Constructive empiricism in the social sciences

Abstract

'What problems face the aspirant empiricist today?' is the question Bas C. van Fraassen asks in his seminal work The Scientific Image (1980). In this thesis, I interpret this question as a challenge to develop constructive empiricism [CE] in a field of scientific inquiry other than the context of physics in which it was conceived. The first part of the thesis expounds CE with reference to classical empiricism, discloses some of its fundamental assumptions, and spells out in detail its account of science. In the second part of the thesis, CE is extended to social science. Since CE was developed in the context of natural science, I take an articulation of the alleged fundamental differences between natural and social science as indicating challenges a CE-outlook on social science must address. I also provide a brief history of the gap between the sciences. Then, in the bulk of this thesis, I argue that CE's model view accommodates social science, that description, prediction and explanation in the light of CE are proper fruits of inquiry in social science, and that CE is able to make sense of the differences in the concepts used in natural and social science. In the discussion of the feasibility of CE for social science, I show concurrently that contemporary articulations of the differences between the natural and the social sciences pose no insuperable problems for the constructive empiricist.

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Acknowledgements

I would like to thank dr. Guido Bacciagaluppi and dr. Ruud Abma for their valuable time, comments and help during this thesis project, dr. Jaap Bos for being third examiner, and Willeke Verduijn for support throughout the year.

I would also like to thank my family, Albert van Dijk, Leonor Villa, Cristhian Felipe Pinzon, and Teun and Corrie Verduijn, among others, for always supporting me.

Contents

Introduction

'What problems face the aspirant empiricist today?'[1] is the question Bas C. Van Fraassen asks in the introduction of his seminal work *The Scientific Image* (1980). Let us pause to draw my interpretation of that question. Someone who aspires to be an *empiricist* has a particular theory of knowledge, hence, if we maintain that science is humankind's most systematized and careful attempt to obtain knowledge, that person has a particular epistemological view about science. Someone who aspires to be an empiricist *today* faces a variety of scientific disciplines that all claim to provide knowledge, so, that person must take a particular epistemological stance towards all of them. Thus, one way to understand the *problems* the aspirant empiricist faces is the challenge to develop and defend empiricist views in new areas.

My way of coming to grips with the initial question proceeds by the following strategy. In part I of this thesis I provide a comprehensive articulation of constructive empiricism. In chapter one, I introduce some classical empiricist tenets and contrast them with the 'constructive' flavour of empiricism that is developed in Van Fraassen's *The Scientific Image*. In chapter two I discuss constructive empiricism as a philosophy of the aims and structure of science.

In part II of this thesis I face the challenge to develop and defend constructive empiricism in the social sciences. I show that a constructive empiricist outlook in the social sciences is both possible and fruitful. Yet, since constructive empiricism was developed in the context of the natural sciences - mainly physics - spelling out a constructive empiricist approach of the social sciences requires that we consider some of the alleged fundamental differences between the natural and the social sciences, and use them as points of departure for the articulation of constructive empiricism in the social sciences. In chapter three I provide a brief history of the gulf between the natural and social sciences.

In the remaining chapters, I interpret the explication of the 'fundamental' differences between natural and social science in Jerome Kagan's *The Three Cultures. Natural Sciences, Social Sciences, and the Humanities in the 21st Century* (2009) as challenges for the articulation of constructive empiricism in the social sciences.

In chapter four I first illustrate some amendments Van Fraassen made to his

[1] Bas C. van Fraassen, *The Scientific Image* (Oxford: Clarendon Press, 1980), 3.

‘early’ model view, and then I show how they pave the way for a constructive empiricist account of phenomena, appearances, measurements, data, models, and empirical adequacy in the social sciences.

In chapter five I argue that the distinct questions each culture asks are no problem for constructive empiricism. In addition, I claim that description, explanation and prediction are proper ‘products of inquiry’ of social science in the light of constructive empiricism. Moreover, I argue that pragmatic explanation saves us from metaphysics in the social realm.

In chapter six a discussion of the nature of concepts in both natural and social sciences is provided. I illustrate the variety of social concepts used to explain social phenomena and show how pragmatic explanation allows us to make sense of this predicament of the social sciences.

The thought underlying this thesis is that the fruits empiricism has to offer the social sciences, have not been reaped by behaviourism, just as the fruits empiricism has to offer general philosophy of science and actual science have not been reaped by logical positivism.

Before I embark on this project, allow me to make some preliminary remarks to understand this project correctly. Constructive empiricism, since its general conception and presentation in 1980, has undergone quite some development. For example, in *The Empirical Stance* (2002) Van Fraassen pauses to reflect on the fundamentals of empiricism – the idea that experience is the sole source of information about the world – which his earlier work took more or less for granted, and develops empiricism as a rational attitude or stance one may adopt towards empirical science.[2] In *Scientific Representation: Paradoxes of Perspective* (2008) Van Fraassen develops an answer to a lacuna disclosed in *The Scientific Image:* the question how models can represent the observable world, since models are abstract structures and observable things are not abstract but concrete.[3]

Needless to say, besides internal developments, there are many interpretations and, equally unsurprising, vastly more disagreements about what constructive empiricism actually says, which is already apparent from the essays bundled in *Images of Science* (1985). In this thesis I give constructive empiricism a general interpretation

[2] Bas C. van Fraassen, *The Empirical Stance* (New Haven: Yale University Press, 2002), xviii, 32-40, 43.

[3] Bas C. van Fraassen, *Scientific Representation: Paradoxes of Perspective* (New York: Oxford University Press, 2008), 421.

that focuses more on the 'early' Van Fraassen, epitomized by *The Scientific Image*. Going into the details of constructive empiricism is unfortunately beyond its scope.

One caveat for this thesis is that talk about 'social science' is an abstraction. Surely something more must be said about what social science is and which disciplines are social sciences. This is a fair request. In my opinion, every science that carries out systematic inquiry into social phenomena qualifies as social science, with social phenomena roughly understood as the observable behaviours of humans. This thesis will mainly draw on sociological and psychological studies, but anthropology, pedagogy, and political sciences are social sciences on this count as well. There lingers of course a serious risk of oversimplifying these disciplines, but it is beyond the scope of this thesis to spell out all the details of the various disciplines, compare them, and see which aspects of their nature match my crude definition and which do not.

I do not see much use in discussions about which disciplines are proper social sciences according to this or that criterion and which are not. Is causal explanation a benchmark of social sciences? Or rather the description of unique events? I think that most scholars and laymen have some prima facie understanding of what social sciences are and what it is that makes them different from the natural sciences. That is good enough for present purposes.

Consider as illustration the haphazardness in the disciplines that comprise social science faculties. In all Dutch universities psychology belongs to the faculty of social sciences.[4] Yet, a quick search on the internet learns that in other countries psychology can be found under the medical sciences and natural sciences as well.[5,6] The search for a rigorous definition of social science and concurrent disciplines is vain. Hence, although my discussion of the social sciences in relation to the natural sciences is at some points inevitably general, the second part of the thesis is replete with references to concrete social studies.

4 Ruud Abma, *Over de grenzen van disciplines. Plaatsbepaling van de sociale wetenschappen* (Nijmegen: Uitgeverij Vantilt, 2011), 14.

5 "Medical Sciences Division," Division and Departments, Oxford University, accessed March 13, 2018, http://www.ox.ac.uk/about/divisions-and-departments.

6 "Natural Sciences," Areas of Study, Princeton University, accessed March 13, 2018, https://www.princeton.edu/academics/areas-of-study/natural-sciences.

Part I. Empiricism and constructive empiricism

Chapter 1. Classical Empiricism and constructive empiricism

1.1 Fundamentals of classical empiricism

In spelling out empiricist doctrine, we will proceed from the discussion of some basic principles to discussion of Van Fraassen's specific form of 'constructive empiricism'. Empiricism has a long history. A 'slogan' formulation will certainly fail to do it justice, but for illustrative purposes I will give it anyway: the fundamental tenet of empiricism is that knowledge is delineated by experience.[7] That is, we can only have knowledge through experience.[8] This claim can be traced back roughly to John Locke, George Berkeley and David Hume.[9,10] We take as illustration of 'classical' empiricism Berkeley's explication in his famous *Three Dialogues between Hylas and Philonous in opposition to Sceptics and Atheists* (1713)*:*

Here's what I think, in plain words. The real things are the very things I see and feel and perceive by my senses. I know these; and because I find that they satisfy all the needs and purposes of life, I have no worry about any other unknown things [...] *That a thing should be really perceived by my senses and at the same time not exist, is to me a plain contradiction* [...] *Away, then, with all that scepticism, all those ridiculous philosophical doubts!*[11]

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[7] Van Fraassen is well aware of the complicated history of empiricism and the problem of identifying the 'principles' of the tradition. To give one example, he agrees to some extent with Roger Woolhouse's point that the label 'empiricism' in 'British empiricism' is inapt for protagonists as Locke, Berkeley and Hume, since they would rather characterise themselves in terms of Platonism, Cartesianism, or Aristotelianism. See Van Fraassen, *The Empirical Stance,* 201-02.

[8]A point of caution is in order here. Firstly, in speaking about knowledge, one may argue that mathematical truths should count as knowledge as well, but surely they do not depend on experience in any way. Be that as it may, arguing for an empiricist grounding of mathematics is beyond the scope of this thesis. For the remainder of this thesis we can restrict the concept of 'knowledge' to our propositions about the world. I take mathematical propositions to be empirically empty, therefore, they need not concern us here.

[9]David Hume, *A Treatise of Human Nature*: *Being An Attempt to introduce the experimental Method of Reasoning into Moral Subjects* (London: John Noon, 1739), xvi-xvii.

[10] John Locke, "An Essay Concerning Human Understanding. Book II: Ideas," Jonathan Bennett, last modified August 2007, http://www.earlymoderntexts.com/assets/pdfs/locke1690book2.pdf, 18.

[11] George Berkeley, "Three Dialogues between Hylas and Philonous in opposition to Sceptics and Atheists," Jonathan Bennett, last modified November 2007, http://www.earlymoderntexts.com/assets/pdfs/berkeley1713.pdf, 41-42.

And, regarding how we should conceive of knowledge he continues:

[...] *all I want is to know what ideas are connected together; and the more a man knows of the connection of ideas the more he is said to know of the nature of things. If our ideas are variable, and our senses are not always affected with the same appearance - what of it? It doesn't follow that they aren't to be trusted, or that they are inconsistent either with themselves or anything else, except for your preconceived notion that each name stands for I know not what single, unchanged, unperceivable 'real nature'* [...][12]

These statements in the history of philosophy will be clarified, as well as some other epistemological concepts they invoke. It will turn out to be instructive to contrast constructive empiricism with this more 'classical' type.

The fundamental claim that experience is the only source of knowledge in Berkeley's account is relatively easy to grasp from the quotations above. What is real is what is perceived in sense perception. The things perceived in this way Berkeley calls 'ideas'. Humans can only perceive the world and its furniture as a collection of ideas; ideas are not to be taken as signs or appearances of objective things existing behind them. Ideas are not some kind of medium but rather the only things there are to know. The difference is subtle. Berkeley claims that it is not the case that there are things in the world that we can only understand as ideas; he claims that ideas are the only things there are. In addition he maintains that there can be no sceptical discussion about the reality of our experiences. What is experienced, simply exists, otherwise, we end up in a plain contradiction. For something that does not exist, cannot be experienced.

When Berkeley speaks about the things that other philosophers in his time had presumed to be 'single, unchanged, unperceivable', when he speaks about other philosophers invoking a certain 'real nature', he is speaking about the scholars that conceived things to be known *beyond* what is given in our sensory experiences (for example Nicolas Malebranche, who according to Berkeley asserted the existence of an 'absolute external world' in which extended things have 'real' natures, that is, 'true forms' and 'true shapes' that our senses are unable to perceive).[13]

Yet, for Berkeley there is no justification for the existence of such things. With

[12] Berkeley, "Three Dialogues," 52.
[13] Berkeley, "Three Dialogues," 31.

this claim he opposes both metaphysical realism and scepticism. If there is nothing to know beyond our experience, and if, as a result, we cannot make any sensible claims in terms of existence or truth beyond experience, the claim that there is a mind-independent world is redundant and vacuous. Further, contra scepticism, the fact that our ideas are sometimes varied and even chaotic does not mean that they cannot be trusted in principle. Truth should be conceived of as a relation between ideas, that is, as a relation between our experiences. For our experiences will normally exhibit a certain constancy, that is, ideas occur to us in a certain order most of the time. When, for example, we see one billiard ball move towards another stationary one, usually the moving one will hit the stationary one, which will in turn move. With regard to this particular constant conjunction of ideas, truth is defined.

As we will see, Van Fraassen's constructive empiricism also takes some features often invoked in scientific theories as things that should be understood as relations between ideas and not as aspects that describe an entity or process in the world beyond our ideas about it. So, let us outline the fundamentals of constructive empiricism, and see how it is related to the more 'classical' forms.

1.2 Fundamentals of constructive empiricism

Prima facie Van Fraassen has the same basic understanding of empiricism as Locke, Hume, and Berkeley. Although Van Fraassen is not in favour of slogan formulations of empiricism, as an empiricist he definitely has some inklings of the role experience plays in our pursuit of knowledge about the world.[14] William James and Hans Reichenbach maintained as core doctrine of empiricism that experience is the sole and strict source of information about the world, according to Van Fraassen. He further qualifies this limiting role by asserting that experience gives us information about the world only regarding what is both observable and actual.[15]

So here we encounter a new concept: 'observability'. Observability is to be

[14] An old problem for articulating empiricist doctrine is that the resultant doctrines are self-defeating or meaningless. For example, the claim that experience is the sole and strict source of knowledge is itself not an empirical claim. See Van Fraassen, *The Empirical Stance,* 38-41. Since Van Fraassen himself digresses in various works on what empiricism is in terms of core doctrines, I do not see this as much of a problem. See footnote 15 for illustration.

[15] Bas C. van Fraassen, "Empiricism in the Philosophy of Science," in *Images of Science. Essays on Realism and Empiricism with a Reply from Bas C. van Fraassen,* ed. Paul M. Churchland and Clifford A. Hooker (Chicago: Chicago University Press, 1985), 250, 253. In Van Fraassen's *The Scientific Image* a similar statement is made on pp. 202-3. See also Bas C. van Fraassen, *Laws and Symmetry* (New York: Oxford University Press, 1989), 8.

understood as a property things may possess: something is observable or unobservable. An *observation* is something different, namely, one's act of observing a thing that exhibits that property. Van Fraassen spells out observability in the following way:

The human organism is, from the point of view of physics, a certain kind of measuring apparatus. As such it has certain inherent limitations - which will be described in detail in the final physics and biology. It is these limitations to which the 'able' in 'observable' refers, our limitations qua human beings.[16]

From this qualification we can infer two types of things: entities, events and processes that we can naturally observe, and entities, events, and processes that we cannot naturally observe. We call them *observables* and *unobservables*.[17] For example, we can in principle observe cats, rivers, the moons of Jupiter, if we are in the right physical conditions to observe them. What we cannot do, in principle, is observe subatomic particles, genes, desires, and intelligence since these entities by their very natures escape our sensory faculties as measuring apparatuses.[18] Observability is only disclosed fully by our 'final physics and biology'. It is not a philosophical concept that is open to a priori philosophical discussion; it is rather an empirical fact to be discovered. It is a 'function of facts about us *qua* organisms in the world.'[19] Van Fraassen applies the limitations of observation to the community of humans. This means that the observable/unobservable distinction applies exclusively to the human race, that is, what is observable is essentially what is observable-to-us.[20]

In the work of classical empiricists like Berkeley, it is observation as an act that plays a major epistemological role, rather than observability as a property of things. Observability as a property that divides the world in two realms seems redundant in the classic picture. For everything that exists in that picture is by definition observable. Moreover, it is senseless to speak of the properties of things that cannot be perceived since they are by that fact unknowable. As we will see shortly, Van Fraassen adopts a

[16] Van Fraassen, *The Scientific Image,* 17.
[17] The reader will also encounter the word 'phenomenon' (in plural 'phenomena') in this thesis, which is for Van Fraassen synonymous with 'observables'. See Van Fraassen, *The Scientific Image,* 3. There is also a different meaning of the word 'phenomenon' in the philosophical literature that is discussed in part II of this thesis, section 4.1.
[18] Van Fraassen, *The Scientific Image,* 16-17.
[19] Van Fraassen, *The Scientific Image,* 58.
[20] Van Fraassen, *The Scientific Image,* 18-19.

quite different stance towards things that are not observed.

1.3 Experience and existence

With the exposition of 'classical' empiricism in mind, one natural question to ask is whether there is a Berkeleyan relation of implication between observation and existence, since the relation between observation, observability and existence in Berkeley's philosophy is clear but in Van Fraassen's it is not yet. We have a general typology of basic entities but what should be our ontological commitments toward them? It is necessary to explore these questions in depth, since Van Fraassen's famous claim in *The Scientific Image* is that he is an agnostic about the existence of unobservable entities.[21] Surely this statement hints that observability bears in some way upon our ontological commitments.

