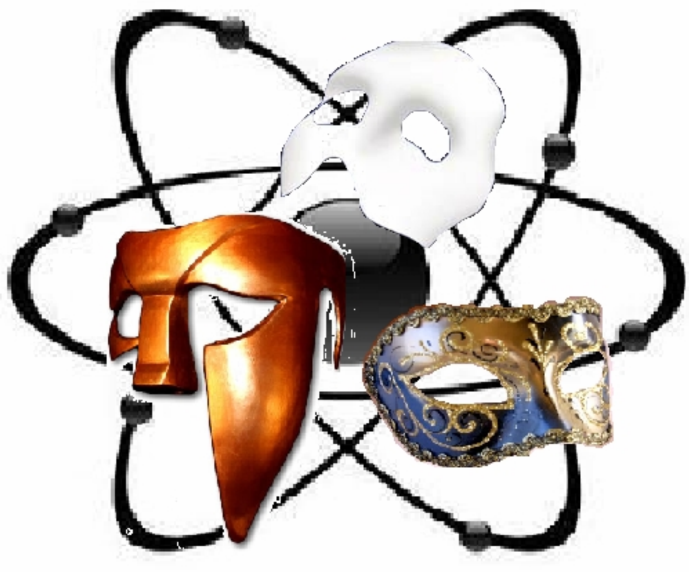
The Multiple Faces of Scientific Unity

Scientific Diversity in a Comparative Study of Two Contemporary Sciences



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First of all, special thanks to those pharmacologists and physicists who have devoted their time and energy to the interviews, without their contribution this paper would not have been possible.

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Introduction

Popular documentaries treat it as a unanimous voice that reveals the mysteries of the universe, and some refer to it as the single solution to all the challenges that the future may hold. Others criticise it as if it is a legal person, they equate it with an oppressive reason, or see in it the disenchantment of world. Both celebratory and critical voices have treated science as a single entity, yet science itself can hardly live up to such unambiguous uses. Even more, regarding the identity of science opinions beg to differ; where one is positive of science, the other is critical, and yet another is somewhere in between. Through the findings of our field we have come to know science as set of practices embedded in society. Through science we find numerous actions, ideas and images which, on close examination, are not all that similar. Sociologist of science Stephan Fuchs explains in his *Against essentialism*: ‘There is a welter of evidence from science studies suggesting that there is no global logical or methodological unity to science. There is no science “as such”. No one has ever done “science in general.” Science, with a capital S, is an invention of philosophers, and one that does not measure up to the empirical evidence pointing at the disunity of the sciences. Some science critics mistake science for its method, than call this method “positivism,” which presumably has something to do with numbers and statistics. These are ideologically and politically suspect.’[1] Be it in a positive or critical way, any unambiguous depiction of science raises questions.

Through histories, sociologies and philosophies of science all of us science studiers have become familiar with the variety of practices, ideas, views and products that have been captured under the denominator of science. The problem we face branches out into two basic types. The first area of debate involves the question what the identity of science is. The cultural and linguistic turns, the Strong Program, postmodernism, the Science Wars; in all of these movements we can identify contention about the nature and identity of science, about whether its advent and development should be explained naturally, socially, culturally or otherwise. Secondly, and parallel to the question of what the identity of science is, runs the second question whether, or to what extent we can actually speak of a single scientific identity. Current commonplace notions within the science studies, such as the locality of knowledge production, the change of scientific knowledge through mediation, and the contingent development of scientific disciplines support Fuchs’ assertion against an

[1] Stephan Fuchs, *Against essentialism: a theory of culture and society* (Cambridge and London, Harvard University Press 2001) 6-7.

essentialist view of science, and thus against a unified understanding scientific identity. Yet on the other hand even Fuchs' *Against essentialism* appears to discern a distinct domain called science, suggesting that after all is said and done science still has an identity distinguishable from other domains of human conduct.

Nevertheless, the idea of a unity of science, or that of various disciplines as part of one unified scientific project, has been put under tremendous stress by what Fuchs calls a welter of evidence from the science studies. And by mobilising his evidence against an essentialist understanding of science Fuchs joins a long academic tradition that has questioned the identity of science. In fact, *Against essentialism* can be understood as a contribution to a longstanding debate about the nature of science and knowledge, a debate that also includes iconic contributions such as Sokal's hoax, *Les mots et les choses*, *The structure of scientific revolutions*. Notably, the publications of each of these icons lead to renewed and severe contention about the identity of science.

This paper takes a step back from all of these identity debates. Nevertheless, the debates themselves do characterise the general problem area in which this paper seeks to contribute. This paper will address the problem of the identity of science by asking how scientists themselves view science and if the differences in views between scientists from different fields count as signs of disunity. The point of departure of this paper is the views of a number of scientists from different sciences. This paper adopts a subjective point of view and will avoid a 'bird's eye'-perspective on science that prescribes a specific nature or definitive culture to it. Only after we have looked into the individual perceptions of science I will discuss science in more general terms such as disunity. However, before we can proceed to cast the words of individual scientists into the categories that we are familiar with from the various discussions of scientific identity the central issues in play require further attention.

Disunited identities

Regarding the problem area of the identity of science one cannot circumvent the authoritative contribution '*The disunity of science: boundaries, contexts and power'* edited by Peter Galison and David Stump. [2] In it we encounter a combination of problems resembling those in *Against essentialism* and other discussions of the nature of science. Interestingly, already in the introduction of the *Disunity of science* we find disagreement when Galison sketches out

[2] Peter Galison, 'Introduction' in: Peter Galison and David J. Stump ed. *The disunity of science: boundaries, contexts, and power* (Stanford, Stanford University Press 1996) 13-18.

the various positions on the extent and gravity of scientific disunity that fill the contributing papers.[3] The book itself is divided into three parts, the first part is titled 'Boundaries', the second 'Contexts' and the third 'Power'. Each of these parts represents a distinct steppingstone into the central issues that are shared by most debates about the nature and identity of science. Steve Clarke nicely sums up the diverse contributions to each part of the book. The part on boundaries mainly concerns itself with the demarcation between science and non-science and between different scientific disciplines. An important frame of reference in this part, and an intellectual background throughout the book, is the Unity of Science movement propagated by members of the Wiener Kreis.[4] An important issue addressed in 'Boundaries' is that a distinct identity for science relies on its distinction from other domains, and consequently that the sub domains captured under the denominator of science have certain qualities in common. Visible throughout the book is the recurrent tendency to formulate the disunity of science as an anti thesis against either the Unity of Science movement, or in some cases against existing historical readings of that movement.

The second part concerns itself with contexts of disunity, and as Steve Clarke aptly discerns each contribution to this part is: 'in one way or another, concerned with attempts to break out of the endless cycle of debate between realists and relativists (aka constructivists) which has characterized much of the interaction between philosophy and science studies in the last twenty years.'[5] When addressing the unified, disunited, social, cultural or natural identity of science one is easily drawn into realist-relativist type of discussions. In realist-relativist debates arguments go beyond a discussion of science as a phenomenon as such, but also directly address scientific knowledge claims. Regardless if one is positive or critical towards such claims, the arguments mobilised in realist versus relativist discussions draw in epistemological, ontological and metaphysical dimensions. In short, discussions concerning the nature and identity of science are not only about the scientific domain, but are also about the claims that domain makes about the rest of the world.

Almost naturally, the third part of *The disunity of science* concerns power. Following any discussion on realism versus relativism issues, one also expects a discussion of the repercussions that a specific conception of science has for the areas in which science makes its claims felt. Besides one paper on the socio-political dimensions of Otto von Neurath's efforts in the Unity movement by Jordi Cat, Nancy Cartwright and Hasok Chang, the other

[3] Galison, 'Introduction' in: *The disunity of science: boundaries, contexts, and power*, 13-18.
[4] Steve Clarke, 'The disunity of science', *Philosophy of science* 66 (3) (1999) 506.
[5] Clarke, 'The disunity of science', 507.

contributions assert more one-sided points. In order of appearance the reader is presented with a richly illustrated piece on the representation of science in natural history museums, a contribution proposing the idea of epistemological sovereignty as the crux in avoiding relativist pitfalls, a discussion of the loss of subjectivity in science and a text on the gender bias in science. Notably, Steve Clarke devotes only three sentences to the collection of five papers that comprise this third part of the book.[6] Where Clarke describes the second part of the book as a collection of efforts to transcend the realism-relativism cycle, the part on power seems to remains firmly rooted in that stalemate. More precisely, for the large part the contributions on power offer very postmodernist discussions of scientific representation, epistemological hegemony, the death of the subject and discursive gender bias.

In line with the second part, Fuchs' *Against essentialism*, and the Science Wars, the third part of the book shows how discussion about the nature and identity of science draws in questions concerning relativism and realism, epistemology and truth. Additionally this third part of *The disunity of science*, in its rather one sided approach to the problem of power, also shows how hard it is to escape existing categories concerned with science and scientific knowledge claims. As a result, the reader is left with only vague clues about the repercussions of unity or disunity, which are mainly described as instances of a pervasive or oppressive structural hierarchy imposed by science.

There are some recurring features to discussions concerning the identity of science. Firstly, identity is a matter of internal and external demarcation; it is both a matter of how one defines science as well as a matter of how one separates the domain of science from other domains of human conduct. Another feature of our problem area is contention, from polite academic debate to the outright verbal hostilities of the Science Wars the identity of science is a source of ongoing disagreement. Although I cannot hope to resolve or settle this historic contention, its existence alone justifies a fresh look at the problem, all the more because the contention itself shows recurring features that are essential in an understanding of the problem of scientific identity. Any discussion of the identity of science draws in science's knowledge claims, its description of the world and its status as the producer of reliable knowledge. As a result any discussion of the nature of science cannot help but involve ideas on what determines the outcome of scientific research, why or if science can justifiably describe our world.

[6] Clarke, 'The disunity of science', 507.

Starting with the last question if science can indeed justifiably prescribe the world it becomes clear how any question on the nature of scientific knowledge also raises questions of social order, social responsibility and the effects of science on society. In other words, discussion of the identity of science tends to involve a discussion of power, sometimes subtly, sometimes explicitly. Explicit notions of social power can for example be found descriptions of science as an oppressive force of rationality that imposes a single view of reality on individual subjects. On the other hand, more implicit notions of power can be found in celebratory descriptions of science as a propeller of societal progress on the basis of its triumphs over the whims of nature. Power can be the possibilities of new technologies or the elusive yet pervasive discipline of scientific culture of society. Yet in some shape or form power is always part and parcel of any discussion of science.

As presented in the foregoing section this paper will adopt a subjective approach by investigating what identity individual scientists attribute to science, by investigating how they perceive science and what in fact science means to them. However, in all likelihood a scientist will not present his opinions and views of science in the vocabulary of *The disunity of science* or *Against essentialism*. Nevertheless, also individual accounts, developed from a background of personal, practical experience, involve philosophical ideas. At the point where an individual provides arguments concerning the status of scientific knowledge, the nature of reality and the merits of science he or she certainly touches philosophical issues. Even casual utterances on science and knowledge by an individual draw in epistemological, ontological, social and metaphysical dimensions, although perhaps not in a strict philosophical form. In short, the way a scientist describes science also involves arguments and reasonings that are devoted to other than just practical proceedings in their field.

Subsequently, the view that a specific person has of science is more than just a concise definition of the word science. In terms of identity, we should expect attributes of science that distinguish it from other domains, and from the same attributes we should expect both distinctions and connections between sciences. The individual scientist will have notions of internal and external demarcations, and again he will draw upon epistemological, ontological and social arguments to justify them. Although in milder form than in the academic debates of the last decades, ideas concerning realism and relativism show their faces in the words of scientists as they explain how and why science is how they perceive it to be.

The other general feature of identity debates that will be visible among scientists is disagreement. In all likelihood scientists from different backgrounds will have different views as to what science is. Of course, individuals from the same department may differ on science among themselves as well. However, the most notable differences can be expected between individuals whose practice, and therefore daily experience of science is totally different. Especially these interdisciplinary differences are interesting in the light of the discussion of disunity. Instead of determining if different practices as a whole are different or alike enough to justify either disunity or a common methodological denominator. my aim is to see if the people who are occupied with those practices differ in opinion enough on the identity of science to justify the label of disunity or unity.

The subjective approach to scientific identity also enables me to deal with the problem of power differently. Instead of an instrumental notion of power as the means of restraining nature or a structuralist notion of power as the restrictive effects of established discourse I will seek power in the relations between the individual and its environment. From the point of view of the individual subject we find power not in the progressive historical movement of the subjection of nature or society as suggested by respectively the prototypical realist or relativist identity of science. From the point of view of the subject power can be described as the possibilities and restrictions of individual action that a specific identity of science facilitates.

A realist scientist will describe science and the world it connects to differently then a relativist scientist, or a pragmatist scientist. Consequently each identity of science will open up the possibility for certain interactions each individual’s environment while neglecting other possibilities. The realist scientist will be preoccupied with the object of his studies, while the relativist may be occupied mostly with the politics of his science, giving both of them a different grasp on science and a different scope of action. This rather tailored use of the concept of power will be elaborated in the pages that follow. For now it suffices to say that I will try to explain the specific identity of science that an individual holds from the relation it creates between the individual and its environment.

The central question, how scientists themselves view science and if the differences in views between scientists from different fields count as signs of disunity, is three fold. It first asks how scientists view science, which I will try to answer from interviews in which I asked scientists about their views. Views, in turn, are the container term I use to capture scientists ideas, opinions, reasonings and arguments concerning science, which together comprise the specific identity of science upheld by a scientist. In the term view the notion of an identity of science has a subjective connotation by implying that there is a subject that holds the view. As suggested in the foregoing sections such views are more than a simple definition of the term science. Views of science involve social, epistemological, moral and ontological ideas associated with its nature and identity.

The leap from the view of a scientist to disunity suggests a second, hidden step. First of all the leap implies the discussion multiple identities of science. Secondly, it implies an analysis of views of science and their differences in such a way that they connect to the discussion of disunity. This paper will focus on two sciences, theoretical high energy physics and psychopharmacology, and the views that are expressed by members of both fields. Each chapter will go by the many aspects and dimensions involved in the views uttered by practitioners of the respective field. The collection of those views, the entire conception of science, will be treated as the shared identity that science has in the individual life worlds of the scientists from a specific field. In my analysis I will then try to determine what relation between subject and environment is suggested and facilitated by the respective identities.

Afterwards, the descriptions and analyses can be compared in a discussion of the disunity of science. In the chapter devoted to this discussion we the word identity will take on a slightly different role. Where the identity of science in the first two chapters is a subjective conception of science based on the views and utterance of individual scientists, the third chapter will directly engage *the* identity of science. The point of interest is if we how we can speak of an identity of science when among scientists there is a variety of such identities. An important argument which will be developed during the first two chapters in this respect is that the identities held by scientist are not a superficial reflection or an epiphenomenon of the practice in question but has a direct relation to it. The third chapter will discuss to what extent the reflections of the individual scientists support the idea of a united, and a disunited science. The findings in the two case studies will by compared to the descriptions of unity and disunity provided by different authors – notably, most of them authors and critics of *The disunity of science*. I argue that both labels have shortcomings as to their ability to describe scientific diversity, and I will propose a redefinition of scientific identity against the background of the identity debates on the basis of the scientific diversity visible in the case studies.

Different sciences, different views

The reality or relativity of scientific truth claims will not be the main focus here, the specific identity of science will also not be the topic of discussion, at least not until the very end of this paper. Instead, I will take off from a much more modest and arguably much more personal point of departure. The idea for this specific approach came to me after long term exposure to a multitude of ideas about science, and a recurrent witnessing of and participation in discussions about the nature and identity of science. The question that came up to me how scientists themselves, although probably not at all involved in any such debates, view science. For, practicing science on a daily basis surely fosters an idea of what it is. In addition, I wondered if scientists from different backgrounds, considering the different ways in which they practice science, might then also reflect differently on science. The idea that different scientific identities exist in different scientific cultures, as one might call them, connects these identities to the problem of disunity and to the question to what extent we can speak of a distinct scientific identity.

Therefore I thought it would be refreshing to go out and ask the scientists themselves. Of course, there are many sciences around, and many scientists to choose from. My choice fell upon theoretical high energy physics and psychopharmacology. Geographical proximity was a factor in my choice, so were accessibility, familiarity, and interest in the topics of both sciences. The more reasoned criterion for my selection was that both sciences featuring this paper should be ‘successful’; they had to be respected practices, hard sciences which made venerated contributions to our understanding. A true challenge to scientific identity would not come from a comparison between marginal fields, or one between practices of different standing, one exact, one soft. Such variety among sciences could easily be relegated to commonplace divisions in science such as the two cultures, pure and applied or even good and bad science. Hence the choice for these distinct specialisms; very different in practice, yet both considered as cutting edge and as working at the boundaries of contemporary knowledge.

However, the road from an opinion of a scientist expressed in xth minute and nth second of an interview, to a notion of scientific identity is not a matter of straightforward inference. Also the relation between a scientist’s view, the scientific identity upheld in a certain practice and the identity of science in general is one that can take many different roads. To clarify my own method and approach a look at an earlier, similar study is worth the effort.

One of the contributions to the *The disunity of science* is by Karin Knorr Cetina and also considers two sciences, their differences and their testimony to the disunity of science. In her essay *The care of the self and blind variation: the disunity of two leading sciences* Knorr Cetina compares experimental high energy physics and molecular biology, a comparison she later elaborated in a monograph titled *Epistemic cultures: how sciences make knowledge.*[7] The work of Knorr Cetina deserves special attention because its approach resembles the approach of this project. Knorr Cetina juxtaposes two sciences in an effort to determine the disunity among sciences. The difference between both of her case studies, she claims, testifies to the disunited nature of science. Her empirical research consists of many hours of ethnographic observations, interviews and the presence of multiple co-workers for long durations of time in two prominent laboratories. Her method reminds one of the early work of Bruno Latour and Steve Woolgar in *Laboratory life: the social construction of scientific facts* in which they also sat down in a laboratory and spent many hours of observing the life in the laboratory as if it were a strange tribe.[8]

Knorr Cetina's methods are of the anthropological/ethnographical kind and her descriptions aims to capture 'the rough build or empirical machineries at work in the two sciences.' Additionally, she does not 'draw on philosophical labels such as realism, instrumentalism, pragmatism, conventionalism and the like' but tries to give a 'richer description', she asserts that: 'if anything is suggested with respect to the philosophy of science, it is that there exists no "scientific method" that extends to all fields. The disunity of scientific practices can be witnessed on many levels: on the level of their orientation toward and treatment of signs, of their relation to themselves, of the forms of alignments they institute between subjects and natural objects, of their general approach to capturing and engaging truth effects in inquiry. It is also located in how these practices set up and include the referent–whether they attempt to form, with the referent, a common life-world or leave the work of dealing with the referent to an interposed machine.'[9] Knorr Cetina also her explanatory approach in this particular section of text. Concepts such as empirical machineries, truth effects and referents draw upon a thoroughly cultural constructivist understanding of scientific practice and reveal an important emphasis on semiotic interpretation.

The first major difference between my work and that of Knorr Cetina is the amount of time and resources available for the actual research. In the case of my research time and resources were much more limited. The empirical data gathered for the purpose of my project

[7] -Karin Knorr Cetina, 'The care of the self and blind variation: the disunity of two leading sciences' in: Peter Galison and David J. Stump ed. *The disunity of science: boundaries, contexts, and power* (Stanford, Stanford University Press 1996) 287-310.
-Karin Knorr Cetina, *Epistemic cultures : how the sciences make knowledge* (Cambridge Ma., Harvard University Press, 1999).

[8] Bruno Latour and Steve Woolgar, *Laboratory life: the social construction of scientific facts* (Beverly Hills etc., Sage 1979).

[9] Knorr Cetina, 'The care of the self and blind variation', 288.

consists of interviews recorded on a laptop computer and a number of publications by interviewees. Of course, the identities sought by me are the identities of science held by scientists, not so much the identitification of their entire practice. And in this simple observation lies the major methodological and conceptual difference between our approaches. Whereas Knorr Cetina aims to present the reader with a characterisation of scientific practices based on the internal workings of their epistemic culture, I locate identities in the eye of the beholder. In this light the difference in time and resources may not have been as much as a disadvantage as it seems. Moreover, a recurrent point of criticism on Karin Knorr Cetina's work is exactly the ethnographic process of identification that characterises her meticulous research.

As is also visible in the work of Latour, anthropological and ethnographic descriptions rely on a somewhat idiosyncratic terminology which translates 'in lab' observations to more general categories. As Stephen Cutcliffe elucidates Knorr Cetina focuses on 'the practices that go into the making of scientific knowledge', and 'the "cultures" that surround and give symbolic meaning to such practices.'[10] A critical commentary comes from Barry Markovsky who describes Knorr Cetina's presentation of the two sciences as a process of 'unrestained interpretive riffs' with the result that 'the imagery builds layer upon layer without the benefit of clearly defined terms or explicit chains of reasoning'.[11]

A somewhat more elaborate critique addressing a similar point can be found in an article by George Gale and Cassandra Pinnick published as part of a discussion with Knorr Cetina and Martina Merz. [12] Their critique boils down to an analysis of the use of language that follows from the ethnographic approach of presumably both Knorr Cetina and Merz. They distinguish a participants' language, which is that of the physicists observed by Knorr Cetina an Merz, an observation language used for ethnographic documentation, and explanatory language to deal with the technicalities of the physics involved. Their first critique is that the different languages are used together in the same sentences, and are mingled, while the status of each language is unclear. In short, Gale an Pinnick assert that it is left to the reader what type of description is offered by the work of the ethnographers; a native one, an explanatory one or an ethnographic one.[13] Another address concerns the language

[10] Stephen Cutcliffe, 'Epistemic cultures : how the sciences make knowledge' *Science, technology and human values* 26 (3) (2001) 390-391.
[11] Barry Markovsky, 'Epistemic cultures: how the sciences make knowledge' *Contemporary sociology* 29 (3) (2000) 556.
[12] George Gale and Cassandra L. Pinnick 'Stalking theoretical physisicists: an ethnography flounders: a response to Merz and Knorr Cetina', *Social studies of science* 27 (1) (1997) 113-123.
[13] Gale and Pinnick, 'Stalking theoretical physicists', 114, 116-117.

Knorr Cetina uses to explain to the reader what is done by the scientists in relation to the physical processes they investigate. In such explanations it remains unclear what the explanation refers to; whether the metaphors in use relate to the scientists practices, are meant to capture symbolic processes or actually describe the objects researched by the scientists remains unspecified.[14] In a similar fashion Latour's work has also been subject to criticism on his idiosyncratic vocabulary. In the famous epistemological chicken debate,Harry Collins and Stephen Yearley deemed Latour's depiction of science a superficial change of vocabulary, emphasising the inability of his anthropological description to escape existing categories and promote a better understanding.[15]

The ethnographic approach is based on translation, it takes practices and symbols out of their context and puts them in new symbolic order to make it relatable to non-natives that are part of the investigating culture. It captures the symbols of a culture, but it does not necessarily do justice to the experience inside the culture; in a sense it paints a reality over a reality. Such symbolic translation can lead to clear theoretical proposals, or leave the reader with the feeling he has to make two translations: between oneself and the work one is reading and between the text and the practise described in it. Whichever is the outcome, the common issue remains; there is a measure of objectification to the outsider view of the ethnographer or anthropologist, however sensible to the nuances of the foreign culture the researcher may be.

As a researcher on the topic of the disunity of science one can only look up to the massive contribution made by Karin Knorr Cetina. And the work of Bruno Latour is extremely elucidating in the way it connects to actual practices, and also in the way he uses his description to explain the intellectual stalemates that surround modern science. However, their work may also provide a basis to push oneself away from, hopefully in the direction of a better understanding. In this case that basis is the 'cultural approach'; it appears to describe cultures as they are and as they work, while the cultural perspective rejects straightforward description by emphasising the relative position of both observer and observed. The problem with such descriptions, is that they allows for only one end state; the symbolic machineries of Knorr Cetina, the collectives of Latour, or even the discourse of Foucault, they present a single picture of what a culture or community does full stop. What this paper tries to avoid is giving of the impression that what is written here describes the epistemic culture, that what the reader can take away from the following pages is the entirety of social, cultural, physical

[14] Gale and Pinnick, 'Stalking theoretical physicists', 118-121.
[15] H.M. Collins and Steven Yearley, 'Epistemological chicken' in: Andrew Pickering, *Science as practice and culture* (Chicago and London, University of Chicago Press 1992) 321-323.

and symbolic dimensions of theoretical high energy physics and psychopharmacology. Consequently, I would not dare to claim that the following pages describe *how sciences make knowledge.* Instead, I adopt a more modest approach which confines the problem of the identity of science to the relation between individuals and their respective, perceived environments.

Notably, the personal view that an individual has of science, the identity or meaning that science has in someone's life, differs from the idea of a cultural identity discernable as an exterior feature of a person, group or practice by a researcher. My starting point will explicitly be the former, even the bulk of the information on the organisation of the fields investigated in this paper are based on personal accounts. Interviews in which I engaged with scientists about their practice, their ideas on other practices, society and science in general make up the largest part of the empirical data. I also used other first hand accounts such as articles and lecture presentation sheets as reference material.

Obviously, the descriptions offered in the pages to come will also be constructions, and features will be introduced into the analysis that are probably not native to the scientists' life worlds. However, it is not concepts and generalisations that this paper is trying to avoid, it is the presentation of such concepts as an exterior feature of an entire scientific culture. In my analysis I will explicitly try to relate the collection of opinions, arguments, and descriptions uttered by scientists of science by scientists to familiar categories such as nature, realism, correspondence and truth. Such categories provide a common ground between the subjective identities upheld by scientists and the identities that are the recurrent topic of academic debate.

The most fundamental basis for generalisation in this paper will be the relation between subject and environment. I have asked scientists about their practice, about what a typical day at the department looks like, about their methods, about epistemological tensions in their methods, and hence about their attitude towards scientific claims. I asked scientists about their regard for other sciences, and about their view of science's role in society, its use, merits and necessity. I asked scientists how they view the concepts they worked on, about their ideas on natural reality. Their answers can be understood as more than just a description of science, but also as testimonies to the way science fits their idea of natural and social reality. Together these views describe the logic and the place of science in their world.

Of those who write on science we know their explicit views. Their ideas on what science is and how it operates are well articulated and most often pretty clear. We discuss their ideas and their underlying assumptions among the members of our field in writing and in speech. With the scientists we should not expect the same articulation. Of course, science being their daily occupation scientists also have a pretty clear idea about what it is they do. However, they rarely engage in the debates in our field, therefore we should not hold on to the expectation that the scientists interviewed here will express their views in terms of the problems we discuss in our field.

There exists a tension between the observation that there are always social, epistemological and other philosophical dimensions to a conception of science on the one hand, and the expectation not to find them as such during the interviews. On the one hand I have tailored my questions to draw out scientists views in terms of familiar categories and philosophical position. For example I have sought out obvious tensions between methods and knowledge claims to tempt the interviewees to take a position on both methodology and the status of scientific knowledge. On the other hand I will use a more informal understanding of technical philosophical concepts, loosening their analytic definitions so they can be accommodated by the subjective experience of the scientist. Of course, such alternative usage requires explication as well as precision.

Subjective philosophical systems

The alternative approach and the slightly alternative use of normally well defined philosophical vocabulary that goes with it require explication. Let us start with the already often mentioned subjective approach itself. What I mean with a subjective approach is that I intend to start my analysis from the experience of the individual. Hence I speak of views and conceptions, because views and conceptions suggest a person to hold a view or conceive of a notion. Whereas words like identity or nature convey a more stand alone, settled and a priori character. The identity of science that I aim to establish from scientists' views again relates to the respective scientists; it is the identity for them – the way they identify science. The same idea of identity can also be transposed to the many identities conceived for science from our field, and is illustrated by the quarrel between proponents of different identities. In this sense also depictions of science as objective or purely cultural only exist by the virtue of some scholar or philosopher attributing to science that objectivity or culture.

In common parlance the word subjective is often regarded with a measure of suspicion; it is associated with caprice, whim, unpredictability and sometimes danger. In its common daily usage subjectivity carries these connotations as the opposite of objectivity, such as associated with journalistic objectivity. In contrast, my understanding of subjectivity has a more philosophical origin in the sense of the human subject as opposed to the natural object. Of course, the more commonplace usage also relates to this philosophical division by maintaining that objectivity stays true to nature, whereas subjectivity means relying on mans own creativity. Note that the central question of this paper does not ask about the scientific identity as a yet undiscovered, or as of yet inaptly reasoned given, but asks about the perception individuals have of science. As a result my approach leaves the question about the nature of science as a whole aside for the moment.

The most illustrative explanation of subjectivity in the way I intend to use it comes from William Barret in a popular explanation of Martin Heidegger's Dasein. In an astounding feat of explanation Barret describes the abstract concept of Dasein in a relatable way: 'He is in the world because, existing, he is involved in it totally. Existence itself, according to Heidegger, means to stand outside oneself, to be beyond oneself. My Being is not something that takes place inside my skin (or inside an immaterial substance inside that skin) my Being rather is spread over a field or region which is the world of its care and concern', which he vividly illustrates; 'Think of a magnetic field without the solid body of the magnet at its center; man's Being is such a field, but there is no soul substance or ego substance at the center from which that field radiates.'[16] How true Barret's outline remains to Heidegger's original thought I cannot say, my knowledge of Heidegger's philosophy is far to limited to pass such judgement. However, the interesting point conveyed by Barret through Heidegger is that subjectivity can be depicted as a field that accommodates one's world rather than a matter of consciousness inside man's head.

If we speak of subjectivity in this sense we do not reduce it to the level of personal opinion implied by the more commonplace usage. Subjectivity, in the existentialist sense of the word, can be pictured as the individual life world, as experienced by a person. By adopting the idea of a sort subjective bubble of existence that each person experiences, we can speak of identities as the meaningful phenomena in that that bubble.

A subjective approach cannot answer, or rather does not ask what actual identity of science lies beyond the individual sphere of experience. Against the background of a supposed disunity of science it does ask us what the identity of science is inside that sphere. In Barret's almost pictorial explanation of subjectivity, the identity of science can be understood as the place of science the world of, in this case, scientists. Similar to the way discussion of the identity of science draws in epistemological, ontological an social

[16] William Barret, *Irrational man: a study in existential philosophy* (London, Mercury Books 1964) 194.

arguments, a scientist's subjective identity of also supposes views on science and natural reality, science and society, and on the way knowledge connects subject and environment.. Therefore, by asking about the identity of science we also ask about the world it connects to and the scientist's relation to that world. So what we are looking to establish, to phrase it in another expressive metaphor, is the floor plan of scientists' subjective field of existence to see were science is and how it is related to the rest of the map. Of course, 'the world' is far too broad a term in this sense. Concretely, we are looking for the way science is positioned in relation to nature, to society and the place of different sciences in that arrangement.

From the subjective point of view ontological statements can be considered as those statements that concern the world, nature or objective reality. Additionally, reality can have different characters, it can be viewed as materialist as opposed symbolic or idealist. In the same sense the processes that determine the world can be described as purely physical interaction as opposed to for example metaphysical processes. One scientist might view the reality as the result of strictly causal relations, while another sees it as the result of open ended processes or as arbitrary symbolic arrangements.

In the same subjective line of reasoning epistemology can be found in descriptions of how people know the world and what the status of that knowledge is in relation to the actual state of things. Statements regarding knowledge may be empiricist or rationalist, pragmatic, constructivist or otherwise. For example, a scientist's statements can be characterised as empiricist based on a tendency to emphasise sensory observation in finding knowledge. However, this observation is markedly different from deeming the scientist a philosophical empiricist, as the scientist may not truly conform to the technical definition of that label. In the same sense, the measure of success that a scientist attributes to the ability of knowledge to connect to objective reality can be understood in terms of epistemological realism and relativism. However such a characterisation does not automatically equate him or her to either one of the proponents in the Science Wars.

Epistemology, or the question of knowledge, plays a particularly interesting role in the subjective analysis of science because it directly describes a relation between the subject and the environment. Notably, epistemological arguments have a 'cosmological tendency' to imply a state of the world, the place of man as well as the way these relate. As such they summarize the relation between subject and environment on which a specific identity of science is founded. All the more, we can picture a specific epistemology – the reality, the subject and the distance and connection between both – as a subjective field of experience.

Arguably, the same ‘cosmological tendency’ is also the heart of many realist-relativist type of debates, and forms the fuel for high running emotions during such debates. Similarly arguments concerning knowledge and epistemology also play a prominent role in works such as *Against essentialism, Epistemic cultures*, and *The disunity of science*. Therefore the chapters on theoretical high energy physics and psychopharmacology will, besides discussing both the practical surroundings of the scientists in question and their views on science, reserve special attention for scientist’s views on knowledge.

I am aware that the subjective approach may strike the reader as idiosyncratic, however I hope to show its virtues in the light of the identity debates in the course of this text. Still, the particular understanding of subjectivity in this paper, and its rather pictorial understanding of a relation between the subject and its environment through individual conceptions may be rather vague. Some examples may make an understanding of subjectivity more familiar. The simple idea behind the subjective approach is that the prototypical scientific realist will have a different conception of reality than for example a culturalist or a network theorist. The former views reality as objective and natural and will connect science to that reality with a specific idea on how knowledge connects to nature. The culturalist views science as a cultural practice and relates the reality of scientific knowledge to culture. The idea of reality that accompanies the culturalist view is that, regardless of whether there is a objective realm, human reality is essentially cultural. If we picture such subjective realities as a field we can position science somewhere in this field in a specific relation to the subject, reality, society and so on.

