traits who is trying to read what I wrote on this screen just a minute ago.

But wait, here we encounter another element in the analysis: An action; namely the act of writing. If we start from a relatively stable conception of a reader/writer and a screen, it might be hard to explain how they can ever connect. However, if I actively introduce my process of writing into the analysis, it becomes clear to me that the letters I see appearing on this screen are not an inherent property of this screen at all; and to you, the reader, it becomes evident that this text has a history, a specific place of production (For a discussion of reflexivity see Woolgar 1988). The history of this text is intertwined with my history, and also with yours.

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[4] See McDowell 1996 for a good analytical treatment of this anxiety of the modern world. McDowell analyses that the space of concepts, the space of reasons, and the external world cannot be regarded as completely separate. Also see Latour 1993 for the impossibility of upholding the dichotomies central to modern thought, such as that between subject and object, and between culture and nature.

The shift to actions makes us analyze the *relations* between the elements of this story that we previously considered as fully separable and predetermined. Things are not just 'out there' waiting to be represented but they are *done*. Moreover, words are not just true or false descriptions but have material effects and can be considered as actions (Austin 1975). In the case of this text, you might say that it is produced by me, the author. However, I am not the only factor in its production. The text is also produced by the educational structure that made me write this text in the first place. It does not stop there though. This text would have been different if I wouldn't have had a student flat where I could write this piece. Here we see that my living situation and the educational structure in the Netherlands have some distinct effects on me that make me write this text (in this way). At this point you might even wonder about this 'me' that you see being mentioned in this text. 'I' am no less a product of these structures than this (electronic) piece of paper. It would be more apt to look at all the elements in the analysis as being *produced* in specific ways by specific structures than to see them as having inherent properties. By using the term *production,* we step away from trying to attain accurate representations, and towards a "critical ontology of ourselves" in which we analyze how we came to be what we are now (Foucault 1984).

Now, if *production* sounds quite mechanical and concerned with material factors in a deterministic way it might be combined with the term *performance*. *Performance* conjures up the image of a stage with some performers on it. The performers try to make the audience believe that they are some character by telling a story that supports their fiction. The story is performed on stage with the help of the actors, their costumes, the décor, and some props. All these things on the stage have some function in the story. We might draw an analogy and say, as some sociologists did in the previous century, that in daily life we are also performing some story about ourselves (See Goffman 1959, as quoted in Mol 2002, 34-6). Things that we do in daily life support a story about ourselves that we see as our identity.

But, if we compare life to a play, does that mean that we have a self that we are pretending to be, the performance, and an actual self that is revealed underneath all the costumes when we go backstage? This sounds eerily similar to the problem of wanting to know the *true* meaning of a text. To avoid this trap, our conception of *performativity* must not make such distinctions between frontstage and backstage. Instead, we can view performing as a kind of production. Performing ourselves in daily life does not hide a deeper self but constitutes the self: It produces identities by incrementally building on the histories of how they have been performed before.[5]

[5] This notion of performativity is taken from Judith Butler who uses the notion of performativity to theorize the production of sexual difference and the constitution of gender identities. See *Gender Trouble* ([1990] 1999) and *Bodies that Matter* (1993).

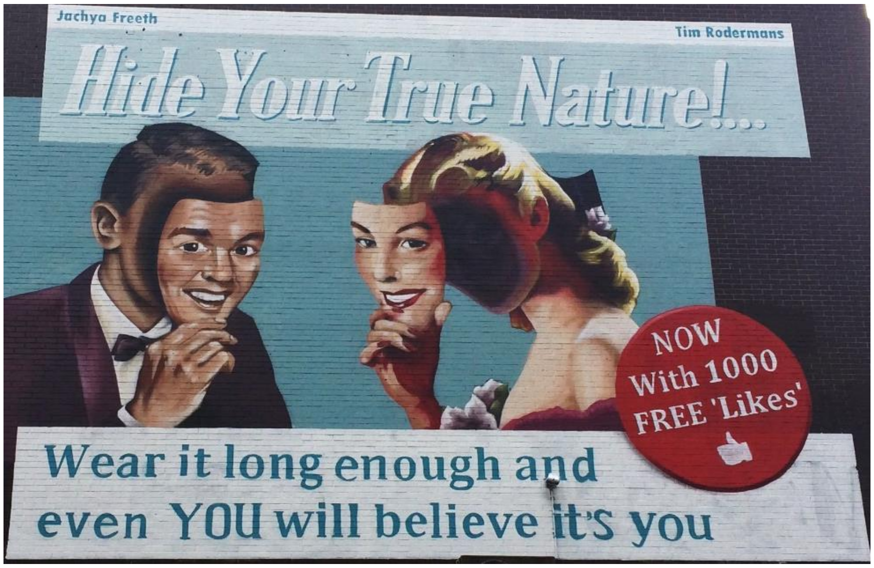

Figure 1: "Hide Your True Nature!... Wear it long enough and even YOU will believe it's you. Now With 1000 FREE 'Likes'". By Jachya Freeth and Tim Rodermans. Picture taken by author at Weesperplein, Amsterdam.

We can try to use this framework to analyze the situation that you are in right now as a reader. Reading this paper is a performance which includes you, the reader, a screen or paper to read from and some place where you read this. The things in this scenario are not 'things in themselves' but things that gain their identities through their function in the performance. An important shift in this framework is that this paper does not have an inherent identity or meaning (backstage), but that the meaning is constituted through other signifying elements in the performance such as the time at which it is read, who is reading it, which words are being read, and in which font the text is presented. It is not the case that the meaning of the text unidirectionally determines how and with what props you should read the text. Instead, how you read the text is a back-and-forth between the props and the meaning of the text: On the one hand, the props (paper/screen), stage (wherever you are right now) and actors (you), perform the meaning of the text; on the other hand, the meaning of the text influences what you need (a quiet space; a screen or paper to display the text) to read the text. Moreover, the performance builds on previous performances of you and of the text. Instead of a true meaning being hidden behind this particular reading of this paper, this reading is built upon, and adds to, your history of reading texts, as well as on the specificities of the situations in which I, the author, wrote it.

But does this mean that the meaning of this text only depends on how you and I perform it? Is the meaning of this text solely a product of our opinions about it? Using the term *performance*, it may seem like this analysis of the problem of understanding what is written in this text recedes into the realm of words and social signification. This might be the way that some sociologists would analyze this scenario, claiming that the text and the reader are *social constructs* (see Hacking, 1999). A helpful contribution of social constructivism is that, by claiming that both text and reader are socially constructed, the historicity of these elements is stressed. This undermines notions of inherency. At the same time, however, emphasizing social signification does not actually solve the problem of *words and things* because it neglects the things: Claiming that the meaning of this text is only constructed through our social understanding of it makes the problem recede into the realm of subjective interpretations. In this realm there is (again!) no satisfying way to have a connection to what the interpretation is about.

Social constructivism does not close the gap between the social and the natural, or between knowing and being, because it erases the natural/ontic. Let's recall that performances also require a lot of props. The room I am in, the chair I am sitting in, the desk I am sitting at, the computer I use, and the food I just ate are necessary for this text to be written in this way. These nonhumans are necessary for the performance of my identity as a writer, but also for the identity of the text (i.e. its meaning). We can even investigate identities of very mundane things like cups by evaluating the performances (actions/uses/experiments) they are engaged in (e.g. it is a being that can contain fluids. And it can be broken when it encounters a large force). To do this, we can drop the word 'social' and describe beings in the world as *constructs* of all kinds of factors.[6]

This brings us to one of the cornerstones of the relational philosophies considered in this text: The conception of practices as *material-discursive* constructs/performances. Having either a physical or a social account of practices of knowledge like reading or writing reaffirms the very source of the problem of representationalism by strengthening the chasm between knowledge-representations and what they are supposed to represent. Instead, shifting our focus to practices closes the gap by showing the role of meaning in structuring matter and the role of matter in enabling or limiting the possibilities of signification. In practice, there is no clear distinction or unilinear causality between so-called material and discursive elements. Let's refocus on what I am doing. My fingers are touching the keyboard of my computer according to which words I want to type. As I do so, the words appear on the screen, which I perceive with my eyes which enables me to read what I wrote. In this feedback loop of typing and perceiving, the next words slowly emerge in the process of writing until sentences appear. The sentences are produced and performed by my fingers, the computer, other parts of my body, the software of the computer, the battery of the computer and the energy stored in it. In this list it is not immediately clear which elements are material and which are discursive. In practice, things are a combination of the two. Or rather, the very concepts of materiality and discursivity are abstracted notions that can make me produce lists in which I separate the elements that are supposedly material or discursive, but they are not concepts that automatically emerge from an analysis of practice. In a way, even claiming that the components of my list above are hybrids of material and discursive elements reaffirms the split between words and things again.[7]

If we move to a performative, material-discursive account of the practice of reading to explain how you can read this text, we might ask who the audience is for whom something is being performed. Are we side-stepping the question of how *you* can read what's written on this screen by taking a third-person perspective? Let's review what Judith Butler, who uses the word *performance* without assuming the existence of a backstage (i.e. underlying identity), has to say about the performativity of gender:

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[6] Bruno Latour and Steve Woolgar also went through this conceptual change when republishing their book *Laboratory Life*. Whereas the subtitle of the first edition (1979) was "The Social Construction of Scientific Facts", they dropped the word "social" from the subtitle of the second edition (1986).

[7] In the analogical case of analyzing the "natural" and "social" factors in a research, Donna Haraway has suggested the term naturecultures to overcome an implicit split also known as the nature/nurture debate. See *The Companion Species Manifesto: Dogs, People, and Significant Otherness* (2003). Also see Latour, *We Have Never Been Modern* (1993), for an analysis of how, in the modern world, the nature-culture divide is transgressed in practice but still effectively maintained in the moderns' attitude towards the world.

"Significantly, if gender is instituted through acts which are internally discontinuous, then the appearance of substance is precisely that, a constructed identity, a performative accomplishment which the mundane social audience, including the actors themselves, come to believe and to perform in the mode of belief" (Butler 1999, 179).

Butler observes here that the actors themselves start believing in their performance, effectively joining the rest of the audience (See figure 1, in which the characters believe in the reality of their masks).[8] That means that *performativity* is not meant as an outsider perspective; it is a tool to understand our relations to others *and* to ourselves.

The crucial step in understanding relational philosophies by contemplating how one reads is that the shift to performativity entails that you execute certain actions in order to perform 'reading this paper'. In that process both your own identity is (re)shaped by the performative elements needed to view yourself as a reader, and the identity/meaning of this paper is constituted by the specificities of how you read it. The intellectual, physical and other histories of you, this text, me, and other beings involved in this practice matter to the specificities of how you read this. Moreover, by focusing on practices, we not only stress historicity, but relationality more widely. By stressing relationality, the conceptual gap between words and things might become narrower and, at the very least, does not seem unbridgeable anymore. I might even say that the bridge becomes primary and that, by exploring the practice/performance of bridging, the two opposing sides of *knower* and *known* get constituted (Haraway 2003, 6).

I must admit that framing the problem of knowing in this relational way does not solve it in the terms of traditional epistemology. Rather, it shifts the question towards a new vantage point that, instead of encouraging doubt and cynicism, can inspire new questions (See also Latour 2004). The question is not anymore whether 'your' idea of what is written here is accurate, but how you relate to the paper and how this relation performs you and the paper. We shift our focus to the practice of writing and reading, in which meanings, identities, electrons, other texts, and an endless list of other beings constantly get reconfigured in relation to each other. In this reconfiguration 'I' do not have a final say over the content of the text as the author. 'You', the reader, have a say as well: As a writer, I have the reader in mind when writing this text in an attempt to make it understandable. Moreover, as a writer I am also a reader. I constantly read what I have written before; the difference is that I edit, and add to, what I read. In addition, you might be someone who has influenced my thinking through other practices such as conversations before I wrote this. You might even have commented on an earlier version of this text, having become a co-author in a quite direct sense. But even if there is not such an evident relation between you as reader and me as author, the fact that I am writing, and you are reading this text means there is some relation. In this relation, the content of the text is not a given, but a constant renegotiation between all the beings involved.

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[8] We might want to drop the word "social" in front of "audience" in Butler's quote too, moving towards a post-humanist understanding of performativity. This is one of the pillars of Barad's agential realism (2007).

How to do posthumanist ethnography

How can I extend this mode of thinking to conduct research? As Bruno Latour noted in ‘Why has critique run out of steam’, we need ways of inquiring into science and technology, or ‘technoscience’, that do not criticize it up to the point of annihilation, but that can inspire new questions (2004). Thinking about technoscience by inquiring into the material-discursive performances of the relational elements in technoscientific practices can achieve this. By not taking any essential identities for granted in this analysis we might stumble upon unexpected ways in which these elements relate and find unexpected roles that they take on. As in my analysis of reading above, we need a quite fine-grained analysis of technoscience to discover these roles and local identities. Such local knowledge can be obtained by using the methods of anthropology.

In this field, thinking about gathering knowledge from relationality has gained traction in the last decades. Kathleen DeWalt and Billie DeWalt, in their handbook *Participant Observation*, argue that phenomena can only be observed when they intersect with the observer (2011, 92). Ethnography, the method of anthropology, is characterized by the interaction of “the people being studied (the Other) with the anthropologist (the Self)” (36). In other words, the tool of the anthropologists is their own participation in a situation (111). In this participation there is always an ambiguity as to what the anthropologist should be observing. The researcher can access the ‘object’ of observation because of their participation in the situation in which this object plays a role. The anthropologist observes the phenomenon of interest by “attending to the events and people in the context they are studying. This is not just a visual phenomenon, but includes all of the senses” (80). One of the people in this context is the researcher. Anthropologists self-consciously study their own participation in a phenomenon because their access is constituted by all the senses that are engaged in participating. By partly becoming part of the phenomenon under investigation, anthropologists can develop knowledge about it.