Van Fraassen is clear about observability: 'the term 'observable' classifies putative entities, and has logically nothing to do with existence.'[22] So, in Van Fraassen's view, whether something is *observable* (exhibits the property) does not imply anything about its existence. He continues:

The term 'observable' classifies putative entities (entities which may or may not exist). A flying horse is observable - that is why we are so sure there aren't any - and the number 17 is not.[23]

Although not explicitly stated or defended by Van Fraassen, I hold that a correct interpretation of the quotations above requires us to take constructive empiricism as ontologically committed to the mind-independent existence of (at least) the observable entities, processes, and events that comprise our world. In other words, the observable aspects of the world do for their existence not depend on any human activity and are thereby ontologically taken for granted. This is a *metaphysical realist presumption*. The idea is close to what is called 'common sense realism'. Common sense realism is the common-sense presumption that the world and the entities, processes, and events that comprise it, regardless of whether they are observable or unobservable, exist independently of our activity and our minds. However, constructive empiricism is only

[21] Van Fraassen, *The Scientific Image,* 72.
[22] Van Fraassen, *The Scientific Image,* 15, 18. Similar statements can be found on pp. 82, 197.
[23] Van Fraassen, *The Scientific Image,* 15, 197.

committed to the weaker realist presumption that there are entities, processes and events that exist independent of our minds and activities, that we are in addition able to observe. Mind-independence in the fashion just explained is synonymous to objectivity for Van Fraassen.[24]

Let us tease out the implications of this presumption. First, we have the observable world as a given. This is simply the collection of entities, events and processes that we can observe and that we thereby take as being metaphysically real. Second, we have the unobservable part of the world. Although we may well be able to find out the details and limits of our observational capacities, we cannot infer the existence or non-existence of the unobservable entities from that. We refrain from any ontological commitment here. That is, the only thing we can do is remain agnostic about the unobservable part of the world. We can construct the following typology of things and examples:

	Observable	*Unobservable*
Existent	Horse	Atom?
Non-existent	Flying horse	Atom?

The 'flying horse' example might be prima facie puzzling. Does it show that because a flying horse is unobservable, it is non-existent? Does Van Fraassen commit the fallacy here of claiming that observability entails something about existence? A flying horse is, by the very definition of observability, and by our understanding of horses and flying things, something that we can in principle observe. When we would observe a flying horse – an *act* - we would take it to exist. This tenet is clearly Berkeleyan, except that Berkeley would obviously not accept that the act of observation establishes that something exists in the metaphysically real or mind-independent sense – it would be an idea. Still, the observability – a *property* - of something is inert with regard to its existence.

The realist presumption respects the logical independence of observability and existence Van Fraassen insists on. In the two sections that follow I will try to show that

[24] In *Laws and Symmetry*, Van Fraassen explains objectivity in more detail than in *The Scientific Image*. In *Laws and Symmetry*, objectivity is explained with regard to laws and probability a few times as a property that indicates that something is independent of psychological, subjective, or otherwise anthropocentric and historical facts (pp. 36, 43, 64, 82, 132). Van Fraassen explains objectivity only briefly in *The Scientific Image*, in relation to probability on p. 165.

the realist presumption is imperative in understanding Van Fraassen's antipathy to scepticism, and in understanding his endorsement of a 'correspondence' notion of truth.[25]

1.4 Scepticism

Let us start with scepticism. Van Fraassen addresses the issue in the following way:

At this point, it may be objected that I have drawn an arbitrary line. Surely the observable objects and processes we recognize in our world, are also postulated entities, believed in because they best explain and systematize the sense-experience or series of sense-data which are at bottom the only real evidence we have? [...] *But it is easy for me to add at least this: such events as experiences, and such entities as sense-data, when they are not already understood in the framework of observable phenomena ordinarily recognized, are theoretical entities. They are, what is worse, the theoretical entities of an armchair psychology that cannot even rightfully claim to be scientific. I wish merely to be agnostic about the existence of the unobservable aspects of the world described by science—but sense-data, I am sure, do not exist.*[26]

The 'arbitrary line' refers to Van Fraassen's alleged arbitrary attitude towards unobservable entities. Paul M. Churchman has argued that with regard to scepticism, our cognition in general is prone to false strategies and other limitations as a consequence of our evolution. Hence, a sceptical attitude towards unobservables only is unwarranted: Churchland holds that 'our observational ontology is rendered *exactly as dubious* as our nonobservational ontology.'[27] So why would the observable parts of the world be better off? The answer is simple. I think that Churchland underestimates the import of his point, since it implies a wholesale agnosticism about both observables and unobservables. The realist presumption simply bars this situation outright. We presume the existence of at least some part of the world that is furnished with observable entities, events, and processes.

In philosophical thought about the relation between perception, mind and

[25] In his later work Van Fraassen is much more explicit about his realist commitments, see for example *Scientific Representation: Paradoxes of Perspective*, 3.
[26] Van Fraassen, *The Scientific Image,* 72.
[27] Paul M. Churchland, "The Ontological Status of Observables: In Praise of the Superempirical Virtues," in *Images of Science. Essays on Realism and Empiricism, with a Reply from Bas C. van Fraassen,* ed. Paul M. Churchland and Clifford A. Hooker (Chicago: The University of Chicago Press, 1985), 36.

world, scepticism has certainly been an issue. One of these solutions to sceptical issues Van Fraassen refers to in the quotation above are 'sense data'. The philosopher Bertrand Russell came from the possibility of chronic illusion and error in ordinary perception to the postulation of sense data as remedy. Although we may err chronically in our ordinary experiences, we have direct and infallible knowledge of sense data according to Russell, hence Van Fraassen's remark that 'sense data are at bottom the only real evidence we have'.[28] That ordinary objects are complex logico-linguistic systematizations of sense data is exactly what Russell at some point held (one cannot help to think of Berkeley's ideas as objects here).[29] So although we are sceptical about the existence of observable entities, either because we are forced to be agnostic about them for 'parity of reasoning' arguments mentioned earlier, or because we deem our observations to be fallible because we humans are rather limited beings, we can be sure about the existence of and our access to sense data, as a kind of neutral object 'in between' world and perceiver.

Van Fraassen, however, is not content with proposals of this kind. In the quotation above he seems to imply that there is no need to postulate sense data, if we simply presume that the observables we observe exist in a realist way, independently of observation. For everything there is to our experiences can then simply be captured in the 'framework of observable phenomena ordinarily recognized'. If I perceive a cat, then I do not perceive sense-data of various cat-like properties. I rather perceive a cat that exists independently of me and is an observable entity. Cats, in contrast to sense-data, are understood in the framework of observable phenomena, and observable phenomena are ontologically taken for granted. This is why in my opinion, Van Fraassen relies implicitly on the realist presumption to rule out scepticism.

1.5 Truth

The realist presumption of Van Fraassen matches quite well with a 'correspondence' conception of truth. That the latter is advocated in *The Scientific Image* is made clear by Van Fraassen's insistence on a 'literal construal' of the language of science:

First, on a literal construal, the apparent statements of science really are statements, capable of being true or false. Secondly, although a literal construal can elaborate, it

[28] Bertrand Russell, *The Problems of Philosophy* (London: Williams and Norgate, 1912), 217, 234-35.
[29] Russell, *The Problems of Philosophy,* 12, 46-47.

cannot change logical relationships.[30]

Let us start with the first part of the quote. That idea that the statements of science must be capable of being true or false, implies that for these statements it is possible to conceive of truth conditions that specify whether a statement is true or false. In other words, whether a statement is true, depends on whether the conditions specified by the statement obtain.

Michael Dummett defines realism in terms of 'external' truth conditions. According to him, a realist maintains that a statement, whether in a theory or some other discourse, is true or false in virtue of something external, but this 'externality' should not be understood as sense data, ideas, or language, since they are not mind-independent. A statement is true or false in virtue of a reality existing independently of us.[31] For Berkeley, a statement is true in relation to other ideas, for Russell it is true in virtue of particular configurations of sense data. They thereby both oppose metaphysical realism of observable entities as Dummett understands it.

Van Fraassen maintains like Dummett that our statements are true, or equivalently, that they correspond to reality, when the conditions they specify obtain:

The contrary position of constructive empiricism [...] *also assumes scientific statements to have truth-conditions entirely independent of human activity or knowledge.*[32]

That there exist for the statements that we devise truth-conditions that are 'entirely independent of human activity or knowledge', sits well with the realist presumption that there is a world independent of human activity and mind, that we may in addition observe. Indeed, in other words by Van Fraassen, in science we strive for *objective* descriptions of nature.[33,34] The correspondence theory of truth entails that in a lot of

[30] Van Fraassen, *The Scientific Image,* 10.
[31]: '[...] the realist holds that the meanings of statements of the disputed class are not directly tied to the kind of evidence for them that we can have, but consist in the manner of their determination as true or false by states of affairs whose existence is not dependent on our possession of evidence for them.' Michael Dummett, *Truth and Other Enigmas* (Cambridge, Massachusetts: Harvard University Press, 1978), 146.
[32] Van Fraassen, *The Scientific Image,* 38, 197.
[33] Van Fraassen, *The Scientific Image,* 116.
[34] This claim can also be found in *Laws and Symmetry* (p. 177) where Van Fraassen says: 'To begin with the question of objective truth and right opinion. Certainly our opinion is right or wrong, and this depends on what the world (the facts we make judgments about) is like.'

cases, by means of observation we can assess the truth of our statements. As we will see, truth about the observables is a significant property of acceptable scientific theories. Van Fraassen does, in his insistence on a literal construal of the language of science, not deny that statements about unobservable entities can in principle be true or false. This is where the second part of the quote comes in.

The second part saves the intelligibility and meaningfulness of the language of science by connecting it to our natural 'literal' use of language. We take an empirical statement in natural language to say something about what the world is like. This is the same for the language of science: theories say something about what the world is like in the same way. The statements of science must be understood in a natural fashion, they are not pseudo-statements that have to be understood instrumentally or metaphorically. Hence, Van Fraassen says: 'If the theory's statements include "There are electrons", then the theory says that there are electrons.'[35] It is in Van Fraassen's account not possible to reconstruct 'electrons' as mere instrumental devices devoid of any reference to the world: that would violate a logical relation of implication. This is counter to the logical positivist movement, which roughly held that statements referring to unobservables are only meaningful insofar they can be reduced to statements about observables.[36] In constructive empiricism, if we encounter unobservables in our statements, we still take our language literally while simply 'bracketing' the truth-values for them.

1.6 Constructive… empiricism?

Yet, one premonition can no longer be ignored: with the preceding exposition in mind, can we even say that Van Fraassen is an empiricist after all? Presuming the existence of mind-independent entities, and adopting a correspondence criterion of truth, is not quite what one would expect of an empiricist. In my opinion, the previous exposition is important exactly because it shows the relevance of this question. As we have seen, contrasting constructive empiricism with more 'classical' forms of empiricism discloses the fundamental assumptions of the former sharply. However, I think that ultimately, it is innocuous to conclude that in his basic framework Van Fraassen is quite some way from the more classical forms of empiricism. Still, in the 'early' works

[35] Van Fraassen, *The Scientific Image,* 11.
[36] Van Fraassen, *The Scientific Image,* 11.

considered in this chapter, for example *The Scientific Image* and *Laws and Symmetry,* no explicit realist presumptions are made by Van Fraassen. Now that we have laid out some of the fundamentals of constructive empiricism, we should continue examining the philosophy of science that is built on them.

Chapter 2. Constructive empiricism, a philosophy of science

2.1 What is science?

According to Van Fraassen, the relatively uncontroversial view of science that most philosophers hold is that scientists devise scientific theories to account for the observable phenomena, by postulating unobservables. Further, if we aim to describe a system in the world then we describe that system in a scientific theory in terms of its possible states.[37]

Yet this account of the *structure* of scientific theories is only one part of the story. We need in addition an account of *the relation between a theory and the world*, and accordingly an account of *what it is to accept a scientific theory*.[38] These two features are distinctive of constructive empiricism and will be explained in the following two sections.

2.2 The relation between theory and world

A straightforward view of the relation between theory and world is to hold that a theory aims to be true of the world. This is an idea close to the 'correspondence' theory of truth we have seen earlier: our theories are true if and only if they correspond to the world. A theory is true if and only if the truth-conditions that are specified by the theory obtain. With the passage above in mind, this seems to mean that a theory as an account of the observable phenomena (entities, processes, events) is true if the (un)observable phenomena (entities, processes, events) it postulates correspond to the world. This view of the aim of science can be called 'scientific realism'. It maintains, according to Van Fraassen, that science aims to give us theories that are literal and true descriptions of the world in both its observable and unobservable aspects.[39]

However, there are numerous criticisms of this view and in this thesis, we will not dwell on the particular arguments against scientific realism. Yet it is vital to invoke scientific realism in the explication of constructive empiricism, since the latter is defined in sharp contrast to scientific realism: it *restricts our access to truth to the correspondence of the empirical consequences of a theory with the observable aspects*

[37] Van Fraassen, *The Scientific Image,* 3.
[38] Van Fraassen, *The Scientific Image,* 4.
[39] Van Fraassen, *The Scientific Image,* 8. Van Fraassen, *Laws and* Symmetry, 191.

of the world.[40] If a theory is true about observables, it is *empirically adequate*.[41] That is enough for a scientific theory to be acceptable for Van Fraassen, who asserts that 'the thesis of constructive empiricism is that in science, what matters is empirical adequacy and not questions of truth going beyond that.'[42] So the difference of constructive empiricism from scientific realism is that realism aims for the correspondence of the theory's unobservable entities with the world *in addition* to the theory's correspondence of the observable aspects with the world.

2.3 Models and empirical adequacy

We need to say a little bit more about the structure of scientific theories and how they can be empirically adequate. For Van Fraassen, a theory is best understood as a presentation of a set of models:

To present a theory is to specify a family of structures, its models, and secondly, to specify certain parts of those models (the empirical substructures) as candidates for the direct representation of observable phenomena.[43]

A model comprises entities and relations among these entities.[44] Its entities may be abstract or concrete; the relations between these entities comprise the model's structure.[45] To say that a theory is empirically adequate is to say that the theory has at least one model that accommodates the observable phenomena.[46] The model that accommodates the observable phenomena is an *empirical substructure*; it specifies observable entities and relations between then.

For example, a Newtonian physical theory has, among other possible (empirical) models, a certain spatiotemporal configuration of the planets in our solar system as empirical substructure, that 'fits' the celestial phenomena we can observe. This 'fit' means that in science we try to observe phenomena in the world that are isomorphic to the empirical substructure of our theory. Empirical adequacy means

[40] Van Fraassen, *The Scientific Image,* 12.
[41] Empirically adequate means true about observable consequences in past, present and future.
[42] Van Fraassen, *The Scientific Image,* 61.
[43] Van Fraassen, *The Scientific Image,* 64.
[44] Van Fraassen, *Laws and Symmetry,* 220, fn. 2.
[45] So, a structure is basically a set of relations between entities in some domain. This conception is borrowed from Michel Ghins, "Models, Truth and Realism: Assessing Bas van Fraassen's views on Scientific Representation," in *Manuscrito* 34, no. 1 (January/June 2011): 209.
[46] Van Fraassen, *Laws and Symmetry,* 218.

that we have at least one model in which the observable phenomena 'fit'.[47]

2.4 Families of models or deductive systems

Van Fraassen contrasts the semantic model approach of scientific theories with the syntactic approach. He regards the latter as too preoccupied with issues in language. In the syntactic approach, theories are bodies of theorems in a particular language, that bear particular syntactic relations to each other, for example, implication and negation.[48] From the axioms in that body, theorems and empirical laws and consequences can be rigorously deduced. In contrast, the semantic model approach sees constructing models as constructing possible realizations and interpretations of these bodies of sentences, that is, as providing possible meanings for them.[49]

The advocates of the syntactic approach, for example the logical positivists, had serious problems with defining the meaning of these sets of statements. For they insisted that the meaning of a sentence consisted solely in what it said about the observable part of the world.[50] But in scientific theories, we not only have observational but also theoretical terms, that is, terms that are only remotely related to observations. So how should we explain what theoretical terms mean? The syntactic approach, because it was wedded to language, tried to solve this problem in purely linguistic terms by dividing language in an observational part and a theoretical part. The meaning of theoretical terms depended on complex logico-linguistic reconstructions of purely observational terms. (One cannot help but think of Russell's reconstructions of ordinary objects out of sense data here as well.)