Let us now consider a more elaborate example of the way such subjective relations and individual identities of science can be inferred from personal views. Consider example, the deficit model of science communication discussed by Massimiano Bucchi. The deficit model is characterises by the author as the often implicit assumption that science communication is the direct communication of scientific knowledge to society.[17] In the deficit model knowledge is just passed down to society which can than use it to its advantage. A number of assumptions are the basis for this depiction of science communication. The first assumption is that science produces objectively true knowledge. The second assumption is that knowledge remains essentially the same in the process of communication. The third is a

[17] Massimiano Bucchi, ‘Of deficits, deviations and dialogues. Theories of public communication of science’ in: Massimiano Bucchi and Brian Trench ed. *Handbook of public communication of science and technology* (London and New York, Routledge 2008) 57-60.

very tacit assumption that useful things can only be done with true knowledge and that society in inherently incapable of producing such knowledge.

As a result the deficit model depicts science communication as a matter of one way traffic, the traffic being knowledge. Additionally, science is depicted as a distinctly objective domain and society as distinctly not objective. This matter of affairs suggests an objective understanding of knowledge and a realist epistemology in the sense that scientific knowledge is an accurate description of nature. It also suggests a realist ontology so there is something to be accurately described in the first place. Society is deemed unable to produce such accuracy because it has no epistemological connection to the objective state of affairs. Society, however, is bound to the properties of the same objective reality that only science is able to describe accurately. Hence, the only progress in dealing with the world can be through scientific discovery, and the consequent communication of discoveries. The deficit model heavily relies on realist notions of scientific objectivity. In comparison, the type of science prescribed by the deficit model would be poorly supported by a constructivist conception of knowledge that introduces subjectivity into scientific practice. Neither would the deficit model be well supported by an understanding of reality as discourse, which would take away the possibility for accuracy and objectivity in knowledge.

An epistemological argument close to the one implied by the deficit model of science communication can also be found in Barry Markovsky's critique of Karin Knorr Cetina's *Epistemic Cultures*. Markovsky states: 'This book [*Epistemic* Cultures] is replete with generalizations to laboratory sciences, to molecular biology, to physics, and to all of science. Finally, Knorr Cetina demonstrates (if the reader was not convinced already) that at least some corners of her focal disciplines operate very differently form one another. I disagree, however, with the contention that this is evidence for the disunity of science—a presumption expressed early on in the book. I agree more with those who contend that the fabric of science is unified at a more abstract level, its pieces knitted together not by the concrete activities of individual or collaborating scientists, but by the underlying logic or the theoretical and empirical methods that they collectively employ. If that is so, then *Epistemic Cultures*, generates potentially useful insights about activities inside two scientific disciplines, but in doing so *overlooks the actual science that ties them at deeper levels* [not italicised in original print]'[18] In the end Markovsky's disagreement with Knorr Cetina is a matter of disagreement

[18] Markovsky, 'Epistemic cultures: how the sciences make knowledge', 556-557.

with the identity of science suggested by the latter, despite all the criticism on method and presentation mentioned earlier.

The views of scientists and science studiers on the identity of science are similar in the sense that they mobilise the same type of arguments and both involve claims about knowledge, nature and society. When hearing the views of scientists we should expect nothing short of personal philosophical systems comprised of epistemological, ontological, social, moral and metaphysical ideas. Summarizing these ideas as a relation between subject and environment is the goal of the first two chapters. Although perhaps on a more tacit level, we can distinguish among scientists, positions on the identity of science that resemble, and are comparable to the positions of our peers in the identity debates. But before we can discuss identities of science at length it is the psychopharmacologists' and physicists' turn to speak.

Case one: Psychopharmacology

Prelude to the first case: a controversy

Feeling dissatisfied with your life? Feeling down? Are you unable to enjoy the things you usually enjoy? William S. Appleton, M.D., and author of *Prozac and the new antidepressants: what you need to know about prozac, zoloft, paxil, wellbutrin, effexor, serzone and more* explains a depression may well be the cause of your distress. He warns not to quit your job when you are dissatisfied with it, or leave your spouse if you have grown distasteful of his or her company. You would be doing away with things that have nothing to do with the way you feel; a depression might well be the 'underlying cause'.[19] According to Appleton, many diagnoses of depression are missed exactly because people attribute their mood to their job, spouse or to other environmental factors. Meanwhile, 'depression is a treatable disease, and you can be helped.'[20] Many depressives 'do not see themselves as depressed' neither does their social environment 'realize they are ill.'[21] Depression can be 'destroying' your life without you even realizing it, all because you attribute your problems to your surroundings.[22] Moreover, not knowing you are suffering from a depression worsens its effects on your life. Luckily, medical science has devised effective remedies; third generation antidepressants. Appleton concedes antidepressants are hardly perfect remedies, let alone true cures.[23] However, they can effectively relief despondency, wittingly or unwittingly ignoring them will get you nowhere.

Somewhere else on the spectrum is Trudy Dehue, a professor at the University of Groningen in the Netherlands. Her research area is the history and theory of science, especially of the social sciences. Immediately one notices her work has a markedly different tone and style than Appleton's. In her recent work the *Depressie epidimie* (The depression epidemic) she makes an elaborate case for social responsibility in the use of antidepressants and the diagnosis of depressive disorders. Dehue distinguishes three common arguments in the explanation of steeply rising numbers of depressive people and the use of antidepressants.[24] Firstly, there is the argument attributing the rise in depressions to the fact

[19] William S. Appleton, *Prozac and the new antidepressants: what you need to know about prozac, zoloft, paxil, wellbutrin, effexor, serzone and more* (New York, Plume 1997) XIII-XIV.
[20] Appleton, *Prozac and the new antidepressants*, 9.
[21] Ibidem, 14.
[22] Ibidem, 13.
[23] Ibidem, 50.
[24] Trudy Dehue, *De depressie-epidemie : over de plicht het lot in eigen hand te nemen* (Amsterdam, Augustus 2008) 17.

we have only recently become able to diagnose it correctly. This argument is based on the idea that depression is diagnosed more often nowadays, because science has only recently discovered its real biological nature, and we now know what to look for. A set of critical arguments points at the pharmaceutical industry. This set of arguments explains the rise in depressions as a conscious effect of marketing policies by pharmaceutical firms, actually talking people ill for sales. A third line of argument points at the increase of government care in all facets of citizens' lives as the cause for the steep rise is in depression. This argument holds that people have been pampered and have become squeamish under the soft protective blanket of governments' social legislation. As a result people now lack the sense of responsibility to deal with problems themselves.

Professor Dehue does not do away with any of the foregoing arguments (however clear her own preferences for some arguments over others become in the course of the book), she merely wants to add an explanation to those mentioned above. She makes sure to press the reader her argument is an addition to, and not a replacement of, foregoing arguments. Dehue identifies an important cause for the rise in depressions in the rise of what she calls the 'performance society'. Dehue argues that modern psychology equates depression with earlier disorders such as nerve illnesses and melancholia, while the difference in meaning between such labels cannot justify such an equation.[25] Using this contextual perspective Dehue proposes the performance society as an explanation for the depression epidemic, because this type of society has given rise to a new formulation of deviance and depression. In a society where people strive for success, perfection, and an unflawed life any problem that interferes with the dominant ideal demands a quick fix. Depression in its current epidemic form is not the result of an increase in feelings of unhappiness, these have always been part of life. Instead, the rise is the result of collectively striving for perfection and not allowing ourselves to feel down or moody in any way.[26] Granted, Dehue's explanation potentially accommodates the three foregoing arguments, however it accommodates some more easily than others.

As one would expect Dehue's stance on psychopharmaca is quite different from Appleton's. While Dehue views depression as a concept rooted in contemporary socio-cultural arrangements, Appleton sees it strictly as a medical condition. Whereas Appleton treats depression as a medical condition, Dehue deals with it as a problematic construct and a societal problem. Consequently, Appleton promotes a psychopharmacological solution to the problem, while Dehue's solution to the 'real' problem is changing social arrangements by

[25] Dehue, *De depressie-epidemie*, 255.
[26] Ibidem, 256-260.

'realising a more social society'. [27] Notably, Appleton pinpoints the problem specifically outside of the social realm (don't blame the job or the husband), to him the fact that depression is a physical disease is the 'real' problem.

To Appleton, medical science is the way to betterment. Science, by finding the real problem, can hand you real solutions. An illustrating testimony to Appleton's view of science can be found in his discussion of a dr. Hagenson. Hagenson saw radical masectomy as the only cure for breast cancer, he saw it as the only way to make sure the cancer would not persist and spread. Nowadays, radical masectomy is no longer the only effective procedure available to doctors and patients. In fact, the procedure can be avoided in a significant number of cases. Appleton concludes from this sequence of events that: 'it seems he [dr. Hagenson] did not have the all evidence'.[28] Dehue does not treat scientific knowledge as straightforward accumulation of evidence. In contrast to Dr. Appleton, she views science as a principle player in shaping the socio-cultural labels that are put on feelings of unhappiness.

As a result, we are now left with two fairly different views on psychoactive drugs, depression, and on science. However, we should resist the temptation to choose a wrong and a right at this point. As we proceed to the field of psychopharmacology we should also avoid the urge to look into the scientists' words to look for the answer. What this short prelude shows, firstly, is that there is contention about disorder and therapy. And secondly, it shows that different opinions about disorder and therapy refer to different views of science; it shows how conflicting views mobilize science differently. Doctor Appleton mobilizes science as an objective player in the struggle against depressive disorder. Appleton's goal is to make people see there are drugs that can help them, and convince the unhappy to consult a general practitioner before they start turning their lives upside down. In contrast, professor Dehue downplays science's authoritative say in mental disorder. Her goal is to convince people not to label themselves as depressive when they experience feelings of unhappiness, consequently she makes a stance against taking the psychoactive drugs that affirm such labels. Her solution to her problem is to lower the societal pressures that urge people individuals to perform and to be perfect. Notably, such a societal change also requires science to alter the definition of depression. This short prelude makes us enter psychopharmacological field from the contended side of psychoactive substances. However, this contention does not ask us to force onto one of either sides, from the perspective of disunity it simply asks us how psychopharmacologists view science.

[27] Dehue, *De depressie-epidemie*, 260.
[28] Appleton, *Prozac and the new antidepressants*, 52.

The practice at Utrecht University

Psychopharmacology is a branch of the pharmaceutical sciences. The specific group I studied resides at Utrecht University, in a separate wing of the department for pharmaceutical sciences in the Wentbuilding at the Uithof university park. Not every university houses a specialized psychopharmacological research group, though most pharmaceutical science departments inevitably do some work in or related to the psychopharmacological field. The field features a significant number of established journals and can be counted as an established specialism. Nonetheless, interviewees at Utrecht University still described their group as relatively small.[29]

The field of psychopharmacology is an offspring of the general pharmaceutical sciences. In the Utrecht University case this shows from the shared building. Obviously, a considerable part of the inflow of academics has an educational background in the pharmaceutical sciences, so education is also a shared factor. Notably, the public debates concerning the pharmaceutical sciences; animal testing, research ethics, ghost authorship and the influence of the pharmaceutical industry, also apply to psychopharmacology. Psychopharmacology occupies an interdisciplinary position in the scientific landscape, and additionally, is also closely involved with societal domains such as healthcare, industry and policy.

In the case of psychopharmacology the most distinct academic connection is that to psychiatry. Whereas the general pharmaceutical sciences maintain strong bonds with general healthcare – hospitals, GP's etcetera – psychopharmacology distinguishes itself by involving itself with psychiatry, for the obvious reason of a shared research object. Dr. Groenink mentioned 'we are in the psychocorner'; psychopharmacology relies on psychiatric observations and descriptions to get a lead on the disorders that it tries to help remedy. [30] A publicly accessible exponent of this academic relation can be found in psychopharmacological articles and lectures, which regularly feature quotations from the DSM IV-TR – the diagnostics manual most widely used by psychiatrists and psychologists today.

Analogous to the role of the general pharmaceutical sciences in general healthcare, psychopharmacology's role in mental healthcare is the development of effective drugs. An important characteristic of the psychopharmacology as an academic field, as a practice and as

[29] L. Groenink 11.36 Names of interviewees will be abbreviated in the footnotes; Dr. Lucianne Groenink as LG and Prof. Dr. Berend Olivier as BO. The interviews are enclosed in digital form in the appendix. Times in the footnotes are in hours, minutes and seconds.
[30] LG (1) 3.30.

a professional culture is its allegiance to healthcare. The way the general field and our specialism contribute to healthcare and medicine is through knowledge of the chemical substances involved in illness and treatment. In the case of psychopharmacology these substances are mainly located in the brain, and the contribution of the field lies in the possibility to intervene in the brain chemistry that causes disorder and normalcy. The psychopharmacological approach to remedying disorder relies on chemical and physiological research, in particular in neurochemistry. Social exponents of psychopharmacology's specific make up can be found in its residence among the pharmaceutical sciences, and its ties the Utrecht University Hospital, neurology and psychiatry.[31]

However, the pharmaceutical network is larger than universities and hospitals alone. Another important player in psychopharmacology's network is the pharmaceutical industry. Hence, the industry is also an associate in pharmaceutical research. *The industry* is often wearily frowned upon in the public sphere; suspicion easily rises when you start making money of people who are ill. Academic critics such as Trudy Dehue support the argument that marketing campaigns by pharmaceutical firms have actually created psychological diseases by associating strong psychoactive drugs to normal feelings of unhappiness, effectively 'elevating' common distress to something like depression.[32]

When asked about the apparent conflict of interests when pharmaceutical sciences mix with pharmaceutical industries, Prof. dr. Olivier remarked that developing drugs – effective and save drugs – takes time and money. Having worked in the industry himself he explained that even as a researcher for a pharmaceutical company you are hardly troubled by the commercial aspects of the expedition. 'A pharmaceutical company cannot do without making money, it's not a government organisation. In that sense making money is not a dirty word. As an employee in that company, you work with many others on a project, you weren't troubled by that. What everyone does want is a medicine that is possibly new, has the least possible side effects […] and that it is also available and affordable, for a lot of people, of course […] there are few researches who what to make their more to expensive to be ever used.' [33] At the same time, the potential beneficiaries are far away at the point of substance research as well; between research and production there is still an extensive time of development and testing.

[31] BO 5.30.
[32] Dehue, *De depressie-epidimie*, 259.
[33] BO 2.00-3.00.

Within the Utrecht University group cooperation with industry is common. One of the reasons is the amount of time, money and effort that goes into the introduction of new drugs on the market. Pharmaceutical companies have the resources for such development, and the business model to maintain a viable pipeline of potential new drugs. Both these aspects of drug development are far beyond the means of any university department. Cooperation spans from having companies provide substances or prepared test animals, to doing actual research for a pharmaceutical firm.

However, regarding outside research projects psychopharmacologists – notably pressed by the same university demand to generate money flows from third parties as any department – are selective as to which offers they accept from industry. Although money from third parties makes up the largest part of the group's income, research always has to contribute to the group's own academic interests. Hypothetically, there is a lot more money to be made if the department were to accept every research offer from the industry. Although at times research only may contribute only marginally to the groups own academic interests, there is always a reciprocity criterion.

Amid the potential research offered by third parties and because of the group's reliance on industry money the need arises to keep research activities coherent. 'We realize we are a relatively small group, and that we can only maintain significance by focussing. But if you pick a focus that isn't interesting to society or isn't economically interesting, or is now but perhaps will no longer be interesting in two years time, you won't get any money in. So in principle we say, we are working on anhedonia – that you can't enjoy – that is in itself our core business. On the other hand, we are quite opportunistic. You have to get your finances from somewhere, and from university you don't get it. I have a project I've called psychotic depression; the depressive side belongs to the scope of anhedonia, but the psychotic part has more to do with schizophrenia. In that way I can develop myself in the area of schizophrenia while staying under the banner of anhedonia'.[34] The group is selective and needs to balance its financial and academic interests, at times with opportunism.

The main strength and goal of the psychopharmacology group at Utrecht University is the development of animal models for mental disorder. These models are based on experiments with small mammals subjected to different neurochemical conditions – for example by genetically modifying test animals, by inducing a chemical deficiency by surgical or chemical intervention or by administering substances – often a combination of methods is

[34] LG (1) 11.00-12.30.

used. Statistically significant results in changes of animal behaviour consequently form the basis for models describing neurochemical processes. Researchers' scientific interests go beyond pharmaceutical applications associated with their allegiance to healthcare' and involve a strong academic interest in general knowledge of the brain, its power and complexity. Notably, academic research strikes another note on this point than its industrial counterpart, where specific applications are more important.

Carrying out experiments with numbers of animals capable of producing statistically significant results requires considerable amounts of time, space, equipment, labour and planning. The type of experiments conducted by the psychopharmacology group – those designed to establish a link between a substance, behaviour, brain processes, and eventually disorder - are exceptionally labour intensive. Obviously, single experiments do not allow for conclusive models of neurochemical processes which involve more than one substance. Although experimental observation is gradually automated, considerable hours are allocated to the setup, maintenance and personal observation of large numbers of small mammals. Conditions have to be controlled, animals have to be observed as they go about their business and their behaviour is to be recorded and translated into statistical results. Data from different experiments is produced and gathered, mostly by PhD students, before it is analysed and communicated to a major scientist in a report.

Both people I interviewed at this research group were major scientists; their activities mainly involved organizing, analyzing and guiding research rather than executing experiments. One of the first things that immediately caught my attention was the division of labour within the research group. Compared to the stereotypical idea of the lone scientist in a laboratory, the psychopharmacology group reminds one of a commercial organization. The main function of major scientists can be characterised as being a centre for communication on all kinds of levels; guiding the PhD students that carry out experiments, guiding the analytical staff, synthesizing experimental results, formulating feedback, acquiring funding, organizing new research, devising experiments, maintaining ties with specialists from other fields and so forth. The scientists interviewed for the purpose of this case study hardly did any laboratory work themselves, their activities were mainly managerial.

A specifically important managerial function is directing research. As discussed above, there are numerous research offers from the side of industry. One of the functions of the major scientists is selecting the research that fits the group's own academic direction. Major scientists are gatekeepers, not only by selectively taking on outside projects, but also by shielding the group from hypes and publication trends.[35] Many of the related players have different goals then the group itself. The group 'doesn't work on substances [like firms], but on brain mechanisms.'[36] And unlike psychiatrists, the psychopharmacology group does not design therapies. Amid all the forces coming together in the world of psychopharmaceuticals it is important for the group to maintain its own course. As one interviewee described 'we are a knowledge generating system', which sums up the distinct identity of the group as a science. Furthermore, in relation to the foregoing, this characterisation also highlights that psychopharmacology's specific place in the network is to be maintained by the managerial decisions of the groups central members.[37]

The road from experiment to model is long and the road from model to application is perhaps even longer. Administering a substance to a group of animals, while keeping another group as control, might very well produce significant differences in behaviour between both groups. Nevertheless, significance does not immediately produce a conclusive model of substance behaviour on a neurochemical level. An insight in brain mechanisms requires numerous experiments, and each experiment requires careful and thought-through planning. Despite what artistic images in documentaries might suggest, there is no real way to directly observe individual reactions in the brain on a molecular level. The complexity and opacity of the brain forms a demanding research environment from the very start.

The main activity of the psychopharmacology group is animal experimentation and the development of models based on experimental data. However, the way from disorder to animal behaviour is not a straight line. Berend Olivier explained: 'We think [anhedonia] is one of the core symptoms of depression, not being able to experience pleasure from things, no enjoyment. And you can also model it in animals. Look, if you think of suicide, try to come up with an animal model for suicide, that's impossible. That's not a model you can ever use, and an animal can't tell you he's feeling really miserable today either, but you can tell from his behaviour. So for anhedonia, enjoying, we have all kinds of models and we can measure behaviour on it, and this proves to correlate really well with depression and anhedonia in humans.'[38] Still the way from therapeutic findings to conclusive experiments on neurochemistry is not a matter of simple creative translation, on the DSM Berend Olivier commented: 'you have to describe an incredibly heterogeneous clinical picture, depression is

[35] LG (2) 36.00.
[36] BO 3.30.
[37] BO 6.50.
[38] BO 7.00-8.30.

an incredibly heterogeneous clinical picture.' [39] He explained that, although neatly described in the DSM IV-TR, depression is a complex object which defies straightforward definition. Elsewhere he made clear in reference that you can't use the DSM on animals.[40] To bridge the gap between animal behaviour and clinical disorder, the latter is brought down to some general symptoms distinguishable in animal behaviour. The behaviour associated to 'core symptoms' can then be monitored in animal behaviour under varying neurochemical conditions to come to a general understanding of the substances involved in disorder and normality.[41] Anhedonia, or the inability to experience pleasure and to enjoy activities normally enjoyed, has become one of the main foci of the group. Anhedonia is a general symptom of depression; it is always in some measure present in depressive patients. As mentioned by the scientists anhedonia also describes a common symptom in other disorders.

'An animal can't tell you that he's feeling really bad today, but you can tell from his behaviour.'[42] The step from the general symptom in human disorder to the animal is made by linking behaviour to psychological states. 'To do the research you have to take something from the clinic to convert to your test animal'; Lucianne Groenink explained. [43] However, because behaviour tends to differ from subject to subject – as depressive disorder also varies from patient to patient – behaviour is to be brought down to certain actions which can be quantified and measured in large populations in order to attain statistically significant results. 'In essence, you want to get to those areas of the brain which are involved in anhedonia.'[44] And to come to such brain mechanisms the scientists 'look at behaviour, because we are, of course, working in the psychocorner'.[45]

Attaining significant results from test animals means monitoring the changes in their behaviour after administering drugs. The behaviour changes in the animals are then recorded on account of specific activities (for example the mean distance travelled or number of ejaculations) and subsequently processed and analyzed statistically. In this way experiments provide an insight in the way a substance generally effects behaviour/an animal's psychological state. Novel observations form the basis for the articles that are the group's main output. Synthesising experimental results and findings from earlier publication provides

[39] BO 8.00-9.30.
[40] BO 5.30.
[41] BO 8.00-9.30.
[42] BO 7.15.
[43] LG (2) 19.20-19.48.
[44] BO 9.37.
[45] Respectively BO 3.30, LG (1) 2.20.

the basis for models of the neurochemical processes involved in general symptoms of disorder.

The development of psychopharmacological models involves a rather composite methodology, a composition nicely summed up in the words clinical disorder, animal experimentation and behaviour. This method forms the conceptual detour, but practical straight line from disorder to experiment and back. Psychopharmacology's methodology nicely aligns itself with its place in larger scientific and societal networks. Its main objective is neurochemical knowledge for the larger purpose of neurochemical intervention; the instruments of therapy that should eventually follow from its models. For *disorder* psychopharmacology relies on psychiatry: 'We talk to different psychiatrists, with different perspectives. There's a difference between individual psychiatrists, one focuses on the psyche, another looks more at the biological substrate. And we try to pick out the things that are characteristic of depression, anhedonia for example'. [46] This first step belongs to the definition of the problem. The fabrication of neurochemical models of disorder requires the transportation of human disorder to animal disorder; a translation of disorder into general symptoms and general symptoms into behaviour. After the problem definition the psychopharmacologist is able to develop his or her working hypothesis from the link between general symptoms and behaviour. The empirical aspect of research consists of carrying out experiments to satisfy or falsify these hypotheses. The way back up this conceptual track paints a picture of monitoring behaviour under different neurochemical conditions, quantifying specific actions, then statistically analyzing these actions and their difference from control groups to say something about the relation between the conditions and the behaviour, an hence about symptoms of disorder.

The output of the psychopharmacology research group mainly consists of articles. Articles keep to a set structure of introduction, materials and methods, results and a discussion that guides the reader through the way an experiment was conducted, under which conditions and with what results, followed by a discussion of the resuts.[47] Even if one is unfamiliar with the technicalities of the substances used in an experiment, a general idea of the proceedings can be distilled from the articles. Articles have an exposé style reminiscent of Steven Shapin's

[46] BO 8.30-9.00.
[47] Hiske M. van der Stelt, Laus M. Broersen, Berend Olivier, Herman G.M. Westenberg 'Effects of dietary tryptophan on extracellular serotonin in the dorsal hippocampus of rats' *Psychopharmacology* 172 (2004) 137-144.

notion of virtual witnessing.[48] Additionally, the articles provide an illustrative an account of the composition of approaches and methods that come together in psychopharmacological research. For example, there is a specific section on the experimental setup and procedure, and a separate section for statistical analysis of observations.

The lectures posted on the Utrecht University psychopharmacolgy group website also nicely illustrate the psychopharmacology's composite methodology. Slides feature pages from the DSM, schematic pictorials of neurons and neurotransmitters, schematic cross sections of rat brains, and graphs and tables picturing behavioural differences between groups of small mammals subjected to different neurochemical conditions.[49] The relation between disorder, neurochemistry and animal behaviour is not straightforward. The last slide of the second lecture series tellingly reads: 'Conclusion/ summary:', followed by three bullets: 'The ethiology of psychiatric diseases is complex. Gene-environment interactions are probably important. We are far away from a real understanding of brain function.'[50]

There is an epistemological tension in the composite nature of psychopharmacology's method, tension caused by the different approaches that come together in one methodological toolkit. First of all, there is the contended definition of disorder; we find examples of this in the disagreement between Appleton and Dehue, but also in the divide between those psychiatrists that focus on the psyche and those that emphasize the biological substrate. The object matter on to which these definitions are projected is also indisputably complex; scientific understanding of the brain is very limited. Finally, there is the challenge of putting the conjunction of brain and disorder to the test. Disorder becomes behaviour and brains become the chemical conditions in the experimental environment. Individual behaviour is objectified into mean behaviour and statistical significance hints at correlations. Logically, one could question the conjunctions and assumptions made by the field; what defines anhedonia? Is it a purely chemical process or is it also dependent psychological choice of context? Does behaviour represent psychological experience? And all in all, what does an experiment with rats really say about depression?

During the interviews I found that psychopharmacologists were very aware of the limitations of their methodology to the extent that they share the considerations conveyed in the questions above. When asked a critical question about capturing behaviour in statistical notions such as the mean distance travelled, one interviewee humorously reacted

[48] Shapin, Stephen (1984) 'Pump and Circumstance: Robert Boyle's Literary Technology', *Social Studies of Science.* 14 (4) 490-502.
[49] http://www.pharm.uu.nl/psychopharmacology/olivier26sept2005.pdf, 27-05-2010.
[50] http://www.pharm.uu.nl/psychopharmacology/oosting26sept2005.pdf, 27-05-2010.

'hmmm…painfull point [laughter] well, that's of course the definition of, if how much you walk says much about behaviour and emotions. But still, everything we do here is validated. So to your common sense this may be far removed from, well, can you tell from how or how much an animal walks if he's afraid or not, or depressed or not. But from existing drugs we know that if you give an animal an antidepressant he will walk around more. So in that way you have validation. Look, it's probable not entirely optimal, but certainly says something about what we're measuring. And the statistics are there to make sure you're findings aren't a coincidence.'[51]

Strict research standards further diminish the epistemological shortcomings of what at first sight seems as a rather meshed together toolkit. Lucianne Groenink explained she did hypothesis driven research to make sure that measurements satisfy the question at hand. She explained a common fallacy is to take measurements that do not really connect to the hypothesis, but may only seem to do so at first sight. Also, all experiments are carried out blinded to thwart subject fallacies; from the observation of rat behaviour to the analysis of statistical data, researchers are withheld from the information about the precise experimental conditions that different test groups were subjected to during the experiment.[52] For the same purpose observers also go through a training course, before taking part in experiments researchers should be in a minimum of 85 percent agreement with other observers to ensure the consistence of observed results.

Views on science

The above gives a general idea of what psychopharmacologists do and what they are trying to accomplish. It shows main characteristics of the field's modus operandi and represents important features of scientists' daily practice. The image portrayed in the above can be understood as the working environment of scientists, based on scientists' perception of this environment. Of course, the image here is incomplete, following the footsteps of Karin Knorr Cetina or Bruno Latour one could fill many hours of research and devote multiple books on the practice of psychopharmacology. However, we are not so much concerned with the practice of psychopharmacology, our interest is the pharmacological identity of science. The practice comes into play insofar individual views are specific to the practice. As such, the practice also provides a basis for explaining views.

[51] LG (1) 3.20-4.15.
[52] LG (1) 1.00-5.00.

The concise sketch above contains the essential characteristics of psychopharmacology. Now we can move on to psychopharmacologists' views on science, and later their idea of knowledge and their epistemological position. Firstly I will discuss scientists' views on their own field, on science in general, and on other fields. Also the relation between science and society will be discussed. Later our analysis will zoom in on the level of epistemology by discussing scientists' views on the quality of knowledge and some larger world views.

The practice of psychopharmacology is part of a larger societal network committed to providing psychoactive drugs to psychiatric patients. Lucianne Groenink explained the goal of the operation was to 'attain a better understanding of how the brains function' but explained the more 'relevant goal [is] that if you know more about this, you can also develop new and better medication'.[53] When asked about what makes his practice a science, Berend Olivier brought up that in his area of work he found it '…exciting that, despite the fact that you *are* constantly working on a fundamental level, that from the sideline there is also a constant aspect that, hey, a lot of people are ill, depression for example. And that we try to contribute in such a way that it also…, that it isn't purely for the fun of it, but that we also do things that, for others in the future – then of course you are talking about ten, fifteen, twenty years – become applicable.'[54] The interviewees considered themselves doing fundamental research, however there are also applied aspects to their motivations. Notably, the group itself does not develop medicines, but the models they produce are developed as part of a larger pharmaceutical endeavour to produce new and better drugs. Psychopharmacology would struggle to fit the label of an applied science, its 'application mindedness' can be better understood from its healthcare allegiance.

We already witnessed some examples of psychopharmacologists' awareness of methodological limitations. Calling the notion of the mean distance travelled a 'painful point', identifying core symptoms, and mentioning that 'an animal can't tell you he's feeling really bad' testify to a reflective mindset concerning method. In a research environment where disorder, chemistry and behaviour come together as the basis for neurochemical models each individual step of the research process – defining general symptoms, defining symptoms in behaviour, measuring activity, statistical analysis – has to be weighed out against the other steps and methods. With multiple theoretical and practical backgrounds coming together the

[53] LG.(1) 0.39.
[54] BO 0.45.

implicit assumptions and limitations of each approach are laid bear. In all, the composite methodology results in a sober attitude towards method.

On the state of knowledge scientists expressed a similarly sober outlook. The final slide of the ‘Hedonia and andhedonia’ lecture series reads that causative relations in disorder are complex, gene-environment relations are *probably* important, and that the state of knowledge of the brain is far from true understanding. All these sentences convey modesty and stress the limits of knowledge in psychopharmacology and neurosciences in general, rather than anticipating its accuracy or universal validity . Note that these concluding statements are the final words any student taking the ‘Hedonia and anhedonia’ course takes home from it. In an elaboration on science and societal relevance one interviewee explained: ‘But pharmacology, the way we do it, we do the brains, of all the brain disorders we have, from psychiatry and neurology, we don’t understand a thing. We don’t know how they come to be, we don’t know their backgrounds. We act really learned, but we know nothing.’ This is a strong statement about science and the state of knowledge, but it is also requires a very sincere and critical mindset. He continued: ‘So we have to, that is, if we want to help humanity, and I assume we do, of course I don’t really know all of them, then we have to do fundamental research. Then we have to invest money, not all of it in the Netherlands, but that is of course why scientists cooperate internationally, to prevent everyone doing the same thing. Yeah, it are really small steps, that’s what we do in science, science should not pretend that we take really large leaps and that we’ll be there in ten years, really.’[55] Asked if we will ever be there he answered: ‘No, I don’t think so, brains are so complicated that I…. When I started around twenty five years ago I thought, a little while and we’ll have solved it. We really know far less then we did then. Then we didn’t have all these ideas that we know now, so we also have less…we were also unable to suspect what else was there […] they were simpler, if you start with something you start with the simplest theory. For example, feeding behaviour in rats; there was a saturation centre and a hunger centre. The saturation centre made you stop eating and the hunger centre made you start eating. There was a sort of balance between the two, which was presumably altered by the blood, by glucose and so on. It was simple, but it’s absolutely not simple, it is so complicated. Anorexia for example, is such a complicated brain disease of which we know so little’[56]. Professor Oliviers elucidation supports the idea of a sober outlook on method and is in line with the slide discussed above. In his statement he expresses modesty towards both the state and potential of (his) science.

[55] BO 25.00-26.00.
[56] BO 26.00-27.10.

Interestingly, what you know can diminish and what you do not know can grow with a change of awareness about your position in the world.

Following a short elaboration on the role of interpretation in the study of history, Lucianne Groenink made a bridge to her own field: ‘What I do, I’m sitting around with these numbers, deciding if they are significant or not, but how you interpret that can in some cases also go both ways. So perhaps it’s just a sort of illusory objectivity.’[57] This sequence of events may have had something to do with the fact that I had made my own humanities background clear from the start, accounting for some of the nuance and modesty. However, social desirability can not account for the statement as a whole; Lucianne Groenink’s words also convey a sincere questioning of the objectivity of her own methods. Even more her words suggest a mindset that maintains the possibility for questioning matters such as scientific objectivity altogether – in contrast to, for example, the notion that methods just vary in objectivity.

When asked the question what truth is to her, if it is something that is reached, achieved or agreed upon, Lucianne Groenink decidedly answered: ‘No, no, I’m very modest, the truth doesn’t exist at all, but I want… well I think you sometimes make certain assumptions that if this is true, then this and that and that should also be right.[58] Again, these statements display modesty to the extent that science, particularly in the case of psychopharmacology does not simply uncover truth, it is a much more careful and dented process of small steps towards understanding. The collection of these statements as well as the slides mentioned earlier all the more show that when reflecting upon (their) science the limits of scientific method are a prominent aspect of psychopharmacologists considerations. This modest attitude finds its practical exponent in the careful balancing of methodological components discussed in the foregoing section.