DeWalt and DeWalt note that this makes anthropologists walk a thin line between participation and observation, with the danger of getting stuck in either (21). Full participation implies that the researcher becomes part of the context of observation and has no analytical interest in it anymore. The researcher, arguably, gains a most advanced ‘insider’s perspective’, but loses the connection to the research. Full observation, on the other hand, implies that the anthropologist removes herself from the situation as much as possible and seeks not to influence it in any way. If the researcher gets stuck in this position, she cannot produce any knowledge because she has no access to what she is studying. She cannot just let the phenomenon ‘speak for itself’, because it only ‘speaks’ when asked the right questions and approached in the appropriate manner. If the researcher succeeds in balancing between participation and observation, anthropology becomes an iterative practice in which “our ideas and notions are continually challenged and ‘resisted’ by the actions and words of those within the setting” (15).

Even though anthropology is grouped among the social sciences, it is not only humans that are observed in ethnography. Ethnography lends itself well to a theory of performativity. The social phenomena that an anthropologist is conventionally interested in are contexts in which an aspect of a culture is performed. These performances, as discussed above, happen through a variety of factors, some of which are material. By participating in performing the situation of interest, the anthropologist can observe the props, roles and the stage that are needed for the performance, and report about them; not regarding them as customary, ritual things but as things that perform the roles that make the situation work. Phrased in this way, it seems unfitting to name this discipline 'anthropology'. The method of study just outlined does not have to center around the 'anthropos', whoever that might be.

Annemarie Mol, in *The Body Multiple*, claims that diseases can be studied using the methods of ethnography (2002). With this she does not mean a study of the cultural meanings of a disease, but an ethnographic observation of the scientific practice around the disease. Conventionally, social scientists were confined to studying illnesses, the cultural meanings of diseases. But, Mol notes, this schism made philosophers and sociologists lose any power to discuss scientific categorization (9). This was an unwanted situation because this categorization is subject to, and of, a lot of power, and it is not unreasonable to say that such power should be available to scrutiny by critical scholars. However, it is not the case that Mol aimed to study disease separately from illness. She didn't just aim to engage with medical science, but with how knowledge about the body is constituted in practices that also carry cultural meanings, such as how patients feel about their illness.

Ethnography is a good method to do this, because it allows a researcher to gain access to situations in which diseases and illnesses are performed together. In practical situations there is not an immediate divide between scientific objects and their cultural meanings. Patients have diseases (science) and have to deal with them (society) in the same hospital bed. When studying practices, a researcher can critically assess how scientific categories are constituted and how they relate to cultural categories. The open-minded, critically self-reflexive method of anthropology transforms into "praxiography", a study of practices (Mol 2002, 31).

Praxiography is a combination of various methods that focus on practices. Ethnography, or participant observation, as discussed, is a method that focuses on the performance of phenomena in their contexts. It is well-suited to discuss practicalities because of the participation of the observer in situated contexts. If the phenomena under consideration are scientific, a similar focus on the practicalities of a scientific object can be gained by studying the methods section of scientific articles (Mol 2002, 158). If scientists play by their own rules, these should contain all the relevant practicalities that played a role in the scientific investigation. A last resource for praxiography is to enlist informants as auto-ethnographers (Heuts and Mol 2013, 128). By asking the right questions in interviews it is possible that the interviewees recall the practicalities of the situations they describe. It is important here to device specific questions, like 'what should I know if I were to take over your job tomorrow?', that center practices, instead of asking people for their opinions. This gives a researcher access to situations that they couldn't attend to themselves.

2. Superconductivity in practices

This chapter is divided into a main- and subtext. They can be read separately, after each other, or through each other, using the footnote numbers to switch between texts. It is up to you to decide! [9]

In the main text of this chapter, I consider the case of a Dutch university-laboratory at which quantum-electronic properties of materials are investigated. I have followed a research group at this laboratory that researches electronic phenomena, such as superconductivity, of novel materials at extreme conditions. Let us call them 'group S'. Group S allowed me to conduct an experiment into a superconducting material myself. They showed me how to investigate the superconducting phase of the material beta-palladium-bismuth-two (β-$PdBi_2$). In this chapter, I describe what superconductivity is by considering what I observed during my research at group S's laboratory. My aim in writing this description is to include the ways in which facts about superconductivity are *produced* into the description of what it *is*. To do this, I cannot simply relay facts from physical theory to you; I want to analyze how physicists get to make claims about physical reality, and not simply repeat what physicists have said. Therefore, I will describe to you what they do, or what they say that they do. You will see that, in practice, the question of what superconductivity is, is not clear and not singular.[10]

Science as practice

[9] In this subtext, I relate to the literature relevant to the discussion in the main text. I weave these literatures together into a genealogy of my theoretical framework. The story starts in the 1980s, when many meta-scholars of science, i.e. historians, philosophers, and sociologists of science, shifted their attention from historical and macrosocial analyses to the study of scientific practice (Pickering 1992, 2). By this time, these meta-scholars had already taken a different route than the philosophers that preceded them by incorporating (macro)social elements into their descriptions of science (See Golinski 2005 for an overview of the origins of constructivist approaches in science studies). Key publications that preluded the study of scientific practice were Bruno Latour and Steve Woolgar's *Laboratory Life* (1979) and Karin Knorr Cetina's *The Manufacture of Knowledge* (1981). Both publications brought anthropological methods to the study of science. One of the effects of studying science as an unknown culture was that the constructivist stance in science studies could move away from a social determinist vantage point. Studying scientific practice highlights its local, contextual nature, which escapes universalist explanations; both 'natural' and 'social' ones.

[10] In addition, the turn to practice has made seeing scientific knowledge as a coherent, stable network of concepts untenable. In practice, a scientific culture is too complex to be well-described by singular conceptual networks (Pickering 1992, 5). Although untenable in such an all-encompassing form, analyses of scientific knowledge as conceptual networks have the advantage that they can provide a theory of the underdetermination of scientific knowledge. Think of Willard Quine's 'web of beliefs', in which all propositions in a network of beliefs about the world can be changed if the auxiliary beliefs are changed such that the network does not fall apart (Quine

What is a superconductor?

Before I started my experiment at group S, the group leader (I will call him Alex here) had sent me a chapter from a physics textbook, as well as a paper on β-$PdBi_2$, so that I could study the theory before I started my experiment. In its summary, the chapter claims that "superconductors are materials that have zero dc resistance below a certain temperature T_c, called the critical temperature" (Serway 1997, 514). This description gives an experimental answer to the question 'what is a superconductor'. According to conventional scientific intuition, it should be possible to verify this claim in a laboratory by doing experiments. At group S, Alex helped me to carry out such an experiment on β-$PdBi_2$. Using a commercial apparatus, the material had been cooled down from approximately 100 Kelvin to 4 Kelvin while measuring its electrical resistance. The apparatus had logged these resistance measurements, as well as the temperature of the sample, on a graph on a computer screen. This was one of my first encounters with superconductivity in the laboratory:[11]

1951). Underdetermination-theories allow us to move away from explaining the development of scientific knowledge as a teleological progression towards theories that are increasingly closer to the truth, such as outlined in Ernest Nagel's *The Structure of Science* (1961; quoted in Hacking 1992).

Quine, and, later, Thomas Kuhn (1962), managed to replace such a teleological view of the production of knowledge with one in which science is an internally consistent set of beliefs. Kuhn, and especially his followers, attributed the closing of disputes within such a web, or between incommensurable webs (paradigms), to social values such as accuracy and consistency. The Kuhnians' analysis presumes two things: First, that the closing of controversies happens through discussion in the scientific community and, second, that, when controversies are closed, scientists revert to a coherent 'normal science', without open controversies. The second presumption implies that the day-to-day business of scientists is a coherent enterprise. This is not a surprising standpoint for a historian like Kuhn. Historians study records. When people record their knowledge, they usually present it in a coherent manner. Later in this chapter, I will question whether the day-to-day functioning of science is coherent at all.

The first presumption, i.e. that controversies are managed and closed in the social/theoretical realm, has been considered and extended by Ian Hacking: Hacking proposes to see scientific knowledge in a way that is similar to Quine's web of beliefs, except that he adds scientific apparatuses and analyses to this web (1992). Hacking's thesis is that "as a laboratory science [i.e. a science that isolates phenomena and interferes with them under controlled circumstances] matures, it develops a body of types of theory and types of apparatus and types of analysis that are mutually adjusted to each other" (30). Hacking adds material factors to Quine's web of beliefs, implying that they are adjusted to theoretical factors and to each other in the same way as beliefs might be adjusted to each other.

[11] Hacking categorizes the elements that are adjusted to each other in his extended Duhem-Quine thesis into three groups: ideas, things, and marks. Elements that belong to these groups, such as questions, samples, and graphs are adjusted to each other in experiments. This shifts the analysis of scientific knowledge from a set of ideas that hangs together in the minds of scientists to something that is done in

*When I arrived at the lab the next day (the measurement system had been executing my measurement sequence overnight), I saw a sharp decline in the graph around a temperature of five Kelvin. Alex arrived slightly later and looked at the results. **He said that there was a reasonably sharp drop in resistance.***

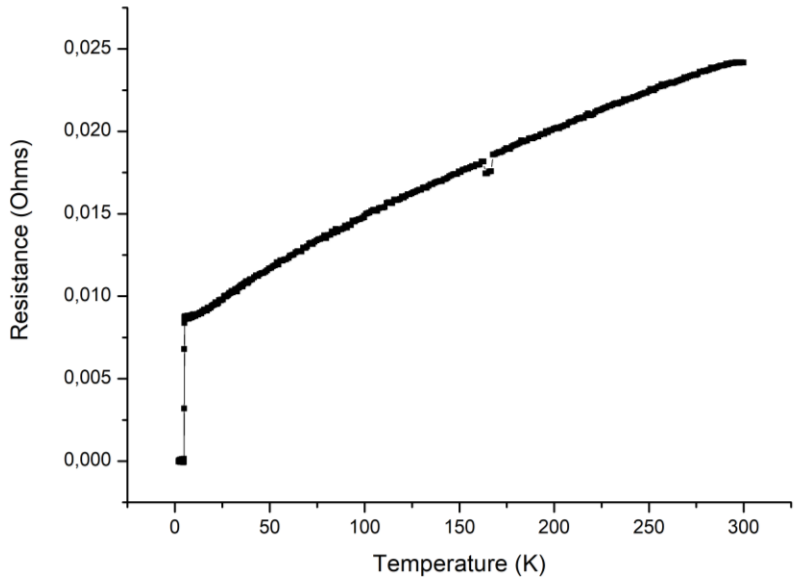

Figure 2: Resistance of β-$PdBi_2$ versus temperature. The almost vertical line on the left is the "sharp drop" mentioned above.

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laboratories and other scientific institutions. It makes scientific knowledge something that is present in scientific practices. According to Hacking, experimentation in the laboratory sciences is conducted in such a way that these sciences are "self-vindicating": Their elements adjust to each other in such a way that they are stable sets of practices.

Importantly, this way of viewing science emphasizes that the stability of science is a product of both theoretical and experimental/material factors. Hacking's extended web of beliefs, I could call it a 'web of practices', is not merely stabilized by adjusting theories, but by adjusting experiments, ways of recording data, and ways of coordinating between laboratories as well. Furthermore, if theory and experiment adjust to each other to stabilize a science, they *co-produce* each other as the science develops or "matures" (1992, 31). Viewed in this way, theories or networks of concepts do not capture the 'natural' world increasingly well, but the phenomena that are produced in the laboratory and the theories that make sense of them 'grow up' together, each accountable to the other.

Crudely, the genealogy of science studies I have given so far takes us from naïve realism, i.e. regarding scientific facts as the truth, to different forms of social constructivism, in which what scientists *say* determines what constitutes a scientific fact, to models of co-production in which what scientists *do* coordinates how scientific knowledge and the phenomena and apparatuses that this knowledge is accountable to develop in a mutually dependent way. Such co-production is crucially different from a social constructivist analysis of science:

The "sharp drop in resistance" happens at a point that is called the critical temperature, T_c. Below this temperature, superconductors have no resistance to electrical currents. This story is very much in line with the definition in the textbook that Alex had sent me. However, a story from one day earlier complicates things:

> *I got to the lab at 9:15. I looked at what the measurement system had done during the evening. First, I thought I saw good results as I saw a very sharp decline of the resistance in the graph. However, the decline took place at T=80K. This is way too high for the material I was researching. Alex had already been in the lab, because he had already written down what had happened in the log book. It said: "V- contact lost. R properly measured until 80K, then R-->0 because voltage contact lost."*

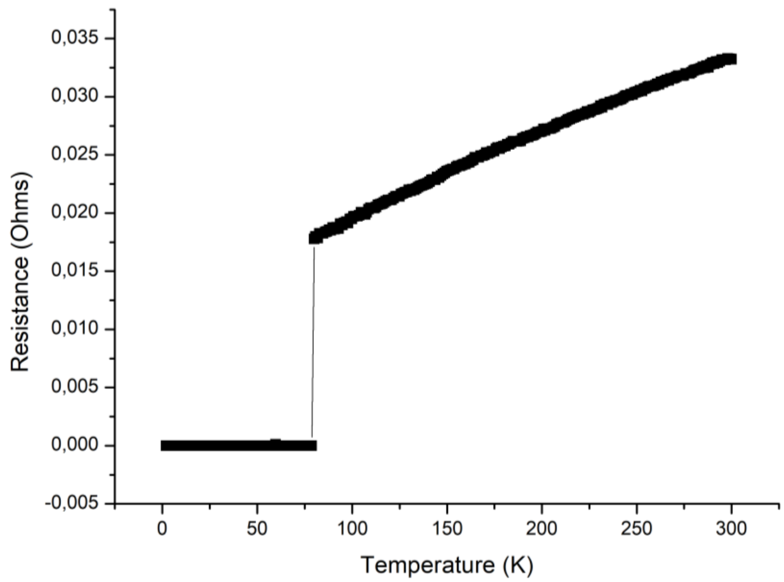

Figure 3: Resistance of β-$PdBi_2$ versus temperature. The vertical line indicates the temperature at which the V- contact was lost.