Many authors, as well as Van Fraassen, have rejected this division, for various reasons.[51] One reason is that in a language there is no part to be found that is not in some way connected to theoretical terms. Further, as we have seen, Van Fraassen thinks that demarcating the observable from the non-observable is not a philosophical, linguistic or theoretic issue at all, but an empirical fact about the organisms that observe.[52] On the practical side, Van Fraassen deems the syntactic approach a far cry

[47] Van Fraassen, *The Scientific Image,* 64.
[48] Van Fraassen, *The Scientific Image,* 42.
[49] Another important difference is that models often cannot be axiomatized in a non-trivial sense. That is, they cannot always be formulated in terms of 'first principles' or axioms, which entail empirical laws, and empirical consequences. Van Fraassen, *Laws and Symmetry,* p. 188.
[50] Carl G. Hempel, "Problems and Changes in the Empiricist Criterion of Meaning," in *Revue Internationale de Philosophie* 4, no. 11 (January 1950), 44.
[51] Van Fraassen, The *Scientific Image,* ch. 2, ch 3.
[52] Van Fraassen, *The Scientific Image,* 57.

from actual scientific practice. Scientists do not seem too occupied with axioms and the deduction of observable consequences, but rather with constructing models.

The model approach provides for a possible meaning of a body of theorems by specifying entities and relations between entities that satisfy the syntactic statements of the theory in question but does not draw essentially on one particular language. Many different models with a variety of entities and relations are possible that will satisfy the same syntactic requirements. Models need to be internally consistent; they need to be models of the body of sentences they stem from. To be scientifically good, they need to be empirically adequate as well.[53] That is not to say that models are free of language and interpretation; any formal system that is purely mathematical is not useful in empirical science. So some interpretation of the entities in the model and their relations in terms of reference and meaning, will always be established, as is the case with the empirical substructures.

2.5 Theory acceptance: epistemic and pragmatic reasons

So now we have a grasp of empirical adequacy as the relation between a theory and world: scientific theories 'latch' onto the world in terms of being true about observable consequences. With regard to the issue what of it is to accept a scientific theory, Van Fraassen distinguishes an 'epistemic' and a 'pragmatic' dimension.[54] With regard to the epistemic dimension we ask 'How much belief is involved in theory acceptance?', and with regard to the pragmatic dimension we ask 'What else is involved besides belief?'. Both issued will be addressed in this section.

We have seen that for Van Fraassen, what matters in science is empirical adequacy. When we say that we accept a theory to be true (or not), we believe that it is adequate with regard to its observable consequences (or not), but no more than that. But surely a scientific theory is more than its empirical substructures and observable consequences. Often there is a whole network of unobservable entities, processes and events that is postulated. In the words of Van Fraassen:

If we look at a model of a scientific theory, we discern important substructures which do not correspond to anything observable; and we also see substructures that do not correspond to anything actual. [...] *So as far as empirical adequacy is concerned, the*

[53] Van Fraassen, *The Scientific Image,* 94, 218.
[54] Van Fraassen, *The Scientific Image,* 4.

theory would be just as good if there existed nothing at all that was either unobservable or not actual. Acceptance of the theory does not commit us to belief in the reality of either sort of thing.[55]

Regardless of what the unobservable entities in a given theory are exactly, the upshot is that we are not committed to believing in their existence, since empirical adequacy is the only necessary and sufficient condition for the acceptance of a theory.

However, we do not accept a theory solely on the basis of believing in its empirical adequacy. When we accept a theory, according to Van Fraassen, we also commit ourselves to a certain 'research programme', that is, we are immersed in the language of the theory and we confront observable phenomena in terms of the theory in question.[56] Our reasons for doing so are *pragmatic*: they are reasons for accepting a theory that are not epistemic since they are not related to truth. In other words, pragmatic reasons are blind to empirical adequacy.[57]

For Van Fraassen this is most salient in the case of empirical underdetermination: a situation where two theories are logically incompatible with regard to the unobservable entities they employ, but have nevertheless the same empirical consequences. That means that the choice between the two will be underdetermined by the empirical evidence, since they are both empirically adequate.[58] The choice between the two theories, therefore, cannot be made on epistemic grounds, and the pragmatic dimension of theory acceptance enters the fray. According to Van Fraassen, we accept theories besides empirical adequacy for considerations of elegance, simplicity, scope, completeness, and unification. Intuitively explanation is one of the most salient aspects; it will be discussed in the next section.[59]

2.6 Explanation

In *The Scientific Image,* a whole chapter is devoted to spelling out an account of explanation that abjures any realist virtue ascribed to explanation. The idea that explanations are salient and significant in scientific theories is innocuous, as we will

[55] Van Fraassen, *The Scientific Image,* 197.
[56] Van Fraassen, *The Scientific Image,* 12.
[57] Van Fraassen, *The Scientific Image,* 4.
[58] Van Fraassen, *The Scientific Image,* 4, 12, 46. Arguably Van Fraassen says here that when we are 'immersed' we are working in a Kuhnian paradigm.
[59] Van Fraassen, *The Scientific Image,* 87.

see, even in constructive empiricism, but for other reasons than one might expect.

The traditional realist idea is that explanations tell us why we observe the phenomena we observe, by means of positing unobservable entities, processes and events that are true representations of the world. And truth in the sense of correspondence is surely a necessary condition of an explanation if it is to explain at all.[60] But from historical examples, among other reasons, one can infer that we had entities, processes and structures in theories that at some point explained, predicted and were empirically adequate, but that were discarded later anyway. One can think of the caloric theory of heat, the phlogiston theory of combustion, the mechanical gravitational ether theory, and Newtonian gravity, among others.[61] That a theory explains is apparently independent of its full-blown truth or empirical adequacy.

One can also infer this from the point of empirical equivalence: if two theories are logically incompatible in the sense that they proffer disjunctive explanations, but have nevertheless the same observable consequences, then explanations are inert with regard to truth (although this does not entail that they cannot explain, as will be made clear shortly). This is why Van Fraassen holds that explanation is not 'a special additional feature' that gives philosophers, scientists or laymen good reasons for believing a theory over and above its empirical adequacy.[62]

According to Van Fraassen, in the traditional accounts of 'explanatory power' by Carl. G. Hempel and Wesley C. Salmon, there is really no more to explanation than empirical adequacy.[63] For the road Salmon takes is arguing that an explanation is statistically relevant to the occurrence of the observed phenomena that we want to explain.[64] Hempel thought that providing an explanation in terms of showing why some event occurs, entails no more than showing that the observed event in question is necessary, on the condition that it can be subsumed under a law.[65]

According to Van Fraassen, on these views explanation coincides with showing that the phenomenon's occurrence is no objection to the claim of empirical adequacy of one's theory.[66] But in constructive empiricism, in any case, we refrain from saying that this or that explanation, if it exhibits unobservables, renders a theory empirically

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[60] Van Fraassen, *The Scientific Image,* 97.
[61] Van Fraassen, *The Scientific Image,* 98. See also Larry Laudan, "A Confutation of Convergent Realism," in *Philosophy of Science* 48, no. 1 (March 1981), 27, 33, 45.
[62] Van Fraassen, *The Scientific Image,* 100.
[63] Van Fraassen, *The Scientific Image,* 108.
[64] Van Fraassen, *The Scientific Image,* 107.
[65] Van Fraassen, *The Scientific Image,* 108-09.
[66] Van Fraassen, *The Scientific Image,* 109.

adequate. Many other possible logically incompatible explanations may do so. So there is no point in saying: this explanation makes the phenomenon observed necessary or more likely. Explanation is a business independent of a theory being a good description.[67]

What is an explanation, then? For Van Fraassen explanation is a business exhibiting its own peculiar logic that can be laid out completely in terms of questions and answers. Explanations are no more than *answers* to requests for information, that is, they are pieces of information offered to propositions that exhibit a particular interrogative.[68] Both the question and the answer are essentially *relative* and *context-dependent*; they depend on particular features of inquirer and audience.

It is humans who are in the business of asking questions. In doing so there is always some background knowledge *K* involved. For if we ask for an explanation for the observed phenomenon *P*, we first of all presume that *P* is true, for example, otherwise the question would make no sense.[69] What is more, when we ask 'Why *P*?', we always ask why *P* obtains instead of some other alternative states of affairs *Q, R, S* that are also possible in that context, because they share the 'topic of concern' with *P*.[70] That is, we do not ask why of all things that may possibly happen, *P* happens, just as when we ask for a causal history of *P*, we think of some relevant causal factors out of a bewildering totality of causes that may be listed in principle. We always ask out of a particular interest.

So, if the chemist asks, 'Why did this substance *explode*?' s/he may actually intend to ask, 'Why did this substance explode *instead of melt*?' or 'Why did *this substance* explode and not the other I added?' If a lawyer asks the chemist 'Why *did* this substance explode?' s/he may intend to ask 'Why did this substance explode at all given the security protocols?' What contrast is made depends on the context: on the interests and the background knowledge of the inquirer. Different contrasts also anticipate different possible answers.

Van Fraassen has a more or less 'Bayesian' take on the evaluation of answers: the updating of belief, background knowledge, additional information, and subjective probabilities all play a role. The answers we formulate to address questions are statements that contain events that are taken to be *reasons* for *P*. Let us call those

[67] Van Fraassen, *The Scientific Image,* 93-94, 153-54.
[68] Van Fraassen, *The Scientific Image,* 126-27.
[69] Van Fraassen, *The Scientific Image,* 110, 144.
[70] Van Fraassen, *The Scientific Image,* 127.

reasons *R*; that means that an answer *A* has the form '*P,* and not some alternative, because *R*'. Reasons need not be entailed by *K*, but *R* may of course not be denied outright by *K*.[71]

What we do when we evaluate answers is divided in three parts. To begin with, we want to know whether *R* is likely at all, given *K*. Second, we want to know how well *R* does in favouring *P* against alternatives *Q*, *S*, *T*. So, what we basically do is calculate the probability distribution of *P* and alternatives *Q*, *S, T* in the light of our background knowledge *K*, and then we have the 'prior' probabilities. Then we see how *A* affects this distribution by checking how well *P* does together with *R* and some auxiliary conditions (facts that are known but do not imply *P*) in the light of a relevant part of our background knowledge *K(A)*.[72] Third, we examine the relations between the answers. We check again in the light of *K* whether *R* is likely, whether it favours *P,* and whether it is not made irrelevant ('screened off)' by other answers.[73] We may not have a function that combines the probabilities of all the steps, but Van Fraassen doubts the value of an integrated function that weighs cogently the importance of 'how likely an answer is to be true' against 'how favourable the information is which it provides'.[74]

According to Van Fraassen, 'the probability to be used in evaluating answers is not at all the probability given all my background information, but rather, the probability given some of the general theories I accept plus some selection of my data.'[75] All my background information implies *P* and *R* already, but that is no explanation of *P*. After all, a question is a request for information beyond *K*. So, for evaluating answer *A* as explanation to a question, besides accepting *P* and *R* I need only a general account of *P* (which is a part of *K*) say, some low-level generalizations such as 'heating things up may cause them to explode', and some further facts about the environment.

Background assumptions are highly specific and context-dependent. If on the grounds of my K_b I accept *P* but deny *R*, I agree that there is a question, but I will not evaluate *A* as a good answer because I deny *R*. For another person with a given K_o *R* may obtain and actually be a good answer. If someone else does deny *P*, then that

[71] Van Fraassen, *The Scientific Image,* 143-45.
[72] Van Fraassen thinks that *R* favours *P* if it bestows a higher probability on *P* than on any alternative. This does not mean that *P* has to have a high probability, or that *R* increases the probabilities of *P* since it might just lower the probabilities of the alternatives *Q,S,T,…* relative to *P*.
[73] Van Fraassen, *The Scientific Image,* 146-147.
[74] Van Fraassen, *The Scientific Image,* 150.
[75] Van Fraassen, *The Scientific Image,* 147.

person rejects the question, rejects the request for explanation, maybe because this person thinks that I am under a false impression that *P* is true. The background knowledge an inquirer or audience possesses is thoroughly subjective.

But do our presuppositions of *P* and *R* in asking questions and providing answers, our requirement that *P* and *R* obtain in order for the questions and answers to make sense at all, not require us to presuppose unobservables, since it is often the case that *R* employs unobservables? No, says Van Fraassen, because we can use the theoretical language on the supposition that the theory is empirically adequate. And surely, when scientists are immersed in a theory they say from within a theoretical picture which unobservable entities there are, how they are related to others, and so on. But ontological commitments 'are not to be read off from their language.'[76]

We now have a picture of explanation that comprises theory, fact, and context. What is asked, varies per context; The background knowledge and additional data vary from context to context; even the particular part *K*(*A*) used in evaluation depends on the context. As Van Fraassen puts it:

So to say that a given theory can be used to explain a certain fact, is always elliptic for: there is a proposition which is a telling answer, relative to this theory, to a request for information about certain facts, (those counted as relevant for this question) that bears on a comparison between this fact which is the case, and certain (contextually specified) alternatives which are not the case.[77]

Scholars as John J.C. Smart, Richard Boyd, Wilfrid Sellars, and Wesley Salmon, proffered arguments that boil down to the claim that non-realistic explanation certainly encounters the problem that we cannot explain *why* these explanations are so successful. What explains the fact all our observations, specified by theory *T,* fit theory *T*?[78] The answer these scholars desire is, of course, that we are successful at explaining and predicting observations because theories are *true* in the scientific realist's sense. According to Van Fraassen, we should be nominalists here in the sense of holding that the regularities we observe are a brute *given*; we acknowledge their existence but whether they have an explanation is irrelevant to whether a theory is

[76] Van Fraassen, *The Scientific Image,* 152.
[77] Van Fraassen, *The Scientific Image,* 156.
[78] Van Fraassen, *The Scientific Image,* 23, 77-80, 32-34, 107-09.

good or useful.[79] This is some sort of addition to the ontological picture we drew earlier. In the occurrence of things and events we observe and presume to exist, there are regularities to be recognized as a brute given fact about these observables. The regularities require no explanation. In other words, for Van Fraassen, there can be no 'unrestricted demand for explanation'.[80] Illustrative is the following quote:

If explanation of the facts were required in the way consistency with the facts is, then every theory would have to explain every fact in its domain. Newton would have to add an explanation of gravity to his celestial mechanics before presenting it at all.[81]

But as Van Fraassen argues, gravity was for Newton no more than a mathematical description of what we observe, that is, the movements of the celestial bodies; it was not intended as a realist explanation.[82]

2.7 Causation and counterfactuals

Explanations often employ causation. Indeed, we often expect of an explanation that it provides understanding how the event to be explained came about the way it did. We desire accounts that show us how the events leading up to the explanandum are related. Explaining in terms of causal relations may even be held to be a criterion for scientific explanation, as Reichenbach did.[83] For Van Fraassen, explanation in causal terms is something quite different from explicating causation as such, which can be quite difficult. As an empiricist, Van Fraassen shuns the explication of causality 'in itself' in terms of necessary relations between events, or productive powers in things that exist independent of human minds.[84]

Van Fraassen prefers to understand causation in explanations as a type of answer to a question as 'Why did event *E* happen?' that identifies the salient factors in the causal network of events. These 'salient factors' are the *causes* of *E*. A causal explanation (or answer) is committed to saying that there is a structure of causal relations of a certain sort, which could in principle be described in detail. We however always provide salient factors, since there may well be an infinite list of causal factors

[79] Van Fraassen, *The Scientific Image,* 24.
[80] Van Fraassen, *The Scientific Image,* 31, 211.
[81] Van Fraassen, *The Scientific Image,* 94.
[82] Van Fraassen, *The Scientific Image,* 94.
[83] Van Fraassen, *The Scientific Image,* 26.
[84] Van Fraassen, *The Scientific Image,* 3, 158, 196.

that contributed to the occurrence of the explanandum, and answering with that list is often not the desired answer.[85] Which factors are salient, depends again on the context.

Similarly to how we compare the event we want to explain to possible alternatives, we compare causes by means of competing 'counterfactuals'. So for instance, when causally explaining a riot of football supporters in the historical centre of Utrecht, the sociologist may point to the failure of the Utrecht police force to exert authority, while keeping the psychological facts of the fans constant. That is to say, this claim amounts to saying that if the police had exerted authority, the riot would not have happened. So the police's inability to exert authority is the counterfactual cause here. Yet, the social psychologist may point to peer pressure, while keeping the social institutional facts constant. This amounts to identifying the cause that had there been no peer pressure, there would have been no riot, while keeping social institutional facts constant.

All of this is not to say that explanation cannot employ causal relations; in fact, it often will do so. And this is no problem for constructive empiricism. If we are 'immersed' in Newtonian mechanics, our explanations may exhibit gravity as the cause of certain configurations of celestial bodies. That is perfectly fine, if we understand Newtonian mechanics as the body of background knowledge from and in which we explain. Note, however, that in all the examples the entities alluded to are unobservable. More will be said about them in part II.