Another important aspect of an individual’s conception of science is the attitude towards different areas of science and academia. Any determination of which practices are regarded as sciences says something about what makes a practice scientific, it reveals someone’s personal demarcation criteria. Secondly, any hierarchy among sciences reveals more about these demarcation criteria by pinpointing the crucial attributes a practice has to posses to be considered a science. Hierarchy, by distinguishing between more and less scientific, implies a structure in the scientific landscape, for example by implying that all sciences are reducible to

[57] LG (2) 8.50-9.15.
[58] LG (2) 9.30-10.00.

one fundamental method, nature or science. Thirdly, in a more practical sense, the way scientists judge their ties to other fields reveals their tolerance and valuation of methods and approaches other than their own. Concerning the interviews I must say that scientists are generally really correct about other sciences, at times almost politically correct. Of course, when asked about the status of other fields relative to ones own by somewhat of a stranger few academics would immediately draw an outspoken picture of the ranks and picking order of the academic landscape. In all likelihood, when asked about your opinion about another field in an interview you would answer reserved and respectful, whereas in private or among peers you might express a stronger opinion. This social factor has certainly been present in the interviews, although I did find the interviews to be open and informal. During the interviews I have tried to compensate this social desirability effect by asking thoroughly but indirectly for opinions on other fields, by asking about what makes science scientific and if other fields, perhaps, complied with these criteria more then others. In the case of psychopharmacology I first asked about scientists' regard of related fields, such as psychiatry.

Psychiatry is an especially interesting relation because it is a contributor to psychopharmacology's composite methodology. But at the same it operates in a totally different way: it works with patients, it is part of the healthcare system, adopts altogether different methods in research and is generally not regarded as a natural science. Psychiatrists deal with the mentally ill, prescribe pharmaceuticals and decide, between them, at what point a condition is counted as a disorder. The Utrecht psychopharmacology group maintains close ties with psychiatry: 'Well, you use the DSM and psychiatrists as sparring partners and as continuous source. They provide us with information, we provide them with information; you are dependent of it, you can't say "well guys, I'm just sitting here in my little room, I think out some fancy stuff, and I don't really have anything to do with the outside world." No, we have intensive contact with psychiatry, with the [university medical centre] here in Utrecht, and also in Amsterdam and abroad'[59] Psychiatry provides a connection to the outside world, moreover, on a motivational level the field of psychiatry is also important in the light of their healthcare allegiance.

Berend Olivier stressed: '15 percent of all people has to deal with depression at some point in their life, sometimes severe, sometimes less severe, 15 percent.' Recalling Dehue's argument I proposed this also might have something to do with the definition of depression, he reacted: 'Yes also, but if you ask how much of that 15 percent really suffers from hardcore

[59] BO 5.10-6.00.

depression, then that's 3 to 5 percent, who won't get out of it without help, who might feel a bit better at times, but in truth are always depressive, well, and if you don't do anything, are very likely to commit suicide. Well, do we want that, no [and following an agreeing no on my part] no, so…'[60] After discussing the relevance and ethics of research in different disorders he made a heartfelt statement: 'I think, in psychiatry, if you see how much is suffered there, from depression, anxiety disorders, schizophrenia, there is a lot of suffering there. And it occurs in almost every family.'[61]

Lucianne Groenink also referred to this psychiatric objective when she discussed psychiatry as part of the composite methodology of psychopharmacology: 'It's somewhat opportunistic, we can put our articles in better journals if there's a clinical sauce over them. So ehm, that's what we do. Besides that, I think also in psychiatry they have come to the point where they say, well we call someone schizophrenic or depressive, but we should really let go of that whole classification. That is also why we choose for anhedonia. People say it falls under depression, but it also belongs to schizophrenia; someone who suffers from schizophrenia also isn't feeling happy. Well, I think it comes down to some really big steps, like why are we doing this research in the first place? Because I have the dream, well for me it's a really strong motivation, that it is relevant, and that you eventually can help people with it, although it might take 50 years from now. But that is always your point of departure.'[62]

Psychiatry goes about its business differently from what we encountered in psychopharmacology; this would be a rather obvious observation to anyone. Concordantly, there appears to be a tension between the natural scientific objective in neurochemical reactions and the social/medical scientific treatment of disorder in psychiatry.[63] If we take the standard diagnostics manual for psychiatrists, the DSM, as an example, it becomes apparent we are dealing with contended subject matter. First of all, the DSM has received a significant amount of criticism, not in the least from the academics like Trudy Dehue. Such criticism mainly focuses on the natural status attributed to the disorders described by it; in particular criticism focuses on the practical exponent of treating the psychiatric categorizations as natural, etiological entities. The same contention is visible in psychopharmacologists' attitude toward the psychiatric input in their field. When asked about her opinion of the DSM

[60] BO 27.10-27.50.
[61] BO 28.50-29.20.
[62] LG(2) 18.10-19.20.
[63] Although psychiatry is in practice a medical specialism it can hardly be separated from its social scientific cousin psychology.

Lucianne Groenink humorously commented it should be tossed.[64] At the same time she also explained she felt that psychiatry ‘leans towards the social sciences and is methodologically solid. The definition of how depressive a person is, in the end, is objectified by a score on a questionnaire. So,….I’m not really negative towards that’. [65]

We already witnessed Berend Olivier’s description of psychiatry as a sparring partner, a characterisation conveying both likeness and disagreement.[66] He mentioned the DSM and psychiatric categorization more specifically in an explanation of the goal of anhedonia research: ‘Well, you see, depression is, of course, the area where anhedonia is most present, but bipolar disorders, schizophrenia for example, other psychiatric disorders where it also plays part. So perhaps following this path you will come to ideas for developing drugs to treat anhedonia. So you say, when someone is anhedonic, for a long time, so a in a way depressed, then this is a way to treat it. So perhaps we are deviating from the DSM IV categorisation, which is rather artificial, and are forming new categories. If you talk to psychiatrists, they often speak… they speak in terms of the DSM IV, but they have their own ideas on illnesses’.[67] Lucianne Groenink made a similar statement, after her comment to toss the DSM: ‘No, well, I am really a very nuanced person, I think the categorisation is really practical, and people like to have a label of which they can say “I have this”, although it is never desirable to have a psychiatric disorder, but if you have something then it is nice to have something that says; this is what you have. […] And I think that in seven out of ten cases it actually is that simple, in terms of treatment. But in terms of research I think it makes more sense to look at symptoms, or what are nowadays called endogenous types, and that you will make more progress, scientifically, in this way then when you look at a somewhat artificially put together whole and think; now we need a test animal.’[68] Although confiding that the state of psychiatric knowledge on disorder is far from optimal, psychopharmacologists also made clear their field is dependent on the crossbreeding between organic chemistry and psychiatric healthcare. The statements above underscore this double sided stance in sometimes critical, often grateful and at times even praising words towards psychiatry.

Further removed from the own field, where social ties are looser or absent, opinions about the state of science become all the more interesting. Psychiatry is relatively familiar terrain for

[64] LG (2) 23.45.
[65] LG (2) 7.20-7.50.
[66] BO 5.37.
[67] BO 9.30-10.23.
[68] LG (2) 23.50-24.44.

psychopharmacologists, because major scientists have direct and regular contact with psychiatry. By moving to more distant areas of research we enter a more conceptual level of reflection on science. A psychopharmacologist may never have set foot in a physics lab, yet his conception and opinion of it reveal how he views science and what supports that view. Of course, the social effects of the interview discussed earlier, combined with the personal awareness of the limited knowledge of other fields, result in a collection of reserved comments regarding the scientific status of such fields. Interviewees were asked about their views on fields other then their own; preceded by questions about their own research standards I asked if other fields were perhaps more or less able to live up to their standards. Answers immediately sketch a scientific landscape, with psychopharmacology somewhere in the resulting stratification.

On first mentioning of the question what fields she regarded as softer or harder then her own Lucianne Groenink laughingly said: ‘I won’t comment on that’, pursuing an elaboration of some less exemplary practices in her own field instead.[69] And such reservation is common in scientists’ response to questions about other fields, also among the physicists in the following chapter. However, some measure of hierarchy remains present in their answers. Nevertheless, Berend Olivier also started out with a reserved stance on the status of different sciences: ‘Well, I think different fields contribute different things, but no one can allow himself to do bad things. Science just doesn’t accept that, within your field, you send in an article and if that’s bad it doesn’t get published. So I think every field has its own standards for quality to which everyone must abide. So I’m not afraid of that.[70]

We already witnessed the case of psychiatry and Lucianne Groenink’s characterisation of psychiatry being methodologically sound, *because* it leans towards the social sciences, implying stratification among sciences on methodological grounds. Following her abstinence from comments on other fields, some criticism on psychopharmacological fishing expeditions, and her valuation of psychiatry as a social science Lucianne Groenink explained: ‘The psychiatric research that I refer to, in the end, is very methodologically…. you can also do a case study in which you describe someone, and based on that you say then it must be like this or that. Then it becomes vague. … well, the other day I had someone over who did a PhD in history, the third or so- mucht Punic war or something. And I wondered, well you’re going to read all those old sources, in the end it is *your* interpretation of a certain text. And yeah, what is true then. But, well, in writing history that is part of the deal, that you know that it is

[69] LG(2) 6.15.
[70] BO 4.20-4.40.

an interpretation.'[71] Immediately this was followed by the earlier mentioned nuance of objectivity in psychopharmacology.[72] In a comment on a difference in the status of knowledge produced by different fields Berend Olivier established some hierarchy: 'I think that among beta oriented fields [natural sciences] it's all on about the same level, physics those kinds of fields, I don't have a real overview. Psychology is clearly another type of field, but in the beta direction I think it is reasonably good [the status of knowledge].[73]

On the status of the natural sciences, and the question if other sciences live up to scientific standards such as falsification and reproduction better, and if she believed that for example particle physics is harder and more exact Lucianne Groenink answered: 'I don't believe that at all… no, they have an altogether fantastic truth. They also just state something and then proceed to work around it. But in the end the question remains if their point of departure is correct.'[74] Asked directly about scientific hierarchy and reducibility she commented 'I know that image exists', I added it exists with the general public as well, to which she replied 'Well also here on the work floor; "That one has a PhD in medicine, so that's nothing", so I certainly feel that around, and sometimes I have it myself. But I also see PhD's from this department of which I think: apparently that wasn't quite it either.'[75]

In an elaboration on the difference between sciences, social sciences and humanities the conversation turned on philosophy, Lucianne Groenink ended with the following consideration on the status of philosophy; 'writing down or capturing thoughts seems easy and random, but I know, is preceded by a whole selection process. I just told you I don't think there is a hierarchy to it, perhaps in the end philosophy stands higher than physics.'[76] Considerable politeness can be read into this statement, especially when we consider part of the preceding deliberation: 'Yes, well those people….' she hesitantly started out, I pressed: 'You can be really honest, you can truly be really, really straightforward', she went on; 'well I am of course very much in my own little world, so my view of humanities and social sciences, well, depends on which people I happen to know, or what research you pick up along the way. But the research I see is often from the arts, the master neuroscience, about the processing language and so on, and that involves imaging equipment which instantly makes it look like my own research. But what I wanted to say is something like, that generally the people from philosophy have a clean and flawless line of reasoning, but I,.. so that,.. so I think

[71] LG(2) 8.00-8.55.
[72] See footnote 48.
[73] BO 4.50-5.10.
[74] LG (2) 10.30-10.50.
[75] LG(2) 11.00-11.37.
[76] LG(2) 16.15-16.30.

they are true thinkers…, but in the end, despite that, what they come up with remains, in my eyes, still kind of vague. Perhaps because it hasn't been measured.'[77]

So we are left with utterances of politeness in the interviews, which to a large extent seem to be the result of sincere respect towards other fields. However, in the sub sentences, the slips of the tongue, and the spontaneous fast replies we also witness latent forms of hierarchy, hierarchy which may be much more explicit 'on the work floor.' There are certainly demarcation criteria from which psychopharmacologists think of psychology as 'another type of field' or philosophy as 'remaining vague'. These criteria have to do with measurement, testing and empirical objectivity. Although objectivity is epitomized nowhere in the words of the scientists, and even exact fields just do reasonably well epistemologically, there are fields which are regarded as considerably less objective. The more mundane work floor references towards fields such as medicine support this idea of hierarchy.

The question remains, however, what the criteria are for stratification are. Now, we should not try to determine precise criteria, or try to find out what the rational decision making process is makes psychopharmacologists distinguish science from none science, and more scientific from less scientific. Firstly, such a rational model can not be recovered from the interviews. Secondly, we should not expect a rational model for the simple reason that the reasoning behind their hierarchy may not be strictly rational or formally conclusive, but may instead be implicit and emotive. Neither should we try to force the interviewees' answers into such rational models for the simple reason that these answers are in all likelihood based on implicit considerations rather than rational decision making loops. Nevertheless, we can establish some general areas and qualities that play a decisive role in arguing a fields' more or less scientific status.

Measurement and the possibility of measurement are important in distinguishing hard from soft and vague from conclusive science. Method and proximity to tangible objects forms another criterion distinguishing hypothesis driven science from 'see what comes out'- fishing expeditions, and beta oriented fields from psychology. When fields are viewed as methodologically sound their produce is taken seriously, methodologically sound in this sense relates to the ability to objectify results. In contrast, basing conclusions on individual case studies is regarded as questionable and vague.

An important aspect in the psychopharmacological mindset towards other sciences can be found in their ties to other and, judging from the criteria above, somewhat softer fields of

[77] LG(2) 14.00-15.15.

research such as psychiatry. Despite a fondness for hard and fundamental research, psychopharmacologists value the contribution of softer research. Words such as dependence and sparring partners convey both the difference between the fields as well as the need for both to cooperate. A sense of strength in diversity nicely sums up the psychopharmacologists' characterisation of the scientific landscape. Although, a sense of hierarchy between hard and soft sciences is also present.

Another characterising feature of psychopharmacology is its relationship with the pharmaceutics industry and its strong reliance on external funding. Pharmaceutical companies receive fair amounts of criticism in ethical debates ranging from drug safety to animal welfare. Additionally, companies involved with psychoactive drugs find themselves in the field of fire of critics such as Trudy Dehue. The latter's reproach against the marketing strategies of companies has already been mentioned. The public image of the pharmaceutical industry is not that clean to begin with and psychoactive drugs are arguably viewed with even more suspicion. Consequently cooperation with the industry does not immediately fit the ideal of an objective and impartial science.

However, both psychopharmacologists did not see cooperation with industry as a breach of scientific ethics. In contrast, interviewees were exceptionally positive about the influence of industry. Besides the obvious financial benefits the interviewees also described the effects on scientific research as conducive. In answering the question if cooperation with pharmaceutical companies interfered with research Lucianne Groenink explained: 'My personal experience is, and with about three companies I maintain intensive contacts, that I can do things that I would not be able to do if I would not cooperate with one of those companies. And I think it is wrong to say that if you're with NWO, if you have NWO money that you can then completely do your own thing, because there are hypes, I can send in a very solid and even novel proposal. But if this just happens to be the decade of genetics, and there's no genetics in it, well, then I might as well not send it in at all, because I don't have any chance, I'll be out in the qualifying round. So, I think its not really justified that the pharmaceutical industry gets so much criticism.'[78] Specifically of the scientific benefits she commented: 'I also think, I have been around the block, that in the end it [cooperation with industry] keeps your scope wide. You can think; but now I can really specialise. If you work

[78] LG (1) 12.55-13.50. NWO, the Nederlandse Organisatie voor Wetenschappelijk Onderzoek (the Dutch Organisation for Scientific Research) is the independent organisation that allocates state funding to Dutch scientific research.

on this system at one point and on that system at another, which you can use in your area of application, then it is enriching.'[79] Berend Olivier elaborated on the cooperation with industry: 'I personally think it's a good mix, a pharmaceutical company also has to be ethical and also has no interest in messy work. They have the substances and a lot of expertise, we don't have that and you can't get that in a university. We have a different expertise, and I think that mix works really well.'[80]

The possibility of a conflict of interests is recognised. Berend Olivier strongly criticised the process of ghost authorship where top scientists are paid to put their name as first author on company research. In addition he explained that normally funding does not interfere with the content of research, even more: 'what companies want very much, if you do research with them using a new substance and you find something entirely new, something that's potentially interesting to them, then they often have a clause [in their contract with the researchers] which says "we can delay publications for a maximum of three months to give us the opportunity to", for example, "write a patent". But it can not be that if all rats die during an experiment that they say, you can't publish that.'[81]

For the major scientist the role as gatekeeper also comprises the guarantee for research without outside interference. However, being a gatekeeper, in this respect, should not be understood as a purely defensive task. And from the point of view of the psychopharmacologists it need not be because pharmaceutical companies are responsible players themselves. However, in their choice of projects, as in their use of the DSM, psychopharmacologists display a measure of opportunism. Instead of passively guarding their department from the risks of company research, major scientists display engagement, welcoming cooperation instead of keeping it at a safe distance, or seeing it as a necessary breach of scientific ethics. Science, in the case of psychopharmacology, should not be seen as impartial, but as keeping to its own agenda.

A similar line of thought was visible in scientists' views on the relation between science and society at large. We already witnessed Berend Olivier comment regarding the cooperation with psychiatry '…you can't say "well guys, I'm just sitting here in my little room, I think out some fancy stuff, and I don't really have anything to do with the outside world"'.[82] However, this creed does not only apply to psychopharmacology and its relation to

[79] LG (1) 14.10-14.35.
[80] BO 15.00-15.23.
[81] BO 14.40, 17.00-17.35.
[82] BO 5.36.

psychiatry. Berend Olivier explained on the importance of science: ‘Look, if you have been in science all your life, than its part of your life. I personally think science is, indispensable in all facets of our society. Therefore, I think it’s a pity that science, in that sense, is still somewhat isolated, and that by some it is still seen a something of an ivory tower. I would really like to see scientists being put to a more socially relevant use. And that is not always their fault, but a theoretical physicist is also perfectly well capable of explaining what he could perhaps do for society and what kind of role that is. And if he can’t, then he can always say; “Well, I just do this because I find it so fantastically great and I have examples from the past of which I said what are we doing? What is the use for it? And which became useful later on.” So I think that science is very important. Scientists have to learn better how to speak a language that everyone can understand. That’s really important.’[83]

In a reaction to the fact that some might view the cooperation of science and industry as a conflict of interest and both should remain separated Berend Olivier stated: ‘We can no longer afford that in academia, there is not enough money, fundamental money to take such a position…´[84] Confronted with the position that sees the isolation of science as its guard against conflicting interest Berend Olivier added: ‘Societal engagement does not mean a conflict of interest. Isolation also carries a great risk, if you let go of every notion of societal relevance you run the risk of being shut down at the first major budget cut. Or if you really can’t explain why you are doing certain research...’[85] Engagement, it seems, is a requisite for the science that psychopharmacologists envision. Not only should scientists be more engaged just because it is morally preferable, science cannot afford to be isolated, societal relevance is integral to the justification of practising science in the first place. Societal relevance is a cue for good science, even the value of fundamental science relies on such relevance; ‘If we want to help humanity we have to do fundamental research’.[86]

This type of argument runs in line with scientists’ expressions that it is their aim to relieve psychiatric suffering – what I have called their healthcare allegiance in the above. At the same time psychopharmacologists do consider their work as fundamental research. We already witnessed Berend Olivier’s characterisation of psychopharmacology as a knowledge generating system, and his determination that he worked on a fundamental level, but just not for the fun of it.[87] Lucianne Groenink similarly mentioned the greater goal of psychiatric

[83] BO 23.15-24.30.
[84] BO 13.40-13.50.
[85] BO 24.30-25.10.
[86] BO 25.30.
[87] BO 1.00, 6.50.

relief, and explained, when asked what science with a capital S is: ‘Science, science…. science is curiosity, but….. it is also by…. something about truth comes up but that’s not completely accurate, science is discovering things by excluding other things.’[88]

On top of a healthcare allegiance we can also identify opportunism as a factor in embracing engagement, for example in their collaboration with industry, and in the use of the diagnostic criteria from psychiatry in publications. Even on the level of the daily practice we find a pragmatic attitude towards the approaches, categories and methods that come together to form psychopharmacology’s composite method. Both the coming together of different methods as well as the complexity of the research object has bred continuous awareness of the limits to what you can do with for example statistical analysis and animal experiments, and what the results say about neurochemical patterns. Nevertheless, psychopharmacologists consider their research as fundamental research. Science, in psychopharmacological terms, is associated with societal involvement. The practice of psychopharmacology, the organization and selection of research directly reflect this involvement.

Views on knowledge

Central to any view on science is a notion about what it does on the level of knowledge; what the status of the produced knowledge is and how this status is acquired. To recapitulate the example from the introduction, the deficit model of science communication depends on a notion of scientific knowledge as an accurate depiction of objective reality which can be communicated as it is, in its truth, and does not need alteration when it passes through different domains of human conduct. The idea of an isolated science underpinning the deficit model grants primacy to an objective realm with direct contact to nature, over the capricious subjectivity of society at large. The notion of science as an isolated objective realm, and the argument that places this objectivity above subjectivity, also heavily rely on the objective truth finding process that supposedly goes on at the core of the scientific practice, between science and nature.

As already discussed in the introduction views on science and society, or the stratification among sciences also involve epistemological, ontological, moral and metaphysical arguments. From the subjective point of view such arguments imply a specific order to the environment of the individual scientist. Arguments concerning knowledge, or the

[88] LG (2) 12.55-13.06.

reality to which that knowledge relates, give an account of the order to which the individual relates to in his conception of science. In the introduction I resembled the epistemological, ontological, social and moral convictions that come together in a specific identity of science to a philosophical system. With every part of the system describing a specific aspect of the world perceived by the scientist, the system itself gives something of a floor plan of the relation between the subject and its environment. We can establish the psychopharmacological identity of science from the position of science in that order. Utterances concerning the relation between science and nature, science and society, knowledge and reality suggest such a position by relating science to the subject's view of the environment. Of course, the map is not the territory, in other words scientists do not propagate well defined philosophical systems. Nevertheless, utterances with an epistemological character testify to a very basic and fundamental system of relations between object and environment on which science is founded.

It would go to far to as say that views on science spring from a clear cut epistemological position, or that we find with scientists well articulated philosophical positions similar to the ones we find in handbooks and monographs from our own field. Still, a number of views expressed by scientists in the foregoing paragraph already suggest specific epistemological convictions. And by zooming in on the epistemological arguments in scientists' views on science we find where science fits into the relation between scientist and environment. This specific position reveals the identity and meaning attributed to science by the scientists.

The notion of truth offers an important pointer in the direction of epistemology and has already been discussed to some extent in the above. Scientists' response to questions about truth effectively was that *the* truth – the ultimate truth – is unattainable. Following her statement that science is discovering by excluding options I asked Lucianne Groenink if science uncovered a reality, she elaborated: 'Well, yes, at that point I thought, hmmm, because discovering things… then the discovered should be the truth. Yes, the discovering is an uncovering, unveiling […], and at that point in time that is true. Let us keep it at,… now I do arrive at that truth again, but at this point it is our truth.'[89] Truth is formulated here as a matter somewhere in between correspondence, coherence and consensus. Correspondence comes into play in the act of unveiling, although the truth status of the unveiled is of a

[89] LG (2) 13.15-13.40.

temporary nature. The foregoing statement on discovery by excluding options, together with earlier mentioned statements on methodological soundness and solidity also convey a coherence type of understanding regarding scientific truth. Lastly, although mentioned in the margins of the sentence, ‘at that point in time this is our truth’ refers to a temporality of truth as the result of a communal decision or convention. Such a – in comparison to a correspondence notion – limited conception of truth regarding scientific knowledge also fits statements that *the* truth does not exist, and that we will never be quite *there*.

Additionally, both interviewees expressed reservations towards the current state of knowledge; ‘painful points’ in the relation between experiment and disorder, and the idea that one ‘knows less then he did before’ imply an acute experience of modesty in the face of the true state of affairs. Epistemological realism in the sense that scientific knowledge depicts matters as they ultimately are appears to be absent from the science of the psychopharmacologists. We can explain this reserved stance on the absolute truth of knowledge partly from methodological awareness as a result of a composite methodology, and partly from the perceived complexity of the brain. Furthermore, a statement such as ‘never being really there’, or Lucianne Groenink’s attempts to avoid referring to truth in her explanation of science suggests reservations toward the intelligibility of the world.

Notably, this somewhat limited faith in scientific explanation applies not only to the status of psychopharmacology but to science at large. Lucianne Groenink explained: ‘I believe that in every science you make certain assumptions, I gained this insight when I was doing some chemical analyses, because behaviour remains a case of how fast I move my fingers from side to side, if it does some specific thing or not. But if you measure concentrations, or substances, that will be really exact. Until I saw the real data [measurements of a certain substance], and depending on the way I drew my calibration line across the individual points it was fifty milligrams per millilitre or twenty.’[90] We already discussed Lucianne Groenink’s statement on physicists as assuming one thing and than working around that assumption. On her own field she commented in a similar fashion when posed with the Dehueian question if psychopharmacology also supported a specific understanding of disorder: ‘Well, we are in a loop, somewhere I also said you have specific models “how many metres does an animal travel,” and yes, well, look if I give it Prozac and then it travels somewhat less, so this says something about depression. In that sense we are in a recurrent loop, because you are looking for a substance that works better than Prozac, but

[90] LG (2) 11.35-12.10.

you validate the standard of your readings with existing substances.'[91] So in the process of research as well as in the state of knowledge after research the hand of man is taken into account.

To some degree the modesty concerning scientific truth claims, and the explanatory range of research results may have had something to do with the social setting of the interview. Because interviewees were aware that the interviewer was a science studier, someone with a critical eye for science, my presence may have created an atmosphere in which critical thought on science was perceived as welcome. However, cross referencing psychopharmacologists' use of, and attitudes towards different methodologies, their views on both their own, and on other fields, as well as references to the inherent complexity of research object suggest that psychopharmacologists allow considerable space fore unknowns and uncomprehensibilities in their view of science.

However, the foregoing compilation of statements should not be understood as evidence for a psychopharmacological critique of science of the kind we are familiar with from our field; the fact that they do not express epistemological realism in their views does not make them constructivists or relativists. Consider the following explanation of research by Lucianne Groenink in which she both acknowledges the methodological tensions in her field and downplays them in the case of her own research: 'Well, what is more characteristic of my research than of that of others here, is that I preferably measure categories that can also be measured in humans, that also have been measured in humans. I do research in stress hormones, and the regulation of stress hormones in rodents is identical to humans. And if I know that in depressive humans we find a lot of stress hormones, and we also find those in rodents, than we are talking. Because, indeed, my rat can't say "I don't feel like doing anything" […] I already had that about fifteen years ago, that I wanted to take measurement through which I can think of, and justify, a connection with humans. And that also goes for the behaviour I looked at in the article that you didn't read. Certain ways of processing information of which we know that these are dysfunctional in patients suffering from schizophrenia, and these are then also dysfunctional in my test animals. Apparently, these are such longstanding systems in evolution that they are, in any case comparable on a one to one basis.'[92] Besides reservation towards the potential of scientific explanation there is an equal

[91] LG (2) 33.00 -33.35.
[92] LG(2) 20.00-21-22.

amount of positive reflection on scientific accomplishments and abilities of the psychopharmacological practice.

The earlier mentioned focus on hypotheses that can be measured also conforms to a more positive understanding of scientific potential. Furthermore the commonplace use of scientific concepts reveals a measure of correspondence that sets psychopharmacologists far apart from relativists. The unexplained and taken for granted use of practice specific or more generally used scientific concepts suggests that the place of those concepts in an individual's perception of reality remains relatively unquestioned. Casually referring to identical hormone regulatory systems or evolution suggests these concepts are perceived as real, and the scientific description of those concepts as true in the sense that they describe what is really going on. In the foregoing statement we encounter such a correspondence relation at the point where identical stress hormone regulation is brought up as a direct connection between experiment and objective reality of disorder. Again we encounter a correspondence relation in the casual, and presumably taken for granted use of evolution as a natural reality against which the main argument on hormone systems seems fitting.

However, from a Deheuian point of view one could ask if in fact the identical regulatory systems really produce the same disorders and the same experience of disorder. And from this question one could also question if evolution truly has that much to do with the argument. Whether or not evolution corresponds with nature to the extent that these statements on hormonal regulatory systems in different mammals are valid is not the central issue at hand, however. The fact that evolution is brought up as an auxiliary argument simply suggest a frame of reference and a perception of reality in which specific scientific, in this case biological, convictions play a prominent part.

From the point of view of epistemology such a frame of reference is important because it characterises an individual's perception of reality, the reality which scientific practice must try to unveil or describe. On the level of personal epistemologies we also find ontological assumptions; convictions about reality distinguishable in larger world views. Ontological ideas and assumptions are inherent the the epistemological relation between subject and environment described by a specific conception of knowledge. In short, and idea of knowledge of the world relies on a notion of that world, its structure and its intelligibility.

We already came across a number of statements that insinuate ontological convictions in support of epistemological statements. Berend Olivier's statement that 'we really know far less then we did before' and also utterance from the last lecture slide that our understanding of brain functions is very limited can be placed under the banner of epistemology. However, if we dissect Berend Olivier's words, we also find it conveys a diachronic shift in intelligibility, a changing perception of reality yet to uncover. First brain processes were perceived as simpler, now they prove to be more complex, a proposition supported by the slide in the lecture. However, instead of finding out or proving the brain *is* more complex, the extent of complexity of the brain, and as Olivier mentions later of disorders such as anorexia, retains a measure of mystery. What lies at the basis of the perceived complexity of brain processes remains undecided. Berend Olivier's statement supports a more negative understanding of knowledge in the sense that it downplays the descriptive range of knowledge, and hence the ability to acquire true understanding of the brain.

A more positive explanation of psychopharmacological scientific potential from the side of Berend Olivier can be found in his explanation of his field's societal role. Berend Olivier explained: 'Well, we can, because we do pharmacology of the psyche, that is the brains, we can play an important role in for example warning […] and because you have knowledge of that area, and also weighed out knowledge, you can say 'you have to look out for this' or 'this is nonsense', because in general a lot of nonsense is used as well. I think psychopharmacology has an important societal role to play in that area. Also in prevention, I'm regularly attending symposia, in interviews, newspaper articles. Something happens and then I am called like: 'what do you think about this?'[93]

And in line with their view of science as a beneficial practice by virtue of its for society, psychopharmacologists do not distance their practice from society. Hence, there is room for engagement and room for contention. In line with their 'healthcare allegiance' we again find engagement to be a central incentive in the practice of psychopharmacology. Notably, the interaction between psychopharmacology and society as described by the scientists in question does not conform to the authoritative dissipation of knowledge suggested by the deficit model. Again we see how a specific view on knowledge facilitates a concordant view on science and society.

Another point of interest in the foregoing quote is an ontological assumption, this time concerning of the nature of the mind. The part of the sentence 'we do psychopharmacology of the psyche, that is the brain' again casually hints at a taken for granted scientific conception of reality. Although it might feel as if I am intellectually nitpicking at this point, the fact that the psyche is equated with the brain without question does require a specific frame of

[93] BO 20.30-21.25.

reference, and excludes certain other philosophies of mind. For example, the same expression would probably not be heard from a devout Christian who believes in a transcendent sole, or Trudy Dehue who pressures the materialist conception of the mind. The seemingly effortless conversion of psyche into brains shows how literal customary assumptions and commonplace knowledge become to scientists. In that sense the statement shows an important aspect of the psychopharmacological worldview.

An anecdotal but illustrative remark in the same direction comes from Lucianne Groenink who, when asked about her view of man and consciousness, referred back to a hypothetical doctor's appointment earlier in the interview and remarked: 'Well, I was just thinking, you are at that doctor's and you have a little conversation, are those people ill? Well, that's all neurochemistry of course. Because you have a conversation, well, without neurotransmitters nothing happens in the brain.'[94] Although, there is reservation towards the range of current knowledge of the brain, we can also identify a measure of ontological realism in statements such as these. Where understanding of brain process may still be in its relative infancy, the reality of psychopharmacology's goal – that neurochemistry is the fundamental determinant of psychological reality and disorder – remains largely unquestioned. This should not come as a surprise, obviously working on brain chemistry on a daily basis makes one highlight its importance. Moreover, the initial motivation to commence a study of brain chemistry in the first place also involves a belief in its importance.

From a commonsensical point of view the argument that neurochemistry is important in the functioning of the brain and thus in the psychological state experienced by an individual also makes sense. However, at the point where one treats neurochemistry as the beginning and end of brain function and psychological experience – although perhaps only casually – we encounter a distinctly less commonsensical perception of reality. Again, we would expect another view on the relation between neurochemistry and experience from a psychotherapist or Trudy Dehue. And although psychopharmacologists make clear distinctions between minor feelings of depression and severe clinical depressive disorder, both seem to follow from the equation of brain chemistry and the psyche. In the same line of reasoning one would not expect an outspoken humanistic or metaphysical view from the side of psychopharmacologists; in their daily practice the neurochemical conception of the psyche works in their favour.

[94] LG (2) 37.45-38.10.