In this case the "sharp drop" is not a sign of superconductivity, but a sign that the experiment failed. I knew that the impressively sharp decline in resistance in figure 3 was not a sign of superconductivity because of my expectations. The authors of the paper that Alex had sent me reported that they had found a T_c of 5.4K for their sample of β-$PdBi_2$ (Imai et al. 2012). Next to this expectation, there was Alex who said that the contact point was lost, indicating a failure of the experiment. It turns out that we must add the agreement of a supervisor, or, alternatively, the agreement with theoretical expectations, to the textbook definition: Superconductors are materials that exhibit a complete loss of electrical resistance below their critical temperature *when your supervisor agrees that nothing went wrong.*[12] Of course, this definition is too thoughtless to be kept; not because expectations and agreement

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[12] A strawman social constructivist would say that how scientific facts are made is unidirectionally influenced, or even determined, by social interest (Law 2017, 34). Co-production does not nullify the insight that science is shaped by social authority; it adds that such authority can be constituted by scientific knowledge and methods.

in the scientific community have nothing to do with what superconductivity is, but because the social element in research depends on material and technical elements as well. Alex did not make up an opinion about the V- contact being broken but tested this with an extra piece of apparatus. However, incorporating the two stories so bluntly immediately reveals that there are many conditions that definitions depend upon in practice. It would be impossible to find a precise definition of superconductivity that works perfectly in every possible experiment. Instead, definitions rely on the situations they are used in.[13]

What remains is a "flat ontology" that does not prioritize any group of elements to explain another (Law 2019, 4). An important question is whether it remains possible to speak of difference in such a framework.

Situated knowledges

[13] A commitment to steering clear from categorical differences does not imply that we have to do away with all differences. In fact, it can make things infinitely more complex because types or classes fall away as explanatory structures. For example, it would not be enough to say that scientific knowledge is decided on by humans because they have access to some sort of 'conceptual realm' or 'space of reasons'. If we do not prioritize the category 'human' as having any special properties by default, we must start to ask *how* and *where* decisions are made, and *which* humans (and which nonhumans) can participate in such processes. Moreover, the turn towards a flat ontology should not be understood as an attempt to construct an overview from some kind of "third-person perspective" that sees everything in the analysis 'from above' (see criticism in Rosenberger and Verbeek 2015, 20). Instead, it should be understood as a move towards specificities that includes the position of the one making claims about the subject matter. Such an analysis concedes that an observation or statement always comes from a specific circumstance instead of being disembodied and dis-attached.

Using a flat ontology in this way means acknowledging that observations and statements are always situated and partial. Donna Haraway notes that partial perspectives, and not relativism, can annul naïve realism: "Relativism is a way of being nowhere while claiming to be everywhere equally" (Haraway 1988, 584). It stands close to realism in that it is not accountable to its own position. It claims that an object can be viewed from any angle without consequence, instead of analyzing how its own vantage point influences the way it sees. The only way to follow through with the move towards a 'web of practices' or a 'flat ontology' without falling into traps of naïve realism, or similarly, relativism, that we wanted to escape is to recognize that our knowledge is situated.

A 'web of practices' in a Harawayian sense should be measured up to her concept of "webs of connections". Iris van der Tuin identifies three ways in which Haraway uses this concept (2019, 11): Systematic webs that provide the historically shaped dimensions of reality, webs that are partial and sensitive to oppressions arising from history, and webs that articulate differences between positions. Observing these three uses, our 'web of practices' is simultaneously an analysis of how the scientific practices that we analyze came to be like they are, and sensitive to our own political and historical positioning in this web.

Different definitions in different situations

If you go to a laboratory to ask physicists what a superconductor is, most of them will give you a definition that they have learned during their education. This can be an experimental definition like the one about superconductors exhibiting a complete loss of resistance. But if pressed, physicists will tell you a theoretical definition about Cooper pairs. In this theoretical story, resistance to an electrical current is explained by saying that the electrons that carry the current in a material are slowed down because of many forms of scattering. You could say they bump into each other and into the atoms that shape the material, which slows them down. Under appropriate conditions, such as very low temperatures, however, electrons pair up and form 'Cooper pairs'. When they do this, all these pairs move collectively. This means that to slow down just one of them, you have to disturb all of them. Disturbing all the pairs requires more energy than disturbing just a single electron. Consequently, if the disturbances, such as thermal fluctuations, are not big enough to disturb all the pairs simultaneously, the electrons will move without resistance (Serway 1997, 501). Cooper pairs are what defines a superconductor in theory.[14]

Now, because the researchers are in a laboratory, they will probably not only tell you the theoretical definition of superconductivity. They will come with a more applied definition that relates to the experimental setup around them.[15] In the case of group S, the researchers define superconductivity as

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[14] How can we account for the fact that science is not presented to us in a partial, but in a coherent and stable way? First, this has to do with the way in which we investigate science. We must be critical of the modes in which we conduct our investigations and what effect this has on the answers we get. If we look for singular, coherent answers, chances are that we will find them somewhere. In addition, the coherent presentation of science originates in how the education of our objects of study, scientists, has been carried out. Hacking notes that "before even entering the research laboratory, the student, like it or not, finds that many mature sciences are pedagogically stable. […] [Science is taught] as if it were dead. In a way that is right" (1992, 39).

Karin Knorr Cetina adds to this analysis that the subjectivity of the scientist in a laboratory is as constructed as the objects of investigation. Their 'social' position is as malleable as the 'natural' phenomena that are researched (1992, 119). The formation of the scientific subject is part of the web of practices that assures the self-vindicating nature of the laboratory sciences. Scientific education is carried out in such a way that students are 'workable tools' in relation to scientific phenomena. So, if we observe that a laboratory science seems stable, this is partly attributable to that its foundation has been presented as stable in teaching.

[15] At the same time, laboratory scientists need to be able to deal with uncertainties and incoherent results. In *Science in Action* (1987), Bruno Latour identifies that scientists have at least two modes in which they operate: The mode of knowing the 'truth' and the mode of constructing that same truth. The process that marks the passage of newly constructed knowledge to established fact, he called the process of "black-boxing". Applying Haraway's mandate of privileging partial perspectives to Latour's work, it is clear that the way in which we investigate scientists, and what our own education has been, affects which side of the process of black-boxing we get to see.

a loss of resistance at suitably low temperatures and magnetic fields. This relates to another feature of superconductors: They do not only have a critical temperature, but also a critical external magnetic field, H_{c2}. If an external magnetic field exceeds H_{c2}, a superconducting material is a conventional conductor. To explain definitions like this one and how these relate to their own research, physicists need a piece of paper, or, preferably, a whiteboard:

> *Alex took me to the whiteboard again and started drawing graphs of the critical magnetic field, H_{c2}, versus temperature. He explained that there are different curves for the different directions of the sample: the ab-direction and the c-direction are the highest and lowest, respectively. He then drew the angle-dependence of the resistance and the magnetic field. Using the first drawing, he explained that you can keep the temperature constant and measure the resistance over an increasing magnetic field, for different temperatures, to obtain the H_{c2}-T curve; or you can keep the magnetic field constant, and change the temperature while you measure the resistance, for different fields. We would do the latter.*

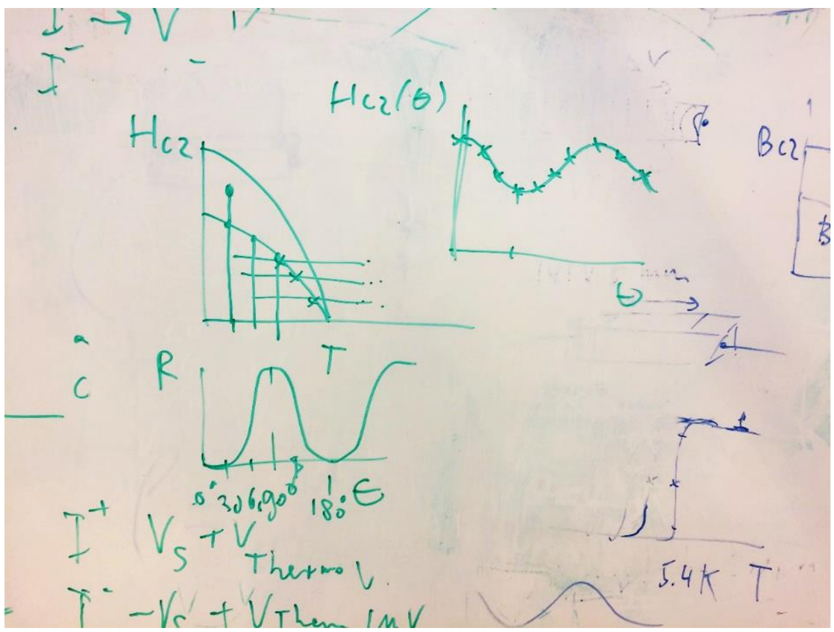

Figure 4: Sketches of graphs on a white board at the laboratory of group S. The sketch on the top left shows a general phase diagram for a type-II superconductor. The area underneath the upper curve characterizes the superconducting phases.

This vignette points the way to another way of defining superconductors that is related to a set of measurements. Alex's upper left sketch on the whiteboard is a visual representation of this definition

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Up to this point I have traced a path from seeing knowledge as something in the realm of human reason to something present in practices composed of heterogeneous elements; from universal to situated and partial. Here, my discussion of the Kuhnians' first presumption, that disputes are resolved by social values, links to their second presumption, normal science's coherence and stability. If using a flat ontology in which experiment and theory are co-produced requires us to see knowledge as partial, then what is it a part of?

(figure 4). The representation defines a superconductor as a material that has a superconducting phase below certain combinations of magnetic fields and temperatures. In the vignette above, Alex was explaining to me which measurements I could do to determine a collection of points from which a boundary between the normal and the superconducting phase of β-$PdBi_2$ could be constructed. This boundary is the upper curve in the sketch. Each point underneath the curve indicates a combination of a magnetic field and temperature at which the material is in the superconducting phase. Each point above the curve is a combination at which the material is in the normal phase.

This definition of phases fits very nicely with the definition of superconductors being materials that show a sudden drop of resistance below a critical temperature.[16] Namely, when putting the material in a fixed magnetic field, it is possible to measure the resistance in the material while changing its temperature. Then, if there is a sudden drop in resistance when cooling down, such as in figure 2, you know that the combination of the strength of the magnetic field and the temperature at which you saw the drop in resistance is one of the points on the boundary that separates the superconducting phase from the normal phase. You can then repeat the same experiment with a different magnetic field strength and find other points on the boundary. The phase-definition also fits very well with the definition of materials being superconducting if the external magnetic field is lower than the critical magnetic field. Namely, while keeping the temperature of the material constant below T_c, you can slowly increase the magnetic field while measuring the resistance over the material. Then, when you see a sharp increase in resistance at a certain magnetic field strength, you know that the combination of that magnetic field strength and the temperature of the sample is another point on the same boundary that defines the superconducting phase. The combination of measurements of critical temperature and critical magnetic field form a coherent two-dimensional definition of superconductivity as a drop in resistance: the phase-definition.

We can also connect this phase-definition to the theoretical definition by saying that thermal energy and magnetic fields can be disturbances that break up the Cooper pairs into single electrons, after which the material is a conventional conductor. The energy that is needed to break apart the Cooper pairs in a superconductor is called the energy gap. Another team of researchers at the laboratory, Group A, does experiments that measure this energy gap. The technique they use is called 'angle-resolved photoemission spectroscopy' (ARPES). Group A's experiments are located on the opposite side of the laboratory from group S's. They have a machine that 'shoots' light onto a sample in such a way that an electron 'jumps' out

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[16] A first instinct might be to say that a situated perspective is a part of the 'whole'. Even if I concede that I can only witness phenomena from my own perspective, maybe, if I join my perspective with that of many others, we can, together, construct a complete picture of what the world is like. This is, very roughly, what Sandra Harding outlines in *The Science Question in Feminism* (1986). Harding notes that science is often androcentric by virtue of the male standpoint being regarded as a standard or neutral position. In this way, a partial perspective is taken as a complete description, which erases the perspectives of oppressed positions on the same issues. What if we add research from female, or other subjugated perspectives to androcentric science? We would hope that this would result in a better representation of reality for various subject positions. Maybe, if every possible subject position would be considered, it would even result in a complete picture of the reality of that issue. I will call this 'addition'.

of the sample onto a detector plate. From the angle at which the electron jumps out of the material and the energy of the light, the researchers in group A can determine how big the material's energy gap is. While doing this, they can control the temperature of their sample. When the temperature is above the critical temperature of the sample, there is no energy gap. But when the sample becomes cool enough, an energy gap starts to appear in their graphs. When using ARPES, a superconductor is defined as a material in which an energy gap is measured when the material is cooled below its critical temperature.