Let us return to counterfactuals for a moment. They are sometimes used as a defining mark of causality. Van Fraassen discusses David Lewis' proposal to identify '*P* causes *Q*' with 'if *P* had not happened, neither would have *Q*'.[86] Again, context-dependence is the key. Van Fraassen denies that counterfactual statements are 'objectively true', that is, true independent of human minds and activity. [87] In making a counterfactual statement, the truth-conditions of counterfactual statements depend on the inquirer. If we use the example above, in pointing to institutional facts as the cause of the riot instead of to social psychological facts, the sociologist is keeping the latter constant (among other things). In other words, the sociologist says that if the institutional fact specified had not obtained, that is, if the Utrecht police force had

[85] Van Fraassen, *The Scientific Image,* 124-25.
[86] Van Fraassen, *The Scientific Image,* 114.
[87] Van Fraassen, *The Scientific Image,* 13.

exerted authority, then the riot would not have happened. So, the conditions for the truth of the counterfactual statement depend completely on the sociologist's particular interest. S/he defines what is held to be constant, in order for the explanation to be true. Hence, truth-conditions in counterfactual statements are context-dependent. That does not mean that the constructive empiricist cannot use counterfactual statements in explanation, they simply are not true independently of the inquirer.

2.8 Modality and probability

Modal claims bear some similarity to counterfactual claims. My rough definition of modal claims is that they are statements concerned with possibility, contingency and necessity. The counterfactual claim that 'If *P* had not happened, neither would have *Q*,' can be understood modally as 'It is necessary that *P* happens if *Q* is to happen.' One can explain counterfactual statements in modal terms, by saying that there is a relation of necessity that exists between *P* and *Q* that renders *P* necessary to *Q*. This relation can be understood realistically by holding that, for example, relations of necessity are objective (mind-independent) features out there in the world, for example in the form of laws of nature.[88] Then one claims that it is law of nature *L* that renders *P* necessary to *Q*.

We said that for Van Fraassen, counterfactual statements do not have objective (mind-independent) truth-conditions, because they depend on the inquirer's background knowledge.[89] In the same vein, thinking of counterfactual claims in terms of objective relations of necessity or possibility is counter to constructive empiricism. Empiricists eschew reifying necessity and possibility. Necessity and possibility are mental heuristics that facilitate our descriptions of observable phenomena, which are just relations among ideas, and not things that exist independently of our minds and activities - this reminds us of Berkeley once again, although Van Fraassen is also very clear on this issue.[90] So whether employing relations of necessity is fruitful at all in explanations depends entirely on the issue, and on what kind of request for information is made. Often, just as is the case with causality, we will think in terms of necessity in our explanations, for example when we invoke laws of nature. But as is the case with causality, necessity itself has no objective counterpart in the world.

[88] Van Fraassen, *Laws and Symmetry,* 28-30, 36.
[89] Van Fraassen, *The Scientific Image,* 118.
[90] Van Fraassen, *The Scientific Image,* 3-4, 134-35, 158.

One special form of modality for Van Fraassen is probability. He conceives of it as a kind of 'graded' possibility.[91] Probability as it occurs in physical theory receives a 'modal frequency' interpretation by the constructive empiricist. The *frequentist* interpretation of probability, in general, uses the objective occurrence and sequence of events to induce a measure to ground their theoretical probabilities.[92] Events always occur with regard to a particular reference class and their probabilities need to be defined with respect to that particular reference class. The stricter frequentists, such as Reichenbach, argue that relative frequency and probability ultimately coincide.[93] Be that as it may, how frequencies tend to the values as specified by our theoretical probability statements, how actual frequencies 'calibrate' our theoretical probability statements, needs to be spelled out by any frequentist.

Although Van Fraassen is a frequentist, he is not a strict one. It is standard mathematical practice to conceive of probabilities in terms of probability spaces. We can model experiments (broadly understood as including events and chance set-ups as well) using probability spaces. Probability spaces employ a sample space that is the set of all possible outcomes, a set of events in which each event contains zero or more outcomes, and a probability measure which translates events to probabilities. This set of events is a Borel field with particular properties. Probability spaces are standard practice in physics because they allow the physicist to model infinitely repeated experiments, among other things. Van Fraassen's point is that the strict frequentist's relative frequency function, such as Reichenbach's,[94] does not exhibit the same structure as probability spaces and therefore does not have the same properties. For example, the strict frequentist function does not have countable additivity as a property. So it is out of touch with a standard mathematical practice with regard to probability.[95]

Because probability spaces as a model of experiments attach probabilities to *possible* outcomes of experiments, this is called the *modal* frequency interpretation. Probability spaces consist of families of events each of which represent an alternative possible configuration of outcome-events.[96] So they are modal in that probability

[91] Van Fraassen, *The Scientific Image,* 198.
[92] David Howie, *Interpreting probability. Controversies and Developments in the Early Twentieth Century* (Cambridge: Cambridge University Press, 2002), 2.
[93] Van Fraassen, *The Scientific Image,* 181.
[94] Van Fraassen, *The Scientific Image,* 183.
[95] Van Fraassen, *The Scientific Image,* 183-85.
[96] Van Fraassen, *The Scientific Image,* 196.

spaces say what could be the case and not only what is actually the case. But to see how this model of experiments relates the actual to the ideal, that is, frequencies to probabilities, the probability function in the model has to be linked with the frequencies of occurrences of outcomes.[97] Remember, Van Fraassen does not think that frequencies are probabilities, but he does think that probabilities are frequencies.[98] And we can link observable frequencies to probabilities by thinking of repeating experiments under ideal conditions infinitely many times. The relation between the ideal and the actual is then that the actual experiment is thought of in terms of its possible extensions to ideal repeated experiments. Then we should expect an 'intimate relation between frequencies and probability so that the model can be directly compared with the theory under consideration.'[99] And so we arrive at Van Fraassen's 'slogan formulation' of probabilities:

The probability of an event A equals the relative frequency with which it would occur, were a suitably designed experiment performed often enough under suitable conditions.[100]

A statistical theory is empirically adequate if there is no statistically significant difference between predicted and actual frequencies in the observable phenomena in the model at issue.[101]

This is according to Van Fraassen the most economical view of physical probabilities we can have. There is no model conceivable where observed frequencies coincide with probabilities, that is, where they are identified with observable phenomena.[102] We can do no more than idealise experiments using probability spaces, repeat the experiment infinitely many times and see which of the possible outcome sequences corresponds to the frequencies which we actually observe. The upshot of this view of modality is that our talk of 'graded' possibility in terms of probability spaces, is at bottom talk about mathematical entities. If the modal elements in the models of our scientific theories detail alternative courses of events, then a complete correspondence of theory with reality may entail modal realism about these alternative

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[97] Van Fraassen, *The Scientific Image,* 190.
[98] Van Fraassen, *Laws and Symmetry,* 197.
[99] Van Fraassen, *The Scientific Image,* 191.
[100] Van Fraassen, *The Scientific Image,* 194.
[101] Van Fraassen, *The Scientific Image,* 196.
[102] Van Fraassen, *The Scientific Image,* 196.

courses, real but non-actual possible worlds, real but non-actual entities or real but non-actual states. But since the constructive empiricist rests content with acceptance of a theory in the sense of having a model that covers both what is actual and observable, he does not feel committed to espouse realism about alternative courses as specified by our probabilistic theories.[103] For they are ultimately non-actual and non-observable; it does not affect the empirical adequacy of our theories, which is in the end all that matters.

Further, that we are committed to modal language in describing the world once we accept a theory and the language it invokes, again, does not commit us to ascribing metaphysical reality to possible courses of events. Just as we can talk about causality, counterfactuals, and unobservable entities, without being committed to their mind-independent existence, we can employ modal language in the context of a scientific theory we have accepted. It is the theories we accept which provide our language with a logical structure that leads us naturally to discourse that includes causality, modality, laws, and so on.[104]

2.9 Laws

The exposition of constructive empiricism as a philosophy of science is not complete without an elaboration on its views of laws; after all, Van Fraassen has devoted an entire monograph to it, called *Laws and Symmetry* (1989).

Philosophical accounts of law purport to provide a theory of explanation, confirmation, an account of necessity, and most important of all, a way of understanding the aim and structure of science.[105] Laws take a central role in the work of many philosophers, for example, Charles S. Peirce, David M. Armstrong, and David Lewis.[106] Perhaps this is why Van Fraassen's ideas on laws receive separate treatment in *Laws and Symmetry*. Van Fraassen argues in the book that there are no laws of nature and, a fortiori, that science as conceived of by the constructive empiricist does not need them.

The two general problems all philosophical accounts of laws founder on, according to Van Fraassen, are the problem of inference and the problem of identification. The two problems are a dilemma in the sense that the solution to the

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[103] Van Fraassen, *The Scientific Image,* 197.
[104] Van Fraassen, *The Scientific Image,* 198-99. Van Fraassen, *Laws & Symmetry*, 213.
[105] Van Fraassen, *Laws & Symmetry*, 184.
[106] Van Fraassen, *Laws & Symmetry*, 1-14, 17-18, 55.

first translates into a problem for the second. The first problem is spelling out the consequent of a law. Surely the statement 'It is a law that *A'* should imply *A* if we are to have a compelling account of laws. So laws should necessitate their consequences. But we should do more than just saying that '*A*' is a logical necessity of the statement 'It is a law that *A*'; we need to know what renders *A* necessarily *actual*. That brings us to the second problem of identification, which is laying bare a feature of the world that yields laws their necessary character. What part of the world necessitates *A*? If necessity is the sustaining power behind facts and states of affair in the world it would be bad tactics to identify it with a part of the world. However, claiming that it is a primitive fact would only make it an inert label without logical force, without implying actuality. [107]

An even bigger issue Van Fraassen identifies is that many philosophers of science in their writings about laws presuppose that the search for laws of nature is the point of actual scientific practice. But for Fraassen this is wrong.[108] Still, the constructive empiricist has to explain how to understand the scientific enterprise without laws. The brief answer is that the account of explanation as already provided does not need to employ laws as entities, processes, power or structures located in the unobservable world. Often, we will talk in terms of laws, but this is a natural consequence of the theoretical picture we find ourselves in. In the semantic view of theories we discussed, laws may appear, but they are the particular laws of models and not objective mind-independent laws of the world. They can appear as fundamental equations of models, as their basic principles. On the basis of these models we expect certain observable regularities, but we do not expect our laws to have counterparts in mind-independent reality.[109]

Confirmation is a kind of pseudo-issue for Van Fraassen; we have seen earlier that it is not necessary to suppose that unobservables establish the empirical adequacy of a theory; this is also the case for laws. The only thing we do when confronted with scientific theories (which may or may not employ laws) is rationally change our opinions about the world of observable objects and events in a subjectivist probabilist fashion. In the previous section we already said that talk of necessity must be understood as a tenet of our use of language, embedded in the broader context of a

[107] Van Fraassen, *Laws & Symmetry*, 39.
[108] Van Fraassen, *Laws & Symmetry*, 39.
[109] Van Fraassen, *Laws & Symmetry*, 188.

certain accepted theory and its models.[110] With regard to the aim and structure of science, the constructive empiricist can rest content with the goals of empirical adequacy and the semantic conception of theories, without reifying laws or anything else that is beyond what is observable.

[110] Van Fraassen, *Laws & Symmetry*, 187, 213.

Part II. Constructive empiricism in the social sciences

Take a novel. From an objective perspective, it is a set of letters, printed on paper and bound in a book. But this objective given is not what makes it a novel. We as readers do that ourselves, because we give the words a place in our imagination. It is a 'representation', an image. Do you see the clear parallel with the scientist who graphically pictures human DNA in a diagram? The same sort parallels are to be found in mathematics and in all sciences.[111]

In part II of this thesis I face the challenge to defend and develop empiricism in the social sciences. My aim is to show that a constructive empiricist outlook in the social sciences is both possible and fruitful.

In chapter three I provide a brief history of the gulf between the natural and social sciences, since constructive empiricism was developed in the context of the natural sciences, hence, must address the alleged fundamental differences between them. In the remaining chapters, I use the explication of the 'fundamental' differences between the sciences in Jerome Kagan's *The Three Cultures* as challenges for the articulation of constructive empiricism in the social sciences.

In chapter four I first illustrate some amendments Van Fraassen made to his 'early' model view, and then I show how they pave the way for a constructive empiricist account of phenomena, appearances, measurements, and empirical adequacy, among other things, in the social sciences.

In chapter five I argue that the distinct questions each culture asks are no problem for constructive empiricism. In addition, I claim that description, explanation and prediction are proper 'products of inquiry' for social science in the light of constructive empiricism. Moreover, I argue that pragmatic explanation saves us from metaphysics in the social realm.

In chapter six a discussion of the nature of concepts in both natural and social sciences is provided. I illustrate the variety of social concepts used to explain social phenomena and show how pragmatic explanation allows us to make sense of this predicament of the social sciences.

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[111] "Eredoctor Bas C. van Fraassen: 'Letters op papier zijn nog geen roman,'" KU Leuven nieuws, KU Leuven, last modified 15 December 2008, https://nieuws.kuleuven.be/nl/campuskrant/0809/04/eredoctor-bas-c.-van-fraassen--201cletters-op-papier-zijn-nog-geen-roman201d. Translation mine.

Chapter 3. A brief history of an old issue

One immediately encounters a severe issue when one tries to extend constructive empiricism to the social sciences. Constructive empiricism was developed in the context of the natural sciences, mainly physics. So the worry is that there are insuperable differences between the natural sciences and the social sciences that prevent any straightforward extrapolation. This worry is prima facie a compelling one. As Ruud Abma points out in *Over de grenzen van disciplines* (2011), it is a kind of echo of an older ideological dispute about the nature of science that originally took place between scholars in the humanities and the natural sciences. Let us discuss this dispute briefly.

Newtonian mechanics in the 18th century was quite successful regarding the unmatched exactness of its empirical predictions. In addition, it rendered possible a host of technological inventions with an impact on society, for example the steam engine. These academic and social impacts propelled the emancipation of the natural sciences, in both their specialization into sub-disciplines as chemistry and biology and in their expansion into a freshly established faculty in universities in the second half of the 19th century. Before that time, the 'natural sciences' often formed an integrated whole with philosophy, theology and history.[112] As illustration, consider a 17th century scholar like Newton who was not so much a 'natural scientist' in our modern sense of the term as he was a theologian, since half of his writings were devoted to Bible exegesis.[113]

These developments stimulated the scholars in the humanities in turn to reflect on the nature and methodology of their endeavours. These developments are at the root of the development of the idea that the natural sciences aim to disclose universal laws of nature, which explain their success, whereas the rationale for the humanities did not lie in the quest for laws, but in their ability to describe and interpret the complex and unique phenomena in history and society.[114] Wilhelm Dilthey is the famous exponent of the separation of the natural sciences and the humanities, that commences in his *Introduction to the Human Sciences* (1883). The humanities are a unity distinct from the natural sciences since the former are rooted in human self-

[112] Abma, *Over de grenzen van disciplines,* 48-49.
[113] Richard H. Popkin, *The Third Force in Seventeenth-Century Thought* (Leiden: E.J. Brill, 1992), 172, 173-88.
[114] Abma, *Over de grenzen van disciplines,* 49.

consciousness. Self-consciousness encompasses typical human features such as spirit, free will, thought, and responsibility for actions – these freedoms are manifest in the 'realm of history' as opposed to the 'realm of nature' that manifests mere objective necessity. The natural realm is just the mechanical course of events.[115] Hence, as Abma writes, according to Dilthey, the humanities draw on interpretation of the realm of history and less on showing how one world-state causally follows from another.[116] Of course, in the 18th and 19th century there was not something that we, from our modern vantage point, call social science, but rather the dawn of the articulation of what the nature of a thing like social science could be. The pioneers of social science, in their thinking about the nature and methodology of a thing as social science, definitely influenced the discussion about different 'scientific cultures'.[117] To take sociology as an illustration here, Abma describes how the first 'proto-sociologists' found themselves in between the two cultures and their dispute.