Justified nitpicking aside, we should be careful of putting psychopharmacology in a philosophical box. The views expressed by the interviewees are more nuanced then a simple materialist conception of the mind. To the psychopharmacologists themselves such a view would be hard to justify from their pragmatic description of knowledge and the inherent mystery to the brain. Following a question on the nature of the human mind Berend Olivier replied: 'What am I supposed to say about that?' in an attempt at precision I asked if man is psyche, in his response he shed a light on his view on the nature of mind: 'You know, you have your brains, and in some way or another that is something that is specific to an individual, and we just don't understand how that works. Look, every human being has the same things, everyone walks, strolls, talks, but on those themes there are lots of possible variations. Heredity plays a part in that, your upbringing, all kinds of matters. Why does one person become depressive, while another doesn't? That has to do with a certain sensibility, but also if one has by chance been confronted with certain traumas. So you can't disconnect the psyche from the biological substrate, and the other way around. So in that sense, I think that good research in the biological substrate, a good insight in what happens there, can also provide an insight in the psyche. But to say that when I raise the level of this neurotransmitter I know exactly what happens, then no, it will never be that way.' When I concluded with that for him there was something separate from the biological substrate he elaborated: 'I don't see that as separate from the biological substrate. The soul for example, I wouldn't know how to do research on that. […] But we also know that something such as moral awareness for example, is certainly anchored in the brain. [gesturing at my forehead he said:] I can make a lesion in you that will make your moral awareness disappear. A biological substrate is needed for that, but how it is organised, and what else is needed, and what things from your memory play a part in it, that is huge. But that is also part of the enjoyment in brain research.'[95]

This attitude falls in line with psychopharmacologists' aim to contribute to those three to five percent that will not get through their depression by themselves, and those three out of ten who are not helped by a clear cut diagnosis during a doctor's appointment. It is the psychopharmacologist's aim to help those for whom depression is the result of deficiencies in their biological structure, Lucianne Groenink explained: 'I make an effort for the really heavily depressed people, and they are really ill. […] That is, the people that visit their GP and leave with box of antidepressants, or a good conversation, and are helped by that. Well, I don't really know about them, these people already receive good treatment and therefore they

[95] BO 10.25-12.30.

are not a really interesting group to me. But there are also people that, I think that also in the scale of depression, or that there is a scale in how severely someone is suffering from that illness. And there are people of whom I say, this is not a societal problem, this is really a brain disease. And this is also, I was just talking about those stress hormones, these are also the people in who you find that their stress hormone balance is disrupted. So I think it's of course not black and white but nicely grey.'[96] So as much as we are unable to blend the psychopharmacological view with the view of Trudy Dehue, we also find a discrepancy between the psychopharmacologists and Appleton. Although certainly leaning towards a materialist conception of their research object in their neurochemical approach, psychopharmacologists express a much more differentiated view on disorder than Appleton. Borrowing Lucianne Groenink's choice of words, Appleton is much more black and white in the sense that he exhibits a stronger tendency to locate depression on the body side of the classical mind-body problem. Additionally the foregoing explanations of disorder and brain function reveals the psychopharmacologists would not be as eager to quell all dissatisfaction medically; sometimes a conversation will do the trick.

Another interesting statement in support of this differentiated outlook on body and mind came from Berend Olivier in response to a question on religion. After the conclusion that science comprises an important factor in Berend Olivier's world view, I ventured the open door of asking him if he believed in life after death: 'No, no, that doesn't do anything for me. But that is, it's, you can also view that biologically, well that's your genes that you pass on to next generations, that is life after death. In that sense I do believe in it, but not in a god consciousness and those kinds of things. But that doesn't mean that people can't have that right, other people. Our brains have the ability to create something of a god consciousness. And that is something that is strongly influenced by upbringing, people are not born with a god consciousness, but are able to develop it.'[97] Upon asking he went on to elaborate such a god consciousness and explained that it can be very useful in fighting disease by giving direction to someone's life and through the support that someone experiences from a religious community. A paradoxical view from a neurochemist, perhaps explained from the sober pragmatism that we witnessed throughout this chapter: in method, in the attitude towards other fields and in the balance between financial and academic prowess.

[96] LG (2) 22.25-23.45.
[97] BO 29.50-30.40.

With god and religion we have surely reached the conceptual endpoint of any worldview, although there was surely much more interesting to discuss in the interviews. We have seen both positive and negative references to scientific knowledge and concepts alternate in the last sections. Additionally, ontological views were mixed with ideas on knowledge and views on science's role in society. The views surrounding knowledge are very diverse indeed. However, there is a something of a system, or rather a measure of coherence between the different views, discernable in the above.

An interesting starting point is the brain, it houses both the chemical foundation of psyche and disorder, as well as a complexity that restricts simple causation and predictive power. In the last statement by Berend Olivier this shows from the genetic heredity that takes on the transcendent role of afterlife, the brain's ability to produce a god consciousness, and his plain acceptance of both phenomena. The views discussed in this chapter can perhaps best be characterised as biological views of reality. Biological principles seem to form the backbone of the world as perceived by psychopharmacology, besides the casual and intentional uses of evolution, also the primacy of the biological substrate in determining disorder support this. If we acknowledge a measure of stipulation in the use of the term biological, we can set biological principles apart from mechanistic or physical principles by their lack of deterministic causation. In modern biology phenomena are often not considered to be directly caused by a singe factor, but are seen to develop from a collection of circumstances. The result of causation is not linear or exact; results may differ in regard to their context of development. This characterisation of biology is highly abstract and defective as a general principle for the description of that field at large. Nevertheless, in the case of psychopharmacology 'the biological attitude' does capture the unwillingness or inability to display epistemological realism regarding brains and disorder.

In line with the shroud of unintelligibility surrounding the mysteries of the brain we have witnessed statements that convey modesty, if not reservation, towards the ability of method to reveal the nature of brain functions and disorder. Another salient attitude in the same direction can be found in the role of psychopharmacology in society. The aim is to contribute to the treatment of the severe cases of psychiatric illness, in other words those cases that are most definitively biological cases. On this point psychopharmacologists set themselves apart from people such as Appleton who seek to expand the use of psychopharmaca rather than reserve it for a select group. The ultimate objective of psychopharmacology is to come up with a complete and true explanation of brain function.

Nevertheless, the hope for meeting this objective is continuously restrained by an equally fundamental conviction that the ultimate truth will remain beyond reach.

Knowledge and its truthful and accurate status are temporal and can even diminish as the field develops. In a reference to the history of his field Berend Olivier testified to a changing attitude towards the intelligibility of the brain as a result of new general schemata for brain processes. The sober attitudes towards the status of knowledge puts psychopharmacologists, combined with their societal engagement can be described as pragmatic. This pragmatic attitude towards science and scientific knowledge is clearly visible in psychopharmacologists emphasis on psychiatric relief over the universality of results. The same pragmatism can be identified in scientists' treatment of the different approaches that come together in psychopharmacology's composite methodological toolkit.

Also the opportunism we witnessed in the selection and presentation of research, such as the use of psychiatric diagnostic criteria in publications nicely aligns with a pragmatic look on science: from a strictly realist or positivist point of view such flirtations with the politics of science would be abject. As a general rule psychopharmacology, rather than justifying science from knowledge for its own sake, justifies science and knowledge from its use for society. In the same line of argument fundamental research is joined with societal engagement. On multiple occasions interviewees expressed how the different interests that come together in their area of work forms a requisite for durable research rather then an impediment.

Science to psychopharmacologists

Having juxtaposed and discussed all these different utterances on science, and having characterised these utterances as positions on method, demarcation, scientific hierarchy, the relation between science and society, epistemology and ontology we have gathered all the parts to formulate nothing short of a philosophical system encompassing the notion of science. However, philosophy is not the psychopharmacologists' trade, and although their views certainly relate to philosophical positions, their casual articulation during the interviews would not justify the presentation of a psychopharmacological philosophy of science. From the perspective of the subject the scientists views are a logical response to his or her environment. From the subjective point of view we can therefore substitute the philosophical system with the subjective relation between individual and environment in which science has its place.

Ideas on knowledge, society, industry and so forth, although not exclusively about science, but characterise the identity of science upheld by the psychopharmacologists. From the subjective point of view we are left with the task of putting all of these views together to determine what the world looks like for the psychopharmacologist and how science connects to this world. The views on knowledge and the nature of reality (on epistemology and ontology) discussed in the foregoing chapter not only describe to nature of reality, society, and science but also position the individual scientist in relation to that world. Epistemological and ontological statements are testimonies to the relation between the subject and its subjective environment because they concern the subject's knowledge of the world. Therefore our first step will be to explain the positions in the foregoing section as such a relation. Following this description we can turn to positions on demarcation, hierarchy, and method to see where the specific identity of science proposed by psychopharmacologists fits into their world. Notably, in a subjective approach the context or environment cannot be understood as a predetermined given, or an objective environment to which scientists relates. Instead, from the subjective point of view the context should be viewed as the context perceived by the scientist in question.

In the foregoing section I reviewed an outspoken relation between scientist and environment through knowledge. I described the psychopharmacological attitude towards science and scientific knowledge as pragmatic. Now we should discuss what this pragmatic attitude means from the point of view of the subject. The first point to recapitulate is that to the Utrecht University psychopharmacologists knowledge does not equal truth in the sense that it corresponds to objective reality. There is a distance between what is said in knowledge and the state of reality as such, in other words knowledge does not accomplish a direct or complete connection to nature. Obviously, in any attempt to gain knowledge resides the presumption that one does not have full knowledge yet. However, the distance between knowledge and reality is different in the case of the psychopharmacologists. Notably, knowledge is not only limited by amount (that not everything has been discovered yet), it is also limited in its ability to traverse the distance between what is known and what is actually going on. Truth therefore has a temporal character and refers to what is known at a certain time. And the state of knowledge at any time is therefore inherently limited in comparison with an epistemological realist understanding of knowledge. In the words of the interviewees this particular point has been explicated in statements such as ‘scientists reason in continual loops around their first assumption’ and ‘we know less then we did before’.

In a practical sense this ‘less-then-direct’ relation between knowledge and to nature was also apparent in their views on method. There was a similar distance visible in scientists’ views on the observational results in animal tests and the nature of disorder/brain function. Another example of this can be found in the way psychopharmacologists deal with the input from psychiatry. Current descriptions of disorder are used and incorporated in psychopharmacological research, but they are hardly taken as literal descriptions of the actual problems underlying disorder. Interestingly distance between knowledge and nature was also ascribed to other natural sciences. Physicists cope with the same assumption loops as psychopharmacologists, whether they realize it or not. In the natural sciences the state of knowledge amounts to a ‘reasonably well’, a mark that corresponds with the absence of any upward stratification. Consequently, the fact that a true understanding of brain function is still far away, as the lecture slide stated, is no objection against the fundamental nature of psychopharmacological research.

Downward hierarchy was apparent in the views of the scientists. Psychopharmacologists do not view other, perhaps more exact sciences, as better sciences. Their regard of softer, less exact fields, is that these are less objective. Fields like medicine, psychology, history and philosophy received cautious critical commentary, mainly for the limited ability of these fields to conclusively test their hypotheses in objectified observations. Political correctness left aside, psychopharmacologists do regard some other fields as less objective, and raise questions about the validity of the results presented by these fields. At the same time also exact fields got away with the mark ‘reasonably good’, so the difference between natural sciences and other sciences should not be stressed to a distinct segregation of hard and soft sciences.

The measure for the hardness or softness of a practice appears to be the ability to objectify results through method. The challenges to objectification in other fields were acknowledged. Generally, psychopharmacologists display respect for the accomplishments of other fields. Psychiatry offers an exemplary case of this; it is much softer –much less able to objectify observations – then psychopharmacology, but is still unquestionably appreciated for its accomplishments. Nevertheless, objectivity seems to be a measure for how scientific a practice is. Objectivity, to the psychopharmacologists, is not in the first place a matter of proximity to objective natural reality. Instead, objectivity is associated with the process of experimentation, with objectifying result and the precision of measurements. Psychopharmacologists's faith in the validity of their methods as means to determine what is true and not is strong. Although, truth should again not be understood as a fixed truth, rather it is truth in the sense of 'when this is true, then also this, this and this must be true.' Truth carries a strong component of consistency, whereas the ability to correspond to nature is viewed as limited. Hence objectivity means validated methods and results that can be objectified. Concretely, objective results are a matter of taking measurements that relate to the hypothesis and trained observation. As such some fields of research produce less objective results, because the do not use validated methods and precise measurements.

The epistemological distance between scientist and nature that is to be traversed by knowledge is an important factor in the psychopharmacologists' ideas about more or less scientific fields. Although it is certainly not simply a case of who describes nature best. There is also an ontological factor to the equation: it is in psychopharmacologists' ideas about reality that we find another obstacle to correspondence. In the case of their own practice the nature of the brain is considered an innate complexity. In a similar fashion there is ambiguity about disorder; it is the severe cases to which psychopharmacologists direct their efforts, but also in less severe cases or casual psychological problems the same chemical variables take part. There are social and hereditary aspects that play a role, an interplay that is itself hard to describe or to objectify. Also in the interactions between neurotransmitters plainly describing the process is out of the question. Although psychopharmacologists clearly explain disorder, psyche and the brain from physical or material properties, the processes that describe the behaviour of material substances are viewed as complex. Biological principles do block the road to materialist determinism. The emergent properties of the complexity of the brain for example, make it impossible to say that if you raise the level of this neurotransmitter this happens. Their sober outlook on scientific explanatory power is thus also facilitated by a rather non determinist conception of reality.

Their idea of truth and knowledge in relation to natural reality is also transposed to other sciences and makes for a modest outlook on the representational powers of science in general. Hence, other sciences are not distinctly harder of more fundamental, while decidedly softer fields do not drop in status proportionally to their ability in to objectively depict reality. The psychopharmacological idea of knowledge values use over correspondence. The psychopharmacological view diverges significantly from the deficit model discussed in the introduction. Science should contribute to society at large, and not by dissipating truths which others can take to heart, but by contributing to society through direct lines of cooperation. Social engagement by scientists, and the input by commercial and healthcare players in science were applauded by scientists.

Again in conflict with the deficit model, the intertwining of interests was not seen as a breach of scientific ethics. In an epistemological sense, science was not described as an absolute description reality. And because scientific knowledge is not viewed as an absolutely objective representation of reality by psychopharmacology, other then scientific interests are not seen as an obstacle in doing science. In contrast, psychopharmacologists themselves regularly referred to the commercial and political aspects of their own practice. Hence, to psychopharmacology, the ‘subjectivity’ of society is not an impediment to the practice of objective science. Even more, pharmaceutical companies were described as conscientious and capable researchers. In fact, to the psychopharmacologists society is an important factor to assure that scientific produce is useful. Science should not be for the fun of it, for the sake of truth alone, but should contribute to the lives of members of society where it can. In the case of psychopharmacology this attitude materialises in the aim to help the mentally ill.

Psychopharmacologists embrace engagement, in their practical experience society is often closer then nature. The psychopharmacologist is more likely to judge a scientist for social isolation than for defying an ideal objectivity. Science should be about contributing to humankind and not about finding truth for its own sake. Objectivity is not guaranteed by isolation from the whims of society, or staying true to nature. If anything objectivity is a matter of method. It is precisely in isolation that science becomes unaware of its own methodological loops and derails into the production of unquestioned, invalidated and useless results. Hence, it is precisely its connections psychiatry and the pharmaceutical market that allow psychopharmacology to be a fundamental science, and to solve questions. To psychopharmacology cooperation with society, engagement and fundamental scientific contributions are a package deal.

If we now shortly return to the prelude to this chapter we immediately encounter considerable differences between psychopharmacologists and Dr. Appleton and between psychopharmacologist and Trudy Dehue. Not only do we see a difference in the willingness to prescribe drugs to the psychologically distressed, we also find a different view of science and a different relationship between science and the social and natural environment.

Compared to Appleton psychopharmacologists display a resemblance in the physicalist approach to disorder, and in their faith in the ability of science to contribute to mental health. However, the conception of Appleton and psychopharmacology diverge at the point where psychopharmacologists limit scientific explanatory power concerning mental faculties and the proficiency of psychoactive substances to constitute a solution to any form of depression. In the same line of reasoning psychopharmacologist also describe the current diagnostic understanding of depression as artificial, a convenient a product of convention, while Appleton understands it as a distinct illness.

This attitude moves psychopharmacology towards the position of Trudy Dehue, who also strongly questions existing diagnostic categories. The resemblance stops at the point where Dehue argues that scientific methods are unable to create objective criteria of disorder and will always be subjected to social and normative understandings of mental normalcy and abnormally. Their positive attitude towards the ability of science to make a difference with fundamental research and validated results sets psychopharmacologists apart from Trudy Dehue. Moreover, psychopharmacology’s emphasis on the biological and neurochemical basis of disorder conflicts with Dehue’s view on the social construction of disorder, creating a bigger divide between psychopharmacology and Dehue, than between psychopharmacology and Appleton.

Interestingly, Dehue views scientific knowledge as a social construct and therefore as partial, whereas Appleton positions science close to nature, where it is just a matter of gathering evidence to come to a real solution. The type of reality both authors suggest and the relation between that reality and science through knowledge sets them apart from each other and from the psychopharmacologists interviewed here. The identities of science held by these three ‘parties’ differs on the basis of the position of science in these three subjective fields of experience. With Dehue science is positioned firmly in society, while nature is much further away; only perceivable through the socio-cultural produce of science . For Appleton, M.D., nature is much closer, it is tangible and interfering in nature fosters direct effects. These effects themselves are universal. Therefore, attributing social causes to natural problems such as distress is merely a diversion from what is really going on.

From the point of view of the subject there is a logic to each of the identities of science proposed here in the sense that views on knowledge, method, nature, society and so on fit together. Moral, social, epistemological and ontological ideas form something of a personal philosophical system that justifies the specific identity science has in a particular worldview. The psychopharmacologists' motivation to contribute to psychological healthcare, their engagement in other domains and fields of research, their pragmatic attitude toward interactions with other parties, their pragmatic view of knowledge and non determinist view of natural reality 'fit together' in a similar way. There is a logic to their specific arrangement of science and society and respectively science and nature that supports and justifies their specific idea of what science is and should be.

From the point of the subjects discussed above the identity of science is not just a matter of a well formulated definition, but involves epistemological, ontological, social and moral dimensions. Moreover, incorporating these dimensions as part of an identity of science provides substance to identities as well as a strong basis to distinguish different identities. The views of pscychopharmacologists on knowledge, reality, the use and status of science can establish a particular 'philosophical' position. The idea of knowledge for the sake of knowledge, useful by the virtue of being a true and accurate description of nature, finds little support in the views of psychopharmacologists. Although psychopharmracologists are inclined to explain scientific knowledge and disorder, morality and mind from a natural reality, they also display reservations towards the correspondence between natural explanations and the properties of nature itself. Their attitude towards the explanatory potential of scientific knowledge is modest rather than literal. Their valuation of science is strongly based on merit and use in relation to society besides accuracy in relation to natural objects. And in terms of the floor plan of psychopharmacologists' subjective relation to the environment; their attitude towards knowledge, natural reality, the connections to society, other sciences and even their statements on disorder or religion fit together. Together they facilitate interactions – practical interactions during research as well as social interactions with third parties – which can be characterised as pragmatic.

Explaining the subjective

The observation that views on different aspects of science are mutually supportive and fit together to in support of an identity of science is one step in our analysis. A second step is to give substance to the coherence between views. I have tried to accomplish this by portraying them as a personal philosophical system. Personal views that concern on epistemology, science, society and scientific hierarchy can be understood from the logic in which they mutually support each other. However, to work towards an explanation of identities of science we should take another step. I have positioned both the identity of science and the system of convictions that supports it in the subjective field of experience, between the subject and its environment. This paper is not a historical analysis, and the development of identities of sciences is not out main concern. Although it would be interesting enough to find how different scientific identities have come to be, both the scope and the main question of this paper ask no such explanation. However to understand identities of science from the context in which we encounter them, from a subjective perspective, requires us to it relates to the environment of the subject.

In *Against essentialism s*ociologist Stephan Fuchs toils with the same problem area as this paper, although from a very different angle and to a very different purpose. His work is an attempt to pull sociology out of what Steve Clarke calls 'the endless cycle of debate between realists and relativists' by developing a sociological theory of culture, and hence of science. Fuchs identifies the greatest pitfall for sociology in any such attempt is falling back on to the essentialism in scientific realism and postmodernist relativism.[98] Therefore he adopts a viewpoint in which culture is not an entity or discrete whole emanating from a core belief or attitude, but as the result social interaction. Culture or personhood is not the result of an inherent quality of a group or individual, but a product of the sustained interactions in the form of a network of relations. Adopting a sociological approach to our problem area he explains 'variations in social structure correspond to variations in cultures, at this point the sociology of knowledge comes in'. He elaborates that 'degrees of realism or relativism' have to do with a culture's confidence.[99] Confident cultures do not worry about their core assumptions, or may even be completely unaware of them. Cultures with a lower degree of confidence are, according to Fuchs, more sceptical and are divided about their identity. These less confident cultures are 'more prone to various degrees and shades of relativism.'[100]

[98] Fuchs, *Against essentialism,* 1-3.
[99] Ibidem, 4-5.
[100] Ibidem, 4-5.

Applying this typology to identities of science one could deem realism a sign of confidence, whereas relativism is a sign of questioning an doubt.

Applying Fuchs' explanation to psychopharmacology we find a confident, though not totally unquestioned scientific culture. The way that Fuchs formulates his typology and makes it applicable to cultures in general also makes it a basis for a contextual explanation of cultural differences. National cultures untouched or relatively untouchable by other cultures are not likely to reconsider their entire identity, the dominant way of life is successful and the successes speak in favour of that way of life. Open and connected cultures will be continuously confronted with foreign customs and unfamiliar points of view putting their own views and customs in a larger perspective and making them questionable in both the literal and figurative sense. Psychopharmacology resembles an open country, because it relies on many social transactions with other cultures such as industry and psychiatry. Therefore, the field is continuously negotiating foreign input, a process which forces it to continually question and recalibrate its own convictions. Adopting this sociological framework we could say that psychopharmacology, because it is required to deal with the interests and attitudes of foreign players, is also required to accommodate for example commercial interests or 'softer' psychiatric observations in its own philosophical system.

Using Fuchs' sociological approach we are forced take the environment described by the psychopharmacologists literally, or at least to suppose a certain environment to which the psychopharmacological scientific culture relates. Of course, few people would deny that when it comes to for example social connections to other domains there really is much activity in the practice of psychopharmacology. However, from the subjective point of view we should remember that it is not the actual but the perceived environment that will steer the actions and shape the views of individual scientist. In any case, the environment provides an interesting point of departure to substantiate the relation between subject and environment that follows from the logic that connects different views into a coherent system of convictions.

One particular contribution to *The disunity of science* by Mario Biagioli also presents the reader with an explanation of scientific culture from the interaction with its environment central. Taking Thomas Kuhn's paradigm as his starting point Biagioli uses an analogy to evolution to explain the rise and fall, and the incommensurability between different paradigms. He calls his view contingentism, a term by which he aims to emphasise that neither paradigms nor their success are a matter of a priori qualities but a result of successful adaptation to the socio-historic environment.[101]

Biagioli's explanation of paradigmatic development sets of by comparing the establishment of a new scientific paradigm to the evolutionary beginning of a new species. A new 'scientific species' is formed at the moment when it becomes unable to communicate with the paradigm it emerges from, just like a new species is formed when it is no longer able to breed back with the original species.[102] Pursuing this line of reasoning somewhat further Biagioli explains how new paradigms are successful – in the simple sense that they exist for a certain amount of time – because they fit their socio-historic environment. Other paradigms might wane and disappear or not even reach any lasting potential through their failure to adapt to the same historical circumstances. However, analogously to evolution, the success of a paradigm does not mean it is objectively better, that it is determined by an underlying rationality or that it, in the case of science, refers 'to the closeness between the physical world and the group's representation of it'.[103] To fit the environment in this sense means is to adapt to its demands, and when this adaptation brings a group's endeavours far enough from the practice of the original group incommensurability arises – the moment Biagioli deems it a paradigm.

Success or failure in the evolutionary race of scientific paradigms, then, in Biagioli's eyes is not measured by the success of connecting to the research object. Instead, 'local contingencies (rather than the hidden hand of rationality) have a lot to do with it.'[104] Regarding the views of the surviving paradigm he makes clear that '…intergroup justification of beliefs is not generally necessary. Somebody belonging to a given scientific group does not necessarily need to justify his or her beliefs to members of other groups. Simply those beliefs are the only ones he or she has.' Of the view itself he explains that 'a scientific tribe ontologizes its worldview not simply because it does not have access to alternatives, but also because such a worldview embodies (by the very fact of having survived) the result of the successful interaction between that tribe and the environment (both natural and social) with which it happened to interact'.

In the case of psychopharmacology we can use Biagioli's interaction model between a paradigm and its environment to further explain the pragmatism towards knowledge, method

[101] Mario Biagioli, 'From relativism to congingentism' in: Peter Galison and David J. Stump ed. *The disunity of science: boundaries, contexts, and power* (Stanford, Stanford University Press 1996) 196-202.
[102] Biagioli, 'From relativism to congingentism', 196.
[103] Ibidem, 197-200, quote on 199.
[104] Ibidem, 198.

other sciences and societal players as successful adaptations to the demands of the environment of its particular scientific niche. Psychopharmacology is required interact with commercial and scientific tribes. It is required to justify its own beliefs at least to some measure because of this interaction. In Fuchs' words this self questioning can be viewed as a very light shade of relativism in response to the input of foreign values. This measure of relativism, in the sense that scientific results are not understood a direct and completely accurate representations of natural reality, is captured in the psychopharmacologists pragmatic attitude.

In Biagioli's line of reasoning this pragmatism can even be explained as a necessity for existence; as a successful adaptation in an environment where highly different interests come together. From this point of view, the evolutionary niche filled by psychopharmacology more or less requires any group of people that would fill it successfully to be capable of taking foreign cultures as seriously as its own. In a place where the overlap between different interests and approaches constitutes the space offered by that place there is no room for epistemological absolutism. That is to say, any epistemological absolutism or realism would impair successful engagement with the fields constituting players. At the same time, this specific niche also requires a practice not to be to pliable; the niche being a separate niche requires the ability to retain independence from neighbouring domains. Retaining a separate identity asks for resoluteness and an individual sense of direction, a feature we have encountered explicitly in the group's pragmatism and, at times, opportunism.

Returning to the point of view of the subject, the discussion of Fuch's and Biagioli's view on the interaction between subjects and their environment suggests that a specific identity of science (as part of a certain culture) facilitates specific interactions the subject and its environment. A specific identity of science and the philosophical system that supports it point out priorities and important processes in that environment, it describes a specific locus of control and a focus of attention in the relation between subject and environment. In the case of the major scientists interviewed at the psychopharmacology group in Utrecht this comes down to a keen eye for the social environment of their practice as well as the practice itself. In their scientific world it is not only revealing nature that makes for good science, it is also acquiring funding, selecting research projects, and engaging with experts from other fields. Especially the major scientists are required to continuously balance financial and academic interests. The relation to natural reality is less direct, due to the limits of scientific methods as well as inherent qualities in nature. Then again, the psychopharmacologists' motivation in doing science is also not the revelation of universal truths per se, but the promise of gradual progress in the relief of psychiatric suffering. The emphasis on satisfactory result is visible in their conception of knowledge, their views on method and their valuation of science in general. In the eyes of the psychopharmacologists science is only justified satisfies societal purposes.

Case two: High Energy Physics

Prelude to the second case: a consensus

Wouldn't you like to know how everything around us works? An explanation for all there is to know? Stephen Hawking, one of contemporary physics' great minds argues we might be well underway towards a theory of everything.[105] Hawking explains that up till now we have found only partial theories. We have satisfactory theories about gravity, the strong and weak nuclear forces and electromagnetism. But we do not have a unified theory that explains all these forces from the same set of laws. However, Hawking cautiously hints we 'may now be near the end.'[106] He narrows down the future of theoretical physics to three options:

> '- There really is a complete unified theory, which we will someday discover if we are smart enough.
> - There is no ultimate theory of the universe, just an infinite sequence of theories that describe the universe more and more accurately.
> - There is no theory of the universe. Events cannot be predicted beyond a certain extent but occur in a random and arbitrary manner.'[107]

The third possibility is not in line with the goal of modern science according to Hawking. Even the theological arguments in favour of the third option do not hold in the light of the findings of modern physics. The second option is in line with 'our experience so far'.[108] We have uncovered more and more of the world, explaining ever smaller and larger phenomena. And, Hawking hints, there may be a limit to 'this sequence of "boxes within boxes"'.[109] Hawking points out that if a particle would exceed Planck energy it would cut itself loose from the rest of the universe. So there is a limit to the scale of observation and, so it appears, to the scale inherent to the universe. At some point, Hawking argues, we would be at a point where our findings hit a boundary and our theories converge.

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[105] Stephen W. Hawking, *The theory of everything: the origin and fate of the universe* (Beverly Hills, New Millennium Press 2002).
[106] Hawking, *The theory of everything*, 147-148.
[107] Ibidem, 160.
[108] Ibidem, 161.
[109] Ibidem, 162.

'What would it mean if we actually did discover the ultimate theory of the universe?'[110] Hawking states its discovery would 'bring an end to a long and glorious chapter in the history of our struggle to understand the universe.' Hawking also points out such a theory would 'revolutionize the ordinary person's understanding of the laws that govern the universe'.[111] He argues that, where one was still able to grasp the entirety of human knowledge in the days of Newton, the development of modern science has put that level of comprehension out of reach for us today. Once a unified theory becomes available, it could be summarized and simplified, and its outlines could be taught at schools. Everyone would then be able to grasp the laws which govern the universe, the laws which 'are responsible for our existence'.[112]

'In chemistry […] we can calculate the interactions of atoms without knowing the internal structure of the nucleus of the atom.'[113] The ultimate goal is to 'find a complete, consistent, unified theory' encompassing all partial theories. Hawking hints at a clear way down to the foundations of everything; from chemistry to the atom, from the atom to elementary particles, then perhaps to strings and beyond. Hawking's line of reasoning, *discovering* more complete and consistent theories, reflects both his great expectation as well as the dimensions of his scope. From the universal theory Hawking suggests he would know the laws 'responsible for our existence'. Hawking suggests a loop that knowledge of the external world makes to explain our own lives, besides explaining the structure of the universe. In effect, he assumes that both can be reduced to the same set of laws.

Hawking's quest does not only bring him closer to the ultimate physical laws. If we would find the ultimate theory, we effectively would be done in physics. Hawking explains 'we can ask about the nature of God even if there is only one possible theory that is just a set of equations rules and equations'. Taking away the fear of determinism he proceeds: 'What is it that breathes fire into the equations and makes a universe for them to describe […] Why does the universe go to all the bother of existing? Is the unified theory so compelling that it brings about its own existence? Or does it need a creator, ...'[114]

Today scientists are to busy developing new theories 'to ask the question why'.[115] And philosophers no longer ask the big question because they have not been able to keep up with the developments in science. Science has become a technical specialism and philosophers

[110] Hawking, *The theory of everything*,163.
[111] Ibidem, 163.
[112] Ibidem, 164.
[113] Ibidem, 147.
[114] Ibidem, 165.
[115] Ibidem, 166.

have been forced to reduce their investigations to the realm of language. Hawking comments: 'What a comedown from the great tradition of philosophy from Aristotle to Kant'.[116] The unified theory of the universe would undo all this, Hawking explains. If we know how the universe works we will again be able to discuss *why* it exists. 'And if we find the answer to that, it would be the ultimate triumph of human reason. For then we would know the mind of God.'[117]

These are no modest objectives; a universal theory of everything, explain everything so we can start wondering about why everything bothers to exist in the first place, and possibly even learning the mind of God. Is this how science should be? How it should set it goals? Or is Stephen Hawking being overly idealistic? I will not answer these questions myself, it is clear however that this philosophical, almost existential appeal of physics does not keep itself to the work of Hawking.

It is the way documentaries about the universe start on the National Geographic channel; 'Where do we come from? What is the faith of the universe?...' The same documentaries feature the mysteries and oddities of our universe such as massive black holes, dark matter, super novae, the warping of space, relativity and so forth – topics also pondered by Hawking in his many publications. The appeal of the revolutionary theories of physics proposed during the last hundred years has transgressed disciplinary boundaries and has found acclaim in many philosophical, spiritual and Weltanschauliche publications. The physicists' excavation of the universe has provided many, remarkably different authors with food for thought and has formed an inspiration to a striking variety of ideas.

In New Age publications physical principles such as non-locality or indeterminacy form the basis for, and a scientific justification of revolutionary views on the world. I came across one telling paragraph on the back cover of Benjamin Adamah's *Nulpunt-psychologie* (zero-point psychology), and for the sake of illustration I do not want to withhold it from the reader:

'Let us not fool each other, our world blossoms and thrives when our social climate is rooted in enlightenment, openness and creativity. It grows sickened and soulless when we are gripped by the manipulation of truth, stress and over-control. Durably implementing this simple realization in society is the only way to restart our human evolution fruitfully. Zero point psychology dissects the postmodern farce and shows its bizarre anatomy. In return this

[116] Hawking, *The theory of everything*, 166.
[117] Ibidem, 166-167.

book presents a new, holistic conception of the soul, resistant against a negative collective consciousness and founded on the best of the esoteric traditions, what the author describes as 'the Q factor'. An enticing book for those who want to carry their insight and consciousness to unexpected new hights at the eve of 2012. A dynamic conception of the soul that arms you against the 'Matrix' and puts you back in the real world.'[118]

Again no modest objectives, in the case of Adamah the basis for restarting a supposedly faltering evolution and escaping the postmodern condition is zero point psychology, which is based on the physics of zero point energy. For Adamah, as for many others in this genre, physics' formulation of zero point energy, or the lowest state of energy a quantum mechanical system can have, is proof that there is energy even in a vacuum. The idea that empty space is always energetic to many New Age writers suggests that everything is connected in a single energetic field. And because everything and everyone is constantly embedded in this illusive but all pervasive field that is our cosmos we can interact with our cosmos on a collective level to change our life at the core of its existence; on the level of the most fundamental energy.