At group A's side of the lab, Bob and a fellow researcher were handling some new equipment worth 300,000 euros. Bob had agreed to show me how ARPES works and how it relates to superconductivity. As soon as I had arrived, he took me to his desk to show me figures in papers and presentations on his laptop. After explaining how electrons hop around in crystal lattices and that they like hopping less or more in different crystal structures, he took me to group A's experimental setup. He took out a small device that indicated the strength of magnetic fields around us and pointed out that the setup in front of us shielded the sample from the earth's magnetic field. If there would be a magnetic field in the chamber, it would affect the trajectories of the electrons and the spectrum that comes out would be smeared out.

Even though both the resistivity-definition and the ARPES-definition fit with the theoretical definition of superconductors, they don't fit with each other experimentally. It is impossible to measure the energy gap of a superconductor using ARPES while measuring a sharp decline in the resistance of the sample at the same time. Measuring the resistance of the sample requires running a current through the sample, which would produce a magnetic field. This is incompatible with ARPES, because the magnetic field produced by the current would disturb the angle at which electrons exit the sample. In theory, measuring the energy gap and measuring a loss of resistance are two instantiations of the same phenomenon: The formation of Cooper pairs in the material. In practice, however, two mutually exclusive experimental setups are needed to measure these instantiations.[17]

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[17] If we want to add all perspectives on an issue together, a first step is to find out which perspectives are out there. If we take the (somewhat too big) case of adding a female perspective to normative science, we might ask what the female perspective is. Is there one? Are there many? Annemarie Mol helps us to complicate this question even more by asking "Who knows what a woman is" ([1985] 2015). She notes that various scientific disciplines have different ways of answering this question. Genetics says that a woman is a person with two XX chromosomes; psychoanalysis claims that a woman is someone with certain complexes and identifications; and endocrinology defines a woman as a body with a certain mix of hormones. The definitions of these and other disciplines, and their sub-disciplines, can be analogous to each other and mark the same sets of objects, but very often they do not, which results in controversies. What about people who are categorized as male by the makeup of their hormones and are categorized as female by their anatomy?

The point is that there are clashes between different ways of knowing. How can we add all perspectives together if there is no agreement on which perspectives are there? What makes this more complicated is that it is not a question about how many

ARPES is typically not used as a technique to investigate superconductors that have a critical temperature below 10 Kelvin. For temperatures with a lower T_c, scanning tunneling spectroscopy (STS) is a more suitable technique to investigate the energy gap. STS and ARPES are another example of how a pair of research techniques can complement each other. Both techniques measure the spectra of the electronic band structure of a material. The energy gap can be seen in such spectra. The difference between the techniques is that ARPES measures these spectra for a defined momentum range, while STS measures spectra in a defined area. ARPES 'looks' at the whole area of the sample at once: It functions by shooting light onto a sample with a specific frequency that corresponds to a certain momentum of the electrons. This light interacts with all the electrons of that momentum on the whole surface that the light reaches. Conversely, STS works by hovering a tip above a sample and measuring the tunneling current between the tip and the point on the sample underneath it. This means that STS investigates a localized area on the sample, but that it can interact with electrons with a large range of possible momenta in that area. STS and ARPES both measure the energy gap but do so by averaging over different physical quantities.[18]

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perspectives there are, but about where the boundaries of categories lie. When the sciences say something about who is a woman, they offer different ways of answering this question while all addressing the same category: woman. These different perspectives are not separate: "'Splitting' in this context should be about heterogeneous multiplicities that are simultaneously salient and incapable of being squashed into isomorphic slots or cumulative lists" (Haraway 1988, 586). A phrase in Marilyn Strathern's *Partial Connections* might help to understand this structure: "One is too few but two are too many" (1991, 35). We can't speak of a single female perspective, nor of discretely different ones. Following Mol, I will call this 'multiplicity' (2002).

Entanglements and isolation

When working with a flat ontology, a web of practices, we see knowledge as situated by its context. Differently situated knowledges can sometimes be added up into a bigger whole, they can contradict, and they can also kind of fit together but not completely. This last option could be regarded as the general case with the two other options as its boundary conditions. This last option of relating, multiplicity, is characterized by partial connections. When situated knowledges are partially connected, this complicates the question whether they are the same or different, because the boundary between them is shifting (see Mol and Law 1994 and de Laet and Mol 2000). "Multiplication may, among other things, lead one out of this binary opposition that is also an entanglement: the *self* versus the *other*" (Mol 2002, 135).

Karen Barad connects the analysis of difference pioneered by Haraway to entanglements in physical theory: "The fundamental discontinuity of quantum physics disrupts the nature of difference: the relationship between continuity and discontinuity is not one of radical exteriority but rather of agential separability, each being threaded through with the other. 'Otherness' is an entangled relation of difference" (Barad 2007, 236). Barad's analysis relies heavily on Niels Bohr's philosophy of quantum mechanics, which revolves around the concept of 'complementarity'. According to Barad, ARPES and STS have in common that they probe the surface of a sample, whether that is a localized area on that surface or not. Because of this, ARPES and STS are limited to defining surface states. This implies that both techniques do not only use a specific physical quantity, the energy gap, to answer *what* superconductivity is, but also that they are accountable to a specific location *where* superconductivity takes place.[19] If researchers want to get to know something about the bulk of a superconductor, they

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complementarity can be understood in an ontic way, even though Bohr himself used the concept to indicate the epistemological difficulties of the inseparability of the knower from the known (2007, 127). She notes that, if we accept that knower and known cannot be separated, that knowledge is always situated, it does not make a lot of sense to speak about epistemology and ontology separately (185).

[18] In her 'ontoepistemological' framework, Barad understands Bohr's complementarity as indicating mutually exclusive ways to 'cut up' phenomena into known and knower, measured and measurement, cause and effect. The "agential cut" cuts up a phenomenon into knower and known in a way that depends on the specificities of the apparatus used to produce knowledge (2007, 148). This is a rephrasing of the insight that the way in which we conduct our investigations influences the answers we obtain. In this way, different agential cuts result in differently situated knowledges. In physics, these situated knowledges can be complementary: The description of one physical quantity, say the position of a particle, can require an experimental arrangement that is incompatible with the experimental arrangement needed to measure another quantity, in this case the momentum of that particle. The experimental arrangements and the measured properties are 'mutually exclusive'.

The agential cuts that differentiate observer from observed result from "intra-actions" in Barad's terminology (2003, 815). This neologism implies that intra-acting components are separated from each other 'inside' a bigger ontological unit: inside a 'phenomenon', which is the smallest unit in a relational ontology, i.e. a "relational atom" (2007, 151). This is crucially different from *inter*acting components, because it indicates that the components were not separate to begin with. Intra-actions question the notions of sameness and difference. Barad notes, following Judith Butler, that defining an object through exclusions and boundaries can only be done by virtue of what is excluded (2007, 64). What is normal is defined by what is weird, which means that the normal needs the weird in order to be itself. According to Barad, the differences between opposing pairs like knower and known, social and scientific, and male and female are constituted through such intra-actions (2003, 817). These opposing pairs need each other to exist; they are entangled. This relation of complementarity "entails *two* important features: mutual exclusivity *and* mutual necessity" (2011, 444). It is an "exteriority within" (2003, 825).

[19] Different ways of drawing boundaries between known and knower produces different partial perspectives on *what* the known is, but also on *where* the known is, and for *whom* that matters. Partiality, as explained before, indicates that observers have a standpoint and therefore no complete picture of the observed. What is more is that partiality implies taking sides, as Baukje Prins emphasizes: "Moreover, the possibility of a constant (re)drawing of boundaries has very real, material, and often unexpected effects. Precisely because practices of knowledge constantly generate objects, issues, and experiences that are very real, they cannot be perceived as

either must assume that the surface states of the sample are the same as the bulk states, or they have to use another technique, such as muon spin spectroscopy (µSR):

> *Charlotte showed me a picture of herself standing next to a large apparatus between concrete walls. Group S is interested in superconductivity in a certain material, and Charlotte's project is to investigate this material using µSR. She showed me a slide of a presentation she gave some months earlier with a schematic picture of the µSR-apparatus. A muon beam would pass a muon counter, and then go through a hole in a detector, and reach the sample that was placed between that detector and a detector on the opposite side. The muon enters the sample, and then has an interaction with the magnetic fields in the sample. Depending on that interaction, the muon exits the sample and is detected by either one of the detectors. This is repeated many times, after which Charlotte determines the asymmetry between the measurements in the two detectors. From this, she can characterize the sample. The setup allows the temperature to be controlled such that the sample has a temperature below T_c. Charlotte explained that, using STS measurements, other groups had claimed that there was a certain superconducting state in the material she was working on. But she had found a different state in the material using µSR. She explained this by noting that muons go into the bulk of the material, while STS measurements only have access to the surface.*

Charlotte can infer that the sample in the apparatus is superconducting by looking at the asymmetry of the results. Moreover, the asymmetry measures can be used to characterize values of the critical magnetic field, the current density, the magnetization vector, and sometimes the penetration depth of the magnetic field. However, the resistance of the sample cannot be measured while using µSR. According to Charlotte, this would be very impractical. There would not be any space to fit the wires onto the samples. Furthermore, it would disturb the µSR results by producing a magnetic field. Likewise, it would not be possible to combine µSR with ARPES, because of the magnetic fields of the two beams of the two experiments interfering. And finally, it would simply be too expensive. Even if it would be possible, it would not be worth it because, according to Charlotte, "you just want to measure one physical property, because you do not know how they interact".[20]

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innocent. They have both ethical and political implications" (1995, 356). If knowledge is situated, entangled, or what have you, it means that it is suspended in a web of issues, some of them ethical. This is so, even if abstract sciences claim impartiality.

[20] Why is it that the vision of science as impartial and isolated has such prominence? In their history of objectivity as an "epistemic virtue", Lorraine Daston and Peter Galison identify that, in the mid-nineteenth century, scientists sought to remove themselves more and more from the process of making scientific representations (2010). This fits into a longer history of evaluating and reducing obstacles to attaining knowledge about the natural world. What arose in the mid-nineteenth century was a way of representing that Daston and Galison call "mechanical objectivity". The idea is that scientists use mechanical procedures to produce representations of reality,

So far, I have outlined what superconductors are in various experiments. When asking physicists what a superconductor is, they explain all kinds of things about processes that happen to, in or around a sample of some material. The samples they use in their experiments are not just found somewhere. They are made by yet another researcher in the laboratory:

> *Dennis showed me the furnaces that he uses to make the superconducting samples. He makes a cylinder by pressing powder of different materials together and then hangs this cylinder in an oven. The oven has parabolic mirrors with halogen infrared lamps around them. This setup heats the bottom of the cylinder with a maximum temperature of 2000 degrees. A holder is placed underneath the cylinder at a distance of a couple of millimeters. The cylinder then rotates and melts at the bottom. The molten material drips to the holder underneath it. As the cylinder melts more, the holder is moved so that the material can grow on it. The material then solidifies in a controlled way to make a superconducting crystal. Dennis can control all kinds of variables like pressure, temperature or oxygen levels to make the crystal that the experimenter wants.*

In this vignette, a superconductor is a crystal that is grown in a highly controlled way. Dennis makes the sample in accordance to the wishes of the experimenter, while taking into account the price of the materials and the amount of effort needed to produce a sample that is good enough for the planned experiment. Dennis kept emphasizing that making superconductors is all very simple (which Alex later denied). He just follows the right recipes. These recipes are connected to the theory of condensed matter physics. The theory tells researchers which crystals they want to experiment on, and it tells Dennis how to make the crystals. He also checks whether the crystals that he made have the specifications that they wanted them to have. He does this using powder X-ray diffraction. X-rays diffract from the sample in ways that are characteristic of certain symmetries and 'order parameters' of the material. If the crystal structure of the material that Dennis makes is known, he also knows which peaks to expect to see in the diffraction

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reducing the human factor as much as possible. Even though objectivity in this sense kept a certain prominence in epistemology up to the present day, Daston and Galison show that the notion of objectivity has simultaneously been evolving into new epistemic virtues that coexist with older ones.

From the time at which objectivity was synonymous with non-interference by subjects, mechanical objectivity evolved into "trained judgement". The latter is similar to 'addition', mentioned above, in that it tries to represent a complete picture of an object by showing it in many circumstances: In the early twentieth century, some scientists realized that "accuracy should not be sacrificed to objectivity", in the sense that idiosyncratic pictures of objects could differ too much between each other for one of them to represent the whole group accurately (2010, 321). To increase accuracy, they made atlases with many instances of the object, which the scientist had to learn to interpret correctly. Through knowing a variety of examples of the object of study, the scientist could identify any case that was supposed to belong to that class of objects.

pattern. Next to this, he can also check the chemical composition of the material. Alex pointed out that this is a crucial part of the experiments at group S. It is important to know for sure that you are doing measurements on the right material.[21]

The coordination of instantiations

In practice, we can find different definitions of superconductors in the laboratory in different situations. I have used the word *definition* to talk about superconductors in the situations described above because it is conventional to say that a definition is *used*. I have showed that physicists need to use different definitions in different situations to make their experiments work. However, the word *definition* can be misleading when we conceive of it as indicating that an observer or experimenter *defines* what something in front of them *is*. Namely, in this case all the power of defining what a superconductor is would lie with the experimenter, and this puts the question 'what is a superconductor' firmly in the realm of epistemology.[22] What if the experimenters disagree about their definitions? How do we then decide which one is true?