On the one hand, the pioneers of sociology in the 19th century clearly mimicked the natural sciences in their articulation of empirical precision, the discovery of laws, and a broad scope as the main tenets of sociology.[118] In my view a nice illustration of these ideals can be found in Auguste Comte's *A General View of Positivism* (1865). The issue how the natural sciences are related to the social sciences clearly shaped his philosophy known as 'positivism'. It was the doctrine which held that in science we should focus exclusively on the observation of objective matters of facts in the world.[119] These objective facts exhibit a certain lawlike order, independent of us, hence science should aim to disclose the laws that govern objective matters of facts. [120] Objective facts can be found both in the natural and the social world, so both realms are open to one scientific study that aims to find laws. Comte heralded sociological laws that would

[115] Wilhelm Dilthey, "The Human Sciences Form an Independent Whole alongside the Natural Sciences," in *Wilhelm Dilthey. Selected works. Vol. 1. Introduction to the Human Sciences,* ed. Rudolf A. Makkreel and Frithjof Rodi (Princeton: Princeton University Press, 1989), 56-59.
[116] Abma, *Over de grenzen van disciplines,* 51, 113, 128.
[117] The different 'scientific cultures' alluded to here refer to Kagan's understanding of scientific cultures as different scientific disciplines and their actual research, for example natural science and social science. This is how scientific cultures are understood in this thesis. However, it must be made clear that Kagan borrows the terminology from Charles P. Snow, unfortunately without further reflection on the difference in meaning. Snow identified as 'isolated cultures' the 'literary scientists' and 'physical scientists', which did not really communicate with each other and were unaware of significant developments in each other's fields, which is a quite different understanding of 'culture'. See Charles P. Snow, *The Two Cultures* (Cambridge: Cambridge University Press, 1998), 2, 4, 14-15.
[118] Abma, *Over de grenzen van disciplines,* 126-127.
[119] Auguste Comte, *A General View of Positivism,* trans. John. H. Bridges (London: Trübner and co., 1865), 35.
[120] Comte, *A General View of Positivism,* 23.

synthesize and systematize every aspect of human life, although the laws of sociology would necessarily be a little bit more approximate than the simple laws of physics.[121] The focus of sociology on laws and a broad scope is clear here, although one should be cautious not to make Comte's positivism sound too modern: positivism was for Comte first of all a comprehensive quasi-religious political program to 'spiritually reorganize' humanity in terms of 'love, order, and progress' that was in his opinion able to surpass medieval Catholicism in this.[122]

Another example of the focus on empirical facts and a broad scope is Emile Durkheim, who at the end of the 19th century argued that the sociologist has to adopt the vantage point of the physicist. Durkheim held that there are social facts in the world that can be studied in sociology, in the same way as there are natural facts in the world that can be studied by physics. Social facts are entities that exert influence over one's life. They should not be understood in terms of an individual's actions, feelings or thoughts, or be reduced to them, but should be understood as collective entities such as the monetary system, religion, customs, that enjoy an existence independent of humans that shape behaviour.[123] The idea was that whereas natural science disclosed the fundamental laws of the natural world, sociology would do the same for the social world as a whole. Durkheim's famous *Suicide* (1897) is emblematic of this view; it draws on large pools of empirical data, and because it attempts to explain suicide in terms of social facts, Durkheim's explanation of suicide applied to the society in question as a whole, which was quite unprecedented in his time.

On the other hand, sociology's inception was clearly influenced by the humanities, in particular history. Abma points out that in the 18th century scholars as Giambattista Vico, Charles de Montesquieu, Adam Ferguson, Adam Smith and John Miller already wrote about politics, economy and culture, from a historical vantage point. We can understand this form of early historical sociology as a precursor of modern social science.[124] Sociology has historical roots, and this is no surprise: both history and sociology draw on and describe the same material: social phenomena. According to Abma, sociology as a science began to differ from the traditional conduct of history when in the 19th century pioneers of sociology began to think more about

[121] Comte, *A General View of Positivism,* 41-42.
[122] Comte, *A General View of Positivism,* 116.
[123] Emile Durkheim, *The Rules of Sociological Method,* ed. Steven Lukes, trans. Wilfred D. Halls (New York: The Free Press, 1982) 50-51, 56, 59, 81-83.
[124] Abma, *Over de grenzen van disciplines,* 127.

causally explaining and engineering the social realm,[125] thereby tending more to the natural sciences by manipulating and laying bare causal relations between social phenomena.

Interestingly, even in sociological practices until recent times, we can find something of the discussions about and defining properties of these two traditions. Carla van El in *Figuraties en verklaringen: stijlgebonden schoolvorming in de Nederlandse sociologie vanaf 1968* (2002) refers to the study of Wolf Lepenies who argued that the proto-sociologists borrowed the tools and concepts that already existed in the domains of the natural sciences and the humanities, so as to construct a new 'third way' for the study of social phenomena. Sociology had to emancipate itself as a serious field of academic inquiry, for as we have seen, history already interpreted society and its development, so it used elements from both older traditions in a completely new setting. The construction of sociology from practices in the humanities and the natural sciences was still visible in two major schools of Dutch sociology until a few decades ago.

The 'Figurative School' of Amsterdam was more inclined toward a literary and historical explanation of social phenomena, and held that the individual and the social are two sides of the same coin: they are constantly interacting and thereby changing each other. Social phenomena could be studied on the psychological level, on the societal level, and on the level of history. Unfortunately this school no longer exists in the form described here.

The 'Explanatory School' of Utrecht and Groningen, on the other hand, which still exists, stresses a methodological individual approach, that deduces hypotheses from general laws and principles about how social conditions affect the behaviour of the individual. The background presumption at work here is that human behaviour is relatively stable. This allows the Explanatory school to construct models of social conditions to test the hypotheses in order to see how social conditions affect the individual's behaviour.[126]

Now that we have a clearer view of the origin of the social sciences, in particular sociology, let us see how contemporary scholars make sense of the relation between the natural and the social sciences.

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[125] Abma, *Over de grenzen van disciplines,* 128.
[126] Carla van El, *Figuraties en verklaringen. Stijlgebonden schoolvorming in de Nederlandse sociologie na 1968* (Amsterdam: Aksant, 2002), 7-9.

Chapter 4. Models

In the remaining three chapters I will identify some alleged fundamental differences between the natural sciences and the social sciences, and use them as points of departure for the articulation of a constructive empiricist view of social science.

The first fundamental difference between the natural sciences and the social sciences Kagan identifies concerns 'the sources of evidence on which inferences are based and the degree of control over the conditions in which the evidence is gathered.'[127] According to Kagan, the natural sciences rely on 'experimentally controlled observations of material entities', whereas social scientists rely on 'verbal statements, less often on biological measures, gathered under conditions in which the contexts cannot always be controlled'.[128]

This way of framing the matter challenges us to spell out how the constructive empiricist makes sense of Van Fraassen's model approach to scientific theories in the context of the social sciences. In section 4.1 I discuss some philosophers that disclosed and criticized simplistic (implicit) assumptions about the relation theory-world. I believe that these criticisms provide some inklings for understanding why Van Fraassen's 'early' semantic model understands the relation theory-world too simply as well. In section 4.2 I discuss the 'later' Van Fraassen's more careful articulations of phenomena, appearances, measurements, data, structure, and empirical adequacy as solution to the problem. I show in sections 4.3 through 4.7 how this amendment enables a fruitful approach for models in the social sciences: the distinctions and notions introduced capture much of the way social scientists deal with models in actual scientific practice. In section 4.8 I elaborate on Kagan's idea of experiments and show that this rather simplistic dichotomy between the two cultures in terms of experimental control should not bother the constructive empiricist much.

4.1 The relation theory-world under philosophical scrutiny

Before we focus on the criticisms, let us first recapitulate Van Fraassen's 'early' model view by considering an example. In 'early' constructive empiricism, to present a theory is to present a family of structures, which are its models, and to specify certain parts

[127] Jerome Kagan, *The Three Cultures. Natural Sciences, Social Sciences, and the Humanities in the 21st Century* (New York: Cambridge University Press, 2009), 2.
[128] Kagan, *The Three Cultures*, 4.

of these models as empirical substructures that are candidates for the direct representation of observable phenomena.[129]

Newton, according to Van Fraassen, distinguished the observable or 'apparent' motions of bodies, say, planets, from their 'true' motions, and 'saved' the phenomena in his mathematical model of motion.[130] An apparent motion is a motion relative to other bodies. Apparent motions form relational structures that can be defined by measurements or observations of the time and position of bodies, with angles of separation, velocity, and distances, calculated as functions of their times and positions. A 'true' motion is a motion relative to 'absolute space' in Newton's mathematical model. Newton held that apparent (relative) motions could be identified as the differences between 'true' motions. In his mathematical model, structures could be defined that were exact reflections of the apparent motions, although these structures were defined in his theory in terms of relations between absolute times and absolute locations. So Newton had some model in his theory such that all actual apparent motions were identifiable with motions of that model. The crucial feature of constructive empiricism is of course, that instead of being ontologically committed to all the unobservables Newton's theory implies, for example absolute space and absolute motion, we could adopt instead the weaker belief that observable phenomena can exist in the structure described by the theory, and be agnostic about all other aspects of its theoretical structure that are unobservable.

Let us start with Nancy Cartwright's critique of a 'conventional' route from scientific theory to world. It is important to remark that in *How the Laws of Physics Lie,* Cartwright did not really identify or voice a specific critique of the gap between theory and world in constructive empiricism. Still, I believe that the book provides a useful general impression of the issue.

Cartwright criticizes the deductive-nomological picture that says that fundamental laws of nature explain in the sense that they tell us how concrete physical systems in the world behave as concrete instances of these laws.[131] Take as example Newton's universal law of gravitation. Concrete massive bodies are subject to gravity

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[129] Van Fraassen, *The Scientific Image,* 64.

[130] For Van Fraassen, 'appearances' or 'apparent motions' are not the same as observable phenomena. The former are the possible outcomes of measurement procedures, whereas the latter are any observable entities, process, events. He acknowledges that he did not too carefully distinguish the two in *The Scientific Image*. Van Fraassen, *Scientific Representation: Paradoxes of Perspective,* 283, fn. 24.

[131] Nancy Cartwright, *How the Laws of Physics Lie* (Oxford: Clarendon Press, 1983), 44.

in the Newtonian sense. However, they are in principle also subject to electromagnetic attractions as specified by Coulomb's law, since massive bodies may also be charged. Newton's law of universal gravitation abstracts away from this complex situation, hence, it is inapplicable to actual massive bodies in the world that are also charged. Cartwright's famous way of phrasing this issue is that Newton's law holds only under 'ceteris paribus' conditions, which means 'other things being equal'.[132] Fundamental laws are not applicable to the messy realm of the concrete matters of fact, hence, they do not explain them.

According to Cartwright, the correct route from scientific theory to world is from fundamental law, to a model of a phenomenon, to phenomenological laws. Fundamental laws are true of the model, phenomenological laws are true of the phenomena.[133] It seems that Cartwright claims that the gap between theory-world can be closed by introducing models in between the observable phenomena in the world and abstract theory, which is, as we will see, part of Van Fraassen's solution to the gap.

However, there are important differences between Cartwright and Van Fraassen that we must mention. Phenomena in Cartwright's account are the events, entities and processes the *physicist* and not the *philosopher* deems observable. Phenomena so conceived are blind to the observable-unobservable distinction of Van Fraassen.[134] Physicists count the process of superfluidity and meson-nucleon scattering as physical phenomena and aim to describe them with phenomenological laws.[135] The description of what happens in superfluids is not phenomenological in Van Fraassen's sense. So in the work of Cartwright, the construction of a model of a phenomenon is not necessarily the construction of a model of some observable entities, events or processes, as it is in constructive empiricism. Cartwright thinks that in science we should aim for satisfactory descriptions of phenomena. We explore the question why the phenomena behave as they do by looking for phenomenological laws. In this quest, a belief in the reality of unobservable entities and causal processes is imperative, claims Cartwright.[136] It will be clear that Van Fraassen does not accept this demand for explanation and the beliefs it involves.

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[132] Cartwright, *How the Laws of Physics Lie,* 45, 56-58.
[133] Cartwright, *How the Laws of Physics Lie,* 1-4.
[134] Cartwright thinks that the observable-unobservable distinction is foremost a philosophical distinction and not a physical one. She seems unaware that Van Fraassen argues in *The Scientific Image* that it is a non-theoretical, biophysical distinction.
[135] Cartwright, *How the Laws of Physics Lie,* 2.
[136] Cartwright, *How the Laws of Physics Lie,* 159-161.

For the purpose of this thesis, however, I extract from Cartwright's work the idea that abstract entities - regardless of whether they are fundamental laws or abstract models and structures in the sense of Van Fraassen - need some kind of entity in between the world and abstract theory that is idealized in some way so as to render these abstract entities relevant to the observable regularities in the world.

Let us now turn to James Bogen and James Woodward. Bogen and Woodward criticise Van Fraassen's idea of empirical adequacy, which says that a good scientific theory must save the phenomena, that is, the observable entities, events, and processes must fit in the theory. Bogen and Woodward have a somewhat similar understanding of phenomena as Cartwright: the 'true' melting point of lead, the decay of a proton, weak neutral currents, and the human capacity to store things in memory, are examples of phenomena in their view.[137] Their point is that when scientists make observations, they will generally obtain scattered data and not phenomena. Consider the following illustration. We cannot simply observe the 'true' melting point of lead, since we virtually never observe the 'true' melting point of lead but rather different data points when we try to measure it. The 'true' melting point of lead (327.5 ± 0.1 degrees Celsius) is, on various assumptions (the purity of the lead, the working of the thermometer, the way it was applied and read) estimated from observed data, on the basis of statistical inferences, but in any case, not observed. Hence, Van Fraassen cannot claim that theories save the phenomena, but must be taken to claim that theories save the data, which is an unreasonable requirement on any theory and is therefore implausible.[138]

In my view, the latter claim is unfair to constructive empiricism since phenomena in Van Fraassen's sense are just observable entities, events, and processes that can be measured; hence, a theory can in principle be empirically adequate of phenomena in that sense. Bogen and Woodward compare their use of 'phenomenon' directly with the way Van Fraassen uses it in the slogan of 'saving the phenomena' which is quite different. It may well be possible to think of a phenomenon in Bogen and Woodward's sense such as the 'true' melting point of lead, as an observable quantity that is the statistical extrapolation of a parameter from a data set.

Still, their argument is fair insofar it requires the constructive empiricist to be

[137] James Bogen and James Woodward, "Saving the Phenomena," in *The Philosophical Review* 97, no. 3 (July 1988): 306.
[138] Bogen and Woodward, "Saving the Phenomena," 308-09, 351.

more explicit about the relations between phenomena (understood as observables), measurement, data, idealization, and models. Van Fraassen says that empirical adequacy means that the observable phenomena fit in the theory's models, but exactly how do we ascend from the observable world to model to theory? This is a theme that does not receives much attention in *The Scientific Image*. Observation is by no means a simple process and in scientific practice, by observing we indeed often obtain scattered data which, under various assumptions and idealizations, is processed to yield just the observable quantities we need.

According to Bogen and Woodward, there are considerations that justify our inferences from data to phenomena. If the data obtained are reliable, if possible confounding factors and errors are under control, if the right methods for data reduction, analysis and statistical testing are used, we are warranted in inferring the existence of phenomena. Whether they are observable or unobservable is not relevant in this inference.[139] Although this may be the conviction of scientists working in some theoretical picture, the point of *The Scientific Image* was that constructive empiricists do not need to make any inferential ontological statement about things unobserved, as long as we have a theory that is correct with respect to what can be observed.

Let us take stock. I have provided some illustrations of the gap between theory-world from the literature. According to Cartwright, abstract generalizations do not say much about the concrete observable things in the world if the phenomena in the world are not idealized in some way, and according to Bogen and Woodward, in actually testing theories in the world we should be aware of the complexity of observation, in the sense that we usually observe scattered data.

4.2 A solution

We will see that idealization and the distinction between phenomena (as Van Fraassen understands them) and data are recurring notions in Van Fraassen's 'later' view of models. But we have not yet identified the crucial problem for his 'early' view. Van Fraassen's 'early' view, epitomized in *The Scientific Image* and illustrated in the example of Newtonian mechanics in section 4.1, maintains that empirical substructures are candidates for the direct representation of observable phenomena, and if phenomena fit in the empirical substructures, empirical adequacy is achieved.

[139] Bogen and Woodward, "Saving the Phenomena," 327.

However, the crucial issue in my view is that in the 'early' Van Fraassen it is underdeveloped what it means to embedded concrete observable phenomena in abstract mathematical structures, and what it means to represent them by doing that. The problem is that phenomena are not abstract entities but concrete entities, and that the 'later' Van Fraassen thinks that talk about structure makes sense only insofar it is talk about the mathematical structures or models we use to represent phenomena.[140] As empiricist he wishes to refrain from supposing structure in nature, since this supposition would smack of a metaphysical outlook on the world.[141] Here the gap between theory and world recurs in all its splendour. Various notions and distinctions are introduced so as to make sense of the problem how abstract structures map onto concrete objects, and how they represent them. This amounts to a more complete and rich view of phenomena, data, measurement, models, and empirical adequacy. Let us discuss these now.

Firstly, Van Fraassen's solution is to posit a distinction between phenomena and appearances. Phenomena are all the observable entities, processes, and events, as they were understood in *The Scientific Image*. Appearances are phenomena as they occur in measurement outcomes. Appearances are determined by a particular measurement set-up, a particular experimental practice, and the theoretical conceptual framework in which the phenomena and measurement procedure are understood, classified and characterized.[142] So the things we ordinarily observe in the world, the behaviour of natural objects and humans, are observable entities or phenomena, and they exhibit the autonomy and 'bruteness' discussed in section 2.6. Still, whenever we are motivated to measure something about them, we get theory-laden appearances. A measurement in this context is an operation that functions as an instrument to gather information. Measurement outcomes are things such as a list of numbers that are meaningful information in that they are intended as representation

[140] Valerio Iranzo writes that in *The Scientific Image* Van Fraassen is a realist about structure in the 'extra-scientific' world since he supposes that empirical substructures directly map onto observable phenomena, although that requires them to be represented in some abstract way, hence, requires the postulation of structure beyond scientific theory. Valerio Iranzo, "Models and Phenomena: Bas Van Fraassen's Empiricist Structuralism," in *Bas Van Fraassen's Approach to Representation and Models in Science,* ed. Wenceslao Gonzalez (Dordrecht: Springer, 2014), 70.