Indeed, there is a lot you can do with physics. Its theories find their way into many seemingly conflicting spheres of society and authorship. But instead of stark controversy over its produce, physics' theories are a source of wonder and thought to most authors. Some thought is taken very seriously, while the worth of other ideas is recognized only by a small group of kindred minds. Both audacious physicists and prophesizing New Age authors employ the theories of physics to envision the future in them. And in both cases, current physics is often treated as a natural given. It is immensely rare for a New Age author to start his work with a refutation of established physics to form the foundation for his or her own ideas. At times an author in esotericism may suggest at a deeper and more profound truth behind physics. Nevertheless, unlike antidepressants the theories of physics are rarely criticized to make room for an alternative interpretation of reality. Instead, the concepts of physics are used to justify alternative interpretations.

Physics does not have much to fear from the world, it stands strong. Even the most alternative minded authors acknowledge its worth and celebrate its discoveries. Alternative works regularly carry titles denoting the work inside to be 'new physics' or a 'physics of consciousness'. A mix of physics and other academic fields of is also a common recipe for a book title, for example 'quantum psychology' or 'zero point psychology'.

[118] Benjamin Adamah, *Nulpunt-psychologie* (Deventer Ankh-Hermes 2008) backcover.

Strikingly, both physics and its alternative followers often refer to their own work as part of a long and great tradition. Stephen Hawking wrote several histories of the more celebratory kind, following the great men of science through their paces.[119] Many New Age authors underscore the equivalence of their view of reality to great Eastern traditions such as Hindu mysticism, Buddhism or Taoist thought, and indeed to Western science. Others celebrate mystical predecessors such as the Gnostics, Essenes or Meister Eckhart. For the popular physics writer, as well as the documentary maker and the New Age author a common way to phrase an introduction is to ask, rather enticingly, what lies 'behind' the physics we now know. And all of them seem to agree, and to expect, that what lies beyond will be even more enticing than what we already know.

[119] Stephen Hawking, *On the shoulders of giants: the great works of physics and astronomy* (Philadelphia etc., Running Press 2002) and Stephen Hawking, *God created the integers: the mathematical breakthroughs that changed history* (Philadelphia etc., Running Press 2005).

The practice at the Rijksuniversiteit Groningen

My investigations in physics took place in the presence and works of the theoretical physics group working on high energy physics (HEP) at the Rijksuniversiteit Groningen in the Netherlands. Similar to the psychopharmacology case I conducted interviews with members of the group and investigated a number of publications. The residence of the theoretical physics department is completely different from that of psychopharmacology in Utrecht. First of all it seemed a bit quieter here. Granted it was mid summer, but surely located at the Nijenborg 4 at the Zernike University complex, together with chemistry and other physics branches, there was bound to be a laboratory somewhere in the vicinity. However, there are no laboratories in theoretical HEP – let alone ones that house large numbers of small rodents. Stereotypically almost, the main workplace of the theoretical physicist is set up around a blackboard; the ideal means for performing algebraic operations on complex equations easily and reversibly.

High energy physics does exactly what its title implies; being a branch of physics it investigates the physical world, the adjective high energy designates its focus on high energy phenomena. The energies the members of the Groningen group work with, or rather try to comprehend, are those of the magnitude of cosmological phenomena, with the pinnacle of energy being the formation of the known universe in the Big Bang. These energies cannot be produced on earth, so the scale of the phenomena in question cannot be reduced to experiment. Therefore, HEP at the Rijksuniversiteit Groningen is a theoretical branch of physics. Surely, large experimental labs such as CERN and Fermilab can also justifiably be called high energy physics, however in the context of the Groningen group the adjective high energy refers to energies even beyond the grasp of such experiments.

Nonetheless, the theoretical branch has a symbiotic relationship with experimental high energy physics. Besides the LHC at Cern or Tevatron at Fermilab also astronomical devices such as the WMAP and Chandra satellite can be counted as empirical counterparts of theoretical HEP. The data from such projects is highly important to the theoretical work, as the interviewees pressed; 'Theory has to connect to the experiment'.[120] The relationship between experimentalists and theoreticians mainly consists of sharing findings through publications. Although practitioners from both branches keep track of developments in each other's field actual collaboration with experimentalists is rare. Still, to the theoretical

[120] EB 7.00, RB 13.35, MR 58.00. The names of interviewees will be abbreviated in the footnotes as follows: Prof. dr. Eric Bergshoeff as EB, Prof. dr. Mees de Roo as MR and Bsc Rob Bremer as RB.

physicists these connections are, although indirect, an elementary part of their practice. Each scientist hopes that his or her theory will eventually lead to new experiments and new data, or better still, that it will be verified by experimental observation. The interrelation between theoretical and experimental high energy physics is thus strong and necessary, but distant and indirect at the same time.

A similar relationship exists between HEP and astronomy. Astronomers and astrophysicist make observations about grand phenomena which would not be producible on earth. For example, gravitational lensing – the deflection of light by large clusters of mass such as a collection of galaxies – would be impossible to recreate on earth in terms of mass and scale. Astronomical observations of gravitational lensing tell the physicist something about gravity and the dispersion of mass in the universe. The other way around, an astronomer may turn his telescope those parts of the sky where unusual phenomena have been predicted by theoretical physics – for example the prediction of gravitational lensing by general relativity.

Professor Eric Bergshoeff explained the interplay between experimentalists (or empiricists) and theoreticians as digging for treasure on a vast beach: ‘It’s like there is a really vast beach, and somewhere there’s a buried treasure. An experimentalist can say; I just start digging here, and then I’ll dig over there, and then there and then I’ll systematically go over the beach so I will sooner or later come across something odd. And the theoretician can say; I’m going to make calculations, come up with a theory where it is. But in the end you need the combination of the two; that the theoretician says to the digger, I’d look over there, because I think that over there […] that there is that interaction, because they both need each other. The experimentalist needs the theoretician as source of inspiration, to motivate for a directed search and the theoretician needs the experimentalist because, well without verification he is just speculating.’[121]

Another academic kin can be found in mathematics, an undoubtedly important part of the practice of physics. However, mathematics is so integral to the study of physics that it can hardly be understood as input from a separate field. Of course, the mathematics and physics department have shared interests such as education, but real collaboration is sparse. Professor Mees de Roo mentioned there are occasions when they, the HE physicists, turn to the mathematical department when they encounter unfamiliar phenomena; ‘do you know anything about that? Are you doing anything in that area?’ In the same section he also pointed

[121] EB 35.58-36.39.

out the distance that divides both fields; 'can't you go any faster?' he remarked on how mathematicians deal with problems quite differently from physicists.[122] About the relations with other sciences in general he remarked: 'I think, at a certain point you need each other,…but not every day'. Although connected, interdisciplinary interaction is neither vital nor common in the practice of HEP.

The specific niche that HEP group in Groningen occupies in the scientific landscape distinguishes itself by investigating physical conditions far beyond experimental means. Because there is no way to directly verify ideas about such conditions the HEP group at Groningen University necessarily practices a theoretical branch of physics. This effectively means that the goal of the group is to formulate theories about the physics beyond experiment, to work out physical problems 'on paper' and devise toy models, again in written form, to see to what extent these theories hold under different conditions. Boundaries for theorizations are set by empirical findings, still this leaves ample room for a multitude of theoretical options to fill in the blanks.

The 'HEP base unit', as the research group is officially labeled, is more loosely organized than the psychopharmacology group in Utrecht. Instead of working in groups centrally organized under the auspices of major scientists, members at the HEP group in Groningen work more independently, more individually. Of course there is a certain hierarchy and frequent cooperation; department members work together, each even having a circle of regular coworkers from outside the institution. However, cooperation is mainly project based and individual members are relatively independent when it comes to their own research. Members of the base unit collaborate in different projects. Such collaborate projects are again quite compactly organized; articles are usually authored by two or three individuals. At the time of the interview one physicist explained he was working on a quite large project where six people were involved. In comparison, authorship of articles from other sciences such as psychopharmacology regularly amount and even exceed such numbers. The entire unit is also relatively small consisting of around ten members, not as big as the one in for example Amsterdam according to one interviewee. Then again, even the Amsterdam group is relatively small in comparison to departments housing other disciplines.

Aside from being compact, theoretical physics is also hardly capital intensive; salaries, office space, and in some branches occasional computer time comprise most of the costs. The

[122] MR 44.00-45.30.

largest part of these constant costs, the salaries and the offices, are carried by the university and at times there is additional funding from NWO for individual projects.[123] Of course, the experimental results that form the parameters for theoretical research do come from some of the most expensive projects imaginable. Experimental probing of high energy phenomena takes projects like CERN's LHC or the WMAP satellite. Such expenses, though, are international efforts largely independent of individual departments, let alone individual scientists. In contrast, work in the field of theoretical physics takes place in front of a blackboard, on paper or in front of a monitor.

Whereas the ties to other disciplines are sparse in comparison to psychopharmacology and the existing ties to other fields are considerably weaker, international connections to other theoreticians are all the more important. One interviewee described the international world of theoretical physics as 'a global village'; a collection of small communities.[124] Working together across institutional and national borders is common. Even more, international collaboration is a central dynamic in the field of theoretical physics; the average day starts with reading preprints from all over the globe. Research is not geographically bound, because it does not depend on the presence extensive laboratories. Even the high priced laboratories of experimental physics orbiting the earth or circling underground are largely international projects.

The goal of the HEP base unit is to develop theory about the phenomena beyond experiment, that is, beyond experimental means so far. The phenomena described by theoretical physics concern the shape and structure of the universe as a whole, the conditions right after the big bang and the fundamental components of the universe. It concerns the big, such as the dimensions and dynamic of the universe, and the small, for example the one-dimensional strings supposedly at the heart of all forces. Moreover, the big and the small have to be in accordance; the former has to follow from the properties of the latter. Of course, 'beyond experiment' is not the criterion for the study of high energy physics, but in the case of the Groningen group it does make it a necessarily theoretical practice.

Alternatively, for the theoreticians in question, there is also an inherent incentive to concern themselves with the gaps in established theory, and in a more general sense with the edges of the known. Eric Bergshoeff's treasure digging metaphor also suggests a distinct role

[123] NWO, the Nederlandse Organisatie voor Wetenschappelijk Onderzoek (the Dutch Organisation for Scientific Research) is the main subsidiary for Dutch academic research.
[124] EB 4.30.

and interest on the part of the theoretician; theorizing treasures already dug up is not part of the theoretician's job description. It is therefore not surprising that a sense of universality comes into play in the work of theoretical physics.

The key word in understanding theoretical physics is theory. As there is no way currently available to measure the dimensions of the space we inhabit or observe dimensions curled up between Planck length and the sensitivity of our instruments, the exploration of the possible structure on such scales is formulated in theory, in other words on paper or blackboard. Inherent to theory in HEP is the fact that it cannot be verified at the moment of formulation. Exceptions left aside, theory is wittingly formulated unconnected to any direct experimental reality. As such, theory transgresses the realm of the known and the verifiable and passes into the unknown. Theoretical physics is a matter of weighing different possibilities out against each other, seeing how theoretical options hold up in the face of established data, against varying theoretical conditions, and against established theory. By weighing out the possibilities the theoretician intends to come to the most probable description of the underlying principles of known phenomena. There is thus an instant epistemological tension in theoretical physics; on the one hand it aims at further understanding and knowledge, yet theory also inherently carries speculative aspect.

Experimental data is therefore highly important to theoretical physics; it narrows down the scope for speculation, making theoretical speculation relevant by excluding possibilities. One physicist stressed 'it is important to connect to an experimental reality', on the other hand if a physicist does not 'that isn't bad in itself, but you have to be aware'.[125] In the end the physicist wants to see the relationship between theory and reality confirmed, to get verification that the theory 'actually is a description of nature […] that it fits with what we know now.'[126]

Ultimately the physicist aims to connect to nature, to the structure behind established theory and knowledge. From the successes of existing theories physicists infer a more far reaching structure beyond the known. It is important to note that a reality beyond the known is a fundamental element in theoretical research which is easily aligned with the speculative aspects of its practice. Additionally, the HE physicist's speculation of greater structure also conveys an expectation of nature being, in resemblance of existing theory, structured and ordered.

[125] EB 33.45.
[126] MR 58.00.

The way to close in on the reality beyond, without being able to check what is really there, requires an epistemological nudge in the back. Strictly speaking, not every theory about the structure of the universe is true or to be taken seriously. At the same time existing theory leaves many questions unanswered, Eric Bergshoeff commented ‘Well, you see, the unsatisfactory thing is, you have a theory, so it works with certain formulas and it works in a certain way. And now it proves that, and that is what triggers us, those laws and so on, if we make the numbers in those formulas go to extreme conditions, it proves, at a certain point everything is blown up, it doesn’t make sense anymore. So the whole mathematics and the theory don’t work any more. And we just think that is…., as a theoretician you find that very peculiar, you can’t live with that. For an experimenter such a thing isn’t an immediate disaster, because the conditions under which theories collapse are often outside the reach of experiment. Then you’re talking about really high energies or really great distances. Nevertheless, what we try is, we start thinking: well, can we modify the theory, in a logical way, that at least the collapse is obstructed, cause we really don’t like that. And then we hope that, and this proves to be really hard, you can only do this in a few ways, if it could work in a hundred different ways then it would be completely random, but now we have found a way in which we can avoid these problems. And then, of course, you hope that you have a theory, but that this theory also has consequences for conditions under which you can perform experimental tests. So that you can indirectly see if you’re on the right track.’ [127] Whereas an experimentalist or engineer could remain untroubled by lacunas in the current theory, completeness and consistence are core values in theoretical physics. As such, theoretical research is a matter of logical deduction. This logical deduction takes shape in the mathematical formulae that are the basis for the understanding and presentation of model theories. At the same time the consistency and completeness that that is achieved with mathematics can also be understood as an epistemological justification of proposed theories.

Concerning the role of mathematics Rob Bremer explained ‘mathematics is really the main support of physics, some things,.., some things can’t be knit together with words, actually, that is the case most of the time. If you take quantum mechanics, at a certain point you get a model, which is a wave model of… phenomena, well, mostly it uses waves, and now you want to cover that with a nice mathematical interpretation, with nice integrals, nice functions. And sometimes you encounter these situations which, for example, you can’t carry out in practice; thought experiments, that’s what theoreticians occupy themselves with a lot.

[127] EB 14.45-16.25.

Then you can use a model, which is thus connected to reality, to make hypotheses like; what would happen if we'd have a particle tunnel its way through a wall for example. Then you can see with a mathematical model how it will behave. That gets fed back to experiments, so then you look at tunnelling, is that possible, well […] and then you look if it really conforms to the hypothetical case. And you keep doing this so you can expand the theory ever nicer [ever more elegantly]. So from mathematics you get really nice [/elegant] predictions.'[128]

Using the Dutch expression 'knit it together/ knit it around with words' Rob Bremer referred to a measure of consistency that ordinary language lacks in comparison with mathematics. Additionally, going through the process of interaction between experiment and theory Rob Bremer mainly based the role mathematics in theory building on its predictive power. Another interesting point that defies direct translation from Dutch is where Rob Bremer uses the adjective 'nice' in concordance with theoretical formulation. The Dutch 'mooi' from which it is translated conveys a more dominant sense of beauty and elegance than nice. Therefore, in line with the ideal consistency and completeness, the idea of elegant formulation conveys an almost Ockham-like ideal of simplicity.

And although it maybe a result of the moment and the at times almost philosophically phrased questions, somewhere along the line of Rob Bremer's explanation the distinction between mathematical precision and a connecting to reality became fuzzy, perhaps indistinguishable. In a similar tone Eric Bergshoeff spoke of 'the unreasonable effectiveness of mathematics' quoting and recommending me a book title.[129] And surely, theoretical physicists as well as many others find an epistemological reassurance in the unreasonable effectiveness of mathematics. In turn, Mees de Roo commented on the predictive powers of physical theory that in the field of HEP 'from 1975 onwards nothing has been done [in experiment] that was not contrived in advance' conceding he could think of only one counterexample.[130]

The prominence of mathematics is directly present in articles by the HEP group. Immediately, one notices the difference in style, structure and formulation from psychopharmacology; where the exposé style of psychopharmacology papers give the reader a sense of what is going on in the paper, what experiment has been done, what happened and how this counts as a result, a HEP article requires much more time for any notion of its content to form. Of course, experiments are not part of the practice of theoretical physics, so

[128] RB 17.50-19.29.
[129] EB 12.16.
[130] MR 34.00.

there is no need for an elaboration of the process that lay at the basis of the results. The abstract of one article tellingly started: ‘We consider two inequivalent truncations of the super D9-brane: the “Heterotic” and the “Type I” truncation’.[131] Theoretical physics papers consist of an argument, they propose a model by explaining the parameters in which it operates, the rules describing its behaviour, and determine the model’s physical consequences. The paper in question elucidated two arguments (two theoretical possibilities) within a string theoretical framework (Type I and Heterotic). Generally papers investigate the viability of theoretical models; Rob Bremer explained the papers as a consideration of pros and cons of certain theoretical options.[132]

The role of experiment left aside, also the content of psychopharmacological publication is generally much more tangible than that of a theoretical physics paper. Whereas the reader can keep track what happens to different groups of rats in a psychopharmacology article, the argumentative style combined with the mathematical formulation makes theoretical physics complex and abstract to the average and even advanced reader. Of course the mathematics is employed with the intent purpose of constructing a possible description of highly abstract structures and processes. In a sense, the concepts conveyed by the mathematical formulation also defy commonsensical understanding.

At the time of the interviews Rob Bremer was about to embark on his second year of master studies. Even with his graduate level background in the field he expressed some worries about the difficulty of the second year. Illustrating the abstractness and complexity of his field he explained one needs a lot of knowledge to do theoretical physics, and that ‘you are constantly working on the edges of your understanding.’[133] In comparison to a literature study such as mine he made clear that in a literature study ‘you read a book, and you have immediately progressed a great deal.’[134] Regarding his own studies he stated ‘you cannot really figure out’ the articles you have to read for master level studies on your own, because the level is much higher then you can tackle with your basic knowledge. When working along in the research group for the first time you are ‘thrown in the deep end’.[135]

A slightly different, and slightly critical statement came from Mees de Roo who explained that the tedious algebraic manipulations on the blackboard did not give him the feeling that he was doing physics. He added he would gladly have a computer do it, under the

[131] Eric Bergshoeff, Mees de Roo, Bert Jansen, Thomas Órtín ‘The super D9-brane and its truncations’ *Nuclear physics* 550 (1999) 289-302.
[132] RB 16.03.
[133] RB 9.00.
[134] RB 9.50.
[135] RB 15.00.

condition that he ‘could get it into a computer efficiently’, he added: ‘it becomes physics once you start thinking about it, when you have completed your model.’[136] Although mathematics is integral to physics, more or less making the theoretical practice of physics possible, it does not coincide with physics itself. Mathematical consistency forms the medium for concise and elegant physical theory. As a mode of argument and deduction in the world of HEP there is no substitute for it. The science of theoretical physics in the end is about the physical properties of the world, and it is in this relation particular to the world that we encounter physicist’s characteristic view of science.

Views on science

Because we now have an idea of the practice of HEP, its organisation, approach and its mode of operation, I will proceed to the views of high energy physicists on science and on scientific knowledge. In line with the previous chapter, the above can be understood as the practical lay out of HEP, and consequently as the practical environment that the members of the HEP base unit in Groningen experience on a daily basis. In the following we will discuss the views of the physicists in question regarding different aspects of their practice and science in general. Topics such as the status of their own field, other fields, the conduct of science in general and the role of science in society will be reviewed in similar to the procedure of the foregoing chapter. Additionally, I will devote special attention to views on knowledge in an attempt to explicate the personal epistemology and ontology of physicists as a relation to their environment.

Theoretical high energy physics is part of a physics conglomerate which also contains experimental physics and astrophysics and concerns itself with the fundamental structure and nature of our physical world. The high energy physics group distinguishes itself by taking a purely theoretical approach to the physical world. As pointed out in the above there are empirically set parameters, nevertheless, the field is only indirectly connected to its empiricist cousins. We have already witnessed theoretical physicists stress the importance of connecting to experimental reality. However, such connection is not a matter of instant verification, the theories of today will probably remain beyond the means of experiment for the time being. Consequently, amid the results of experiment and established theory there is still ample room for new multiple theoretical solutions to fill the lacunas. Also, there is still ample room for

[136] MR 51.00.

any of them to be proven wrong. The work of the HEP base unit therefore has a continuously speculative aspect to it. Therefore, an interesting starting point is the way physicists view their own science, and in particular the theoretical method that characterises it.

What becomes clear from reading HEP articles is that theory is generally not a neatly organised concise set of equations like one would encounter in a high school textbook. Compared to the established theories that makes for textbook material, the theory ‘under construction’ of HEP is more of a discussion of theoretical options. Eric Bergshoeff explains ‘… that is how a theory continuously changes, pieces of it are dropped, they prove not to be any good, and a number of techniques survive that prove to be very useful. You call it a certain,… for example the string theory of twenty years ago is not the contemporary string theory. It changes it, it has a certain interaction to it […] it is a soup, and it has to be stirred and kept warm, and things continuously have to be tried with it. And hopefully, it will really connect to the experiment, and some things will be useful then, but you also have to be realistic in that, as long as things haven’t been checked, undoubtedly a number of things will purely exist in our heads as figments of our imaginations’.[137]

Rob Bremer sketched the interplay between experiment and theory in an example regarding the Standard Model: ‘for example the decay of the proton is not possible according to the Standard Model, but they [physicists] are looking for proton decay.’ ‘To see if its possible’, I responded to which Rob Bremer replied: ‘Exactly, that is how physics works, in a sense. You have a theory and you search for objections against the theory,… and at the same time that is also a confirmation that the theory might be right: if you can’t prove the contrary, you have that much certainty, then you know how strong your theory is.’[138] In response to my question if theory, then, is a reflection of reality he explained: ‘Yes, because you have constant feedback. You have string theory, you’ve heard of that, super symmetry, there’s a lot of theories that are an extension to what we currently know. In fact, they can all be true, but if there’s new data from the LHC only one of these theories can be true. And that’s how it works all the time; you wipe theories of the table and you continue with the theories that conform to it.’[139] In this quite Popperian explanation of physics Rob Bremer – in line with Eric Bergshoeff – portrayed theory as a discussion of options, at least at the point where it is yet to be established. When I asked if this is what makes his field scientific, Rob Bremer tied

[137] EB 17.35-18.17.
[138] RB 23.30-24.00.
[139] RB 24.15-24.48.

theory and experiment closely together: 'What is science? It is really a continuous feedback to tru…to reality.'[140]

We already observed the intricate and integrated relationship between physics and mathematics in modern physics. On the one hand there is mathematics' 'unreasonable effectiveness', in other words its predictive potential. On the other hand we also saw testimony that mathematics is not what physics is about. In a discussion of the role of mathematics I asked Rob Bremer how he thought it works, he explained 'that's the interesting aspect about mathematics, you have for example quantum field theory, or really just quantum mechanics, which mathematically speaking is really elegantly constructed, but it does not explain anything about the underlying cause for why you can describe something with waves. That really pretty much is mathematics; you describe something. It is a really, really powerful tool.' When I asked if it is an alternative language he proceeded: 'Yes, well, what you really do is, you describe what happens, you capture it in a model. So you describe something with waves, and by using,… if you put it down really neatly in mathematics, you can start expanding the theory early on, and write experiments out on paper that have never been performed before, but that prove to be right if you verify them later. That's the power of mathematics, it is more than a language.'[141] Upon asking if mathematics then in some way relates to the proportions of objective reality Rob Bremer went on explaining how, for example, the laws devised by Newton were eventually expanded to Kepler's laws and many other laws. All followed the same simple three mathematically constructed rules, he concluded; 'in that respect I'd say mathematics is more….well, yes, you could describe it as the language of reality, perhaps.'[142]

When asked if mathematics is a way of conceptualising theory or if the mathematics perhaps writes itself Mees de Roo explained: 'Well, you could say of some models you construct that mathematics,… that they are more or less created by mathematics because you, for example, set certain preconditions. To take an example, if you say "I want the super symmetric version of a certain model", "the idea that there is symmetry appeals to me, and I want to know what kind of special properties such a model has." And then you construct that model, but it is really already determined by the conditions that you set at the beginning.

[140] RB 25.02-25.11 (In Dutch the cut of word 'truth' is phonetically close to the word for reality, which makes the slip more natural; 'Wat is wetenschap? Het is eigenlijk een continue terugkoppeling naar de waar…. naar de werkelijkheid.').
[141] RB 26.50-28.00.
[142] RB 28.30-28.55.

Those conditions are not a necessary result of you having a specific application in mind.'[143] In line with Mees de Roo's point of view Eric Bergshoeff pointed out that 'mathematics is used as a language and you really need it as well'. When asked if physics could be practiced differently, without mathematics, he explained that we are culturally conditioned to do research in a particular way and 'the only thing we can do is work from the existent knowledge […] to advance science'[144]

Although the importance of mathematics supersedes labels such as tool, resource or language, the physicists in question also stressed the importance of keeping the theoretical, speculative nature of mathematical descriptions in mind. Rob Bremer stated: 'Mathematics really is only a means, because from the point of view of physics you have to keep in mind that you *are* working on a model, and not on reality.'[145] Mees de Roo even described the tedious manipulations on the blackboard as 'still in the phase between mathematics and physics.'[146] And Eric Bergshoeff explained, in a clarification of his theoretical work, that he tried to come to a theory which also had consequences for testable conditions; 'to indirectly see if you're on the right track, and that's what I'm really trying to find at the moment. Can we in some way or another, either at the particle accelerator at CERN, or now also in astronomical observations find a hint, a fingerprint of that string theory , or that M-theory, that you are on the right track? Intuition says that without experiment you can never guess the answer, because now we are doing a sort of,… we take a purely theoretical approach […] and then hopefully that's how nature is structured. I'm afraid that it is never that simple, nature is always stranger than you think. It's the best you can do at the moment, and we hope, of course, that we will find clues in the future experiments of the LHC or the future values of the Planck satellite, that we at least, that a the theory we are currently using, that a number of ingredients of it are actually used by nature. And my intuition tells me that what we are doing now isn't the truth, it can't, you really need more information for that, but we use, we find bits, useful pieces that will become part of a future theory.'[147] So the theoretical models that physicists work on are certainly acknowledged to contain a large speculative component.

However, concerning the notion of theory theory also another less speculative use the words can be found circulating among HE physicists. In his 2002 inaugural speech Eric Bergshoeff states 'we still do not know what M theory is exactly'. He proceeds: 'our

[143] MR 48.45-49.39.
[144] EB 12.00-14.00.
[145] RB 29.11-29.26.
[146] MR 53.30.
[147] EB 15.55-17.40.

knowledge of M theory, at the moment, is insufficient for making unambiguous predictions about the Standard Model which are testable in experiment'.[148] Now, M theory certainly is not a theory like any other; it is not a coherent set of equations like Newton's laws of motion, in fact M theory has not even been written down yet. Confronted with Bergshoeff's statement and the question what theory then exactly is, Mees de Roo laughed a bit and then explained: 'Well, M theory is something really strange actually. Look, string theory in a certain sense is,…,it has never, is still not really thoroughly worked out. But there's this starting point that everyone calls string theory and because there are different versions of this string theory, at a certain point, one of the great leaders of string theory […] at a certain,.. in any case he showed that all these things were related. That in itself is very special, and he also put together some clues that there is this overarching structure from which all this follows. And this is something that typically receives a lot of attention for a number of years, everyone thinks that if we pull through a little while longer we'll find out what it is.'[149] Although perhaps the result of some fad in physics, M theory is still treated as a reality, while it still very much unproven. Even to theoretical standards it is hypothetical; M theory is still awaiting formulation. What is interesting about Bergshoeff's statement is his mentioning of M theory *as a theory* while it is actually still beyond any, even theoretical grasp. M theory is expected to be there, and this expectation is based on precisely the faith in established theory.

We can identify multiple understandings of theory in HEP, we could divide them into; established theory, theoretical work in progress and anticipated theory. Each of them is used in a distinct relation to physical reality. The first understanding of theory is as a concise set of descriptions that corresponds with the properties nature and is supported by experimental findings. The second meaning of theory is hypothetical. Theoretical work in progress seeks experimental confirmation and looks at established theory and experimental data for its parameters. Furthermore, it is hardly concise and is acknowledged to be largely speculative. In contrast, established theory follows from a compact set of principles which are assumed to be an accurate description of physical reality. The third understanding of theory is again hypothetical, but is also expected to be an accurate description of physical reality. In the perception of the physicist this yet to uncover structure functions forms an existent endpoint of current efforts. Although anticipated theory is not real in the sense that it can be read

[148] Eric A. Bergshoeff, *Van Snaren naar Membranen: op zoek naar de quantumzwaartekracht* (Inaugural speech 2002) 24.
[149] MR 54.00.

somewhere, it is viewed as part of objective reality. As such it is also expected to be a concise description of the universes fundamental organizing principles, much like established theory.

Towards other sciences the theoretical high energy physicists displayed modesty and politeness. Mees de Roo explained that, although in some fields such as astronomy or experimental physics he could judge the importance of scientific results achieved there, in many other fields he did ‘not wish to pass such judgments because I don’t feel I can.’[150] Obviously, being a relatively closed and self supporting field there is no tremendous need for HE physicists to involve themselves with other fields.

Still, also political correctness plays a part here, Mees de Roo stated: ‘But sometimes I can imagine that you are amazed about what research occupies others’, when asked for an example he commenced in a quite indirect form of judgement: ‘Well, a colleague of mine was a member of the university’s scientific committee for a while, where all the research proposals were processed. He said: “Today we talked about the use of the word ‘ut’ in Twents [a Dutch dialect] for over an hour”, which someone wanted to investigate. And that was always his way of illustrating that a lot was done which did not qualify as the most interesting form of science. But this is easily said, I really couldn’t say if it is important or interesting, but I can imagine that, just as we have our community in which we all know what everyone else does and that it is good science, that there are other communities in which I don’t have an insight, which do fine research, also about the word ‘ut’ in Twents. I ‘d rather stay away from judgments about that.’[151] And although he did not pass judgement, a sense of preference is communicated by Mees de Roo’s words.

When asked if he thought other fields were more or less scientific then his own master student Rob Bremer acknowledged: ‘When I started my studies I really had the idea that, yes, a real beta science is more than someone who studies economics. But the further along with your studies, the more your opinion starts…, the more modest your attitude becomes. The further you advance in your studies, the more you find out that, really, you are studying the same sort of model as an economist who constructs a model of the economy.[152] In a comparison with economic models he explained the physicist develops theories without knowing if there is a nice underlying design to the described behaviour, the deeper idea behind theory is also unknown to the physicist.

[150] MR 26.45.
[151] MR 26.50-28.03.
[152] RB 34.50-35.30.

After discussing the relevance of theoretical physics Mees de Roo mentioned a difference between sciences, he explained: ‘It’s not a practical question’, after an interruption where I drew out the difference to for example finding a medicine for schizophrenia, he replied: ‘No, that is a wholly different thing, no it is really, I would almost say it [theoretical physics] is near pure science.’ When I asked him about the difference between pure and applied science he commented: ‘Well, you ask a different type of question, the type of questions that you ask is just really different. And it has…, it perhaps means that at certain times you get more time to do pure science then you get when you do applied science, perhaps the urgency is different. But that it is both scientific is very clear to me, in both cases you see an opportunity to make progress in a question that people ask […] in case of the universe this is perhaps for your mental peace, and in the case of the medicine it clearly has a very practical application.’ [153] When asked about the difference between pure and applied science Eric Bergshoeff responded: ’Well, I would say a gradual difference perhaps, I would say I do “curiosity driven science” [he used the English expression and followed with a Dutch elaboration; ‘so that is science driven by curiosity’]. Whereas with applications you can just apply the theory to well “whatever it is” really, that is not what I am doing but, that can also be done and I think that, in itself, […] solution driven science also is really important… important developments also take place there. For example, the CD, that someone invents, that you can store all this data, clearly making use of all kinds of physical laws, on a CD.’[154]

In a similar line of reasoning Rob Bremer commented on the idea that in terms of exactness physics was perhaps different from economics or mathematics: ‘At the start of my studies I reasoned that we start from below, you reason that you know what is going on behind the grey mass. So you describe a model of something very complex, but you start at, for example, an individual particle and then you see how an entire substance behaves in a space or whatever. And an economist starts from top to bottom so to say, so he starts on the topside: you have one mash, and you don’t have any idea what’s underneath it. Actually, physics does somewhat the same thing, only the starting point is much more primary. But you really do the same thing.’[155] Asked about other fields such as psychiatry or social sciences, and their status as sciences, Rob Bremer mentioned an example: ‘Yes, well, you read a lot or research, for example that a certain food is not good for you or whatever. A medical scientist actually is someone who, a researcher in that field, who takes a test group, a control group and

[153] MR 36.08-37.22.
[154] EB 30.17-31.07.
[155] RB 1.04.30-1.05.29.

an anti test group, the ones whom they administer a placebo. It really is statistical research, and yes, perhaps an economist does the same but it does leave a different taste with me.' When I asked what kind of taste that was, he responded; 'Well that they are doing less science [I hinted 'So science is really if you capture…'] 'Well, they also try to do that, only less fundamentally or something. They look, they administer a plant with a certain toxin for example, they do this with ten plants, they don't with ten other plants and than they observe the difference for example. That's their kind of research.'[156] He pressed he did not pass any judgment on the value of medicine. Nevertheless, he did suggest that as far as science is concerned the type of research in medicine is not as scientific as HEP on the grounds of it being less fundamental. In a similar line of reasoning Rob Bremer commented on expanding knowledge in different fields: 'Well, if you take history, it is reasonably…, you don't do many discoveries back in time. There's a limited number of excavations, and then you start connecting nice lines, it's a, it's, well, a very different kind of science. It's more, perhaps, explaining how things come to be, why is society the way it is; you can make reasonably nice relations, but it actually stays inside the sphere of knowledge that already exists, so to say.' And after he explained a metaphor of inflating a balloon he added; 'It's really a balloon with a lot of points in it that the historian constructs, and he can come to new ideas, but always inside something that already exists, and physics really is, it expands in to the unknown really.'[157]

Scientists expressed an awareness of the limits of their own science, noting that one has to keep in mind he is working on a model rather than nature, and underscoring that some theories will remain figments of the individual mind. In line with this self awareness, scientists are generally modest about their own field and hesitant towards judging other fields. Of course, political correctness in the setting of the interview may also have played a part. Social desirability and sincere modesty aside, we also see clear distinctions between science in terms of pure and applied, fundamental and non-fundamental. The implicit hierarchy from rigid, exact and truly scientific which is able to provide fundamental knowledge, to 'statistical science' which is valuable despite of its inability to truly expand knowledge, suggests an image of science being purely about fundamental knowledge. To be precise, the criterion for pure, or true science is that it produces fundamental knowledge in the sense that it describes constituting and universal principles of our world.