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[21] Daston and Galison find that the most recent development in epistemic virtues is the blurring of the boundary between images that represent reality and presentations of the (microscopic) creations of scientists. They suggest that throughout history, scientists have been combating limitations of accurate knowledge but that, today, we have "anxieties not about whether we have seized the real right but about whether we are instead making the right real" (2010, 415). Even more than was the case with previous epistemic virtues, making your scientific object ready for an image, or a measurement in general, is taking center stage. Making is knowing. This new knowledge-ideal decenters the importance of accurate *re*presentation in favor of successful presentation.

Daston and Galison note that each epistemic virtue has required a different scientific subjectivity to function correctly. In the nineteenth century scientists had to make themselves as standardized as possible, highly limiting influences from their subjectivity. Later, they had to educate themselves more widely and had to be able to say with confidence which representation presented the norm, and which did not. In the current era of presentation, Daston and Galison claim that usability of the scientific object is becoming more prominent, which means scientists are increasingly seeing themselves as entrepreneurs. In the last case it is most evident that science is entangled with social factors such as financial interest, but, recalling Barad's discussion of entanglements, I can claim that a limited subjectivity is highly entangled with an isolated scientific object as well: Exclusions matter as much as inclusions.

(Anti)realism, representationalism, instauration, instantiation

[22] Throughout this subtext, the question of scientific realism has been lurking in the background. What can we make of this issue in the framework I have outlined here? Classically, the question of realism is about whether we should regard scientific theories as describing reality correctly or not. In short, the question is whether you think that not directly perceivable entities, such as quarks, and/or the theories that

Alternatively, I could use the word *essence*. Using this word implies that there is some inherency about the description I have given. For example, I could say that the essence of a superconductor is that it has zero resistance below T_c. This has two problems: It conflicts with the other stories I have told about superconductors in practice because an essence is usually taken as something singular; and it could imply that my description refers to an essence 'out there' in nature.[23] Although this would provide a clear picture of how we conceive of what the stuff of our reality, our ontology, is, it would obscure the ways in which we can get access to this reality 'out there'.

In contrast to what both the word *definition* and the word *essence* imply, the descriptions I have presented about superconductors in practice are not singular and not easily attributable to either natural or cultural causes and actors. There are roles for graphs, interpretations of graphs, commercial apparatuses, samples, experimenters and supervisors, and money, among many more causes that define a situation. What a superconductor is in the described situations, is neither defined by experimenters alone, nor is it just 'out there', present in the superconductors themselves. Instead of continuing to write about definitions, I will start using the word *instantiation* to capture these nuances.[24]

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describe them, are actually existing things in 'nature', or whether you think believing in the reality of our current scientific theories is naïve because scientific theories have been proven to be false over and over again throughout history (See chapter 9 in Curd and Cover 2013 and Chakravartty 2017).

[23] Underlying the whole discussion is the assumption that science represents a reality 'out there'. Realists think it represents it successfully, and antirealists do not. But in the light of the discussion of webs of practices, flat ontologies, and intra-actions this belief in scientific cultures representing an independent 'nature' seems to be an ill-founded assumption. In these frameworks, we try to see knowledge as something that is to be found in practices, not something that represents something less or more accurately. These practices consist of heterogeneous elements such as whiteboards, muons, β-$PdBi_2$ and policies. Seen in this way, knowledge does not describe something 'out there' but is 'out there' itself. Or, more accurately, such a notion of absolute exteriority to human culture and reason becomes questionable.

It is very hard to clearly delineate the boundary between knowledge and matter in practice. For example, if we look at how google maps traces out a route for you to get from one place to the other by car, we might think that the google servers independently calculate the fastest route to your destination according to the street plan. However, google also calculates traffic delays and can redirect you to find a faster route in case of traffic jams. By sending you and your car along this new route, it affects traffic flows, thereby intervening in what it was representing. We see in this case that knowing the world affects how the world is. Moreover, in this example the separability of matter from meaning cannot be taken for granted. Namely, the physical layout of roads (matter) allows for certain routes to be chosen (meaning) and, conversely, the roads have been built according to a design.

[24] Luckily there are alternatives to this 'representationalism'. There is 'construction': In *Laboratory Life*, Latour and Woolgar describe that science does not discover a 'nature' that has always been the way it we see it now but that scientists gradually construct it (1979). Interestingly, in the second edition of the book, the

authors displayed the subtitle, *The Social Construction of Scientific Facts*, without the word 'social'. There is also 'performance': It might be surprising to use this term if we want to avoid representationalism, because the term is associated with (re)presenting something on a stage. Even so, Judith Butler used this term in *Gender Trouble* (1990) and *Bodies that Matter* (1993) to sidestep fixed identities (see section 'How to Read' on page 3). Yet another option is 'enactment': If you, like Annemarie Mol, do not like how the term performance "not only resonate[s] the stage but also success after difficult work", you can speak of your object of study, or yourself, as being 'enacted' in practice (2002, 41).

An option that I recently encountered is 'instauration'. This concept, used by Étienne Souriau in the mid-twentieth century, is close to construction: "In a general way we can say that to know what a being is you have to instaure it, construct it even, either directly (happy are those, in this respect, who *make things*), or indirectly, through representation, up to the moment when, lifted to the highest point of its real presence and entirely determined for what it thus becomes, it is manifested in its entire accomplishment, in its own truth" (Souriau 1938, 25; quoted in Stengers and Latour 2015, 20). The quote subsumes representationalism under constructivism in the sense that a representation is also a construction. The difference between instauration and constructivism, according to Isabelle Stengers and Bruno Latour, is that constructivism implies that someone is responsible for a being's creation, while saying that something is instaured implies that the creator was responsible in a different sense: a creator that instaures a being has been able to respond to the conditions that enabled the realization of the being that had to be constructed. The creator was response-able (Stengers and Latour 2015, 21).

To say something is constructed does not capture the process of discovery that art and science are. It is a word that sounds technical. Instauration brings back some of the magic of discovery in a way that does not imply that 'the discovered' was already there. To say a scientific entity is instaured would be comparable to saying that a sculptor 'discovers' a statue in a block of marble. The statue was not just there inside the material, but the sculptor did also not precisely know what the exact outlines of the sculpture were going to be, because this depends on what the material allows. Sculpting is a process of creative discovery.

Leonard Lawlor observes that instauration comes from the Latin "instaurare" which translates as "renovation", "recommencement", "restoration", or "resumption" (2011). He claims that instauration can be contrasted to restoration, in that they both indicate repetition; but whereas a restoration repeats what was already there, an instauration, for the first time, successfully establishes something that "had not been able to be" before (404). Instauration is done over and over again, each time realizing different beings that can co-exist and/or superpose.

Instauration, then, fits well with Karen Barad's 'agential realism', because it is a non-essentialist concept that, unlike 'enactment', also suggests reiteration. According to Barad, "reality is an ongoing dynamic of intra-activity" (2007, 206). I have stressed the importance of history for how entities can be constructed, performed, enacted or instaured. History is a word that sums up the contingencies that enable and limit how things can be in the present. This is why Barad writes about "(re)iterative intra-actions" often: She wants to attend to the repetitive nature of reality, just

What can we make of the fact that I found different instantiations of superconductors in practice? The first thing to notice is that superconductivity is differently instantiated in different situations. This means that superconductivity is not a singular phenomenon in practice. But neither is it a pluriform phenomenon. Differently instantiated superconductors are not discretely different from each other but are connected: Superconducting samples are used in experiments such as ARPES and µSR. And these experiments set requirements for making superconducting samples. Furthermore, characterizations of physical properties in one experiment can be used to narrow down what values to look for in another experiment. And when papers get published about new superconducting materials, they usually use many techniques to characterize one sample. It would be a big mistake to say that there are different, separate *versions* of superconductors because the different instantiations are clearly related.

It is tempting to claim that that all instantiations together form a coherent picture of what a superconductor is. The different situations would then highlight different sides of the unified picture of what superconductors are. However, I have indicated that, in practice, the resistance-instantiation does not fit with the ARPES- and µSR-instantiation of a superconductor, and that the ARPES- and µSR-instantiations can also not be realized simultaneously. They cannot be unified in one instantiation because they rely on mutually exclusive experimental setups. The instantiations of superconductivity are not unifiable, nor separable. Instead of being singular or pluriform, a superconductor is a *multiple* phenomenon. Its structure is one in which its various instantiations are coordinated with each other, but not into a coherent whole.[25]

In this praxiography of superconductivity, I have shown various types of coordination: There is 'mutual inclusion', such as making samples for research and knowing the specifications of the material to be made by having done research on it; there is 'exclusion', for example when ARPES excludes doing resistivity measurements; There is 'coherent addition', such as when graphs of resistivity drops as a function of temperature and as a function of the magnetic field are combined into the 'phase-definition' of superconductivity; and there is 'fragmented addition' when knowledge about surface states is added to knowledge about bulk states in a sample (Also see the discussion of coordination in Mol 2002, 53-85).

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like Souriau (according to Lawlor 2011). In the main text, I use the word 'instantiation', which is a bit easier to use, as a non-essentialist concept that, following instauration, addresses the iterative feature of making reality manifest, and the continued work that goes into keeping it stable. "Matter isn't as solid and durable as it sometimes appears. And if it does hold together? Well, this is an astonishing achievement" (Law and Mol 1995, 291).

[25] I aim to have the word 'instantiation' include Mol's multiplicity as well. Mol speaks of scientific beings being 'multiples', which means that they are differently enacted, or instantiated, in different practices, and that these different instantiations are partially connected. I write in the main text that superconductivity is a multiple phenomenon, which means that, in practice, there is more than one instantiation of superconductivity, but less than many different and separate instantiations. An instantiation is not a possible representation of an entity in 'nature' but a construction of which the possibility of being performed is discovered in the conditions present in a situation.

This coordination partly happens through theory. Physical theory ties the ARPES-instantiation to experiments with resistivity by linking their theoretical terms and observables. Theory links the energy gap that is measured in STS and ARPES to the magnitude of the critical temperature that is observed in resistivity experiments. Moreover, characterizations such as the magnitudes of the energy gap and the critical temperature are important information for the production of superconducting samples. Condensed matter theory relates these characterizations to material structures and predicts what the observables would be of new materials. Does that mean that physical theory is the essence that unifies all instantiations of superconductivity? If we regard theory in such an essential way, we quickly run into the problem that theory is often wrong.[26] For example, after completing my measurements on β-$PdBi_2$, I used various models to produce graphs that I could compare my data to. However, the data from my experiment had a narrower distribution than both models I used:

> *The thin-film formula seemed to fit a bit better to the data than the Ginzberg-Landau formula. But the data still fell underneath the graph of the thin-film formula. This suggested that the material was 'more than 2d'. Alex leafed through the Klemm-book looking at graphs for different materials to look for comparisons. In the end he said he would have to think about why the data did not fit the theory. He opened a folder on his computer with a lot of papers in them that had filenames of the materials discussed in the paper. He opened a bunch of them and looked through them searching for graphs. He did not find what he was looking for and suggested that I could make a plot in which my data could be compared to both models. He would think about why the models do not fit the data.*

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[26] This way of philosophizing about science severely troubles the (anti)realism debate. The question whether an instantiation if superconductivity is real or not does not really make sense. All instantiations exist in some way. The way in which they exist can differ: Money, for example, has a very wide range of entities that are enlisted in the situations in which it is instantiated, while 'rainbow-tyquing' probably enlists entities in my fantasy only, and of course in yours as well, now that you read my freshly instantiated concept. But how does this connect to notions of 'right' and 'wrong' in science? Surely, we want to be able to say that a certain scientific theory is wrong, right? The trouble is that 'right' and 'wrong' presuppose a representational structure. In this sense, instantiations cannot be 'wrong'. But we can also think of the instantiations being more or less successful in connecting to its desired effects. This moves us to a pragmatist way of evaluating the merit of science. A scientific entity being instantiated has some material-discursive effects that can be useful or desired in its situation.