[141] Van Fraassen, *Scientific Representation,* 238-39, 247. Elsewhere Van Fraassen maintains that empiricist structuralism is a view of science that has no implications for what the world is like. See James Ladyman, Otávio Bueno, Mauricio Suárez, and Bas C. van Fraassen, "Scientific representation: A long journey from pragmatics to pragmatics," in *Metascience* 20, no. 3 (November 2011): 438.

[142] Van Fraassen, *Scientific Representation,* 283-84.

of the entity, event, or process measured. A measurement is an operation that locates something (that is already classified as belonging to the domain of some theory) in a logical space, for example on a scale (which is provided by the theory to represent a range of possible states or properties of something).[143]

Secondly, Van Fraassen invokes 'data models' and 'surface models' on which the appearances bear. A data model is basically a collection of data. A data model summarizes a series of measurements made and appearances obtained, for example the relative frequencies of occurrences of some event. A surface model idealizes mathematically the data available and replaces it with more sophisticated information, for example by transforming relative frequencies to continuous variables.[144] Both surface models and data models are mathematical structures. [145]

Thirdly, there are 'theoretical models' which are highly abstract mathematical structures, for example Newton's law of gravitation as discussed by Cartwright. Van Fraassen now answers the question what the relation between theory and phenomena is in the following way: a phenomenon is *represented* in data models or surface models, which are mathematical entities and exhibit certain structures; embedding is a *matching* of the mathematical structure of the data model or surface model with the structure of an abstract theory.[146] The claim that some theory is empirically adequate no longer means that the theory fits the observable phenomena simpliciter (a two-place relation between theory and world) but rather that it is true of the phenomena as represented by *someone* or by *some scientific community* (a three-place relation between theory, world and user) by means of data models and surface models. Checking whether a theory is empirically adequate is checking whether the structures of the theoretical model and the data model or surface model are isomorphic.[147]

So basically Van Fraassen's solution to the theory-world gap is to say that there is always some user that chooses to represent phenomena with some mathematical structures. Van Fraassen maintains that the observable things in the world can be

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[143] Van Fraassen, *Scientific Representation,* 156, 159.
[144] Van Fraassen, *Scientific Representation,* 166-70.
[145] This invocation of different kinds of models corresponds nicely to Soazig Le Bihan's contention that in order to remedy the simplistic relation between theory and model of the 'strong' semantic view (fn. 149), we should suppose a whole 'hierarchy' of models that lies in between the phenomena and the theory. Soazig Le Bihan, "Defending the Semantic View: what it takes," in *European Journal for Philosophy of Science* 2, no.3 (October 2012): 14.
[146] Bas C. Van Fraassen, "Representation: The Problem for Structuralism," in *Philosophy of Science* 73, no. 5 (December 2006): 543-44.
[147] Van Fraassen, *Scientific Representation,* 258-59.

represented or modelled by a potentially infinite variety of structures.[148] There is nothing in the observable phenomena themselves that determines which structures are models for them; it completely depends on the user's selection of relevant parts of the phenomena, and this deflates the significance of the question how abstract objects are ultimately mapped onto the physical world. So it is basically some user who decides that this surface model represents that phenomenon, and in this situation, it does not make much sense for that user to ask whether s/he has a theory about the phenomena themselves or about the surface model as representation.[149] The only thing we (can) do in science is represent phenomena by means of models.

We must make some concluding remarks on the role of scientific representation in constructive empiricism. Representation is a theme that remained implicit in *The Scientific Image,* but came to fruition in *Scientific Representation: Paradoxes of Perspective.* In my view, the new insight that we represent the phenomena in data models and surface models, and the new account of empirical adequacy resulting from this have some broader implications. For example, the new picture renders the correspondence concept of truth as discussed in section 1.5 obsolete. If there is no direct relation between theory and phenomena anymore, how should one make sense of the idea of truth as correspondence? In addition, can we still understand the language in which we couch our theories in a literal way, since literalness also draws on the idea of truth as correspondence? These implications will not be addressed in this thesis, but they illustrate the implications of the change in views.

In both *The Scientific Image* and *Scientific Representation: Paradoxes of Perspective* scientific representation is essentially mathematical: it is defined as the isomorphic embeddability of mathematical structures. However, Mauricio Suárez has argued that one cannot define representation exclusively in terms of isomorphism. The idea that representation is isomorphism between structures claims that model *P* represents *Q* if and only if *P* and *Q* instantiate isomorphic structures, hence reduces representation to isomorphism. However, as we have seen, model *P* cannot represent physical phenomenon *Q*, if *Q* does not instantiate structure in some way. The problem is that 'every physical object instantiates simultaneously several structures', that is, the physical world underdetermines its mathematical structure. That means that the

[148] Van Fraassen, "Representation," 541.
[149] Van Fraassen, *Scientific Representation,* 258-61.

representation relation itself is underdetermined.[150] According to Suárez, this problem is solved by understanding representation not in terms of necessary and sufficient conditions such as isomorphic embeddability, but rather broadly as a social activity of some community engaged in a social practice, a community that has its own underlying norms of determining what represents (target) and what is represented (source), and which inferences can be validly drawn from the target.[151]

I believe that Van Fraassen deals adequately with this critique by admitting that there are in principle infinitely many ways a theory can represent the observable world, and by being very clear that it is always some *user* (or perhaps a community) that uses, makes, or takes something to represent some other thing as thus or so.[152] In that sense scientific representation is not mere 'isomorphic copy-making' but is essentially pragmatic in the sense that the intended use of a theory occupies centre stage. In this context it is senseless to ask why and how representations are representations of the observable phenomena, since this is a claim that is already 'built-in' in the scientist's use of some theory for describing or pragmatically explaining observable regularities.

4.3 Mathematical models for the social sciences?

As Soazig Le Bihan notices, there are many scientific disciplines that do not proceed by defining laws or axioms, for example, neuroscientists with computational models of the brain or biologists concerned with evolutionary theory.[153] I take this to be the case for the social sciences as well, and I think that some further comments on the leeway in Van Fraassen's discussion of models helps us in making them intelligible for the social sciences.

Van Fraassen has in *The Scientific Image* no definite opinions on the specific formulation of the model-view of theories one should adopt; he 'does not wish to favour any mathematical presentation as the canonical one.'[154] As Le Bihan points out, Van Fraassen thinks of models mainly as a way of presenting a theory but is not intent on a stronger claim, in the sense that theories can be reduced to models, can be

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[150] Mauricio Suárez, "Scientific Representation," in *Philosophy Compass* 5, no. 1 (January 2010): 94-96.
[151] Ladyman et al., 'Scientific representation,' 432-33. See also Suárez, "Scientific Representation," 96, 99.
[152] Van Fraassen, *Scientific Representation,* 23, 259-261.
[153] Le Bihan, " Defending the Semantic View," 5.
[154] Van Fraassen, *The Scientific Image,* 66.

identified with them, or something like that.[155] In the remainder of this thesis a rather straightforward conception of models is adopted, which is borrowed from Michel Ghins:

A model is in the first place a structure that makes true or satisfies a set of statements. Thus, some structure of measurement numbers makes true the statement "Brazil's birth-rate is higher than Belgium's". On the other hand, scientists often stress the representative role of models. In this second sense, models are the possible representors of structures similar to them. [...] *Pursuing in this direction, we may construe scientific theories as classes of models that satisfy some statements (e. g. axioms if the theory is axiomatized) and are possible representations of some concrete structures.* [156]

In my opinion, Ghins' statement above regarding birth-rate is much like the theoretical sentences in the social sciences that hypothesize relations to hold between social concepts, such as "If social integration in a religious society decreases, suicide increases".[157] This is what I understand as abstract theory in the social sciences. Now the models which are structures that attempt to satisfy these statements specify relations between the relevant entities (represented as variables) in statistical correlation and linear regressions analyses. I understand the latter as mathematical models. These models are a ubiquitous feature in social research. They do not get much philosophical attention, yet their use is standard practice at least in substantial parts of sociology, pedagogy, psychology, political science, and the like. They comprise indeed, as Ghins puts it, relations between aggregated and idealized 'measurement numbers' - in our terms data models or 'variables'. They employ a certain structure that is supposed to 'mirror' the structure hypothesized in abstract theory.[158]

My view on structures is straightforward and liberal: as said earlier, they are just relations between entities in some domain (relations between concrete things, numbers, concepts, and so on). Structures of this kind are found in both theoretical or

[155] Le Bihan, " Defending the Semantic View," 5.
[156] Ghins, "Models, truth and realism," 211.
[157] Interestingly, this is exactly how Durkheim phrased his propositions, although of course, he thought that social facts were concrete phenomena in nature and not theoretical at all. It is clear that with regard to the latter, since things as social facts are unobservable and abstract, these propositions are theoretical propositions. See Durkheim, *Suicide,* 167.
[158] Ghins, "Models, truth and realism," 211.

abstract models and in the low-level representational data or surface models, that must implement the relations specified by the theoretical model and at the same time represent observable phenomena in some way.

Yet, the worry can be voiced that this idea of structures is a far cry from the semantic view which understands models, representation, and structure in the context of mathematical logic and set-theory, in which representation may be defined in terms of an isomorphism of structures.[159] Indeed, the situation is different for the concrete studies I consider. The theory and model as structure are often not similar in an isomorphic sense. Similarity is rather loosely and implicitly understood just as similarity of relations between the entities in both domains. But I do not see this as an insuperable problem. In this thesis I suppose with Patrick Suppes that 'any serious statistical treatment of a theory and its relation to experiment does not differ in any essential way from the logical notion of model.'[160] Although I will not be concerned with considering experiments in this thesis, the 'serious' statistical analyses on which I focus share definitely some of the aspects he discusses in his treatments of experiments. Among other things, the estimation of parameters on the basis of data drawn from some sample that are best 'fits' in the sense that they maximize the probability of the observed data, the assessment of the relative independence and normal distribution of variables, and the statistical testing of hypotheses are examples.[161] These analyses ask us to take our models as mathematical entities.[162]

Suppes goes even further in this more 'liberal' direction. He quotes Herbert A. Simon[163] who describes a statistical practice that is quite close to the statistical models that will be discussed in this thesis. Simon holds that in constructing a model, what we do is aggregating and averaging data that we obtained from some sample. Variable X may be measured by locating opinions of sample subjects on numerical scale positions and calculating the mean and standard deviation of opinions in these numbers. Suppes comments that in the behavioural sciences, the set of quantitative assumptions of the theory, the sentences which, if they were specified precisely, constitute the set of

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[159] Van Fraassen does not define representation exclusively in terms of isomorphism: see the last paragraphs of section 4.2.

[160] Patrick Suppes, "Models of Data," in *Logic, Methodology and Philosophy of Science. Proceedings of the 1960 International Congress,* ed. Ernest Nagel, Patrick Suppes, Alfred Tarski (Stanford: Stanford University Press, 1962), 252.

[161] Suppes, "Models of data," 256, 259.

[162] Patrick Suppes, "A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences," in *Synthese* 12, no 2/3 (September 1960): 293.

[163] Suppes, "A Comparison," 288.

axioms of the theory (or at least an intuitive basis for formulating them), are often understood as a model. But in this usage a model would be a linguistic entity and not an extralinguistic entity; the latter is the default view of models in the semantic view. Still, this is no serious difficulty, claims Suppes. In his view, 'the meaning of the concept of model is the same in mathematics and the empirical sciences. The difference to be found in these disciplines is to be found in their use of the concept'. Mathematicians will simply ask different questions about models than empirical scientists.[164]

4.4 A reconstruction

Can we reconstruct social science according to the model view of scientific theory? I discuss the answer to this question by reference to a concrete piece of social research by Kevin D. Breault, who we can interpret as aiming to test two substructures of Emile Durkheim's abstract theory of social integration and suicide. Breault hypothesizes two substructures: the inverse relation between religious integration and egoistic suicide and the inverse relation between family integration and egoistic suicide.[165,166] Durkheim thought that suicide was a social phenomenon that could not be explained solely by reducing it to psychological factors. According to Durkheim, in the scientific explanation of social phenomena, it is necessary to appeal to non-reducible 'social entities' such as a 'great group of states of minds' in which the individual mind participates but which is nevertheless external to it.[167] Once the social ties or bonds of the groups in society weaken, once individuals share fewer beliefs, goals, and attitudes with their group in society and participate less in the 'collective life' of their group, suicide occurs.[168]

Breault aimed to test Durkheim's abstract theory with data from states and counties in the United States, over six different years between 1933 and 1980.[169] We will proceed with a stepwise reconstruction of this research, from phenomena up to

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[164] Suppes, "A Comparison," 289.

[165] For the purpose of this thesis and for the sake of brevity, a simplified version of Breault's extensive study is presented. Only the data concerning U.S. states figures in the following discussion.

[166] 'Egoistic' suicide is suicide resulting from the deterioration of social ties, see Emile Durkheim, *Suicide: A Study in Sociology,* ed. George Simpson, trans. John A. Spaulding and George Simpson (London: Routledge, 1951), 167-168.

[167] Durkheim, *Suicide,* 277-278, 280.

[168] Durkheim, *Suicide,* 125, 167, 170-71.

[169] Kevin D. Breault. "Suicide in America: A Test of Durkheim's Theory of Religious and Family Integration, 1933-1980," in *American Journal of Sociology* 93, no. 3 (November 1986), 628.

the similarity of structures.

4.5 Phenomena, appearances and measurement

Let us start with considering the *social observable phenomena*, the observable regularities. The latter, in my view, just consist of human behaviour, of people doing things in a regular way. I say *social* observable phenomena because it is humans and their behaviour we are looking at and not non-human physical objects. No structures or meanings are *a priori* supposed to exist in the observable social phenomena as they are. We just observe humans allocating, congregating, eating, gesturing, making sounds, and the like. Now of course, when we see someone committing fatal self-harm, we take this to be suicide, an act that is meaningful, an act that carries a certain goal. That is just our innocuous, theoretically-laden way of interpreting our observations, but it is not something that is clear from the observable behaviour itself, something that we can disclose in the phenomena or something that we can unproblematically presume to exist. As Van Fraassen says, this is just the result of being rooted in a particular culture, language, and so on: we play the Wittgensteinian language game in a certain way. So we take ourselves to be observing people committing suicide; people divorcing; people praying, and so on. This is a kind of natural epistemological attitude that can also be found in science.[170] It is basically what Van Fraassen calls the 'objective attitude': the starting point of natural science is to grasp entities as systems. The starting point of the social sciences is to grasp behaviour as meaningful, goal-directed, intentional, and so on.[171] What we in addition observe, is that in different parts (states) of some country, the frequencies of these social phenomena such as suicide are different.

Now Breault's study attempted to develop some statistical models by relying on various aggregate measurements of the observable phenomena discussed above, that would implement the structures he (and Durkheim) hypothesized: the inverse relations between the concepts discussed earlier. Now I hold that these observable phenomena and their regular nature can be said to be 'measured' in the sense that they

[170] I am in favour of the Quinean idea that science is a continuation of common sense, but in a more careful and systematic fashion. We are used to infer the existence of all kinds of things on the basis of a few measurements or observational cues; it is no miracle that this is the default attitude of science, especially of traditional scientific realism. Constructive empiricism improves on this idea by proposing an epistemically modest (agnostic) attitude towards entities that cannot be observed, which concern ultimately many of the entities usually inferred.

[171] Van Fraassen, *The Empirical Stance*, 152-62.

are taken to be some instance of suicide, religious behaviour, or the like by questionnaires, interviews, surveys and official institutions that register social phenomena in some way or other; with a particular question, say. If this sounds rather outlandish to the reader who wishes to cling to the existence of a social realm that is essentially imbued with peoples' meanings, desires and goals, I say: fine, although that is a statement that involves commitments science does not need to be an endeavour that is intelligible at all, so not a statement the constructive empiricist adopts. Although we usually bring intentions, desires, beliefs and the like to the table to make sense of other people's behaviours – in science but also in the most mundane of cases, which is perfectly fine – ultimately, we do not have epistemic access to intentions, desire, beliefs and the like. We may regularly observe people standing still in front of a red light and infer that they all desires to obey the law, but we ultimately have no access to knowing the latter statement – notwithstanding the fact that we commonly think the contrary.