[156] RB 1.06.05-1.07.10.
[157] RB 37.28-38.32.

The remark about the CD's making use of physical laws supports this image, as the knowledge of the physical world supersedes and precedes its use in applications. In a way this remark resembles Hawking's statement about the theory invoking nature into existence. In both cases it is the structure of nature as described by physics is regarded as preceding the material phenomena that testify to that structure. In comparison to the psychopharmacological notion of fundamental science, fundamentality has a much more far reaching meaning in HEP. It concerns more than just 'the basis' or 'origin' of the problem at hand but and conveys a sense universality and even profundity. Subsequently, sciences are stratified according to their ability produce universally valid and profound knowledge. Hence, medical research only contributes marginally to the production of knowledge, whereas history hardly contributes at all. Note that physicist do not rebuke other fields or judge their worth; they may be very useful, but they are not pure sciences.

Interestingly, discussing science's place in society with the HE physicists also turned out to be a rather theoretical topic. Notably, there is no immanent requirement for theoretical physics to involve itself with a multitude of scientific fields, or with societal players such as corporations or policymakers. In fact, there is not much need for direct engagement in society at all, not financially, nor as part of the research process. When doing 'fundamental', 'pure', 'curiosity driven' and theoretical research, it seems that relevance is not described in terms of direct benefits to society.

Mees de Roo explained that his research is scientific because: 'Scientific? Well, I would think because it's new, that you try to bring things somewhat further then they already were. Scientific, well, as far as I am concerned that actually, and not exactly that you ask what science exactly is, but that is science, that you do something new, that you add something, that you look beyond where it was.'[158] Asked about the standards of good scientific research he proceeded: 'Well, yes, certainly, as I said it has to be new, but it also has to, I think it has to, but that's perhaps not always fully hundred percent the case, it also has to have a certain relevance within a certain framework', he continued: 'so science, a scientist who is completely disconnected from all other scientists or every scientific framework is an isolated person who probably will never be read [upon a short interruption on my part he agreed that 'knowledge has to be shared'] and colleagues need to have the feeling that when you finish something a certain contribution has been made.' Science and scientific relevance is thus

[158] MR 22.25-22.55.

described completely within the realm of science, society is altogether absent from the explanation.

In line with Mees de Roo's perspective Eric Bergshoeff explained the importance of applications and theoretical physics: 'I think there always needs to be a small group of people that just has to think about stuff without having applications in mind, just jolly and free, just purely from interest. And then the applications will follow, in the long run applications will follow, by themselves. I don't say everyone has to do it, but a number of people have to do it. And you see when people are just going about it, it is of course the familiar example, but for example at CERN, the world wide web was invented *there*'.[159] And after describing the way the web was developed as a medium for knowledge at CERN he pressed: 'It didn't happen because someone said "hey", a policymaker, "hey it's important that we find applications, I think that internet is something new and important, let's invent it." That's not the way it works.' [160] Interestingly, Eric Bergshoeff makes an indirect connection to socially relevant applications here; it appears that relevant applications follow from good science of the fundamental kind.

He contrasted the course of history and the way the future would turn out with the easily made extrapolations of current developments into the future; there are always unexpected developments. And somewhat later in a discussion of the difference between pure and applied science he added: '…I think you have to do both, then you are specifically looking for applications that, that's trying to find them directly. The fun thing with us is that we can also find unexpected applications, so things, things have been found just by snooping around; take electricity, also found by just snooping around, x-ray radiation, people started snooping around and suddenly someone found x-ray radiation. So I think you shouldn't purely search in preconditioned directions, let some just snoop around, just curiosity driven, and then something will turn out naturally, and if not, too bad.'[161] Although applied fields were definitely considered useful and scientific, and physicists unmistakably acknowledged the importance of technological advance, the comments above suggest physicists view pure science as the frontrunner in scientific, technological and societal advancement. Even more, Bergshoeff's description of serendipitous discoveries even suggests that the truly revolutionary findings come from unconditioned brands of research.

[159] EB 28.15-28.59.
[160] MR 23.05-23.52.
[161] EB 31.24-32.00.

Responding to an earlier comment I asked Mees de Roo what the relevance is of knowing about the beginning of the universe, he explained: ‘Well, that is really the scientific relevance of wanting to understand how the world works and what the relation between different things is. I think anyone who looks at the sky at a clear night at a certain point wonders what all of that is doing there and how far it goes, does it end somewhere or…’[162] After a vivid description of the timeless nature of the scientific quest I asked Eric Bergshoeff what the function of that historic quest is, he responded: ‘No, it’s just fun to think about it and,… well, yes, it doesn’t have a function it just makes it nicer to think about those things. Why is art fun, it’s fun to look at, but what’s the use of art, it’s fun to look at, but what is the use of such a quest, no, yes, it’s fun, just, like art in a sense. Yes, well, why do we live? What’s the purpose? The use? Those are difficult questions, but it is just fun, and yes that’s enough.’[163]

Mees de Roo explained the societal use of science as: ‘Well, on the one hand I think it’s important that you, that people keep asking questions, that they try to better understand the things they see around them. In my view that’s important, and it has in many cases, although it isn’t especially necessary, but in many cases it also has a societal use, things should be,… it is very desirable that science at a certain point also has a societal use […] also if there are calamities threatening the earth, that you can use science to counter them.’[164] When I asked why asking questions is so important, he proceeded: ‘Well, it keeps you going, and this is perhaps very personal [I responded: ‘Well, you are a scientists of course’] ‘No, of course, you have to, if you see something then you have to, you don’t have to wonder “what is it and how can it be, and how can I understand it?” all day long, but you have to have a certain nature. For me in any case it is a really important aspect of thinking that you time and again see things of which you are able to wonder how they work. And the nice thing about it, that you have to have fun doing it, I can imagine there are a lot of people how don’t like it, but if you like to think about it you come across a lot of questions.’[165] Not completely satisfied I asked what would happen if people wouldn’t ask questions, Mees de Roo responded: ‘If it would stop?’, I added ‘Yes, or that they just don’t, perhaps that the questions are there, but that they would be like mwoah, what would…’. He proceeded: ‘I think you would regret that later on, I think things would go wrong then.’ Asking what would go wrong in the given hypothetical situation, Mees de Roo considered; ‘Then it *would* go wrong, then at a certain point, I don’t know, then the power plant goes dead and then no one knows how it

[162] MR 35.21-35.55.
[163] EB 27.20-27.55.
[164] MR 1.06.52-1.07.42.
[165] MR 1.07.55-1.08.28.

works. That's a little practical, but I think you wouldn't keep up, because society is not a simple society anymore. It all became really structured, people also expect a lot from society, they expect to be able to go on holiday to Florida and back and forth in a week […] all kinds of things you can't turn back. And if you were to turn them back, I'm afraid that would cause a lot of misery.'[166]

It appears that the link between fundamental science and society is not very clear. Or rather, in the perception of the theoretical physicist that link is not very thoroughly conceptualised, which in turn gives supports the idea that there is not much need for such a conceptualisation in the daily practice of theoretical physics. On the other hand we have witnessed how theoretical physicists, besides tying science mainly to knowledge, also have technological advances follow fundamental science. After he discussed how physicists work on areas for which they do not know of any applications yet Rob Bremer gave another telling testimony; 'But you have to look back at history.'[167] He pointed out an example in Maxwell's work on waves and the eventual, but unintended consequences for communications that followed, he concluded; 'Physics is always, actually, ahead of the application.'[168] Recapitulating if applications are the justification of physics Rob Bremer disagreed: 'It is precisely, precisely not the justification for its existence, it is just the discovery of the new, and the discoveries [although I presume he meant 'applications' here] are actually a by product that many people underestimate. Because it influences our society greatly, I you would only look at tv's, computers, cell phones, the whole shabam. All of it has its basis….'[169] Science, or rather pure sciences such as theoretical high energy physics, is ahead of technology. Hence, it is also the frontrunner of many advances in society, while it never pursues the specific purpose of societal benefits. The advances are not the intended goal, they are the beneficial, and underestimated by products of proper science.

The importance of fundamental knowledge, the direction knowledge travels to create societal benefits and also the complete absence of arguments for the direct use of fundamental knowledge reminds one of the aforementioned deficit model of science communication. The idea that advances in society and technology trail those in science of the fundamental kind enhances the resemblance by insinuating that science is a separate realm that can in fact be ahead of the rest. Interestingly, in Rob Bremer's explanation in the foregoing section, a historical argument is visible as he puts Maxwell at the foundations of telecommunication,

[166] MR 1.08.40-1.10.01.
[167] RB 46.58.
[168] RB 46.32.
[169] RB 46.46-47.14.

and leaves out other contributing circumstances or consecutive engineering innovations that facilitated the development telecommunication. Science is portrayed as the source of progression here. Additionally, Rob Bremer also argues that advances such as telecommunication are an underestimated effect of good science. With these arguments he also falls in line with the perspective from which Massimiano Bucchi constructed as the deficit model: namely that the challenge to science communication is that society should learn more science. All these aspects contribute to a conception of science as a domain separate and exclusive from society.

Concerning science in general physicists strike a similar chord. We already came across the notion of curiosity driven science. Curiosity, together with novelty was also mentioned by psychopharmacologists as a central feature and incentive of science. In the Dutch of the interviews both words are even closer together in the term ‘nieuwsgierigheid’ which literally means an eagerness for the new. Mees de Roo paid further testament to the comparison when asked if asking questions is what binds the sciences: ‘Yes, curiosity… I think the applied sciences just as well have, or perhaps have it even more than pure scientists.’[170]

At the same time there is also testimony of significant distinction between sciences. As was mentioned earlier Rob Bremer explained physics/science in a balloon metaphor: ‘In the twenties they thought to have completed physics, except for that one problem of the black body radiation. But that was really where the trouble started, at the point where black body radiation, that they came to the conclusion that things were actually quantified, which in the end led to quantum mechanics an entirely new world started, of new discoveries. And you just feel in many theories that the underlying thought is absent. So it would…, well, you can picture it as an inflating balloon; the more knowledge is created inside the balloon, the greater the surface of the discoveries yet to […] there are ever more things that are unexplainable, the more you know, the more you are able to ask.’[171]

Notably, as mentioned above, such inflation is not something that occurs in the study of history, and to a substantially smaller degree in fields like medicine or ‘statistical research’. Another physicist pointed out ‘the ambition to contribute […] to take things a bit further.’ And Mees de Roo explained science furthermore as: ‘that you do something new, that you add something’, on another occasion he mentioned: ‘a scientist who is completely disconnected from all other scientists or every scientific framework is an isolated person who

[170] MR 38.12-38.21.
[171] RB 36.02-36.55.

probably will never be read and colleagues need to have the feeling that when you finish something a certain contribution has been made.' These instances reveal that the physicists define science as contributing to the balloon of knowledge, or perhaps in the eyes of peers. Both the balloon and the circle of colleagues confine the contribution strictly to knowledge. And mere descriptive knowledge contributes less to balloon than fundamental knowledge, which inflates the balloon rather than just filling it with more lines.

On several occasions we have seen physicists claim that 'it is important to connect to reality through an experiment' or that physics and science in general is a 'continuous feedback loop to reality.' Such utterances express two reconcilable referrals. On the one hand they refer to the speculative nature of theory, and the distinction between model and nature. Besides this self reflective position, these utterances also refer to the idea that the experiment directly connects to nature. Although this appears to be a commonplace perception of the way science works, an interesting conceptual move is distinguishable in this second position. In line with the notion of the inflating balloon it implies knowledge is *dis*covered; it suggests knowledge increases through verisimilitude, by getting closer to nature as it is. In comparison with the foregoing case, where categories of disorder are disputed and definitions are recognized to be temporal and current knowledge was seen as limited, the theoretical physicists treat established theory as unchangeable, only to be improved by digging deeper and uncovering the structures that form the basis for known principles. In contrast to the psychopharmacological pragmatism towards science and knowledge, the theoretical physicists show a strong inclination towards treating knowledge as universal and true to nature.

Then again, the physicists interviewed here did not immediately share the Hawkingian belief that science is nearing the end of its search. When after a discussion of developments in physics I asked Eric Bergshoeff what nature looked like to him at the moment he commented: 'The strange thing is; you'll never get there, I don't know how nature works, there is a…. well, you would almost become religious because of it, but there's a huge mystery. We think we're the result of a big bang, right, that's where everything expands. And the theories that we are looking for, we hope to get as close as possible to the big bang with them. That is of course the ultimate extreme condition, how the laws of nature behave at that point in time. But we try to get close to it […] some people think that we'll do it, that is then what some call the theory of everything, others get really itchy about that. I personally think, I think we'll never find it. But you can't…you shouldn't, as a scientist you have to be optimistic. We're just going for it, I'd say we'll just see how far we can get, and that's exciting. But my intuition tells me something will remain uncomprehended.'[172]

At the same time, the idea of never really being *there* does not immediately entail a pragmatic view on physical science. When I confronted him with the friction between the idea of the model as a product of human ingenuity and the ability of theory to capture the reality of the world Rob Bremer explained: 'You can construct models – it's turning off by the way [pointing at my laptop turning to the screensaver mode] like Newton, that actually comprises,….you have a really simplistic model of reality, but it is so powerful that with it you can explain almost everything around you; with just those few simple rules and that simple model. That's nice about a mathematical model, it doesn't have to be the absolute truth, but it approximates it so well, that you can apply it to everything around you.' 'The next best thing', I commented, 'just like the absolute truth.' 'Yeah,..yes exactly', he replied. [173] The belief that physics will come to a point where it can explain everything is a matter of personal conviction, and not shared by all. However, the idea that in one way or another, physics is spanning more and more of nature is common. What I called the 'third understanding' of theory – theory yet to be formulated – shares this growing connection between science and the objective nature. Notably, the expectation of more profound theories in the future, and the gradual approximation of reality both suggest verisimilitude.

In accordance with, and in addition to the foregoing Eric Bergshoeff explained: 'Really understanding everything is impossible, but that also makes it fun. Just suppose for a minute that everything would be discovered, a super formula from which all else follows, wouldn't that be utterly boring? And then we have it…the nice thing about it is that you know something escapes, that it isn't completely right. The idea that it isn't right, but that is science, that the boundary between knowing and not knowing is pushed ahead, to push understanding ahead. What is fun is that really you wouldn't want that.'[174] Science is attractive because it is an ongoing process. Interestingly, somewhat later on he positioned science, or rather the pure type of science his own practice belongs to in a historical background: 'No, well, it's a quest that,…the nice thing about it is that it is a quest that people have been thinking about since ages; the stars how do they work, the sun, does the earth orbit the sun and so forth. And we are just taking part in that quest, and that is just really exciting that you take part in a quest that has been going on for… A lot of things we do are very temporal, they come and go, they

[172] EB 19.38-20.35.
[173] RB 1.03.26-1.04.02.
[174] EB 24.00-24.38.

vanish, many of the things we find important now will be irrelevant in twenty years time, things you busy yourself with. The nice thing about it is that this quest stays. What we are thinking about, they were thinking about it a hundred years ago and chances are that in a hundred years, if mankind is still around, these things will also be though about. Yes, that's exciting, that evokes a good feeling, don't ask me why, but it provides a sense of timelessness to the endeavour.'[175]

On multiple occasions physicists included history into their explanation and justification of science. In particular, they placed their own practice and the project of modern science in a historical narrative of an ongoing human quest for understanding. We already witnessed Mees de Roo's comment that 'everyone who gazes at the stars' on a clear night is tempted to ask questions.[176] When I explained to Rob Bremer that I had chosen theoretical physics as a case study especially because of its public status as the benchmark for science, he added: 'Yes, throughout history actually.' 'Throughout the whole of history? I asked, 'really?'; he proceeded: 'Well, after philosophy that is, because at the time that was seen as the most predominant science, comprehending reality.' I asked if science was a new form of philosophy and Rob Bremer answered: 'It is the successor of philosophy, but it developed from it so then its hard to be anything else. But just the mode of operation and thought has, through all those years of great, really great figures; Socrates, Descartes, if you would put them in the now, then they would be geniuses, because what they put on paper is how contemporary science also works. They have been the founding fathers of our science, the mode of thinking, objectively trying…'[177]

On another occasion, when I asked Rob Bremer about the why in explaining the world he elaborated: 'I think that it is something really human, discovering, wondering why. It's something you see throughout human history. So Greek mythology, you just want to know why, why every now and then lightning bolts come down from the clouds. That's the main motivation, excitement.'[178] On yet another occasion we also heard Mees de Roo testify that asking questions is an 'important aspect of life', at least to him personally.[179] And Eric Bergshoeff described the legacy of wondering in which physics takes part as a major source of its excitement and fun.[180]

[175] EB 26.23-27.09.
[176] MR 35.25.
[177] RB 1.20.18-1.21.23.
[178] RB 33.15-33.44.
[179] MR 44.00.
[180] EB 27.00.

Interestingly, the historical narrative in which physics finds its legacy is one of pure science, of comprehension and of the gradual furthering of knowledge. Interestingly, Socrates and Newton feature in it but not the printing press, the railroads or even the telescope; the physicists historical narrative is specifically not about technological or societal advance. The progress of science is described as a predominantly mental matter of understanding and comprehension. It describes the type of practice in which questions attain relevance from a purely curious interest and science advances by pushing along the boundary between knowing and not knowing.

Views on knowledge

In the physicists' reflections on science and its many aspects we have already come across many references to knowledge. Additionally numerous ideas have been discussed in the above that fit the label of epistemology or ontology. As we have learned this is because knowledge is a central notion in the idea physicists have of science. Nevertheless, we are required to take another look at physicists' views on knowledge in particular. That epistemological and ontological assumptions are part of the physicists' description of science needs no further explication. However, the type of relation between the subject-physicist and what he perceives as his environment should be clarified.

Among the theoretical physicists we again encountered the idea that *the* truth in the sense of the ultimate description of reality, will prove impossible; that we will never be quite there. However, in the case of the HE physicists this determination of the endpoint of knowledge hardly undercuts the explanatory power of their science. Models do not need to be the absolute truth, they may do very well by just approximating it. In a variation on this line of reasoning, when asked a question about objective reality after discussing a certain theoretical concept, Mees de Roo explained: '*The* objective reality? I wouldn't really know what I should want with that, well…, no that is too philosophical to me. Objective reality is, look, the universe, that is just there for me, and we are in it. And ehm, if we weren't there, the universe would be there as well, and so would all those particles, that is pretty clear to me. And in a sense that is a sort of objective reality as well. It just all exists and we are in it. And we are with lots of planets of which we are beginning to know of that orbit other stars, but even if no one lives there, the universe still has its own objective reality, from that point of view the universe exists just the same, and looks the same as we have come to know it.'[181] Mees de Roo first distanced himself from the philosophical concept of objective reality, and then replaced it with the universe being simply there. From his first sentence one might start to wonder to what physics refers to if not the physical properties of the objects that make up reality. However, from the rest of his elaboration one finds that physics refers to nature, to the universe as it presents itself, which is there independent of our thoughts and actions.

Somewhat later we discussed the order of the universe, when Mees de Roo remarked 'In what sense then? It is an organised whole, there are organising principles […] No, no, of course there are a number of basic principles that organise [the universe] and to me that is physics.'[182] So physics is about the basic principles of the universe, the principles from which we can explain its order and appearance. Moreover, this order coincides with the fact that the universe is there, even when we are not, and looks roughly the same from any angle. Hence, the order that is physics is a universal and fundamental order. Consequently, it is the physicist's task or opportunity to approximate the world by describing these organising principles. Interestingly, this attitude shows a large component of correspondence to theoretical physicists' valuation of knowledge and science. Of course there is also a strong impetus towards coherence in altering theory 'in a logical way'; in finding out which few theoretical options might actually work for a given problem. And we have even seen a consensus connotation to truth in the proclamation that when you finish something peers should feel something has been contributed. Still, correspondence makes up the greatest part of scientific success. In fact correspondence can be seen as the ideal approximation; connecting to the experiment verifies a measure of correspondence/approximation to nature.

As already discussed in the above, even in the case of contributing in the eyes of peers the contribution is on the level of knowledge. Inversely, knowledge counts as a real contribution if it approximates reality by describing organising principles. In the same line of reasoning medical science, or history hardly succeed at contributing in this way because they only provide knowledge of particular cases instead of universal organising principles. The distinction seems to be that science becomes pure once it describes fundamental principles. A practice 'does less science' when it is unable to produce knowledge of the pure, fundamental and universally valid kind – although this fact does not undercut the general use or worth of such practices.

[181] MR 1.04.20-1.05.04.
[182] MR 1.05.18-1.05.38.

Although certainly a cautious commentator when it came to other areas of study, Mees de Roo elaborated on pure and applied fields: ‘In both cases you see a chance to do, to make progress in questions that people have. That can be relevant for, well, in the case of the universe, for your peace of mind, to understand more of how it works. And in the case of the medicine it clearly has a practical application.’ When I proposed linguistics and philosophy he remarked: ‘That also brings you further [and when asked if they are pure or applied he proceeded] Well, it perhaps sits a bit in between, I can imagine that in linguistics there is applied stuff, perhaps that if you understand language very well you can make universal translators or something, although I wouldn’t really no what good that would be. But still, you can also ask yourself really abstract questions in that, about the origin, development and change.’[183] Interestingly, the first application that came to mind in terms of of knowledge of language was a universal translator, supposing linguistics captures the universals of language particulars similar to the way that physics describes universal principles of physical behaviour. Another interesting reference can be found in his statement that *also in linguistics* one can ask abstract questions about origin etcetera. Meticulously nitpicking this statement for the sake of argument reveals how ‘also in linguistics’ relates to the practice of theoretical HEP and how the word abstract denotes fundamental or pure. In short, this short excursion into linguistics by Mees de Roo supports the strict idea of pure science we have witnessed earlier, for example in his answer to the question what the relevance of knowing about the beginning of the universe is: ‘Well, I thing that’s really, that’s just the scientific relevance of wanting to understand how the world works and how different things fit together.’ [184]

Alternatively, pure science and fundamental knowledge do not entail the idea that at some point we will know all there is to know. Following Rob Bremer’s description of the modesty he developed towards his own field in the course of his studies he explained: ‘The further you advance in your studies, the more you find out that you really are working on the same kind of model as an economist who constructs a model of the economy’, I suggested that it was not the case that he had grown to value economists more and more, but more that he found out that, and Rob Bremer complemented the sentence: ‘that you yourself, that mankind itself cannot approximate the absolute truth.’[185] On the other hand, we also witnessed him pointing out that a precise description is no condition, and that a close approximation would work well enough.

[183] MR 36.45-38.07.
[184] MR 35.25-35.38.
[185] RB 35.15-35.38.

In the end approximation goes beyond logical consistence. Consistency is a prerequisite for a theory to be considered valid and takes up much of the research time in arduous algebraic manipulations. Nonetheless, approximation and truth are met through the constant feedback to experimental reality. Eric Bergshoeff explained in an historical example: ‘Both in the case of the relativity theory as well as the general theory of relativity public acceptance took place before objective verification justified it. Experimental verification took place only much later on, but it was all so ingenious, it had to be true. [‘It also captures the imagination’, I added] It captures the imagination so it has to be true, but that is really dangerous. Luckily he [Eddington] was lucky, well perhaps he had Einstein for it because he was right.’ After some comments back and forth on the Eddington experiment, Eric Bergshoeff proceeded: ‘Well, that Eddington was a smart person, he was well connected to the press, and it has been enormously important for Einstein to become famous, so Eddington played an important part. But strictly speaking the experiment itself wasn’t convincing enough. Only years later the exact experiment took place, that makes you think, it is not right in a certain sense, it should not happen. In that sense you should remain objective.’ When I asked what remaining objective means he elaborated: ‘Well the experiment, what has been verified and what has not been verified, and what has not been verified is, like I said, speculation.’[186]

The way in which Eric Bergshoeff puts forth the historical example, especially his judgement of the validity of the Eddington experiment is revealing. Time and again he emphasised that theory becomes objectively true, when it is experimentally verified. He slightly raised his voice when he mentioned ‘things to be true because of capturing the imagination’, ‘the Edddington experiment strictly being unconvincing’, and that ‘*it* should not happen’. Experimental verification is the threshold for theories to gain acceptance as an accurate description of objective nature. Additionally, Eric Bergshoeff also used objectivity in relation to the attitude of the scientific subject; being objective in this sense is to withhold from viewing theory as a depiction physical reality until it has connected to physical reality through an experiment.

The universe just being there, connecting to the reality of experiment, approximating absolute truth, it may seem that physical nature is all there is to theoretical physics. From the above it may appear that the objective natural environment is the single point of gravity in the relation between subject and environment in the case of the physicists. However, physicists also

[186] EB 34.30-35.32.

struck some interestingly subjective notes at times. For example all three interviewees mentioned intuition and feeling in relation to their theoretical work. On an earlier occasion we witnessed Rob Bremer pointing out ‘you just feel in many theories that the underlying thought is absent.’ Similarly Eric Bergshoeff’s intuition told him ‘that what we are doing now isn’t the truth, that can’t, you really need more information for that, but we use, we find bits, useful pieces that will become part of a future theory.’ Mees de Roo explained that if he would be asked to do something in theoretical cosmology: ‘Then you miss a certain intuition, to say something like “Oh I will make a project of it and I think in a month or so we will have”, so there always needs to be a certain translation.’[187] On several other occasions intuition also came up, and in line with the examples listed above intuition time and again referred to the viability, completeness and hence the importance of different theoretical ideas.

A well developed physicist’s intuition appears to be a guide in determining the usefulness of different theoretical speculations. In concordance with this observation, and in concordance with the speculative aspects of theory, physicists among themselves may have very different valuations of different theoretical options, the status of established theory and the shape of nature behind the theory. Notably, with the psychopharmacologists we also encountered differences of opinion regarding for example the nature of disorder. We have seen Berend Olivier explain, for example, how each psychiatrist has its own view of how disorder works. Surely, such differences in professional opinion are to be expected among individuals working in the same field. And if there are unsolved questions one should only expect scientists to conceive ideas for possible answers. However, in theoretical HEP we can identify differences of opinion which involve the theoretical options that are in use at that very moment. In theoretical physics different conceptions of hypothetical options immediately involve the subject matter of the practice. An example of such difference of opinion followed Mees de Roo’s explanation of what he saw as the open ends in the physical understanding of nature at this point in time. I brought up dark matter to which he responded: ‘Yes, dark matter is absolutely one yes. But I think the secret of dark matter, on the one hand is located in cosmology, in the sense that in cosmology you can try to find out what it is. You can search for it in the universe. But on the other hand, you can also look for it in particle accelerators. And it is enormously pleasant that both will be done in the coming years. In a certain sense this is going to be, every time is interesting of course, but a really interesting time is dawning at the moment.’ When I hinted, in reference to the inaugural speech by Eric

[187] MR 43.14-43.28.

Bergshoeff, that there was this idea that dark matter and dark energy could be an effect of other world branes (branes being a theoretical concept that facilitates dimensional expanse within a string theoretical framework) Mees de Roo responded: ‘Yes, well look, Bergshoeff is the, he is, well almost the inventor of branes. Well I don’t know, I don’t know entirely, I wouldn’t dare say that I would expect branes as physical objects in the way he understands them, that I would expect those branes as physical objects in space. To me it is also a certain description of a specific process, a […] description of a process that takes place on a very small scale and that manifests itself in a certain way as a brane.’ When I asked if he then wouldn’t count them as objective reality he replied; ‘No, no no, I don’t think so, I don’t believe we will find branes. That is, not that kind, perhaps another type of large structure.’[188]

Although not apparent in theoretical HEP articles, or fitting to the commonplace notions of exactness and rigidity associated with physics, subjective qualities such as belief and intuition are part of the daily practice of theoretical physicists. Even more, individual physicists can have different convictions when it comes to their ideas on nature and the theories that connect to it. Interestingly, also the idea of religion found appreciation among physicists. Mees de Roo did not testify to any religious sensibility, he told me he was not raised religiously, but knew of other physicists who are religious, although none at the department openly advocated it.[189] When asked if he was religious Rob Bremer responded: ‘No, but I’m leaning towards it more and more’, I asked: ‘Really, how so?’ on which he proceeded: ‘Well, really, you can’t exclude anything, that’s the big problem. Really, what we are living in now, with our current knowledge, does not exclude that there ever was an invisible hand [literally from Dutch he used ‘higher hand’ which is common parlance to denote deity]. Perhaps not in the shape that many people imagine it as a person.’ ‘Not in the way that there was a man who..’ and I clapped my hands gesturing instant creation, Rob Bremer went on: ‘Exactly, because that only causes more problems.’ Asking if this tendency towards religion had something to do with his studies, he answered: ‘Yes, that everything fits so inexplicably together, that for example forms like telepathy, really, for example could occur, because you can’t exclude it.’ After I pointed out some analogies between physics and religion in their occupation with the fundamental principles of the world Rob Bremer explained: ‘In physics there are also people who are convinced of a specific school in physics, who really give their entire life for it. For example, when people get into super symmetry or string theory, those people have a belief in it, because it is more a matter of belief than a

188 MR 1.02.02-1.03.58.
189 MR. 1.13.50, 15.00.

feedback to reality. In the future it can turn out to have been the right way, but a lot of people are really convinced they are working on the only truly right idea, so then it is also just a faith.'[190] In line with Hawking, and perhaps als in line with Adamah, physics does not seem to be that far removed from religion. Of course, the testimonies above display considerable differences between physics and religion, and physicists should not be equated with devout followers of organised faith. Nevertheless, all three interviewee pointed out there is room for religiosity in HEP. Moreover, it seems that in physics there are those who make use of this room, whether this is in religion or spirituality as such, or through a literal reading of their physical theory. It would go to far as to say that religion and physics in the end are about the same thing, however there is a kinship between the two given their shared motivation to attain a profound understanding of our world.

An especially spontaneous account on all of this came from Eric Bergshoeff, following the statement that his intuition told him that there will always remain something unexplained. He started of: 'the big bang, right, I can imagine a lot of things can be explained, right after the big bang, but can you explain the big bang itself? ['What was there before it?' I hinted] Well time can cease to exist, but what then triggered the big bang. Now we also have more extensive theories with many universes, that big bangs happen all the time, that it's not unique, that our universe isn't unique [I interrupted: 'But then just you multi scale the problem really'] Yes, well, you can, that's called the multiverse. It's a really interesting question,... Life, is there only life here, is there life somewhere else in the universe? Is it a coincidence? Is it a logical process: under certain conditions life develops and it will also extinguish in time. Are we a light in the void, really? Those are interesting questions, it almost smells like religion, you're not going to understand all of it.' His testimony enticed me to ask him how he viewed life, if it is a logical consequence of natural laws or an anomaly, a very extreme or local condition, he took over: 'On the one hand I can imagine that you explain life from the right circumstances, all kinds of development on the molecular level, at a certain point you get some sort of super molecules, then you get a cell shape, evolution, and then things can, they can develop on a very long timescale….could be. And there are also those who think we will vanish again in time, because if you figure what happens to molecules on earth, how fast evolution goes. Although well, in the future, perhaps we will migrate to other planets…. But say… well… on the one hand it is not impossible to explain, but on the other hand if you then just sit here, you look around you and you see this earth, a

[190] RB 40.40-42.42.

universe, a sun and a solar system, thousands of solar systems, there we are; then you do wonder, what are we doing here? At a certain point it's so impressive, that is really almost a religious feeling in itself, it is hardly comprehensible. So,… I can't figure it out.' Asking if he was religious, or otherwise spiritual Eric Bergshoeff quickly interrupted with his answer: 'I think.. no,.. you would almost deem the wonder about everything, the bubble of wonder, the mystery, it's a mystery of why are we sitting here, that is almost a religious experience, right? It is kind of exciting also, like what are we doing here, how can it be that we are sitting here. Somewhere, logically, you should be able to infer it, on the other hand, you have this big bang, and suddenly we are talking here with a little laptop, about things. That is just a tick to much to me, it is a mystery.'[191] Indeed, this elaboration by Eric Bergshoeff gives an imaginative cross section of the physicists motivation to search for fundamental principles, or rather deeper truths. In the same sense we can also understand Mees de Roo's response to my question what then exactly the relevance is of knowing more about the beginning of the universe: 'Well, that is really the scientific relevance of wanting to understand how the world works and what the relation between different things is. I think anyone who looks at the sky at a clear night at a certain point wonders what all of that is doing there and how far does it go, does it end somewhere or…'

However, we should not read to much religious profundity into the fundamental explanations offered by theoretical high energy physicists. Mees de Roo described man and consciousness upon asking as: 'Consciousness…well it resides in me, right? In a certain way, I don't know what happened or how it works, and it also is a really interesting scientific question, how it works. But in some way or another nature came up with a trick to, to put together our brain cell so we apparently have that consciousness. But, but everyone has that to himself, that consciousness. And ehm,…and yes….I think that it also does not go outside yourself, I mean if man, if someone dies than it disappears. That consciousness concurs with the fact that you live, and when you don't live anymore, or before you are alive, that consciousness isn't there. It concurs with life […] but scientifically speaking it is a top question to understand how that works.'[192] And when I asked if consciousness is a logical consequence of the theory, or something that springs from nature he proceeded: 'It comes from, I think it's from the, how shall I put it, the complexity of the…it obviously is a wholly different type of physics than I do, as far as it is physics of course, but it goes, it's more that if structures get really large and consist of incredibly many particles such as living beings then

[191] EB 20.35-23.27.
[192] MR 1.16.36-1.17.37.

you can also get wholly different processes that have nothing to do with, let's say, the elementary processes that play a part on a atomic or sub-atomic level. They are more a collective event, in which many particles in any case, and also many molecules and many nerves, and things that are composed from molecules play a part. So it's a different type of science in which luckily there is a lot of interest, and in which a lot is done. But it is also a wholly different kind of science, you also see it in physics that really interesting phenomena, take superconduction, actually come to be in systems that are composed of lots of particles. And then it's also a process, a phase transition in which many particles are involved. And that is also why it is hard to describe and you can't derive it from the descriptions of single particles.' I added that it wouldn't fit on the blackboard either, Mees de Roo replied: 'That wouldn't fit on the blackboard, no.'[193] After which he made clear his group does not work on level of collectives, but on the level of particles.