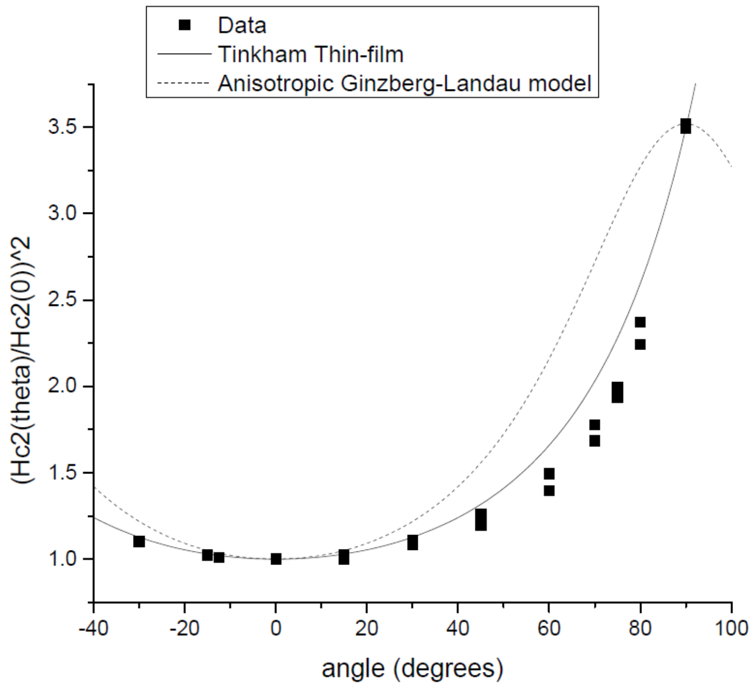

Figure 5: Data of measurements on β-PdBi$_2$ compared to the Tinkham thin-film formula and the anisotropic Ginzberg-Landau model

The mismatch between theory and practice in figure 5 is, arguably, not that big. However, it is just one example of the constant arising of disagreements in science. Alex told me that, for the material that group S is working on, different groups had found greatly varying values for the material's 'London penetration depth'. This quantity can be calculated via the London-model of superconductivity if you know other characterizations of the material: The 'effective mass' of the electrons and the 'charge carrier density'. Inputting these values in the formulas of the London-model gave Alex a value for the penetration depth that was 10 times smaller than the value obtained by a different group that had "really measured it". Yet another group had found a value 5 times as big as the value Alex calculated. He pointed out that "we cannot really explain these differences. But then, the techniques are different, and it is just a somewhat more complex system [i.e. the material]. […] That is the disadvantage with the things that we do: It never really is a nice completed story in which we have been able to precisely solve everything. Then, new questions arise, because the systems are quite complex".[27]

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[27] The question remains, then, for whom an instantiation is useful. If we want to say something about theories being 'right' or 'wrong' this could also be a moral judgement. Thomas Kuhn pointed out that the closure of controversies happens through "value criteria" (2013). He did not mean to say that theory choice is a matter of personal taste and tried to convince his critics that judgement has to be separated from subjectivity (107-8). Such judgements, I would point out, can be made in science because there is a community that upholds the values underlying these judgements, such as a judgement of the accuracy (a value) of a theory. The values and judgements springing forth from them are then coming from a group of people with a certain, i.e. partial, perspective. Such judgements are then, maybe, not personal tastes, but they are informed

For group S, it is never the case that the materials they make are uninteresting compounds that are, as a whole, perfectly described by the physical theory. When disagreements arise, it is hard to determine whether the effect that is found is “really some new, interesting physics”, or whether it is due to experimental difficulties, or to “the sample not being what you think it is”. When this happens, it is a question of weak and strong arguments for and against seeing the effect as a new physical phenomenon. These are played out at conferences and in journals. And new experiments have to be done to provide more arguments.

At these conferences and in these journals, not only experimental controversies are discussed. Theoretical controversies have a role to play as well. As can be seen in Figure 5, there is not one theory or model that defines physical theory. Instead, different models explain different phenomena and can disagree in what they predict for future observations. Although outside the scope of this research, I suspect that the relation between various theories and between theory and experiments is an as complex relational structure as that of instantiations of superconductivity in practice. I suspect that not only grand theories, like general relativity and quantum mechanics, have a complex relation of being partly included into each other while simultaneously making different predictions, but that even on a ‘micro-theoretical’ scale this multiplicity can be found: The details of theoretical models must have the same inconsistencies, inclusions, exclusions, assonances and dissonances in them as scientific practice does. What would a study of ‘physical theory in practice’ look like?[28]

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by moral or political values. Usefulness, and, therefore, pragmatism at large, is not a straightforward criterium.

To analyze reality based on practices is not an easy way out. It is not just a matter of what ‘works’ because usefulness is a value that differs among situations and perspectives. At this point, ethics and politics explicitly enter our discussion of science. Barad, for example, not only erases the boundary between epistemology and ontology, but claims her framework is an ethico-onto-epistemology (2007, 353-96). Latour, in *We Have Never Been Modern*, writes about the inseparability of politics and ‘nature’ (1993), and Mol finishes her book with a discussion of the “politics of what” (2002, 172). I think we need to be careful here not to see this as a very roundabout way back to social constructivism. The claim is not that politics is the layer that ultimately underlies reality. It might sound like that because all these authors elaborate on this point in their conclusions. But politics, in this discussion, does not have a say about *what* is real as opposed to *what* is unreal. That question does not make sense in a nonrepresentationalist framework. Rather, politics matters for *how* the real instantiates itself. For what it looks and sounds like and for how it operates.

What is theory?

[28] Following the authors cited in this citational subtext I have told a story that starts with teleological philosophy, goes through some of the sociology of scientific knowledge, and finally arrives at material-discursive analyses of scientific practices. The last framework avoids representationalism as much as possible by focusing on networks of knowledge practices or other coordination-structures, such as

For the purposes of my argument here I don't have to go further into this question than this. Given the fact that there are various conflicting physical theories that scientists work with, it is implausible that these various theories would be an essence that unifies the multiple instantiations of superconductivity. However, that doesn't mean that theory has no role to play in superconductivity. As shown in the case of the critical temperature and the energy gap, theory links the observables in experiments to each other. In this way it takes on a coordinating role between different instantiations of superconductivity.[29] And not

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entanglements or fluids (Law 2009). Following this argument, we cannot belief in the representational correctness of scientific theories anymore. But then, what is theory?

Instead of starting this question from the privileged position that theory has in representational accounts of science and knowledge, this question can also be answered in practice. Eric Livingstone, for example, studied how mathematicians *do* mathematics in practice (1986). In his work, the study of knowledge-making practices escapes the confines of the laboratory and analyzes highly abstract knowledge such as mathematical proofs as something that happens in specific practices. More recently, Elizabeth de Freitas and Nathalie Sinclair researched the activity of doing and teaching mathematics as a cultural material practice (2014). Mathematics does not appear out of thin air but is devised at blackboards in groups of people arguing over symbols, and involves bodily, as well as intellectual faculties.

[29] By studying theory in such a way, we have dethroned it from its privileged position, enabling us to study it like other knowledge practices. Moreover, this stance gives way to another vision on what theory *does*: In 'Beyond fact or fiction', Amade M'charek argues that it is less interesting to argue over the reality or fictionality of the theoretical concept of race, than to see what the concept does in practices. She explores how biological race is enacted in practice, as a material-discursive object. This way of researching does not want to debunk nor reify the concept of biological race because this would obscure the coordinating role it takes on in practice. M'Charek argues that facts and fictions are made of the same stuff, and that they can play the same roles in practices. Facts can be used to explain differences: ice floats on water, while solid metals sink in molten metals, because the atomic structure of ice is special compared to other solids. But fictions can also be used to explain differences: Parents sometimes say to their children they will grow faster if they eat all their vegetables. This is not necessarily true, but this fiction can nonetheless have some effect. It can make the child eat their vegetables, and make the parents believe they must. Both fact and fiction have effects and coordinate and structure knowledge practices.

Nancy Carthwright argues that we should consider phenomenological laws in physics, i.e. laws that are descriptive rather than providing an explanatory mechanism, to be real (1983). She argues that the truth of general, explanatory laws is, contrary to common sense, not supported by constructing models from these laws and then verifying those models. Cartwright claims that this is a scientific practice that is done more to provide explanatory mechanisms than to make accurate predictions. Accurate predictions are made more readily using phenomenological laws. Accepting her structure, we can see that phenomenological laws 'grow up' together with the experiments that they are about, in the way that Hacking suggests. They form an extended Duhem-Quine thesis: a web of practices. I suggest, however, that we also see the general laws as 'real', only agreements between theory and one or more experiments coordinate superconductivity in practice; mismatches between theory and experiment coordinate how new experiments will be conducted and what they will be looking for.

Physical theories, but also money, feasibility and available equipment, policy, social networks, journals and conferences play such coordinating roles. These, among many more factors, coordinate the instantiations of superconductivity in different ways. Here, at the end of the chapter, we enter the territory of the sociology of science: Helga Nowotny and Ulrike Felt, in their book *After the Breakthrough: The emergence of high-temperature superconductivity as a research field*, write about the coordination of national research agendas, international coordination, the role of the public, and relations between basic and applied science (1997). They use the notion of the "extended laboratory" to describe all the factors that have to come together to make scientific research and technological development possible. What I would like to add to their concept 'extension', is that extended coordination is not only present in the organizational structure of science and technology, but that it is present in what superconductivity *is*: Superconductivity is not something singular that can be researched in many ways but is itself an extended, or multiple, phenomenon. In this ontological, rather than organizational sense, extension does not indicate an assembly of various factors in a lab or between labs. Instead, extension indicates an opening-up of what things are. Superconductivity, instantiated in practices, is an extended, or multiple thing.

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but not in the sense that they represent an absolute truth. Phenomenological laws grow up together with laboratory experiments and provide causal explanations of what happens in the experiments. The unifying, theoretical laws, however, have a coordinating function. When there are disputes or controversies within multiples, or between instantiations, theoretical laws coordinate these differences and produce new inconsistencies within itself or between itself and experiments. This coordinates the further development of science.

3. What is objectivity?

In this chapter, I want to trace out the implications of my analysis in the previous chapter for how we can perform knowledge practices well. What does it mean to juxtapose different ways of knowing? Can we still say anything meaningful about the goals of science? To do this I will discuss the notion of objectivity in feminist philosophy. Feminist views of objectivity have questioned the notion that science provides accurate, unbiased representations, and thereby has informed the literature that questions representationalism that I used in the previous chapter. I will discuss the debate between Sandra Harding and Donna Haraway and move this debate further by considering the multiplicity of objects in practice.[30]

Objectivity, subjectivity and feminism

Objectivity is a nice virtue to strive after if it is on your side. Although a straight-forward conception of objectivity tells us that it is not on anyone's side, this statement has more to it than meets the eye. Namely, everyone has their own perspective on phenomena. When striving for an objective view, the question is how these perspectives can be combined. In the subtext of the previous chapter, I have already outlined the difference between 'addition' and 'multiplicity'. 'Addition' is the hope that we can add many different perspectives on a phenomenon together into a coherent whole that will represent reality correctly. Compared to a naïve standpoint that I might call 'objectivism', it is a big leap forward because it admits that no one has a complete overview alone. Everyone is partial in their view of phenomena because everyone has their own background from which they observe. However, it is not clear that, when adding many partial perspectives together, we will arrive at a coherent view of a phenomenon. It is likely that we will find assonances and dissonances between partial perspectives. The concept of 'multiplicity' aims to capture this insight. It says that partial perspectives on a phenomenon hang together in some way, but that the perspectives do not necessarily have to add up.

Feminist views of objectivity are a response to a naïve view of science that Sandra Harding calls "weak objectivity" (1993): The view of science as being unbiased, value-free, and like a "view from nowhere" (Nagel 1986). This conception of objectivity is conventionally contrasted to its polar opposite, subjectivity. However, subjectivity and objectivity are not simply opposites: In their history of objectivity, Lorraine Daston and Peter Galison nuance the picture of a polar opposition by showing that practices of objectivity require specific ways of scientific subjects disciplining themselves to fit their scientific practice. Making an objective representation of something requires scientists to limit their subjective influence or to shape it in specific ways. They have to fashion themselves to be rigid and systematic, or to be able to form a judgement about what the average is in a collection of scientific images. More precisely, different practices of objectivity have had parallel practices of fashioning a scientific subject (2010, 39 and 191-253).

For a long time, these finely sculpted scientific subjects have had a very narrow background. In *Leviathan and the Air-Pump*, Steven Shapin and Simon Schaffer discuss the societal factors that allowed scientists in seventeenth-century England to produce knowledge (1985). The authority to define what

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[30] This chapter is based on a paper written for the course 'History and Philosophy of Objectivity' in the master's program 'History and Philosophy of Science' in 2018.

counts as knowledge is a problem of social order. In England around the 1660s, this authority came to lie with higher-class men. The subjectivities that the members of this community had to give shape to was necessary for their claims to knowledge. It was not the polar opposite of the scientists' objective representations but an integral part of it. Subjectivity was and is an "exteriority within" objectivity, meaning that what is excluded defines the boundaries of what is included (Barad 2003, 825). Because this necessary subjectivity has historically been a highly gendered one, it is very plausible that the accompanying practices of objectivity have been too.

This claim can be found with feminist philosophers like Simone de Beauvoir and Catharine MacKinnon, who, in 1949 and 1989 respectively, identified the social role of men as objective and the role of women as subjective. This gives men the role of the impartial knower who has an unbiased view of what is happening. This position is gained through a power imbalance between genders that produces all kinds of theories of women that reinforce this same ordering of genders. For example, biological or sociological accounts that make women inherently inferior to men, more emotional, or less innovative than men (see, for example, Longino and Doell 1983, Lloyd 1984).

Standpoint epistemologies and strong objectivity

In *The Science Question in Feminism*, Sandra Harding emphasizes that feminist epistemology is not relativist (1986). Feminist epistemology does not try to "substitute one set of gender loyalties for the other – 'woman-centered' for 'male-centered' hypotheses". Even if it was relativist, it would not be a bad thing compared to 'objectivism'. An epistemology from a woman's point of view would not be more subjective than traditional thought is from a feminist perspective. They are both gendered. However, feminist epistemology does not just aim to formulate an independent set of truths from a non-male point of view, but to "arrive at hypotheses that are free of gender loyalties". Doing so requires the formulation of a science from a female point of view, but this does not necessarily encompass a relativist shift. It is a necessary step to remove androcentric bias from science (1986, 138).