Further, one may suppose that measurement in physics is a straightforward, immutable procedure of simply representing observables as values on a mathematical scale, and that social sciences can never even approach this straightforward, unambiguous process. Although all measurements in Newtonian mechanics (as discussed in section 4.1) may be reduced to a fairly straightforward series of measurements of time and position of bodies in some domain, from which other quantities are calculated,[172] in a different theoretical paradigm the nature of measurement could radically change in the sense that the 'old' way of measuring is incomparable to the new one. As Van Fraassen illustrates, 'What goes on in a measurement process is described differently by classical physics and by quantum theory.'[173] The limits and nature of measurements are not described and settled once and for all. It is a mistake to think that measurements in the natural sciences are immutably unambiguous and straightforward compared to the social sciences.[174] Measurement in the social sciences fits the idea of measurement as representing some 'brute' social phenomena as values on some defined scale (such as counting cases of divorce in some area in some time frame, expressing one's educational level in years

[172] Van Fraassen, *The Scientific Image,* 59.
[173] Van Fraassen, *The Scientific Image,* 57.
[174] An example of problems in measurement and experimentation is given in the 'solar-neutrino problem', that will be discussed in the next section.

of schooling, one's income in gross income per year, and so on).

In principle we are not able to observe an individual divorcing, or an individual belonging to a church. This is merely some theory-laden measurement and concomitant interpretation of some observable behaviour – an *appearance*. I do not think that this is much different from the account of measurement discussed earlier: an operation that locates an item, which belongs to the domain of the theory in question, in some logical space provided by the theory. It is representing observable entities mathematically that are not even abstract in the first place.[175]

4.6 Data models

Let us continue with data models. Data models are just summaries of measurements or appearances. So these are aggregate data about suicides, divorces, and church memberships, obtained at various moments. They are the databases social sciences such as sociology and psychology collect and work with and that are available for statistical analyses, and hence, are somewhat smoothed-out already.

In the study by Breault, for example, they are the suicide rates per 100.000 individuals of various states registered by the U.S. National Center for Health Statistics, that was subject to some idealisations in the research. For instance, not all suicide rates for the different states were registered in completely equal ways. But they are also the church membership rates per 1000 individuals of various states in various years that were subject to correction, as well as the interpolation of a missing year on the basis of the high stability of other years in terms of correlations.[176, 177] They are also the divorce rates, obtained by the same official institution as the suicide rates, per 1000 individuals of various states. They are also the datasets of the income of individuals, unemployment rates, the percentages of immigrants living in some area and so on, collected by official institutions. All these data are aggregated per subject, often averaged and 'smoothed' out with as result that one can compare all the 'standardized' rates for different states, regarding their suicide, church membership, and divorce.[178] These aggregated, averaged and sometimes standardized values are in social science

[175] This is in essence Reichenbach's problem in reverse: what exactly is the significance of mathematical equations for physical objects, since they belong to quite different spheres (natural vs. logical)? Van Fraassen, "Representation,"537.

[176] Rodney Stark, "Correcting Church Membership Rates: 1971 and 1980," in *Review of Religious Research* 29, no. 1 (September 1987): 69-77.

[177] Breault, "Suicide in America," 641.

[178] Breault, "Suicide in America," 637.

often understood as *variables*. I mention unemployment, the percentage of immigration, and the degree of urbanization as variables as well, because they are also added in the model as control variables that, among other control variables, are hypothesized to potentially bear structural relations to suicide as well.

4.7 Surface models

Now the surface model idealizes the information available further by sophisticating it into a more complex mathematical model. It translates the data in the data model into a more suitable form so as to make a comparison of the structure of the entities in the data model with the structure of the abstract theory possible. In our case, the latter are Breault's and Durkheim's abstract theoretical conjectures.

In Breault's work, the surface model is first a correlation model, where relations between sets of variables (not relations between observations) are expressed as correlation coefficients. This model is an idealization in that it assumes, among other things, that the variables in the data model are independent and distributed normally in that they do not have extreme values.[179] Correlation coefficients are numbers that express the degree and direction of linear relationships between paired sets of data or variables. They are numbers between -1 and 1 and a number closer to 1 indicates a positive correlation, whereas a number closer to -1 indicates a negative correlation (inverse relation).[180] For example, with regard to the data from states in 1980, in table 2 one sees that there is a significant correlation of suicide with church membership (r = -.605), which is significant at the alpha .05 level. This means that whenever suicide rates increase, church membership rates decrease and vice versa. The coefficient is significant, which means that the correlation is very unlikely to occur on the assumption that there is no correlation at all, implying that the result is the effect of mere chance.[181] There is also a correlation between divorce and suicide (r = .847), which is significant at the alpha .05 level. It means that whenever divorce increases, suicide increases, and vice versa. So in the statistical surface model, the calculated structures between data are as Breault's and Durkheim's highly abstract hypotheses indicated. One remark here must be, however, that some other relations between

[179] Frederick J. Gravetter and Larry B. Wallnau. *Statistics for the Behavioral Sciences* (Belmont: Cengage, 2008), 533.
[180] Gravetter and Wallnau, *Statistics,* 520-51.
[181] Gravetter and Wallnau, *Statistics,* 249.

variables were significantly and positively correlated as well, for example, the percentage of population growth and migrants living in a state and suicide.[182] As I said, it is generally not possible to obtain a clean and perfect mirroring of structures of theory with surface models in the social sciences.

Breault's analysis continues with a multiple-regression analysis, which can be seen as a follow-up mathematical model in which a linear equation is computed from the data in the data models that provides a precise mathematical description of the correlations at issue. The regression technique involves constructing a line that represents the lowest aggregate of differences between the predicted values and the actual values, in other words, the line which has the smallest total squared error. In a case with multiple predictor variables, it has the form $Y=b_1X_1+b_2X_2+...+a$. This equation allows one to predict Y values for any known X_1, X_2,..., values. The b-coefficients are indicators of the contribution each variable makes in the prediction of Y.

Breault is interested in how well his variables of divorce, church membership, do in predicting suicide, compared to other variables, since we have seen that they are not the only factors correlated with suicide. According to Breault's analysis, church membership and divorce are among the most important factors predicting suicide at state level since their standardized coefficients are significant (they are all below the alpha .05 level except for two which are below the alpha .1 level) in many years (1980, 1970, 1960, 1950 – divorce even in 1940 and 1933 as well, but these years unfortunately lack data on church membership) where other variables are only significant predictors in some years but not all. For example, unemployment is a significant predictor of suicide only in 1950 and 1940. Another indicator is the large sizes of the significant b-coefficients of the predictor variables of interest compared to other significant b-coefficients.[183] This linear regression model is an idealized model in that it assumes, among other things, that the data are normally distributed, and that the predictor variables are not correlated.

4.8 Abstract theory

The abstract theory is often given in the form of a general theoretical conjecture: integration in the family is inversely related to suicide and integration in religious

[182] Breault, "Suicide in America," 642-51.
[183] Breault, "Suicide in America," 642-651.

groups is inversely related to suicide. In the bulk of actual social science, we do not have an abstract mathematical theory in the sense of abstract equations. We often have abstract theoretical statements and conjectures that are susceptible to some interpretation in terms of a mathematical model, that implements the same structure as the structure implied in the abstract theory. The data we collect we take to be measurements or representations of social observable phenomena. At the same time, the data are often said to be the numerical representations or measurements of the unobservable constructs in the theoretical model. This is analogous to measuring the velocity of some object as effect of gravitational attraction in the theoretical context of Newton's law of gravity.

In the study by Breault, the postulation of some structure between the abstract unobservable entities is borne out by the mathematical models that show the relations between data sets, although admittedly with quite some assumptions and sometimes even confounding factors. We have seen that there were significant correlations other than the correlations of divorce and church membership with suicide, just as occasionally other variables were significant predictors of suicide. I see the fact that we do not really have theoretical models and structures that are perfectly similar with the structures in surface models as an unfortunate but nevertheless fundamental predicament of the social sciences, and I do not have a solution at hand for this.

My point was to give an illustration of how we can make sense of phenomena, measurements, appearances, data models, surface models, and abstract theory in the social sciences. However, there are still some issues lingering in the background. We said that social phenomena are just individuals doing things; they are brute phenomena which are not intrinsically meaningful. But as a result of our distinction between phenomena and appearances, there is a difference between an individual inflicting fatal self-harm that we may observe, and suicide as it occurs in the model. In the model we are free to invoke unobservable entities and processes, causal interactions, counterfactual talk relative to what is allowed by a model, law-like generalizations, and the like. In my view this also applies to meanings, goals, intentions, peer pressure, norms, laws, and the like. We are free to represent any observable phenomenon in a way that fits our purpose. We do not speak metaphorically about these things but rather literally, in the sense that for all we know, they may be true of the world, while as of yet, we have no empirical access to them.

I believe that with constructive empiricism we can avoid the swampy discussions about how we measure beliefs, desires, intentions, social structures, since we do not measure them; we rather add them in the interpretation of our models. So we can accept the point of Bogen and Woodward, not that we should infer the existence of phenomena from our data because that is standard practice in science, but that we usually bring unobservable entities to the table to summarize, systematize and explain the data in a way that is in accordance with our contextually shaped needs as humans. Unobservables are entities, events and processes that we may hold as relatively stable, as exhibiting particular characteristics, and that we may take as (pragmatically) explaining the data, for example, a desire. We can, in any case, avoid any ontological commitments by holding that they are just entities, processes and events in our models just as they are features of our interpretations or 'measurements' of behaviour in mundane cases and not aspects of the things we commonly observe. That does, however, not bar us from employing and talking about them in a non-deflationary way, in our theories or everyday language – just as causal claims, counterfactual claims, and the like are not barred from ordinary language.

4.9 Experiments

Let us conclude with a return to and remark on Kagan's claim at the beginning of this chapter. He maintained that the natural sciences rely on 'experimentally controlled observations of material entities', whereas social scientists rely on 'verbal statements, less often on biological measures, gathered under conditions in which the contexts cannot always be controlled'.[184] I hold that experimenting in the natural sciences is a great deal more complicated than Kagan indicates. Let us discuss the case of the 'problem of solar neutrinos'.

Trevor Pinch describes how the detection of neutrinos could confirm an important tenet of stellar-evolution theory of the previous century, namely that stars rely for their energy on nuclear fusion. Neutrinos were thought of as massless and chargeless particles, that are a by-product of nuclear fusion reactions that occur in the sun. However, they interact only weakly with matter, which makes them exceedingly hard to detect. They could not be observed in any straightforward way but had to be observed by a rather intriguing setup: a huge tank with a chlorocarbonic 'dry-cleaning' fluid with which the neutrinos could interact. The interaction produced an argon

[184] Kagan, *The Three Cultures,* 4.

isotope, which was held to be evidence for the passage of neutrino's. The tank was located deep under the earth's surface so as to prevent the interaction of other radiation from outer space that could also trigger a chemical reaction. This is a quite common situation in high energy physics: we can detect some entity only by the interaction with other entities. But this is not all there was to the story. The presence of argon isotopes was inferred indirectly as well. After some time, the argon atoms were extracted from the tank by sweeping it with helium gas, and the argon was stored in a super-cooled charcoal trap. With the help of a tiny Geiger counter its characteristic decay in the form of electron emission was measured. But this is still not the end of the story, since a lot of measures needed to be taken to separate the noise from the genuine signals, for example by employing anti-coincidence devices around the Geiger counter.[185] The raw data were plotted, and some data were deemed genuine signals whereas others were deemed noise. The visual inspection of plotted data provided the information needed to calculate the number of argon decay products. And this in turn was used to calculate the neutrino-induced events. This entire process was far from straightforward; carrying it out necessitated a detailed understanding of the physics of the detection process, the working of the machines in question, and some statistical theory.

Interestingly, the first report of the results of the neutrino experiment ran counter to theoretical predictions from astrophysicists. In their models of stars like the sun, they assumed an initial chemical composition that described the evolution of stars within the model; a model was satisfactory if it was able to reproduce physical properties of the sun today, e.g. its luminosity and mass. Astrophysicists could calculate neutrino energy spectra as an implication of the models they used, from which they computed the neutrino emission which had the energy required to interact with the detector. When the first experimental results were produced, the astrophysical standard solar-model predictions appeared to be three times greater than reported experimental values.[186] The 'solar neutrino problem' was born.

The issue of interest for us is that many different assumptions in the experimental setting were questioned. For example, some astrophysicists expressed

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[185] Trevor J. Pinch, "Towards an Analysis of Scientific Observation: The Externality and Evidential Significance of Observational Reports in Physics," in *Social Studies of Science* 15, no. 1 (February 1985): 5-6.
[186] Trevor J. Pinch, "The Sun-Set: The Presentation of Certainty in Scientific Life," in *Social Studies of Science* 11, no. 1 (February 1981): 139, 135.

doubts about the radiochemical aspects of the experiment, for example about the estimates of the background radiation, the possibility of losing actual observations of argon isotopes in the process, or the difficulty with the ultra-small statistics needed in 'low-counting' or 'marginal' experiments in general. But also aspects of the nuclear physics involved were questioned: for example, the extrapolation of the probability of nuclei interacting as obtained from measurements in the laboratory, down to energies that were thought to be similar to the energies found in the core of the sun.[187] Others criticised the chlorocarbonic substance in the tank that could form polymers that trapped argon ions, thereby preventing them from forming neutral argon atoms, hence preventing them from being measured at all.[188] In any case, many physicists from a variety of subdisciplines within physics could not agree on either the experimental assumptions or the results obtained.

So whereas Kagan's idea that the natural sciences rely on controlled experiments may be plausible, it is unfair to cash an allegedly fundamental difference with the social sciences out in terms of degrees of control. The degree of control and the degree of certainty derived is exactly what is at issue in the minds of the scientists, as demonstrated by Pinch. The history of science demonstrates that it is far from obviously the case that in the natural sciences 'material entities' – it is questionable whether calling neutrinos 'material' has any sense at all – are straightforwardly 'observed' in controlled experimental contexts. It depends on more than experimental set-ups, apparatuses and alleged controlled conditions, namely on agreement among the practitioners themselves.

[187] Pinch, "The Sun-Set," 139-142.
[188] Pinch, "Towards an Analysis,"15.

Chapter 5. Primary interests of the two cultures

Another fundamental difference Kagan discusses concerns the primary interests of scientific cultures.[189] According to Kagan, the 'primary questions' the natural and the social sciences ask are different, as is the extent to which prediction, explanation or description of a phenomenon are the major 'products of inquiry'.[190] Kagan also claims that the 'primary concerns' of social scientists are the prediction and explanation of human behaviours and psychological states, whereas the primary concern of the natural sciences is the prediction and explanation of all natural phenomena.[191] The challenge that is before us is examining whether the existence of different primary questions in each culture is problematic for the constructive empiricist. In addition we should evaluate the roles of description, prediction and explanation as products of scientific inquiry in a constructive empiricist view of social science.

In section 5.1 I argue that the existence of distinct questions is not a unique difference between cultures, but something relevant within cultures as well. I discuss description, prediction, and explanation in sections 5.2 and 5.3, and argue that they are all 'products of inquiry' in the social sciences from a constructive empiricist perspective. In section 5.4 I also argue that constructive empiricism frees the social scientist of metaphysical baggage in discussing explanation in natural and social science.

A comment on Kagan's claim above that the social sciences are concerned with behaviour and psychological states, whereas the natural sciences are concerned with natural phenomena, is that it is true but not really problematic for the constructive empiricist. In section 4.5 I have argued that social phenomena are similar to natural phenomena, except that the former concern humans and their actions whereas the latter concern non-human entities, events, and processes. Hence, they are both eligible to a constructive empiricist view of science.

5.1 Distinct questions

The idea that different cultures ask different questions is a kind of commonplace,

[189] Kagan means by distinct 'cultures' distinct scientific disciplines and actual research in them (social science and natural science, for example). I follow his use in this thesis. See fn. 118 for a comment on Kagan's terminology.
[190] Kagan, *The Three Cultures,* 2.
[191] Kagan, *The Three Cultures,* 4.

because indeed, natural sciences and social sciences as different cultures are concerned with different things in the world (social phenomena as opposed to natural phenomena) and different methods of inquiry (e.g. experiments as opposed to questionnaires). However, we should not jump the gun with this traditional characterization of different aspects of questions that differentiates *between* cultures, but focus on different questions already apparent *within* a culture instead. My strategy in this section is to point to the heterogeneity of scientific questions apparent in physics and social science already, in order to show that we are not dealing with a challenge that is uniquely problematic for an articulation of constructive empiricism in the social sciences.

Consider the following illustration of natural scientists working in the field of string theory. These scientists are concerned with a dauntingly complex family of theories that are supposed to unify both nuclear and gravitational interactions. String theory today is described by Richard Dawid as:

> [...] a *complex web of reasoning consisting of elements of rigorous mathematical analysis, of general conjectures which are based on reasoning in certain limiting cases, of modelling that is done within specified frameworks and of some approximate quantitative assessments. The resulting understanding provides a vast body of structural information and theoretical interconnections between various parts of the theory but leaves unanswered many crucial questions*.[192]

Questions some physicists in this field pursue have for example to do with looking for possible non-perturbative structures of string theory, for example by positing string-dualities, leading to the introduction of a spectrum of various higher-dimensional objects.[193] However, this theory and its questions are generally agreed to be non-empirical in the sense that there is as of today, but probably in the future as well, no empirical testing of string theory possible.[194],[195] So the theory and questions pursued are of a thoroughly theoretical kind.