What stands out from this account is the fact that Mees de Roo immediately connects consciousness to nature in saying nature came up with a trick. His description of consciousness as concurrent with being a live says much about Mees de Roo's world view, but in itself is not a particularly striking move, or something that is distinctly belongs to the HEP point of view. The way he explains his position is al the more interesting, though. He starts his explanation from elementary particles setting life as a complex process apart from the processes going on the elementary level. Next he uses an example on superconduction to explain to the type of collective processes that play a part in life and consciousness. His explanation reveals a distinct frame of reference from which life is explained and admittedly not understood. In Biagioli's words his explanation would be an example of a scientist ontologizing his world view; what is striking is not his physicalist conception of consciousness, or the fact that he explains life as a complex aggregation of matter, but that he starts his explanation from the elementary level he is used to and elaborates it using an example from physics to clarify the relation between the elementary and larger scales.

[193] MR 1.17.45-1.19.25.

Science to theoretical physicists

Physicists relate to their environment differently from psychopharmacologists. Physicists' explanation of science is mainly based on the idea of knowledge. Obviously, any academic field has knowledge as a central interest. Yet, in theoretical HEP knowledge has an especially central place in the sense that the identity of science is confined to a specific understanding of knowledge. Just a summary of test results or facts does not count as truly scientific knowledge. Instead, science can be stratified along the criterion of purity. In this stratification physics is a prototypically pure science because it asks fundamental and curiosity driven science. More applied fields contribute of science as well, but do so 'less than' pure science and applications in the end also 'clearly make use' of pure scientific findings.

'Pure' and 'fundamental' have a special status which presumably is based on the idea that curiosity driven science reveals basic principles, whereas other types of questions entail only problem-specific solutions. The physicists all made clear that in every area of science people see a chance and make an effort to make progress. However, that progress is not measured by its benefits as much as by its contribution to knowledge of fundamental principles. Mees de Roo expressed a similar notion in his division between devising a universal translator and 'more abstract questions' in the case of linguistics. Abstract questions evoke general and fundamental answers, answers which provide an understanding of elementary principles. And by looking what is behind, or what drives known phenomena knowledge of the world at large expands.

Truth is based on correspondence in theoretical physics. Consistency is undoubtedly important in constructing theoretical models, to separate the few from the many options. But to find out single true theoretical option it needs to correspond to the behaviour of nature as presented in experiment or astronomical observation. Consensus has only a very marginal role to play in determining truth. Personal intuition and collective hunches about theory do play an explicit part in the practice of theoretical physics. Nevertheless, correspondence determines truth, take the example of Eddington's relativity observations; acceptance, or the belief that relativity theory was real was deemed unjustified from the data that Eddington provided. Luckily for Eddington Einstein really was right.

Objectivity is also based on correspondence rather than method. Einstein was only objectively right when his theory was verified in a showcase of objective nature. From the perspective of the physicists, Einstein had been right all along because his theory did prove to be an accurate description of nature. Still, Eddington and his contemporaries should have been as objective, and should have postponed their celebration of general relativity until an experiment truly connected the theory to nature. Of course, there is a similarity between Lucianne Groenink's way of remaining objective by ensuring observations are objectified measurements and connect to the hypothesis and the physicists' insistence that theories are speculative until verified. Still, the psychopharmacologists attribute objectivity to the validity of method, whereas the physicists present it as being true to nature.

One can devise a test with groups of plants, one test group, one control group and an anti-test group, and one might attain objectified result from it. Still, to the theoretical physicists the result would not qualify as a significant a scientific contribution, however rigidly the experiment may have been put together. A likeness with physics can be found in the economist's attempt to describe the forces that drive market behaviour. Pure science is about explaining more, explaining deeper principles, pushing the boundary between knowing and not knowing. Interestingly, physicists regularly use spatial metaphors to explain how science works; boundaries are pushed, knowledge furthers, balloons are inflated and knowledge is expanded. And the way to expand knowledge is not through the description of endless particulars 'inside the balloon' but by pushing the understanding over the boundary of the known by discovering hidden universal principles.

Notably, theoretical physicists conceptualise knowledge as what can be called a 'Platonist body of knowledge.'[194] In line with this body of knowledge, physicists also use a historical narrative that portrays the gradual expansion of knowledge. Physics was presented as part of an age old legacy, an inherent human quest for understanding. In the same depiction of scientific history Socrates, Descartes and Newton are viewed as contributors to the same body of knowledge; inflating the same balloon that modern science is inflating now, and will be inflating in a hundred years from now.

As a result, there are sciences that concern themselves with devising solutions, and there is science of the pure kind which concerns itself with the expansion body of knowledge. Science, especially pure science, therefore has a special status. It provides fundamental knowledge from which applications and innovations follow that eventually advance society. Yet society itself, and the way science makes its way to society remains rather vague in the explanations of physicists. The focus of physics is on knowledge for its own sake, which is presented with historical precedents of how knowledge improves our world.

[194] Callinger, Ronald ed. *Vita Mathematica: Historical Research and Integration with Teaching*. (The Mathematical Association of America 1996) 7-8.

In all, physicists conform to a very traditional idea of science; an idea in which science and true knowledge coincide and where science is largely separated from a society which follows the path set by science. Physicists also regularly expressed arguments that conform to a correspondence notion of truth and knowledge. Physicists perceive knowledge as a direct relationship between science and nature, a relationship that leaves out societal influences. Their focus on nature and their emphasis on correspondence fit the label of epistemological realism. However, the realism physicists display differs markedly from the type of realism that relativists and constructivist see as their adversary.

Whereas relativists equate realism with an authoritarian reason that imposes a single worldview, physicists display a much more modest, and arguably more open stance. Their epistemology is realist in the sense that they view verified theories as an accurate description of properties of objective reality. Naturally, they also adhere to the idea that there are universal properties such as basic organising principles independent of human perception. However, they do the display ontological realism that presents its view on nature as the ultimate representation of reality. Even established theory is not impervious to a turnover in understanding if an unexpected discovery would turn up. However, even in a turnover correspondence in established theory would be maintained. Similarly, although not convinced branes are physical objects, Mees de Roo did believe they captured more fundamental processes that manifested themselves in brane form.

Realism captures the direct relation between subject and environment through knowledge assumed by theoretical physicists, and we also encounter this direct relation in their views on the relation between science and society, on science’s role in history and the hierarchy among sciences. Nevertheless, realism should not be understood in the politicised meaning it attained in postmodernist debates and the Science Wars. Realism and physicalism do not entail absolute determinism in the case of physicists. Instead physicists leave the final explanation open, although they also strive to attain it. When a physicists watches his blackboard the theory in front of him is still a model. At that point there are still several options, of which he prefers some over others. Granted, theories that he beliefs have been proven will be seen as true, as an accurate depiction of the ontological state of nature. His view of the nature to which future theory will have to connect, however abstract and changeable, involves an ontological expectation, but also openness. The physicist follows his intuition in his preference for one model over the other, his personal idea of theory yet to come. A personal conception of the actual fundamental principles behind existing theories is an important aspect of the practice of theoretical physics.

As pointed out in the above there are those who adamantly believe in their theoretical solution, as if it should and will correspond to natural reality. In the realm of expected theory we find faith; faith in a specific school of thought, faith in intuition and preference, faith in branes, and in expected theoretical harmony we even find room for religious or spiritual faith. It is in the same open realism that we encounter Stephen Hawkings theory of everything and the possibility of knowing mind of God. In the very same space devoted to speculation and intuition we find authors such as Benjamin Adamah propose their specific idea of fundamental harmony. And from the expectance of fundamental harmony we can also understand the explanation that ‘the use of gazing at the stars and asking why’ is peace of mind.

There are certainly philosophical or existential motivations in physicists efforts. At the point where theoretical structure and nature coincide, where ‘those basic organising principles’ coincide with physics and a theory becomes so compelling that it invokes its own existence, we also encounter idealism. Obviously physicist do not view reality as dependent of thoughts and ideas. However, the other way around, physicists conceptualise their ideas as part of reality, as corresponding to organising structures. To the physicists in question theory is not just a useful way of organising results, it is a possible fundamental description of nature. Subsequently, the structure conveyed by promising models, established theory or a yet to be formulated M theory is perceived to connect to the basic principles of natural reality. Theoretical physicists more or less identify science with fundamental knowledge, in turn fundamental knowledge is identified in nature. Hence, the relationship between the physicist-subject and his environment is direct relation to nature from which society is largely separated.

Explaining the physicists’ identity of science

The various views on science and knowledge expressed by the physicists during the interviews are indeed very different from the views we encountered in the foregoing chapter. Alternatively they display a similarity in the way they fit together and form something kindred to a philosophical system. Of course, the physicists themselves are not explicit proponents of idealism or realism in the philosophical sense, but in the space of this paper these labels characterise their attitude, and indeed the way in which they relate to their perceived environment. This relation between subject and environment, and the specific place of science in that relation is central to our explanation of the physicists identity of science described in the foregoing section.

In line with the approach of the chapter on psychopharmacology one could ask what specific interactions are facilitated and inhibited by the identity of science that physicists uphold and the philosophical system that supports it. Firstly, the physicists' stance fits Fuchs' typology of realist and relativist cultures. The attitude of physicists is very confident; physicists question the viability of different theoretical proposals, they leave open the final interpretation of established theory, yet they hardly question their core assumptions. In pharmacology central notions such as disorder are open for questioning. Also the extent to which experiments correspond with human cases is, on the one hand humorously described as a painful point, and on the other hand continuously refined through validation. In the case of physics such groundwork remains untouched. Then again there are also few players that devote their time to the objectives set by theoretical high energy physics. And even if we count New Age authorship or Weltanschauliche philosophies and religion as other players on the basis of a common interest in the fundamental principles of our world, then still there is little demand for self questioning. In fact, in many cases other players incorporate ideas from theoretical physics rather than the other way around.

Theoretical HEP finds itself in a limited and not exceptionally dynamic social environment. Most of the input comes from other branches of physics which generates little demand for active relation management. The main role for theoretical high energy physics within the physics conglomerate is the development theoretical models which, eventually, can be a source for new experiments and new findings. Because the models describe nature on a deeper level than is currently observable or testable, theories are mainly hypothetical options confined to their home field. Obviously, there is also little direct need or incentive to question the validity of mathematical methods used to change theories in a logical way. Especially on the level of the publications that consider theoretical options there are not many who can take part in the debates apart from the physicists themselves.

Nevertheless, if one considers the speculative aspects to this field of study, the intangible and abstract subject matter, and the great ambition that is cast into the concise mathematical models, a certain measure of idealism can be regarded as conducive. The relation to natural reality produced in theoretical expectations, such as branes and M theory, facilitates the subjective experience that of the physicist that he is actually approximating the fundamental principles of nature.

The same idealism can be found in the historic narrative brought up by physicists on several occasions. The idea that physics is part of a legacy of great minds and great thoughts that contribute to an ever expanding body of knowledge also conveys the expectation that physics, or mankind, is closing in on the mysteries of the universe. It is an expectation of profound understanding which is furthered by the increasing verisimilitude between science and physical reality. In the physicists' view on knowledge, on science and on the historical narrative describing the human advances in understanding, the idea itself is held in high regard. In a daily environment where the most tangible result is an elegant formulation a measure of idealism counts as a motivator. Only within a frame of reference in which abstract fundamental knowledge is important, or even pre-eminent in the advancement of science theoretical work can take flight.

Both in psychopharmacology and theoretical physics I have pointed out how there exists a logic to the views that support the specific conception of science that practitioners from each field have. Views on knowledge, their perception of method, attitudes towards other sciences, science's role in society, and the nature of reality, slot together in the identity science upheld in each field. Additionally I have discussed some perspectives by Fuchs and Biagioli on the relation between cultures and their environment to give substance to the subjective logic of a specific identity and the way it relates to, and facilitates interaction with, the environment of the subject. I have emphasised how the scientists' specific relation to their environment can be described from their conception of knowledge and can be identified in views on other aspects of science as well.

However, the identification of coherence between views in a personal philosophical system maybe striking but it does not provide an explanation, or at least an understanding of it. Similarly, the perspectives imported from the work of Fuchs and Biagioli make a compelling argument that indeed cultures can be understood in relation to their environment but remain void on the precise workings of such environments. Biagioli points out that there is no a priori rationality for determining which paradigms are successful and which wither away before reaching paradigm status. He underscores that the way in which a paradigm fits its socio-historic environment is not the result of a predetermined solution, or the single possible answer, to the demands of that environment. Instead, in his analogy with evolution he points out that adaptation is the matter of contingency rather than a rational result. As he points out 'the only certainty about fit is necessarily an *a posteriori* and *negative* one.'

'Negative' in this sense means 'a paradigm's being still around suggest that, in some way, it does *not not fit* the environment.'[195]

In Fuchs' work we also find no necessary relation between culture and environment. The theory of culture and society that Fuchs develops in *Against essentialism* relies heavily on systems theory; a sociological perspective most famously proposed by Niklas Luhmann which explains social systems (and systems in general) from their interaction with the environment. An important feature of systems theory is that properties of systems are not the necessary result of innate qualities of the nodes that make up the system but are an 'emergent' effect of social relations. In Fuchs' *Against essentialism* we encounter this idea in his explanation of culture and personhood from social structures and interaction, and his refusal to attribute an inherent identity either of these – hence the title of the book.[196]

Another important feature in systems theory is that system behaviour is not strictly rational, at least not in the sense of the rational choice model. Systems theory is able to explain interactions and emergent properties from a system's logic, but the system-environment scheme also entails an inherent restriction to a system's knowledge and perception of its environment and itself. In short, the fact that a system has not yet incorporated the environment as part of the system, and that not every part of the system is directly related to each other part, inherently limits the possibility of knowledge of the environment. Interestingly this particular feature has raised so much attention that it became the central to another strand of systems theory known as complex systems theory. Complexity theory, as it is often abbreviated, focuses on the way a system itself changes in any interaction with itself or the environment, making it impossible to describe system behaviour as a deterministic or linear process. Instead the behaviour of complex systems is often associated with chaotic behaviour.

What we can take away from this brief sidetrack through systems theory, and a second look into the works of Fuchs and Biagioli, is that we cannot expect the relation between subject and environment described by the personal philosophical systems of the scientists to be a straightforward mechanism of interactions. Neither can we view the specific scientific identities upheld by scientists to be the rational result of straightforward interactions. In short, psychopharmacoligists' pragmatic conception of science, and theoretical physicists open realism and relativism are not simply the necessary result of interactions with their environment. Alternatively, as Fuchs and systems theory propose it is only from that

[195] Mario Biagioli, 'From relativism to contingentism', 200.
[196] Fuchs, *Against essentialism*, 6.

interaction that one can attain an understanding of the logic of the system, paradigm, culture or person. A simple causal explanation of the views held in both sciences in question is thus, besides not the goal of this paper, also not a feasible objective in the first place. However, instead of an simple explanation of the scientific identities upheld in each practice, a better understanding is within our reach.

Therefore, I wish to invite the reader to imagine a fairly hypothetical situation which highlights the character of the relation between scientist and environment facilitated by the respective identities of science. Let us consider the hypothetical situation in which we put each of the conceptions of science in the alternative context. Firstly, let us imagine the psychopharmacologists have the realist/idealist 'science as knowledge conception' that we encountered in theoretical HEP. Let us suppose they now measure science to the fundamental contribution of the knowledge. Perhaps, they would start explicating their implicit assumptions about how disorder works and develops. In all likelihood, they would be unsatisfied with the rather statistical nature results derived from animal experiments and view them as only marginal contributions to science. They might experience difficulties as they would time and again assess their test results on their universal value, rather than on their psychiatric merits. They might be found continuously searching for fundamental principles in the data that is delivered to their desks. Additionally they would experience difficulty in achieving academic success because their objectives would no longer be aligned with those of the larger pharmaceutical network, but would strictly concern fundamental academic progress. And if they were to come in contact with pharmaceutical and psychiatric networks, researchers would experience difficulty in sorting out the better from the worse types of engagement under the assumption that collaboration with societal partners leads research away from its pure and fundamental nature in the first place.

The theoreticians from the high energy physics group would wake up to an equally problematic day. Now pragmatic and oriented on societal contribution, the physicists would feel their practice hardly connects to the needs of society. They might start questioning the use of their blackboard work in the light of the only distant prospect of final applications. Now robbed of the idealistic understanding of theory and the gradual advance of pure science they would feel their work is overly abstract. Alternatively, any attempt towards a more pragmatic or engaged approach to the basic principles of nature, would make it hard for them to actually venture beyond established theory. No longer motivated to consider – and painstakingly develop – theoretical option after theoretical option, they would find it hard to advance their field and actually come up with theoretical solutions that might have any prospect of applied success further along the way.

Although highly hypothetical, this scenario first of all underlines how different the respective subjective philosophical systems held by scientists are. Secondly, it serves to show just how the views and the associated identities of scientists are different. This scenario puts into perspective that identities of science are not mere definitions, mere views or opinions apart from the practice, but in fact are a fundamental aspect of the relation between the subject-scientists and his or her environment. An identity of science, and the specific system of arguments and conviction in which it is embedded, actually can be seen to facilitate interaction by establishing priorities; justifying and promoting certain interactions while inhibiting others. In this sense, from a systems theory perspective, and from Biagioli's evolutionary point of view, an identity of science can be described as functional in regard to the purpose of the practice.

Obviously, the 'systems theory perspective' proposed here is hardly a complete or thorough explanation of the dynamic between an individual and his environment. Neither does it provide any explanation of the development of views. However, the hypothetical transposition of conceptions does support the idea that interaction is important in understanding the particular views and the actions held by scientists. Conversely, if we, for a moment, view a scientist or practice as a system, the subjective views of scientists can be understood as functional because they describe, and prescribe the specific interactions that characterise the activity of their practice.

Notably, the dynamic between subject and environment does not ascribe primacy to either of both. Hence, an explanation in the sense of a causal relation between the subject and its environment is not an appropriate description for the dynamic between both to begin with. In other words, the above does not exclude the possibility that a physicist could, possibly, have a drive towards engagement. It does point out, however, that such engagement is not a prerequisite for success in the field of HEP. Interestingly, transposing an identity of science onto another system appears to harm that system's efforts in relating purposefully and effectively to its environment and meet its original objectives. Therefore, it becomes plausible that an identity of science can be understood from its function and use, rather than from its rationality or truth value. Functionality thus merely describes how a set of convictions and views concerning science can be understood from their effectiveness, and their specific use for scientists within the context of a certain practice.

Chapter 3: Comparing apples and oranges

In the foregoing cases I have dealt with two identities of science held by members of two distinct scientific practices. Many different aspects of science have been discussed based on the views expressed by scientists. Additionally, I have discussed views on knowledge and reality separately to determine the way scientists relate to their environment from their specific conception of science. Putting these personal epistemologies, the ontological convictions, the justifications of science, demarcation criteria, and scientific hierarchies together I have pointed there is a logic to both views – analogous to the logic of an epistemology as part of a larger philosophical system. From this logic, and the different views it relates to, I have described and substantiated the specific identity of science upheld within each respective scientific practice. Making this logic and the relation it describes between the scientist and his environment the focus of my analysis I have made an argument for a functional understanding of scientific identities; to understand them from the interactions they facilitate between a scientist and his or her environment. In the following chapter I will discuss the two identities and their differences in the context of the identity debates.

My first step will be to elaborate on the differences between the identities of science proposed in the first two chapters. Consequently I will determine to what extent these differences justify the label disunity. The second part of this chapter will discuss to what extent the commonalities between both sciences justify the idea of unified science, and what kind of identity captures this unity. Whereas the first part of this chapter focuses on the internal demarcations of science, the second part also requires us to discuss the difference between science and non science. For this purpose the second part will also feature some references to non scientific views on science. I argue that the sciences above do not truly justify either the label of disunity or unity. Relying on the subjective starting point of this paper, I propose an alternative understanding of identities of science based on functionality rather than inherent or universal features of science.

Theoretical HEP and psychopharmacology, signs of disunity.

In the preceding chapter I ended with a transposition of the identity of science envisaged by psychopharmacologists into the theoretical high energy physics context and vice versa. Besides supporting a functional understanding of scientific identities, this hypothetical situation underscores that the differences ways both cultures view science is more that just a difference of opinion. From the differences between both their views on knowledge one can also conclude that the dissimilarity concerns the core of scientific practice. Let us consider some further examples of differences in which the views of psychopharmacologists and theoretical physicists principally conflict.

Consider the term ‘fundamental’ which has a quite different meaning in both contexts. Of course, fundamental in both cases differentiates from ‘superficial’ and ‘practical’. In the case of psychopharmacology the word denotes the field’s identity as a knowledge generating system, and sets it apart from the drug research in the pharmaceutical industry. However, ‘fundamental’ science is also closely associated with the integration of psychopharmacology in larger pharmaceutical and psychiatric networks. In this sense the term fundamental characterises psychopharmacology’s place in larger social networks, rather than characterising its separation from such networks.

In the context of the theoretical physicists, ‘fundamental’ more or less coincides with pure and curiosity driven science. Fundamental, in the case of the physicists also, sets apart their practice from what they view as more superficial contributions to knowledge. Moreover, fundamentality is associated with knowledge of organising principles, with the foundations of nature. Similarly their idea of science is strongly associated with knowledge; among physicists science was conceptualised as a growing body of knowledge. Notably, physicists have adopted a more selective threshold between fundamental and not fundamental then their psychopharmacological counterparts. Whereas psychopharmacologists expressed that natural sciences all operate on a similar level of scientific accuracy, the physicists distanced their own practice from ‘statistical’ approaches and applied questions which is central to the psychopharmacologists’ practice.

In the stratification among sciences we encounter a similar difference. Both sciences do not see other sciences as better or harder, but scientists from both fields did suggest a downward hierarchy. In the case of the physicists this hierarchy is measured in terms of fundamental contributions, purity and abstractness. In contrast, psychopharmacologists determine a field’s place on the scientific hierarchy on the basis of its ability to deliver objectified results, and methodological validity. An interesting example can be found in both sciences’ attitude towards philosophy. Among psychopharmacologists we encountered expressions questioning philosophy, mainly on the grounds of its inability to objectify results. In the case of high energy physics philosophy was also not regarded as an exact science – it is ‘a literature study’ and it operates mainly within the restrictions of ‘ordinary language.’ However, philosophy was also held in high regard as part of the legacy which pure sciences now continue, in terms of abstractness and fundamentality the philosophy was also viewed as an intellectual relative. On several occasions philosophy, or great philosophers, were mentioned with regards to their fundamental contribution to knowledge.

Another major difference between both fields' attitude towards science is captured in the world 'contribution'. The psychopharmacologists described science as a way to contribute, and contribution means to contribute to society. Concretely, in the interviews this meant easing psychiatric suffering and performing an active role in advice and public awareness of risks. In the case of theoretical high energy physics contribution is also high on the agenda. However, contribution in the HEP context means adding knowledge and contributing in the eyes of peers. In the contribution of psychopharmacology we explicitly find society, which also counted as a justification for doing science in the first place. The physicists' version of contribution leaves out society by explicitly emphasising scientific progress. Again, it is in knowledge that physicists find justification for doing science. Contributions to society of the kind described by psychopharmacologists were explained as by products trailing pure scientific advance by the physicists. In the same sense, psychopharmacologists explained science should 'not be just for the fun of it' while physicists pointed out asking fundamental questions is 'just fun'.

The differences between psychopharmacology and theoretical high energy physics are considerable, and principal. If we once again describe the views of scientists from each field as philosophical positions we can easily imagine contention between proponents of the respective philosophies. Both descriptions of scientific identity disagree on a multitude of points, from the structure of the scientific landscape, to the status of scientific knowledge, the function of science and so on. However, the question remains if these observations provide enough reason consider science as disunited.

The dictionary states that disunity is a lack of unity or cohesion. The *The disunity of science* does not give such a concise description. Then again, it features multiple descriptions of disunity. The earlier mentioned contribution 'The care of the self and blind variation' by Karin Knorr Cetina also deals with fundamental differences between the life worlds of two sciences. Knorr Cetina casts these differences into a cultural framework; she describes the structures and processes that give symbolic meaning to each practice. Already in her introduction she anticipates an attack on unity: 'If anything is suggested with respect to the philosophy of science, it is that there exists no “scientific method” that extends to all fields.’[197]

In Knorr Cetina’s view these cultural mechanisms determine the identity of sciences, and not a methodological unity. Her critic, Barry Markovsky contends ‘that the fabric of science is unified at a more abstract level, its pieces knitted together not by the concrete activities of the individual or collaborating scientists, but by the underlying logic of the theoretical and empirical methods they employ.’[198] The contention appears to be about what weighs heavier, the culture or the underlying logic and methodology. Not surprisingly this particular identity debate also draws in epistemological and ontological arguments. The cultural perspective of Knorr Cetina views science as a specific culture and culture as the reality that presents itself. The other way around she deems the reality perceived by the scientists as cultural and symbolic, rather than strictly material or natural. Markovsky denies this fundamental difference between sciences, for the reason that both sciences have a shared logic and method. To him cultural differences do not breach that unity. Markovsky treats culture as a layer over reality, rather than as reality itself. From his point of view a difference between practices points at the variation on a theme, rather than a fundamental disunity.

Disunity is advocated from the argument that differences supersede commonalities between sciences. Another example of this can be found in the work of Mario Biagioli. Biagioli’s proposal of contingentism – although introduced as a critique on relativism rather than unity – captures the plurality of science using the term paradigm. The term paradigm, also in the work of Thomas Kuhn that Biagioli refers to, suggests plurality. And in the ‘strong reading’ of Kuhn by many scholars the existence and consecutive rise and fall of different paradigms serves as a critique on the idea of an overarching scientific unity. Besides an evolutionary conception of success, Biagioli’s explanation of paradigms relies heavily on a linguistic assumption. Biagioli defines the threshold for the existence of a new paradigm as the moment at which the new paradigm is unable to communicate back to the original paradigm.[199] He labels this situation incommensurability. By doing so Biagioli pays tribute to a linguistic understanding which is close to the use of culture by Knorr Cetina. Both authors suggest that the difference in paradigm or culture constitutes a completely different reality. In fact, the assumption that paradigms and cultures form realities which take shape largely

[197] Knorr Cetina, ‘The care of the self and blind variation’, 288.
[198] Markovsky, ‘Epistemic cultures: how the sciences make knowledge’, 557.
[199] Biagioli, ‘From relativism to contingentism’, 196.

independent of natural reality enables to have a paradigm, culture or linguistic structure withstand a supposed natural unity to science.

At first sight the creation of a bubble of reality around an individual or practice, in the shape of a culture or paradigm, appears to accommodate the subjective descriptions of science proposed in the first two chapters. In both the cultural and the subjective perspective the natural environment is outside the scope of investigations, as such a group of subjects can perfectly well be described as a culture. However, because the subjective approach adopted here invites one to remain close to the experience of the scientist, as well as the observers, it resists the tendency to take a culture or paradigm as a separate entity. A subjective approach puts the idea of incommensurability under stress by questioning if separate realities are truly unbridgeable. Although the differences between theoretical high energy physics and psychopharmacology are considerable and concern fundamental convictions about science, these differences by themselves do not make for a division. A similar contention is raised by Galison’s work.

Galison’s contribution to the main body of texts in *The disunity of science* also takes scientific diversity as its starting point – and again Kuhn is mentioned in respect to the ‘categorization of group affiliation and disaffiliation.’[200] In his text Galison presents a history of the scientific specialism that developed with the use of computer simulations as part of nuclear weapons testing programs. His work focuses on the way people from a vast array of specialisms were involved with computer simulation. Therefore, Galison calls the practice centered around a these simulations a ‘trading zone’ where nuclear theorists, mathematicians, industrial engineers, computer programmers and many other specialists came together in the development and establishment of computer simulations as an integral part of nuclear weapons testing.[201]

Galison also underlines the idea of a specific reality that was created as a result of the new specialism. Using Biagioli’s vocabulary, Galison shows how the new field ontologized its world: ‘where compact differential equations previously appeared as the essence of simplicity, and numerical approximations looked complex, now the machine readable became simple and differential equations complex […] now Monte Carlo methods appeared to represent truly the deeply acausal structure of the world.’[202] Besides creating a new field, the

[200] Peter Galison, ‘Computer simulations and the trading zone’ in: Peter Galison and David J. Stump *The disunity of science: boundaries, contexts and power* (Stanford, Stanford University Press 1996) 118-119.
[201] Galison, ‘Computer simulations and the trading zone’, 119.
[202] Ibidem, 157.

Monte Carlo procedure of reducing nuclear fission, and later fusion processes to calculable proportions also instigated a new point of view.

On the one hand Galison, Biagioli, and Knorr Cetina are on the same page here, because they all pressure the notion of scientific unity by pointing out how different sciences have developed entirely practice specific paradigms, worldviews, or cultural realities. However, by pointing how a multitude of specialisms and hence, paradigms came together in the development of the new field Galison also downplays the divisions between sciences. Galison explains: ‘the computer began as a “tool” –an object for the manipulation of machines, objects and equations. But bit by bit (byte by byte) computer designers deconstructed the notion of a tool itself as the computer came to stand not for a tool but for nature itself. In the process, discrete scientific fields were linked by strategies of practice that had previously been separated by object of inquiry. Scientists came together who previously would have lived lives apart, and a new subfield came to occupy the boundary area.’[203] Galison’s emphasis on communication and collaboration between specialists from different backgrounds in the development of the specialism of computer simulation cuts across the stark divisions between epistemic cultures suggested by the work of Knorr Cetina and Biagioli. Galison in fact closes with a critique on the idea of a totally ruptured science. He points out how, instead, communication between different fields formed the basis for the development of even more variety among sciences.[204]

Now, the theoretical physicists from Groningen and psychopharmacologists from the Utrecht group are not likely to form a ‘trading zone’ any time soon. However, the more fundamental contention raised by Galison does apply to the conceptions of science described above. The very experience and meaning of science may be fundamentally different for practitioners from different fields. The difference itself may be such than interchanging the views of science between fields would inhibit the functioning of each field. However, this does not necessarily exclude that there are shared values, or that there is a possibility for common ground. Comparing different scientific practices, cultures, paradigms, or in my case the subjective perceptions of scientists from different backgrounds is somewhat like comparing apples and oranges. The common idiom emphasizes that apples and oranges are nothing alike, they defy comparison because they form separate categories – one could even say they are incommensurable. However, the thing with apples and oranges is that from

[203] Galison, ‘Computer simulations and the trading zone’, 157.
[204] Ibidem.

another point of view they can both be considered fruits. Similarly, there may be features to high energy physics and psychopharmacology that justify their identity as sciences.

Psychopharmacology and HEP: a sign of unity

Common ground points at a possible unity of the sciences. Unity is based on the notion that there is a commonality to different sciences that transcends their differences. Unity proposes characteristics that make psychopharmacology and high energy physics identifiably scientific, along with other sciences. Moreover, it separates sciences from non sciences and stratifies sciences amongst themselves. To start with, note that psychopharmacologists and theoretical high energy physicists adopted different criteria for distinguishing more from less scientific. One unifying principle has already been pointed out by Markovsky in his contention that sciences are connected through logic and method.

In support of Markovsky's argument, we have also encountered commonalities between both identities of science discussed in the case studies. Scientists from both fields described science as 'curiosity' and 'finding out new things'. Additionally, both pointed out the importance of experimental verification. In this sense their views conform to the idea of the scientific method. Practitioners from both fields described a procedure of theory/hypothesis, experiments, measurement and verification or falsification that is the textbook version of the scientific method. Interestingly, both fields also mentioned a matter of serendipity in the findings of science. The high energy physicists pointed out the unexpected applications following theoretical or pure findings. And at one point Lucianne Groenink also pointed out coincidental nature of novel findings in an explanation for caution in automating experimental observation: 'Well, because most discoveries are done by coincidence. You can construct an experiment, administer a drug and put a rat in it. And if you then automatically measure with a system that can visualise lines in retrospect and measure them; ok it walked a hundred metres. But perhaps that the animal was jumping around through its cage. No idea, because the only thing you see when you look afterwards is that line, while a rat jumping around, that must be the ultimate antidepressant.'[205] To scientists from both fields novelties are often findings one stumbles upon.