A problem that feminists encounter in this project is the ambivalence of criticizing bad, sexist science by using traditional science. Arguing that science is pervaded by the male point of view precludes the possibility of a scientific critique of androcentric science (Harding 1986, 137-8): It is slightly paradoxical to criticize science that reinforces essentialist, sexist views of women by claiming that the study wasn't scientific enough. If we try to show that traditional science excludes the female point of view, this strategy amounts to fighting an oppressor with his own tools.

This strategy could be summarized as the project of feminist empiricism: By eliminating biases through strict adherence to scientific methods, scientists could fight the androcentric assumptions that fuel traditional science. However, Harding points out that the feminist critique of traditional empiricism challenges the very assumptions on which empiricism is grounded, thereby undermining itself. Wasn't observation of data supposed to be independent of who does the observing? Moreover, feminist empiricism introduces socio-political arguments into the terrain of observation; a terrain that was supposed to be free from politics (Harding 1986, 161-2). This criticism resonates with Willard Quine's criticism of the analytic/synthetic divide: He states that it is impossible to separate statements into a pure experiential part and a linguistic part that describes the experience. Rather, the two are intertwined, making the availability of concepts a part of observation, and experience a part of the language that we use. This invites the possibility of value-ladenness of theory (Harding 1986, 36-41, and Quine 1951). The way that the scientific community is gendered impacts theory because theory is not formed out of the blue. It is instantiated in practices that have ethical and political dimensions. These dimensions leave their marks on scientific descriptions.

In response to these inconsistencies in empiricism, feminist scholars formulated the theory of standpoint epistemology, which Harding claims has been influenced by Hegel's master-slave dialectic: Women have a more complete view of the gender-dialectic, because men are made blind to the reproductive aspects of this relationship, which are conventionally carried out by women. If men are not aware of this work done by women, then the work is done extremely well; it is only when the day-to-day reproductive routines are not carried out that the work becomes noticed. The invisibility of reproductive work enables men to remain in their non-bodily world of abstractions (Smith 1990, esp. 18). Even if this is a very abstracted, generalized picture of gendered relations, it has big impacts on who gets to produce knowledge and who doesn't: Only the oppressor will be able to formulate language to describe this relation, because the oppressed do not have access to the site of knowledge production. To address this dialectic relationship in an all-encompassing (arguably objective) way, we need to account for the position of the oppressed that does not reproduce the same relationship. Harding summarizes this problem as follows:

> *"Briefly, [feminist standpoint epistemology] argues that men's dominating position in social life results in partial and perverse understandings, whereas women's subjugated position provides the possibility of more complete and less perverse understandings" (Harding 1986, 26).*

Standpoint epistemology sees all knowledge as situated in a social context, even (or especially) the traditional objective version. Given the idea that some positions have more information available about social relations than others, the marginalized position is better suited as a starting point to attain objective knowledge (Harding 1993, 56). Harding does not limit this analysis to gendered relations. She takes an intersectional standpoint in that she claims that, in any power-relation, the marginalized side is in a better position to produce objective knowledge. The knowledge that the oppressor produces will in some way serve the purpose of keeping up the power imbalance that gives them a privileged position to produce knowledge in the first place. This argument also wards of relativism for Harding: Universalistic theories fail to acknowledge the context of knowledge production and therefore have no proper response to relativistic claims, except that they have the 'right' claim. Standpoint epistemologies, on the other hand, can account for situatedness of the claim came and add this to their judgements of those claims.

These thoughts on standpoint theories lead to Harding's formulation of strong objectivity in 'Rethinking Standpoint Epistemology: What Is "Strong Objectivity"?':

> *"Strong objectivity requires that the subject of knowledge be placed on the same critical, causal plane as the objects of knowledge. Thus, strong objectivity requires what we can think of as 'strong reflexivity'" (Harding 1993, 69).*

According to Harding, reflexive attitudes will maximize the objectivity of a study, together with the requirement that the people who could be marginalized by a scientific community are included in scientific research. Science that practices strong objectivity should be engaged in "democracy-advancing projects" to achieve this inclusion. To summarize, strong objectivity requires a reflexive attitude of a researcher and active consultation of marginalized viewpoints, especially in studies that affect marginalized people.

Now, taking a step back, what is indicated by the term 'objective' in standpoint epistemology? Although it does not see the subjective as the polar opposite of objective but as an integral part of formulating statements about objects of study, strong objectivity is mainly concerned with managing the subjective. This is reminiscent of the techniques of managing the scientific self that Daston and Galison analyzed in *Objectivity*, although the techniques are less individualized than in the mid-nineteenth century. Strong objectivity is less about minimizing the subjective in an individual researcher and more about managing many subjectivities of an epistemic community. Instead of minimizing an individual's subjective influence, it has to be made explicit so that it can be counteracted or nuanced by a different subjectivity. By balancing out all these subjectivities and expanding the epistemic community to as many different subjectivities as possible, strong objectivity has a completely reversed relationship to traditional objectivity. Rather than an impersonal "view from nowhere", it is a highly populated view from everywhere.

View from nowhere, everywhere, or somewhere?

Harding's paper on strong objectivity uses Donna Haraway's notion of "situated knowledges", which I introduced in the previous chapter (1988). Haraway's writing is a response to Harding's earlier book *The Science Question in Feminism*. While Harding outlines the different trends in feminist epistemology (feminist empiricism and standpoint epistemology combined with postmodern feminism) and shows how they differ, Haraway wants to hold on to both ends; even though she describes this relationship between "radical constructivism versus feminist critical empiricism" as a dichotomy (1988, 580). She can do this because she shortens the distance between the universalistic, all-encompassing view of the oppressor who is nowhere and everywhere at the same time, and the oppressed who, according to Harding, has an objective view of the reproductive work that is needed to put the oppressor on his pedestal. Contrary to Harding, Haraway argues that "the subjugated have a decent chance to be on to the god trick and all its dazzling – and, therefore, blinding – illuminations" (1988, 584). The universalistic trick of taking the position of god, nowhere and everywhere simultaneously, should be critically examined when it occurs among the subjugated too. Reflexivity must also extend in this direction to strive towards something that could be called a feminist objectivity.

For Haraway, this feminist epistemology should not be relativist. However, unlike Harding, Haraway notices that relativism is not opposed to universalism because it reiterates the same universalistic traps that feminists wanted to avoid (see section 'Situated knowledges' on page 14). The threat of relativism (i.e. a separate, independent truth for everyone) catches Harding in an oscillation between universalisms. However, Haraway points out that

> *"the alternative to relativism [is not universalism but] partial, locatable, critical knowledge sustaining the possibility of webs of connections called solidarity in politics and shared conversations in epistemology" (Haraway 1988, 584).*

The view from everywhere is a "god trick" too: it copies traditional science's pretentions of being valid everywhere while not coming from any noteworthy place or time. The difference is that strong objectivity totalizes the contingencies of the production of knowledge. This is the way in which the time and place of the production of knowledge are made unimportant. Time and place are extended to always and everywhere by addition. By noting this similarity, a way out of the dichotomy between contingency and universality comes into view: situated knowledges.

Harding effortlessly incorporates Haraway's thoughts and criticisms into her standpoint epistemologies to formulate her notion of strong objectivity. Strong objectivity and situated knowledges seem to be a good fit: They are concerned with the contingencies of knowledge production and they profess reflexivity. But I would argue that, although compatible in method, situated knowledges and strong objectivity are quite different ideas. We can already see it in the way that the terms are formulated: strong objectivit***y*** (singular) compared to situated knowledge***s*** (plural). Strong objectivity wants to attain maximum objectivity by extending the epistemic community to a variety of voices, which all should be reflexive about what they have to say. However, situatedness is not the only essential feature of situated knowledges; they are necessarily partial. They are always contingent on social, historical and material circumstances. Only from partial viewpoints "the unmarked category [i.e. dominant, universalistic, male, white] would really disappear – quite different from simply repeating a disappearing act" (Haraway 1988, 585). Repeating the disappearance act amounts to taking a critical, reflexive look at the contingencies of science to make it more objective and repeating this until the most objective view has been attained. Haraway apparently does not need an iterative reflection to have an objective view, because she champions views that result from partial viewpoints. The crucial question now becomes: views of what?

"Ontology-in-practice is multiple"[31]

A good way to gather situated knowledges is praxiography. In practices, specificities of objects are highlighted. And because of its anthropological method, praxiography can gather different perspectives on these practices. But as I showed in my own praxiography in the last chapter, these situated perspectives do not necessarily combine into a coherent unity. Another example is Annemarie Mol's study of the leg-vessel-disease atherosclerosis, in practice. She asks a technician to show her the disease during autopsy of a body:

> *"'Look. Now there's your atherosclerosis. That's it. A thickening of the intima. That's really what it is.' And then he adds, after a little pause: 'Under a microscope'" (Mol 2002, 30).*

Under a microscope, atherosclerosis is a thickening of the intima. But in the clinic, atherosclerosis is a patient's trouble with walking. And in surgery, atherosclerosis is a plaque of white paste that needs to be scraped out of a vessel. These different 'enactments' of atherosclerosis are not just different versions of the same object but form a 'multiple' (Mol 2002, 5). Superconductivity in practice is a multiple too, as I have shown in the previous chapter.

Multiplicity could only have been theorized using the insight of partial perspectives and situated knowledges. The discussion about feminist objectivity outlined above is a necessary background for the formation of the tradition that the discussion of multiplicity can be found in: 'material-semiotics', or 'actor network theory'. Law characterizes this tradition as

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[31] Mol 2002, 157.

> *"a disparate family of material-semiotic tools, sensibilities, and methods of analysis that treat everything in the social and natural worlds as a continuously generated effect of the webs of relations within which they are located. It assumes that nothing has reality or form outside the enactment of those relations. Its studies explore and characterize the webs and the practices that carry them. Like other material-semiotic approaches, the actor network approach thus describes the enactment of materially and discursively heterogeneous relations that produce and reshuffle all kinds of actors including objects, subjects, human beings, machines, animals, 'nature,' ideas, organizations, inequalities, scale and sizes, and geographical arrangements" (Law 2009, 141).*

Law's loose definition is informed by Mol's study, which is why I think it is fruitful to show it here. Next to the mixing of different actors, an interesting aspect of this definition is that it sees reality as produced by the "webs of relations in which they are located." This phrase is highly reminiscent of Haraway's "webs of connections" combined with her requirement of situatedness; and it is informed by her work.

Law notes that Mol changes one detail in the theory of reality as a network of practices: Mol's networks do not have to be coordinated to such an extent that they eventually form a "single coherent reality" (2009, 152). Her atherosclerosis can be different depending on the situation in which it is enacted. Sometimes these differences lead to controversies, but more often they are spatially separated, abstracted to such an extent that they can still be agreed upon in meetings, or simply overlooked. Mol rejects that these differences are ways of viewing the same disease that still have to be combined. She argues that each practice produces its own material reality, making the body not singular, like it is in theory, but multiple, like she has observed it in practice. The term 'multiple' indicates this picture of a network in which there is place for different realities. They hang together in specific ways that makes them not the same, but also not completely separate: "there is manyfoldedness, but not pluralism" (Mol 2002, 84).

The formulation of reality as multiple gets to the heart of what I wanted to point out about the difference between Harding and Haraway. Strong objectivity sticks to objectivity in the traditional sense in that all the different situated accounts of an object need to be coordinated into one coherent picture of the object. The object is looked at from many different angles and all these angels can be made equivalent and combined into a complete picture of reality; just like different coordinate systems are made equivalent by mathematical translations (Mol 2002, 154). However, Mol uses a completely different picture. For her there is no object that is looked at from different angles. Her objects come about by linking different practices to each other. This makes her ontology-in-practice not relativist but *relational*. This ontological, relational direction in feminist theory problematizes the term objectivity on a whole new level. And one of the frustrating things is that not so much can be said about it in general, because the key-feature of Mol's strand of material-semiotics is that it takes shape in practice. Mol jokingly remarks that "since this book has generated no universalities, it would be said to fail as philosophy" (2002, 171).

An objective view of what?

Feminists have come up with many ways in which they could make their epistemology representative of a wider audience than the traditional make-up of the scientific community. Most agree that contingencies, subjectivities, situatedness, or whatever you want to call them, must be addressed in a feminist science. But now that this has crystallized out, what is at stake is the way we conceive of the ontology of such a feminist science.

The question of feminist objectivity is a relatively straight-forward one if we conceive of objects in a traditional way: It is concerned with a representation of reality that is as unbiased as possible. But after Haraway and Mol, objects have become multiple. Here, ontology depends on practice and is not concerned anymore with 'mirroring' objects that are out there in 'nature'. Rather, "when moving from object to [scientific] article we do not leave the material realm to enter that of theory and thought, but move, instead, from one sociomaterial practice (observation, experiment) to another (drawing, writing)" (Mol 2002, 154). In this way, representations are not mirrors of reality, but tools in their own right: Angiograms are used to locate a clogged-up vessel and to determine details needed for an operation; atlases full of archetypes of plants are used to categorize other plants for a variety of purposes; images of colliding sub-atomic particles are used to formulate theories of how matter behaves. Some representations are used to treat, some to build, some to engage in discussion, but the important factor that representations share is that they are used. They link practices, rather than static objects.