However, before we rush into the conclusion of unity, this is point also shows divergence between both fields. Lucianne Groenink explained: 'Well, at that moment you have a coincidental finding. But you should do a consecutive experiment, ok it this is so, that

[205] LG (2) 4.46-5.23.

in any case, you come up with a new falsifiable set up.'[206] This testimony immediately illustrates a measure difference between both fields that has also been central in the first two chapters. One could equate Lucianne Groenink's hypothesis with Eric Bergshoeff's theorization of branes, and the falsifiable experiment with the experiments LHC at Cern to point out a shared methodological principle. However, a freak finding in theoretical HEP will not be converted into a falsifiable experiment. It would be theorised and in time verification might be sought by experimentalists. Even a coincidental finding at Cern would not lead to a new set up at the theoretical HEP base unit. Instead it would be worked out on a blackboard, modelled, explained and possibly accommodated in existing theoretical options. In other words, a common principle could be identified, but the practical procedures in both fields are truly different.

The difference most obvious from the subjective approach adopted in the first two chapters is the daily experience of science in both fields. If we compare Eric Bergshoeff's energetic elaboration of the timelessness of his quest and the mysteries of the universe to Berend Olivier's proposal for a more socially involved science – although this is also a matter of apples and oranges – we encounter two entirely different professional worlds, which interact with two entirely different environments. At the one hand the scientist who works on paper and blackboard, negotiating theoretical solutions to fundamental physical problems, who treats science as a curiosity driven quest for knowledge. On the other hand the scientist who guides several large experimental operations, synthesising results, in regular contact with industry and psychiatry, who has a pragmatic and engaged conception of science. Maintaining that the method such different fields is the same in principle at a certain point does not conclusively add up.

The central question is if, in contexts where science, knowledge and theory and experiment have fundamentally different meanings and different uses, general commonalities between method – although very different in procedure – justify methodological unity. If we agree with Knorr Cetina's line of reasoning that the cultural function of measurements, experiment and conclusions is different the idea of methodological unity cannot be upheld. Although I would not go as far as to say that such differences make for a disunited science, they still form serious objection against for methodological unity. The difference in meaning and experience of science does not justify the imposition of a common method. From the perspective of the individual scientist, ascribing such a methodological unity to fundamentally

[206] LG (2) 5.24-5.43.

different practices – in which one develops a falsifiable experiment and another can only wait for such feedback – does more harm to their specific practices, than it does them justice.

Additionally, the assumption of methodological unity would also connect science to domains we as science studiers would not directly count as our research object. Making Markovsky's assumption our definition of science we should also count drug research by the pharmaceutical industry, fighter plane development, test drillings for oil by a petrochemical company, and market analyses by a bank as science because they employ the same general methodological cycle as psychopharmacology and theoretical high energy physics.

The reasonable doubt of the individual's experience over the supposed unity has even clearer repercussions if we look at the nature that scientists from each field try to connect to. Another type of unity attributed to sciences is based on the assumption that in the end all sciences investigate the same world, and the same nature. This type of unity can also be distinguished in Barry Markovsky's proposal of methodological unity, in his assertion that science is connected at deeper, more abstract levels. A similar point of view has been uttered by Stephen Hawking in his expectation of a theory that would explain chemical reaction from theories that describe the inside of an atomic nucleus. Moreover, Hawking suggests that a single set of ultimate natural laws would also be able to explain our existence. Indeed, also the idea of reductionism relies heavily on the assumption of natural unity.

Ian Hacking calls this type of unity metaphysical unity, and he asserts that it is predicated on what he calls the metaphysical sentiment that: 'there is one scientific world, reality, truth.'[207] Furthermore, Hacking explains multiple theses of unity have been developed based on this sentiment, he distinguishes three. First there is the thesis that all kinds of different phenomena must be related to each other, he calls this the interconnectedness thesis. Secondly, he describes a structural thesis which states that there is a structure of logical relations between laws or truths. He also distinguishes a taxonomic thesis which states that there is an ultimate, single classification to all known classifications.[208] Interestingly, Hacking illustrates each point with historical examples. To name one, he describes Faraday's belief that the world had to be of such a nature that magnetism and light affect each other, as an instance of the thesis of interconnectedness. Additionally, he also points out how Faraday's conviction led to his later findings.

[207] Ian Hacking, 'The disunities of the sciences' in: Peter Galison and David J. Stump *The disunity of science: boundaries, contexts and power* (Stanford, Stanford University Press 1996) 44-45.
[208] Hacking, 'The disunities of the sciences', 46-47.

Hacking also presses the reader not to understand the metaphysical sentiment as too exclusive. He states that what is clear in every day life to everyone, without much questioning, is that there are many realities.[209] And although Hacking's statement is mainly aimed at distancing unity from a politicised conception of scientific realism, it also touches upon a subjective reproach against unity as such. Even if we agree with the notion that there is one scientific world, reality and truth, a sentiment that Hacking argues is deeply rooted in our scientific views. And even if we maintain that the scientists interviewed for this paper display this particular sentiment. Then still, we would be faced with two diverging descriptions of that world, reality and truth from the accounts of the scientists above.

What has been integral in my subjective approach is that views are not treated as views on a distince reality, but as the reality of a particular individual. We have seen how psychopharmacologists testify to a less direct, and complete connection to nature than theoretical physicists. Moreover, psychopharmacologists also have a different conception of the principles of nature than theoretical physicists. Whereas physicists speak of 'basic organising principles' and 'the language of reality' conveying a structural unity, psychopharmacologists abstain from such universal expressions and testify to open ended lines of causation and biological principles. As made clear from the start it is not my task to determine if, or what which conception of nature and reality is true. However, the fact remains that scientists from each field relate differently to metaphysical notions such as truth and reality. In light of this, the imposition of a specific metaphysical unity as the identity of science would, much like in the case of method, do more harm to the epistemological and ontological convictions that play such an integral part in a specific practice than it would do them justice.

Unifying principles also are also suggested by more critical perspectives on science. For example, postmodernist readings of scientific history have identified science as a specific type of culture or discourse. Bruno Latour explains how postmodernists display nature as totally separated from cultural reality. He points out how postmodernism creates an unbridgeable divide between the natural and the social.[210] From this situation it becomes possible to display nature as a semiotic construct determined larger linguistic or cultural order. Latour calls this depiction of the world the discourse repertoire. And it is on the basis of this particular repertoire that science can be regarded as an oppressive force of reason with an authoritarian

[209] Hacking, 'The disunities of the sciences', 44.
[210] Latour, Bruno, *We have never been modern* (Harlow etc. 1993) 89.

view of the truth and the nature of reality.[211] However, also this type of unity of science as a specific culture or discourse does not really fit our findings.

First of all, the idea that the same type of authoritarian realism exists among scientists is downplayed by psychopharmacologists' views on softer fields such as psychiatry. Instead of imposing their version of disorder on psychiatry they accept, respect and even welcome psychiatric input. Moreover, psychopharmacologists also did not view their own categorisation of disorder as absolute depictions of the illnesses in psychiatric patients. Strikingly, among theoretical physicists, where the idea of correspondence between knowledge and nature was central and had a rather universal connotation, we also encountered room for intuition, belief and even religion.

If we would take realism as the identifying feature of science we would also have to consider – and equally criticise – for example, Benjamin Adamah's work as scientific discourse. With Adamah, and arguably with many other New Age writers, we find a much more realist conception of scientific knowledge. The scientific concept of zero point energy is understood as a literal representation of reality, scientific knowledge is understood as a literal depiction of reality. Granted, the final reality proposed by Adamah has properties not described by the initial concept, but from the point of view of postmodernist critique this should not be very different from scientists' unjustified depiction of science as reality.

Similarly, scientists do not display either empiricism, rationalism or a systematic mix of the two as an epistemological starting point. Whereas psychopharmacologists heavily rely on experimentation, the validation of methods and objectified measurements are an important point of attention in their science. In contrast, practical attention in theoretical physic is concentrated on logical consistency and elegant formulation. Taking the differences in practice, language and experience into account, both sciences cannot be explained from a common discourse, frame of reference or a general attitude.

All the more, we also find a mix of empiricism and rationalism outside science. Besides the earlier examples of 'scientific method' in non scientific practices, another example can be found in military strategy. Dutch Royal Air Force Colonel Frans Osinga writes war and strategic behaviour as 'fundamentally in flux'. He explains that strategic theory cannot achieve general applicability and validity. Theory will not be all embracing, it will not unify existing partial theories, and it will not have 'a high level of predictive capability, the standard of the "hard sciences".'[212] And even if an underlying pattern would be

[211] Latour, *We have never been modern*, 125.
[212] Osinga, Frans, *Science, strategy and war: the strategic theory of John Boyd.* (Delft, Eburon 2005) 30-31.

discovered and theory would attain some level of predictability 'the paradoxical nature of strategy guarantees that the pattern will be altered […] the very fact that one places a stone so as to construct a foundation alters the environment.'[213] Although Colonel Osinga differentiates between strategic theory and knowledge from that produced by natural sciences, he does describe a feedback loop between theory and observation. He identifies the same loop in the work of another strategist, John Boyd, who forms the main topic of his publication. Osinga writes that both generalization and abstraction to Boyd's work as well as the use empirical data.[214] More concretely, Boyd's main exploit, is the formulation of a circular model of observation, orientation, decision and action that strategists, combatants and combatants alike go through in combat situations.

This short detour through military strategy and New Age authorship, although quite randomly selected from myriad of uses and views on science, show that science can not simply be equated with a specific method, epistemological attitude or ontology. In a more general sense, science can also not be identified as a search for knowledge. Clearly, with Osinga and military strategists we also find an inquisitiveness in the unkown, the development of both practical solutions and of general theory. In this sense we must also conclude that scientists' a view of science as curiosity and finding new things is not a necessarily unifying factor. In fact, curiosity as well as the feedback loop between theory, test, observation and falsification or verification can also be found in non scientific practices. Of course such shared interests and purposes serve as an objection against the idea of a totally fragmented science, however they do not justify they scientific unity either.

From our consideration of the disunity and unity of science we can conclude that the comparison between psychopharmacology and theoretical high energy physics does not justify a conclusive choice for either option. The idea of a disunited science based on incommensurability amplifies the differences between sciences, and ignores common grounds that enable communication and understanding between sciences. If I may informally rephrase Galison's argument it is communication between different sciences that fosters new sciences, and increases variety, as opposed to increasing variety that inhibits communication. Additionally, our two case studies also display commonalities. And certainly one would expect scientists from both fields to be able to communicate about science and their respective practices if they would happen to meet.

[213] Osinga, *Science, strategy and war*, 30-31.
[214] Ibidem, 186, 240-243.

However, scientific unity also finds little support in the diverging testimonies of scientists. In principle general similarities in method, nature and attitude can be discerned in both sciences In practice the meaning and practical experience of such general similarities works out very differently in both cases. Moreover, if unifying principles are to be a sign of unity within the sciences, they should be specific to science, and identify science from non science. As I have illustrated with some examples this is not convincingly the case.

Nevertheless, I have also distinguished between science and non science, I have also suggested demarcations. From the academic point of view of our field of study, we certainly also recognise an object of study which has traits that set it apart from other objects of study. So the conclusion that there is neither unity nor disunity is something quite different from saying that science has no identity, or cannot really be identified. In an effort to understand the identity of science and I propose to take another look at identity, rather than deciding on a specific identity of science.

Identity

Before we go into the problem of scientific identity, we should return to the discussion of unity and disunity. The reader can take away many instances of variety among sciences, and many different accounts of disunity from *The disunity of science*. However, from the material presented – as well as from the sciences discussed in this paper – one cannot give a conclusive answer over the state and extent of disunity in science. The reason for this can be found in the style of the discussion.

An interesting contribution in this respect is by Richard Creath in which he points out there is a multitude of distinguishable unities of science, even within the Unity of Science Movement. He sets apart the unities proposed by Otto Neurath and Rudolf Carnap, and consequently positions Peter Galison's view of science rather close to that of Neurath and Carnap. His line of argument is twofold. First Creath stresses that Carnap did not see unity as a scientific a priori but as reliant on evidence. This is why Creath deems Carnap a 'coherentist' rather than a 'standard foundationalist.'[215] In a similar fashion Creath historicizes Neurath´s supposed holism and physicalism, and replaces them with what he calls social idealism; the idea that 'an observation can in no sense be a comparison of something with reality, and justification can involve only the comparison of one appearance to

[215] Richard Creath, 'The unity of science' in: Peter Galison and David J. Stump *The disunity of science: boundaries, contexts and power* (Stanford, Stanford University Press 1996) 160.

another.'[216] As a result Creath concludes that the favour or criticism toward the Unity of Science Movement espoused by many authors only refers to a distinct type of unity rather than to the unities proposed at the time. The second part of Creath's argument consists of pointing out the compatibility between Galison's view and the views Neurath and Carnap on the grounds of a shared rejection of extreme holism 'that turns various disciplines into island empires.'[217]

The elegance of Creath's contribution to our discussion resides in the way he dissects the debate between unifiers and disunifiers of science by showing the variety inherent to both sides. He downplays the absoluteness of unity proposed by Neurath and Carnap, and he underlines the unifying tendencies in the work of Galison. Additionally, he points out that the biggest fear for most critics of the unity of science is not the actual unities proposed by Neurath or Carnap, but 'the imperialism of physics' which would make other sciences its 'lackey'. Creath immediately takes away that fear by pointing out that 'chemical regularities discovered so far are derivable form established laws of physics. It does not matter. So far it has not diminished the number of chemists, nor is it likely to.'[218]

Creath is not alone in his efforts to historicize unity, over the pursuit of disunity. The contribution of Hacking counts as another prime example of such historical nuance. Already in the first paragraph Hacking points out elements of diversity in earlier accounts of scientific unity put forth by Whewell and Comte.[219] The point that Hacking and Creath authors try to get across is that historically unity has not been the antonym of diversity among sciences. In debates about unity or disunity, however, the disunity of science is brought up against 'the imperialism of physics.' Disunity, by opposing the reductionism on the basis of method or nature, creates a much stronger version of unity than what can be identified among actual historical unity thinkers. In opposing reductionism and the idea of a 'single scientific world, reality, truth', only a total separation of sciences suffices to break down the hierarchy among them. The other way around, the totally fragmented science based on a description of sciences as separate cultures or paradigms and their incommensurability can only be countered by asserting a common identity; a unity on more fundamental levels than culture, practice, paradigm, discourse etcetera.

The problem of disunity not only displays likeness to, but actually shares in the polarisation of the realism-relativism debates. Arguments between disunifiers and unifiers

[216] Creath, 'The unity of science', 165.
[217] Ibidem, 167.
[218] Ibidem, 167-168.
[219] Hacking, 'The disunities of the sciences', 37.

amplify either the differences or the commonalities between sciences in an effort to exclude the opposing option. In the process, both sides draw in ontological and epistemological arguments and assumptions. For example, Karin Knorr Cetina wonders that ‘perhaps it would be time to ask, if we have to have foundations, whether we cannot build a theory of knowledge from circular foundations.’ Later she adds ‘Cultural systems of behaviour, as we know, construe the world in which they [scientists/people in general] live differently.’[220] Stephen Hawking, although not engaged in the disunity academic debate, expresses a clear inclination towards unity accompanied by the appropriate epistemological and ontological assumptions. The reduction of chemistry and our very existence to a single physical theory assumes a direct between theory and the basic principles which structurally unite all phenomena. Similar to Hawking although less explicit, Markovsky also suggests such a structural unity in his assertion of a logic that connects science on deeper and abstract levels.

The dilemma of choosing between disunity and unity is the same as the dilemma in the realist versus relativist type of debates. The positions on each end are not only about science, but also about its truth claims and the nature of reality. Moreover, the positions are polarised because as they are aimed excluding each other. Galison also identifies this problem: ‘At the root of most accounts of the development of science is the covert premise that science is about ontology.’[221] But perhaps the nature of reality is not the most suitable way for identifying science. Let us retrieve the functional understanding of personal philosophical systems as a relation between the subject and the environment. If we now zoom in on the part of our professional environment that is formed by our subject matter, we can conclude that the polarised philosophies behind unity and disunity might not be the most effective frameworks to deal with the variety we observe in science. On the other hand, if this should be our purpose, both positions are perfectly equipped for keeping each other in a deadlock.

Perhaps we need to approach the identity of science in a different way. We require an approach that captures the variety among sciences, but which is also able to grasp commonalities between sciences, without falling back on claims about the nature of science. In short, we need an approach that does justice to the commonalities as well as the differences between sciences, without pinning different practical identities down on one specific unifying principle. The accounts of the scientists above do not provide enough of a basis to formulate a conclusive identity of science or an explanation of scientific variety. Functionality has been

[220] Knorr Cetina, ‘The care of the self and blind variation’, 310.
[221] Galison, ‘Computer simulations and the trading zone’,118.

brought up as a partial explanation of differences between identities of science, by embedding identities in the practical context from which we constructed them. However, functionality cannot be presented the basis of the identity of science, in all likelihood a similar functionality can be discerned in the views of Adamah and Osinga. Despite the fact that the case studies do not give conclusive reason for a choice between unity and disunity, and because the choice between unity and disunity is a choice between exaggerations, I do feel we should attempt to find a more fitting explanation scientific variety.

Obviously, both the subjective identities as well as the functionality of identities as subjective relation are theoretical constructions for the purpose of this paper. Nevertheless, by adopting a subjective perspective I have been able to remain close to the experience of the scientists without presupposing a specific nature of the reality to which this experience relates. And by differentiating between philosophical positions conveyed in individual views I have been able tot get a hold on the fundamental differences dividing those fields. As opposed to creating a culture in which scientists take part, which defines their action and experience, the subjective approach avoided assumptions about the structure of reality. Defining the subjectivity as a field of experience made it possible to have the scientists' define the structure of reality, and to make the relation between scientists, science, nature, and society appear as a logical and functional adaptation to the requirements of their practical environments.

Subjective relations can be understood as functional on the basis of the interactions they facilitate. The power of a specific conception of science, knowledge and reality lies in its ability to cope with the requirements of a specific environment. As Fuchs and Biagioli point out this ability should not be evaluated solely on the basis of scientific achievements, instead scientific achievements can only be made if the environment in its entirety is effectively dealt with. As such I feel I have made a compelling argument that the views of scientists regarding science, and the specific identity of science described by these views, are not as simply matters of opinion, but are a deeply rooted and integral in a scientist's practice. We have seen how scientists view science, and themselves as practitioners of science, in a way that facilitates purposeful interaction in light of their practice.

I propose to return to this functional argument in an attempt to redefine our conception of identity. Let us, for now, assume that the foregoing summary of the role of views in individual action is accurate. At least to some extent most of us will agree that people with different ideas will come up with different responses to identical situations. Now, identity as described from within, from a nature or basic feature of science – in other words an essentialist conception of science – has not provided us with much grip on sciences in their diversity. As an alternative I would like to direct the reader's attention to a specific sociological approach identity. Within a sociological framework we can describe identity from its function. With Fuchs and Luhmann we already witnessed an explanation of identity of a culture or person from the interactions between that culture or person and its environment. Obviously, the environment may consist of other cultures and persons as well as from material surroundings.

In line with Fuchs and Luhmann, sociologist Harrison White also explains identity from interaction: 'persons should be derived from, rather than being presupposed in, basic principles of social action.' Additionally he explains that: 'persons, in the ordinary sense of the term, are neither the first nor the only form in which identities appear. Much theory stipulates persons, takes them as preexisting atoms.'[222] Notably, it is this last mentioned type of theory from which White wishes to part. However, he is also not satisfied with a structuralist approach, because this takes 'control for granted and tries to explain away identity. Structuralism builds on the myth of society as some pre-existing entity.'[223]

We must be careful to equate White's identity with the identity of science. In relation to the subjective perspective, White's identity concerns the identity of the subject-scientist, rather than the identity of science held by that subject-scientist. Similarly, the identities of science proposed by the science studiers should not be viewed as identities. To White it would be the identity of the subject-science studier that he attempts to explain. For the purpose of our explanation we can understand the subjective relation created between the scientist and his or her environment as a Whitean identity.

White proposes an alternative understanding of identity which explains identity as continually formed and shaped in action. Action can be social interaction as well as directed against the identity itself, both should be understood as control efforts. He explains social structures, relations and processes as 'traces of successions of control efforts.'[224] If we apply White's notion of identity to the subjective relations of the scientists, we are able to retain our proximity to the views of the scientist as part of a functional way of interaction. Using his specific idea of identity we can explain the views of a scientist as efforts to control his or her

[222] Harrison C. White, *Identity and control: a structural theory of social action* (Princeton, Princeton University Press 1992) 8.
[223] White, *Identity and control*, 9.
[224] Ibidem.

environment. At the same time White's identity also acknowledges the power of the environment in shaping an individual's particular views.

Now, I am not in the position to immediately apply White's perspective to science at large and provide an identity of science. However, White's explanation of identity is able to accommodate the differences between psychopharmacology and theoretical high energy physics, while maintaining the specific subjectivity of scientists. Additionally, White's explanation also suggests a surprising explanation of commonalities. In line with the insight provided by Fuchs and Biagioli we can explain the subjective philosophical systems from the specific requirements of their social and natural environment. Where Biagioli traces the existence of a paradigm back to its success as an adaptation to a specific socio-historic niche, White explains the development of a specific scientific identity from specific interactions aimed at control of environment input. White explicates subjective action as interaction in the development of identity. In comparison, the explanation of Fuchs and Biagioli proposed in the first chapter depict cultural or paradigmatic identity more as a state of affairs following the interactions with an environment.

Pro forma we can add this notion of activity to the earlier explanations, although the basic argument in the explanation remains roughly the same. In the case of psychopharmacology we can rephrase the explanation that the pragmatic conception of is a necessary adaptation to the input from other social actors. With White we may add that the pragmatic identity displayed by psychopharmacologists also is an active attempt to structure that input into a conducive research environment. In other words, its pragmatism is the result of persistent interactions aimed at controlling both social and practical complexities, and is continually shaped by the interactions that characterise the daily practice. Concretely the views of psychopharmacologists not only displays the requirements of their specific niche for neurochemical research in mental disorder, these views are actually both the sign and the means of ongoing engagement with the environment perceived in that specific niche.

In a similar line of reasoning we can rephrase the idealistic conception of both scientific progress and the range of theory and its ability to bridge the distance between the scientist and the organising principles of nature (through correspondence). The open realism and idealism do not only serve as a motivational minimum required for doing abstract theoretical work which is distanced from societal use and practical exponents. The idealistic conception of epistemological correspondence on the part of physicists is also and active effort in realising that particular, eventual value in their work.

Although all this may seem highly abstract and perhaps trivial, from a subjective point of view White's conception underlines the importance of views as part of the daily interactions that make for a scientific practice. Instead of explaining the identity of a practice from social interactions, he explains it as a central tenet of those interactions. The identity of science that develops in one practice is not only functional but essential in that specific practice. In other words, there may be no a priori rationality to the shape of a specific paradigm, but had the views/control efforts of the subjects involved for whatever reason been different, then the shape of the paradigm would not have been the same. In short, White's perspective highlights that if the views of scientists had developed differently an altogether different practice would have been the result. Disunity, or rather the variety among sciences, can thus be accommodated and more specifically understood using White's notions of identity and control.

However, as both Galison as well as our two case studies support even fundamental differences between sciences do not necessarily justify viewing them as totally separated. Interestingly, in respect to these commonalities White describes identity from recurrent interactions. If we adopt this interactional perspective and look past the immense variety of cultures or paradigms that make science seem separated, we can also distinguish common grounds. From the viewpoint of interaction psychopharmacologists and theoretical physicists also have persistent interactions in common. Even the development of altogether different paradigms and scientific cultures took place as part of the same social structures. A common ground between psychopharmacology and high energy physics can perfectly well be identified by the fact that both practices take part within the institution of the university. Scientist from both fields are embedded in this taken for granted, and easily overlooked set of persistent interactions. Policy, salaries, department buildings, educational duties form examples of a common set of relations that have shaped both scientific identities. And perhaps, but this is a bold conjecture, it is from these institutional preconditions that scientists', as well as our own implicit demarcation criteria can be explained.

This paper may seem to have developed into the longest possible argument for an institutional perspective on science. However, also from the subjective point of view, from the daily experience of scientists a common or comparable work environment and social structure provide valid grounds for questioning the disunity thesis that science is fundamentally fragmented. In a mundane example, even if the subjective experience of science by two scientists from different fields is entirely different to the extent that both have fundamentally different views on what science is, why science is important, what knowledge is and how it relates to reality, and have an entirely dissimilar conception of the nature of that reality. Even if both scientist use and experience supposedly shared notions and methods in a completely different way. Even with the most far reaching understanding of culture and paradigm, an observer would not be able declare a state of disunity, because there is also common experience based on common interactive structures. In an even more mundane example, we can perfectly well imagine a conversation about science between a psychopharmacologist and theoretical physicist, the variety comes into play at the point when they start to disagree.

Strikingly this institutional identity, or rather the observation of a specific type of actions and interactions that scientists of different fields share as part of their daily practice, is also able to do justice to the experiences and scientific identities that are specific to different practices. Amid all the ontologized and politicised notions of principally unified or disunited sciences this common ground between practices is almost hidden in the taken for granted every day routine of scientists. When witnessing the identity debates – in the questions if science is unified or disunited, if it is socially or naturally determined, if it represents nature or the cultural state of affairs – our attention unwittingly drifts towards questions about epistemology, ontology and the status of knowledge. However, in terms of an insight in the actual practice of science such debates pose any reader with a choice between two irreconcilable, opposite and equally inapt descriptions of science. Neither of any of the opposing options gives a satisfying description of science or only begins to do justice to the variety of scientific practices. Rather, one would like to pick a bit of both; characteristics of unity and disunity, of nature and society, of realism and relativism apply to science. However either side of any identity debate defends an essential identity of science that irrevocably conflicts with its opposite, making ‘a bit of both’ an intellectual no go area. That I would say is the reason for the endless cycle described by Steve Clarke.

From a functional point of view such polarised philosophies and identities work fine in an environment where the opposite is an actuality. However, as Richard Creath pointed out, even if chemistry can be reduced to physics, as long as the number of chemists does not drop nothing really changes. Part of the escape from Clarke’s endless cycle Clark can be accomplished by withholding from such definitive identifications of science, and naturally also of the reality it relates to. The identity of science, in short, should not be presumed.

Conclusions

In the foregoing chapters I have tried to develop a subjective approach to science. The reason for this was not so much the universal applicability of the approach to the study of science. Instead, the virtue of the subjective approach exists against the background of ongoing identity debates. It asks no a priori statement about the nature of reality; it just supposes it as part of the subjective experience of the environment. Ontological questions remain open in the subjective approach. And so do questions about epistemology and the social status of science. In the first two chapters I have left the task of answering these questions to the scientist.

Subsequently I have developed a conception of subjectivity in which the scientists' views can be understood as and from the interactions between the scientist and his or her environment. And because the views of scientists on science also involve assumptions about their environment – its nature, structure and the way that science interacts with it – we were able to construct a subjective identity from the web of convictions that related scientists to science, society, and nature. Although the identities and the philosophies that support them count as prime examples of literary construction, they are supported by the actual testimonies of sciences. Furthermore, by casting the many utterances of the scientists from the interviews into familiar philosophical categories the subjective identities of science became comparable to our own views.

By adopting an expanded understanding of the subject as a field of experience the philosophical positions attributed to scientists remained close to their personal views. In psychopharmacology we encountered a pragmatic attitude towards the status of scientific results, method, the organisation of research and the cooperation with third parties. The argument from the subjective point of view is that the attitudes not only fit together, but work together to facilitate the interactions required for the practice to be successful. In the second chapter we came across a wholly different practice and an equally different view of science and all the different arguments that support that view. Knowledge was viewed as more correspondent to reality, ideas of reductionism and a stronger sense of scientific hierarchy characterised also characterised the physicists' conception. These views were supported by a structural metaphysical unity of basic organizing principles. Science was defined under stricter criteria based on notions of purity and fundamentality of scientific knowledge. What I have called a an idealistic view of science, combined with physicists' faith in correspondence between theory and reality, were explained as fitting adaptations to a rather closed social environment where both practice and produce consist of theoretical, abstract and hardly tangible ideas.

A first finding of a personal nature was how far the professional life and the ideas of scientists are removed from the conflicts over the identity of science that a student in the history and philosophy of science so regularly digests. An equally interesting finding to me – after an academic trajectory firmly rooted in the critical tradition – was that scientists are not all realists in the postmodernist sense. In addition, those that do conform to many of the postulates regarding scientific realism, as was the case with the theoretical physicists, not at all display the authoritarian attitude towards truth and reality as suggested by postmodernist critique. Indeed, theoretical physicists did uphold a structural metaphysical conception of unity, however, they generally left the open what the final description of that unity would look like. They even allowed room for religiosity, intuition and feeling in their practice. With the psychopharmacologists I expected stark physicalism towards problems of mind, psyche and disorder. However, displaying an utterly practical, as opposed to principal conception of mind they held fast to the biological substrate but also allowed for distinctly psychological aspects ranging from god coconsciousness to mild forms of depression treatable with a good conversation. All in all, I found it an academically invigorating experience to talk to scientists about their work and their views.

The interviews were certainly interesting, and they have put many of my ideas on science in a new perspective. As we have seen many of the convictions shared and learned in academic papers concerning the identity of science fell short in providing an explanation. Surprisingly, even the notion of disunity – although one might expect it to describe a principal variety among sciences – proved incapable of living up to its claims. Disunity supposes, often only implicitly, a disunited science in the sense of totally separated incommensurable fields. I have concluded that although the differences between theoretical high energy physics and psychopharmacology are considerable – even more, that they are fundamental in the functioning their respective practices – they do not justify the type of incommensurability that supposes an inability to communicate across cultures.

Equally, common grounds provided by methodological, metaphysical and even critical theses of unity also proved incapable to accommodate the fundamental differences in experience between psychopharmacology and theoretical high energy physics. In addition, using some examples from non scientific domains I have made the argument that these unities do not confine the supposed unity to sciences alone, but would also involve other domains.

Although the examples qualify as illustrations rather than evidence, I feel the argument itself poses a strong objection against any essentialist identification of science.

The polarisations the unity and disunity of science do not divide all authors into opposing camps, as was visible in many of the historicizing and nuanced contributions to *The disunity of science*. Nevertheless, the polarisation itself is not a straw man. For, what is visible among each of the contributors is that they all in one way or another deal with this polarisation. The arguments proposed by for example Galison, Creath and Hacking, although all unmistakably nuanced toward the supposed unity or disunity of science, are all developed against the background of an apparent opposition. Interestingly, these oppositions greatly impair our grasp of science. Realism or relativism, natural or cultural reality, unity or disunity, form false choices as neither of each opposition can truly do justice to the variety among scientific practices and individual experiences of science. However, from the viewpoint of each opposing option not only science, but also its truth claims and the reality it relates to are described accurately.

If we recapitulate the idea of identity proposed by White, and apply it to the specific subjectivity of scientists, we can see how it functions as a facilitator of interaction with the environment. Conversely, White's identity can also explain a scientists' conception of science as the result of recurrent control efforts towards his or her environment. The idea of persistent interaction and relations forms the basis for the idea that shared interactions in the form of institutions can possibly account for the common ground between sciences. And indeed from the perspective of the individual experience an occupation at a university forms a common experiential world, and a common cultural background.

However, there is another interesting application for White's view of identity which brings the identity debates even closer. I have already turned the tables on our own field by questioning if the polarised approach of science is such a functional means of grasping science. A consequent question might be what environment and what interaction enabled the polarised and essentialist approach to science to become successful. As already hinted earlier the opposition of disunity and unity, realism and relativism, cultural and natural reality are above all very effective in denying the claims of their opposite. Therefore, they can be regarded as functional in a perceived environment where any encroachment of a certain set of views on one's own perspective is regarded as highly undesirable, or even existentially threatening.

This conclusion may seem presumptuous based on the fragile theoretical framework offered by this paper. However, I see it as a natural next step towards self understanding and therefore as an almost obligatory part of this paper. Arguably, from a historical standpoint most of the incentives for the polarisation of views on science originated from the side of the humanities and literary criticism. We might consider relativist, disunited and culturalist identities of science as attempts to control encroachments of naturalism and scientism. Of course, such considerations require a more historical substance than I can provide at this point. Nevertheless polarised, ontologized and essentialist notions of science may have served a purpose at some time in the history of our field they currently inhibit an accurate understanding of our subject. Following White's notion of identity through action, if the polarised oppositions remain the basis for debate and the point of reference for our views, our field will develop itself as a most effective means for fighting the previous science war. In the technological and technocratic age we live in we might consider more useful directions for our field's development. Even more, in a pressing environment of budget cuts and diminishing resources the search for a more up to date purpose can be viewed as a requirement, rather than a preference.

A first step would be to disentangle our works from the lasting oppositions; a more fundamental step would be to stop mobilizing science in support of our claims about the nature of reality. A possible practical lesson to any investigation of science might be to avoid 'object fallacies', reading an immediate ontological argument in the work of scientists (as opposed to the subject fallacy deemed so counterproductive in more exact fields). Obviously, entangling objective reality and scientific practice also requires us to reconsider of our use of many commonplace notions such as culture and paradigm. As a personal lesson I take away from this project that staying close to the subject of enquiry enables one to set a course independent from the presupposed and politicised categories that, often implicitly, dominate our field.

Sources

The interviews were conducted in Dutch and recorded with an audio recorder from a laptop computer with an integrated microphone. The audio files are enclosed in the CD-Rom attached to the paper version. All audio files are in MP3 format and can be played from compatible media players.

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