But, according to Karen Barad, the impossibility of an absolute exteriority of representations to what they represent does not make objectivity impossible. In her 'agential realism', the primary unit of analysis is the 'phenomenon', a "relational atom" that incorporates the whole apparatus of bodily production: The object that is investigated *and* the apparatus of investigation, including the subject (2007, 151). The word 'atom' is a remarkable choice in this context because etymologically it indicates something that cannot be divided. Barad plays with this by writing about separations within phenomena, while stating that phenomena cannot be divided absolutely. 'Phenomena' are not atoms because they are small, or a fundamental, single kind of substance, but because contingencies cannot be removed from objects of knowledge.

Knowledge practices, for Barad, are processes in which an "agential cut" is made within the phenomenon, which separates the known from the knower, or, effect from cause (148). In such a way, the knowledge created within the phenomenon becomes an "exteriority within" (2003, 825). For Barad, objectivity is characterized by "accountability to marks on bodies" (2007, 340). One part of the phenomenon becomes accountable to the other part in the phenomenon's "intra-action" (2003, 815). The point is not to be accountable to the truth of a pre-existing object but to be accountable to how objects materialize in 'intra-actions'. Different (knowledge) practices materialize different instantiations of objects and objectivity is about accountability to these materializations (2007, 61).

Barad holds on to objectivity by changing the referent from 'objects-in-themselves' to 'objects-in-phenomena' (2007, 340 and footnote 118). Understanding the objective referent in this way is meant to ensure that the contingent nature of the 'object-in-the-phenomenon' is attended to. In this way, Barad aims to provide a solid ontological basis for the problems of situatedness outlined above. However, so far it did not add anything new to the discussion of objectivity. Barad formalizes the impossibility of having knowledge about pre-existing objects by changing the ontological element that we attend to, to a concept that includes the contingencies of knowledge practices.

At the same time, Barad acknowledges that there cannot be a 'third-person-view' that can objectively describe the 'whole' phenomenon objectively, because this would produce a new dichotomy between knower and known, where the known now includes the conditions of measurement of the 'whole' phenomenon that we try to objectively represent, and the knower would be some kind of 'third-person-perspective'. That new 'third-person-perspective' would then also have to be captured to provide an objective view of the new 'whole' phenomenon, and so forth. This means that the 'intra-action' of phenomena is a process that is open-ended (2007, 170): Trying to fully capture a relation between an object and the apparatus of knowledge production creates a new relation of object versus subject. In this sense, this analysis of 'objects-in-phenomena' and an objective description of these phenomena reaches its limits here. Holism is not an option.

But there is another important aspect of Barad's concept of 'phenomena': They are understood as iterative materializations (2007, 210). 'objects-in-phenomena', or instantiations, are not just materialized once, after which they solidify, but their reality is reshaped over and over again in material-discursive practices. If you recall my discussion of Étienne's Souriau's concept of 'instauration', you see that his conception of the manifestation of reality is similar to Barad's (see page 25). Instauration implies a discovery of something that did not preexist. Someone who instaures a work of art does so in the sense of being response-able, or accountable, to how the conditions at hand enable the instauration of the artwork. This conception of knowledge practices starts to lead us towards dynamism, or some other type of open-ended process-thinking.

To figure out what such dynamic thinking can do for or conception of objectivity, we first need to rethink what the underlying desire of using the word 'objective' is. As already discussed, the word could indicate a desire for impartiality, which I showed to be untenable. Next, it could also indicate a need for trust in science. This criterion reframes the desire for impartiality to a need for an epistemological community that has a shared belief in the benevolence and usefulness of scientific knowledge. Reiss and Sprenger suggests that, to achieve this, we could uphold an instrumentalist view of objectivity, in which all features of science that promote trust in it are deemed to be objective (2017). This option seems unsatisfactory to me because it is unclear about who oversees science in this view, and how the process of promoting trust is steered. This could, in the worst case, degenerate into a totalitarian promotion of whatever is deemed to be scientific by those in power. In the best case, it could make science trustworthy, but this does not explain why we could see this science as the best, 'objective' choice among other knowledges. By striving for objectivity, we want to say that 'our' way of inquiring into the world is somehow special and better than other ways; that it attends to reality more successfully than other knowledges.

However, if we recall that we are trying to formulate a criterion for objectivity in science while seeing objects of knowledge as 'multiple' objects, this hierarchy between knowledges becomes less important. The multiplicity of objects of knowledge and the variety of subject-positions that are co-produced with these objects even become part of the analysis. Now, objectivity does not need to indicate a hierarchy between knowledges anymore. What do we want from objectivity if this hierarchy is not the point? Recall that Haraway pointed out the ambiguity between critical empiricism and radical constructivism in feminist philosophy of science (page 35). On the one hand, we want to empirically show that certain views of the world are not an accurate representation of reality. Here, we need scientific methods to counteract oppressive stereotypes. On the other hand, we want to criticize the notion that science can provide complete and correct representations. The latter might seem to trouble the former, because it seems we cannot use science to replace incorrect views of the world with correct ones if we reject the possibility of true scientific representations.

Yet, the ability to criticize stereotypes is not the same as providing an alternative correct picture. Criticism can be done by assuming that there is a final truth of the matter and criticizing an existing view for not having attained that truth yet; but it can also be done by convincingly showing possibilities that do not correspond to a stereotype, thus weakening this stereotype and opening up a new space of possibilities for (looking at) reality. If we accept multiplicity of objects and juxtaposition of ways of knowing as our onto-epistemology rather than singularity or cohesion, this last option becomes a tenable one. Our desire behind using the word 'objective' could be translated into a desire for possibilities.

Can we go from a representationalist objectivity to a dynamic creation of possibilities? If we want to do this, we cannot talk of instantiations or instaurations as objective, because this assumes that we can represent objects correctly. We can never be representationally objective if the referent includes the agencies of observation because this results in an infinite loop. But the process of materialization can be responsible to how it instantiates reality. This process must be open-ended to avoid representationalism. It does not move into a fixed direction in which reality materializes correctly. Because objects are multiple, the values associated with them point into different directions. This goes for accuracy, as well as for usability and political values such as emancipation. A 'good' superconductor might be one that has few impurities in its crystal lattice in scanning-tunneling microscopy, while in technological applications 'goodness' might indicate a suitably high critical temperature. These different 'goods' from different situations can coincide but they can also conflict. They pull the process in various directions in local practices (Heuts and Mol 2013). Such a process could be called a process of "tinkering", indicating a constant negotiation of details without a clear end-goal in mind, at least not a single end-goal (Mol 2008). This tinkering can uncover, step by step, what are the possibilities in our world given our circumstances.

Another way to say this, is to follow Daston and Galison's observation, in their book *Objectivity*, that after representation a next epistemic virtue is coming up that they call "presentation" (2010). Scientists in fields such as nanotechnology now do not try to represent 'nature' as best as they can but present what they can do with matter. This presentation involves producing the (nano-)object, imaging it (sometimes with the same tools that were used to produce it), and presenting it, often with a technological use in mind. A similar observation could be made with the scientists at the laboratory that I studied. They create materials that they want to study and then present interesting features about those materials. They do not try to represent the natural world correctly but aim to show new states of matter before another group does. They aim to present "new, interesting physics".

So, what is objectivity? If I want to be consistent with my previous analysis, I cannot give a singular answer to this question here, but I can follow my analysis to its last step. Objectivity has been various things in the past, and many of these continue existing in the present. A new instantiation of objectivity might arise when we see that a desire for objectivity can be translated into a need for new, realistic possibilities of being and knowing (See also Haraway 2016). Knowledge practices such as those found in laboratories can present such possibilities. It is crucial to note, here, that laboratory sciences are driven by technological interests, and that the objects that laboratories present serve the interest of technology because of the financing-structure of contemporary science. In this context, feminist objectivity could mean that the presentations of science must be attentive to a multiplicity of possibilities that can arise from the situation at hand and must 'tinker' with these situated possibilities instead of following the path of the least resistance, which is strongly entangled with the normative and powerful. Objectivity could mean not to follow any goal single-mindedly but to conduct knowledge practices in a way that opens up new ways of knowing and being.

Conclusion, or, what is this text?

At the start of this text, I outlined my way of thinking by discussing the practice of reading and of writing, and I explained how ethnographical methods can be used in research that aims to foreground practices, transforming it into a praxiography. After introducing these methods, I answered the question 'what is superconductivity', using my praxiographical observations from a Dutch university-laboratory and using various literatures that foreground the study of practices. Lastly, I considered what objectivity has meant and could mean through the lens of feminist philosophy of science. These discussions all developed openness as an important epistemic attitude: The last chapter concluded that the desire for objectivity in feminist philosophy could be translated to an openness to new ways of knowing and being. In this final chapter, I want to consider to what extent my research has met this standard. Moreover, in the second chapter I stated that theory can be regarded as performing a coordinating function. If this is so, then what does the theory that I have presented here coordinate?

Let me start with addressing what the main objective referent of my research has been. When researching the question 'what is superconductivity', superconductivity as it is instantiated in the laboratory is the 'object-in-the-phenomenon' that is agentially separated from the apparatus of investigation, which is part of the same 'phenomenon' (i.e. the same relational atom). In this investigation, there are two main categories, or maybe levels, of 'phenomena' to be distinguished. First, there is the relation between an instantiation of superconductivity as object; and the experimental apparatus, including the researchers at the laboratory, as subject. Second, there is the relation between a whole experimental apparatus as object, including superconductors, experimental arrangements and physicists; and me, my participation in the lab, my theoretical background and this text, as subject.

Now, I am not explicitly going to construct a third phenomenon, a third relation between knower and known, with my whole research as the object of critical reflection and this conclusion as the subject. This leads to an infinite loop of reflection upon reflection, without a chance of a more objective representation. However, if you like abstraction, you might consider that you, the reader, are part of the 'subject-in-the-phenomenon' of such a third-level (or maybe even fourth-level) relation. Note, also, that these hastily constructed levels of phenomena are not discrete. While I, as the author, am the subject of the second-level phenomenon, I was also one of the researchers in the laboratory, and therefore also a subject of the first-level phenomenon, and thus my own object of investigation in the second-level phenomenon. It is precisely this ambiguity between subject and object that characterizes the method of participant observation and praxiography. By participating in practices that are my object of investigation, I could make my subjectivity in those situations the object of my reflections.

Getting back to evaluating this research (back to level two), we see that my research question, 'what is superconductivity', refers to superconductivity, as well as the experimental arrangements that are used to investigate it. Using praxiographical methods and a theoretical body that combines philosophy of science, sociology of science, actor-network theory and feminist material semiotics, I have shown that superconductivity is instantiated differently in different (experimental) situations. What superconductivity *is*, is different in all these situations. This does not mean, however that there are many distinct types of superconductivity, because these different instantiations cannot be discretely separated from each other. But neither can they be combined into a coherent, holistic view of what superconductivity *is*. Instead, I have argued that superconductivity is a 'multiple': It is an object that is not the same in every situation, but still hangs together somehow. Superconductivity is 'more than one and less than many'.

Before I started my fieldwork, I already suspected that superconductivity was a 'multiple'. I had studied the literature that you can find in the bibliography (especially *The Body Multiple*) and wondered whether the objects of physics could also be described using the praxiographical methods that I had studied. In this sense my conclusion is unsurprising. In a way, the conclusion that superconductivity is a multiple is more a methodological statement than an attribute of the object superconductivity. It says something about how I went about answering this question. At the same time, the fact that 'multiplicity' is not really an attribute of superconductivity is precisely the point. Objects do not have essential attributes but are instantiated in practices. So, maybe, this research just provides an example for the type of thinking that I found in the literature. It is a sort of theoretical exercise using superconductivity as an example. Still, the fact remains that the analysis could be done, which is not a trivial result: The examples used for the theoretical exposition of the multiplicity of objects provide constraints to the analysis. Undoubtedly, there are many situated ways to consider what my research is. Probably, it is a multiple too.

Turning to the coordinating function of theory, we might ask what the theoretical expositions in this text coordinate. The first thing that comes to my mind is that it has made me do all kinds of work over the past year. It has made other people work too. Moreover, it has opened up new questions that may have an effect on future work, such as 'what is physical theory in practice?', 'how are the different instantiations of superconductivity coordinated more specifically?', and 'how can science studies research its subject matter in a way that opens up new prospects for being and knowing, going beyond pure critique?'. Next, this text relates different texts to each other and discusses where they develop upon each other, conflict, or agree. I have presented a genealogy of meta-studies of science starting from the turn to practice in the eighties. These texts, and this one as well, coordinate epistemological problems and a need for answers to the anxieties of the modern world (McDowell 1996).

Finally, I want to ask whether my research is objective, in the way I have developed in the last chapter. In other words, has this research opened up new ways of knowing and being without following a goal single-mindedly? As I indicated, I have followed a clear goal throughout my research, namely, showing that superconductivity is a multiple. However, this is not a singular goal because multiplicity necessarily entails various directions in which the research can go. The object is observed in a variety of situations. With regards to the physical theories of superconductivity, I have not opened up new ways to conduct research or know about superconductivity. With respect to physical theory this research can be seen as rather deconstructive. It deconstructs the notion of a singular object that can be studied in the laboratory. However, throughout this text I have indicated that critique that aims to deconstruct the current objects of our knowledge in favor of a more accurate future representation of the world is a dead end. By showing this impossibility, this research is a step towards new ways of knowing and being in (knowledge) practices to come. Possibly, the most objective feature of the research is that it has illustrated the impossibility of representationalist objectivity.

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