Consider an analogy for this situation in psychology. Donald Hoffman

[192] Richard Dawid, *String Theory and the Scientific Method* (New York: Cambridge University Press, 2013), 17.
[193] Dawid, *String Theory,* 14-16.
[194] Dawid, *String Theory,* 6.
[195] Richard Dawid, "Constructive Empiricism, Elementary Particle Physics and Scientific Motivation," accessed March 4, 2018, http://homepage.univie.ac.at/richard.dawid/Eigene%20Texte/11.pdf 3,4.

understands consciousness formally in terms of a conscious agent that consists of six components. Three are measurable spaces: the world, experience, and action. The other three components are relations between these spaces. Perceiving is the relation between world and experience by means of interaction; deciding is the relation between experience and action on the basis of experience; acting is the relation between action and world on the basis of a decision.

These relations are formally understood in terms of conditional probabilities: perception, for example, is a Markovian kernel that specifies the probabilities for the various conscious experiences that might result if the state of the world is $w1$, $w2$, and so on.[196] Now Hoffman's idea is that the world consists entirely of conscious agents and therefore, cannot be properly called an external physical world as commonly understood (Hoffman's ideas are almost a modern psychological expression of Berkeley's philosophy expounded in part I). Hoffman aims to construct the world and the objects in it mathematically by combining interacting conscious agents in dynamical systems. He reinterprets physical properties as position and momentum as properties of interaction. This prevents him from having to refer dualistically to an external physical world. I will not elaborate further on Hoffman's formal conception of consciousness, since it is quite complex. Still, the point for present purposes is that the study of the particular kind of questions involved, for example how the familiar perception of objects in the world emerges from such dynamics is completely theoretical. There is as of today no empirical test of this theory of consciousness, nor is there any appeal to empirical testing as commonly understood, that is to say, no non-computational or simulational evidence.

This situation poses a dilemma: one the one hand, from the viewpoint of constructive empiricism, we are dealing with two theories that are not empirically adequate but border on metaphysical speculation. There is nothing in the theories susceptible to empirical testing. I suppose that Van Fraassen would lament the unavailability of empirical models or consequences of current developments in string theory; he is not the only one.[197] On the other hand, do we want to cut these branches

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[196] Donald D. Hoffman and Chetan Prakash, "Objects of Consciousness," in *Frontiers in Psychology* 5, (June 2014): 6.

[197] There is a divide between physicists that defend that it is unproblematic for a theory to not have empirical consequences for some time, and physicists that claim that string theory is going too far astray. Dawid describes the camp of critics of string theory as defending a classical empirical paradigm of theory assessment, in which Lee Smolin and Roger Penrose are located. See Dawid, *String Theory,* 22-24.

of scientific inquiry that are still understood by many physicists as genuine science? We should indeed be cautious not to interpret this situation too simplistically. For is this situation not a rather common one in the history of science? Indeed, it is; as Carlo Rovelli argues, in a lot of epochs in the history of science, theories were pursued and developed without reliance on clear empirical arguments. Kepler was in favour of Copernicus' theory before its empirical consequences surpassed Ptolemy's theory. Einstein pursued general relativity before the bending of rays of light by the sun could be detected.[198] According to Peter Achinstein, Maxwell's development of the kinetic theory of gas was based on some assumptions that he could not expect to be true or even probable, because they involved the postulation of unobserved particles and their unobserved motions. According to Achinstein, one reason why Maxwell supposed that gas particles move in straight lines with uniform velocity, may be that he was influenced by the work of other physicist of his time, for example, Joule, Bernoulli, Clausius, among others, which he mentions and who also supposed this.[199]

I think that we have to introduce some leeway in the development of scientific theories in the sense that for some of them, the empirical testing of their models is not always evidently available. Van Fraassen has a somewhat ambivalent attitude towards this situation. On the one hand, Van Fraassen acknowledges that metaphysical aspects of a theory are sometimes necessary theoretical detours that in the end yield manageable descriptions of the observable world; in that sense he does not wish to call these aspects 'metaphysical baggage', only when 'these detours yield no practical gain' they are metaphysical aberrations. Still, useless 'metaphysical baggage' - because it lacks empirical consequences - may have potentialities for future use.[200] In my opinion we should not reject theories that in their process of development lack empirical adequacy. As Achinstein and Rovelli show, the development of scientific theories sometimes occurs in absence of opportunities for empirical testing.

There is also another route to the development of theories in science that lack straightforward empirical testability. Although the aim of science as a 'game' is to erect a theory that is true about its observable consequences, this is not the sole motive involved in participating, as Van Fraassen's chess analogy indicates.[201] Scientists may

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[198] Carlo Rovelli, "The dangers of non-empirical confirmation," accessed April 4, 2018. https://arxiv.org/pdf/1609.01966.pdf 1.
[199] Peter Achinstein, "Scientific Discovery and Maxwell's Kinetic Theory," in *Philosophy of Science* 54, no. 3 (September 1987): 410, 433.
[200] Van Fraassen, *Scientific Image,* 68.
[201] Van Fraassen, *Scientific Image,* 8.

be individually interested in pursuing problems for other reasons such as intellectual challenge, competition with colleagues, and so on, while still cherishing the idea that string theory or a formal theory of consciousness will in some way or other improve our understanding of the world by having some empirical relevance. According to Van Fraassen, the only problem is that the science in question cannot be regarded as a successful science yet since it is not empirically adequate yet.

The contrast class to this kind of theoretical questions in the natural sciences are the empirical questions. Think, for example, about scientists that are trying to tease out the empirical implications of the principles of quantum theory. According to Jonathan P. Dowling and Gerard J. Milburn, these principles are quantization, the uncertainty principle, quantum superposition, tunnelling, entanglement, and decoherence, and each of these principles is applied in various contemporary emerging technologies.[202] Dowling and Milburn provide the example of quantum cryptography, more particularly quantum key distribution systems. They hold that 'recent advances in single-photon optical engineering allows the distribution of quantum entangled photons over about a hundred kilometres of optical fibre'.[203] Cryptographic keys that are distributed in this fashion are virtually impossible to hack, due to Heisenberg's uncertainty principle.[204]

To complete the story for the social sciences, an analogy for an empirical question would be the test a possible extension of a well-established theoretical approach of intergroup behaviour.[205] Social identity theory holds that individuals tend to derive a positive social identity from group membership, comparing the group to which they belong (in-group) to another group (out-group) and concluding that one's own group is preferable to the out-group (by positive evaluations, liking, or allocations of resources). Some scholars tried to refine the theory by proposing that the perceived similarity of members of the in-group causes hostility toward the out-group in certain situations, thereby promoting aggression understood as harmful behaviour towards members of the out-group. The question asked was whether in-group 'favouritism' is related to a socially disruptive form of intergroup bias in the form of aggression

[202] Jonathan P. Dowling and Gerard J. Milburn, "Quantum technology: the second quantum revolution," in *Philosophical Transactions of the Royal Society A* 361, no. 1809 (August 2003): 1656-1657.
[203] Dowling and Milburn, "Quantum technology," 1659.
[204] Dowling and Milburn, "Quantum technology," 1659.
[205] Naomi Struch and Shalom H. Schwarz, "Intergroup Aggression: Its Predictors and Distinctness from In-Group Bias," in *Journal of Personality and Social Psychology* 56, no. 3 (April 1989): 364-366.

towards the out-group. This experiment was conducted with Israeli adults, who reported perceptions of their own religious group (in-group) and of an ultraorthodox Jewish out-group.[206] Its conclusion, among other things, was that in-group favouritism is not a good predictor of aggression towards the outgroup, thereby empirically disconfirming the proposed link between social identity theory and hostility toward out-groups.[207]

My point with these illustrations is that completely different questions in principle do not pose a problem that is unique for a constructive empiricist vantage point of social science. Natural science faces the same problem. There is the possibility of acknowledging theories with a 'metaphysical' character as genuinely scientific if we acknowledge that a lack of empirical testability is not uncommon in science.

5.2 Products of inquiry

Let us now examine what role description, explanation and prediction play in a constructive empiricist social science and see to what extent they are 'major products of inquiry'. Let us start with description. Our claim here is a repetition: we have a satisfying description of social phenomena if our theory is empirically adequate of the phenomena as they are represented in a surface model.

I have already argued that there is more to the story of a proper description of the observable phenomena by a scientific theory than is explicit in *The Scientific Image*. A scientific theory that is a good description, includes some abstract theory that exhibits some structure that fits the structure of the surface model, which is in turn a model or representation of the observable world. I want to re-emphasize here that it is no problem if the models we use in science are idealized to quite some extent.

Let us consider an example. In the kinetic theory of gases, as Van Fraassen remarks himself, 'bodies of gas are identified as aggregates of molecules, temperature as mean kinetic energy, and so on.'[208] Indeed, the kinetic theory of gases has mathematical models that specify how ensembles of gas molecules as microstates can be identified with particular macrostates, which appear to us as particular pressures, volumes, and temperatures.[209] Textbook physics tells us, however, that these micromodels are subject to quite some idealizations about the nature and the

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[206] Struch and Schwarz, "Intergroup Aggression," 364-366.
[207] Struch and Schwarz, "Intergroup Aggression," 371.
[208] Van Fraassen, *The Scientific Image,* 10.
[209] Van Fraassen, *The Scientific Image,* 166.

behaviour of the individual molecules in a particular system. Among other things, that the volume of these tiny individual particles is negligible; that particles are moving randomly and bump on the walls of the container and into each other, and that there are no forces of attraction or repelling between the particles; and that they move in straight lines. Further, as a kind of limit on its applicability, the model breaks down in cases of high pressures or low temperatures, since then the macro-properties of the gas at issue differ significantly from the predictions of the model.[210]

There is also the situation where assumptions are ad-hoc added to a theory so as to make it empirically more accurate. According to Cartwright this is common practice in quantum field theory, a theory that is held to be empirically quite successful.[211] Examples are the discarding of infinite self-energies and vacuum polarizations, spontaneous symmetry breaking, permanently confined quarks, a negative-energy sea of electrons, forced renormalization in gauge theories, all assumptions that she considers as bordering on the outlandish so that one may seriously wonder whether nature is 'seriously supposed to be like that'.[212]

The message I would like to drive home is that it is in principle no problem if models are highly idealized, have an applicability only in particular contexts, and may have assumptions that are added ad-hoc. We should now discuss a concrete study in the social sciences to see what this looks like. Before we do that, let me first say something about prediction so that the discussion of the social study can be a fitting illustration of both description and prediction.

5.3 Prediction

For the constructive empiricist, prediction is rendered possible by the very definition of empirical adequacy. For if some scientific theory and one of its models is empirically adequate, then this is the same as saying that it is empirically adequate in *past, present and future*. Van Fraassen's way of putting this is that constructive empiricists are 'sticking their neck out', in the sense that - just as is the case with a simple perceptual judgment - a judgment of empirical adequacy

[210] Steven S. Zuhmdahl and Donald J. Decoste, *Introductory Chemistry. A Foundation* (Belmont: Cengage Learning, 2010), 430-31.
[211] George A.D. Briggs, Jeremy N. Butterfield, and Anton Zeilinger, "The Oxford Questions on the foundations of quantum physics," in *Proceedings of the Royal Society A* 469, no. 2175 (September 2013), 2.
[212] Cartwright, *How the Laws of Physics Lie,* 7. Cartwright even adds local gauge invariance to this list.

[...] *goes far beyond what we can know at any given time. (All the results of measurement are not in; they will never all be in; and in any case, we won't measure everything that can be measured.)* [213]

If we accept a certain theory, we expect that the abstract theory remains empirically adequate, that is, that the structure of the data model or surface model fits the structure of the theoretical model, until there is some other theory or model that improves on it, for instance. Van Fraassen locates improvement in predictions not so much in growing theories but rather in the replacement of theories; some abstract theory or model that is better at describing surface models may always arise.[214] That we have theories that are quite successful in their predictions is explained in a Darwinian sense: only the theories that have latched onto the brute regularities measured in appearances, are the ones that survive.[215] Let us now discuss a concrete social study.

Jefferey Paige's *Agrarian Revolution. Social Movements and Export Agriculture in the Underdeveloped World* is a seminal and lauded cross-sectional statistical study of various economic types of agricultural systems in independent states and colonies in the underdeveloped world, and their relations to social and class relations. Let us first discuss Paige's abstract theory, its implications, and then reconstruct this - admittedly anachronistically - from a constructive empiricist vantage point. I must stress that Paige's work is of a dazzling depth; I provide merely a crude simplification of some of its aspects essential for my argument.

Paige proposes a general theory of rural class conflict which grasps the patterns of recurring conflicts in terms of interactions between the political and economic behaviour of 'cultivators' and 'noncultivators'. Generally, cultivators are the rural labourers that completely depend on labour as their critical means of survival. Cultivators come in two kinds: cultivators that receive income by rights to the land (usufruct) and cultivators that depend on wage incomes. Noncultivators, on the other hand, possess land, capital, machinery and the like as critical means of production, and also come in two classes: noncultivators that depend solely upon land ownership for income and noncultivators who receive returns from investing commercial

[213] Van Fraassen, *The Scientific Image,* 69, 72.
[214] Van Fraassen, "Representation,"537.
[215] Van Fraassen, *The Scientific Image,* 40.

capital.[216] In different agricultural systems, combinations of these different kinds of cultivators and noncultivators may be discerned. Paige's abstract theoretical structure about these configurations is as follows.

Noncultivators that depend on landed property for income are vulnerable and economically weak and rely on political restrictions to land ownership. This tends to focus conflict on distribution of landed property. However, noncultivators of this kind depend on (semi)servile labour and cannot allow economic or political rights to their cultivators. Conflicts are usually politicized. Usually it is hard to achieve compromise in economic conflicts.[217]

Noncultivators that depend for income on capital investments in industry or plantations are economically strong and require less political protection, so conflict tends to be focused on distribution of income from property and not property itself. They are usually dependent on free labour so can tolerate more economic and political rights for their cultivators. Labour conflicts tend to be economic, compromise is possible.[218]

Cultivators that solely depend on land for income (e.g. usufruct subsistence) tend to avoid risks and revolutionary political movements. They tend to be less politically organized and more isolated from other land-dependent cultivators, since they depend completely on the noncultivator's land. There is no pressure for social solidarity.[219]

Cultivators that depend on wages tend to accept risks and are more susceptible to revolutionary appeals. The incentive for political organisation is strong. There is a greater structural interdependence of wage-dependent cultivators and stronger pressure for political solidarity.[220]

Paige's theory sees social movements as the result of various types of economical agricultural organisation. The combinations of various sources of income lead to particular forms of social movements, which in his model of rural class conflict are summarized in the following hypotheses. [221]

A combination of noncultivators and cultivators that both depend on land leads

[216] Jefferey Paige, *Agrarian Revolution. Social Movements and Export Agriculture in the Underdeveloped World* (New York: The Free Press, 1975), 10-11.
[217] Paige, *Agrarian Revolution,* 18, 21, 23.
[218] Paige, *Agrarian Revolution,* 18, 21, 23.
[219] Paige, *Agrarian Revolution,* 26, 30, 34.
[220] Paige, *Agrarian Revolution,* 26, 30, 34.
[221] Paige, *Agrarian Revolution,* 70-71.

to an agrarian revolt; agrarian revolts are mainly directed at the redistribution of land but carry no further political or economic agenda. This is the ‘commercial hacienda’ type.

A combination of noncultivators dependent on income from capital investment and cultivators dependent on land leads to a reform commodity movement, limited economic protest, which aims to reform the control of the market; there is no pressure for redistribution of property nor a seizure of state power. This is the ‘small holding’ type.

A combination of noncultivators dependent on capital investment and cultivators dependent on wages lead to reform labour movements; it is mainly directed towards limited economic demands of better working conditions and higher wages. This is the ‘plantation’ type.

A combination of noncultivators dependent on land and cultivators dependent on wages leads to revolution. This movement enforces the redistribution of landed property through seizures of the state. This is the ‘sharecropping’ or ‘migratory labour’ type.

Now, from a constructive empiricist viewpoint, the question is how this highly abstract theoretical structure is interpreted in terms of a model, and how it is related to the appearances. For the sake of brevity I can only provide some partial analysis of Paige's study here as illustration.

I understand the mathematical surface model basically as the statistical correlation model of types of agricultural organisation with the frequency of rural protests. In this model a particular structure in the data is hypothesized - a structure mirroring the structure of the theory explained above – that must be borne out by the data. The variable of agricultural organization, for example, is built from aggregated and smoothed-out data or ‘scores’ on enterprise ownership, the use of power-driven machinery, and the kind of labour (e.g. wage, usufruct use, migratory, owning family). In principle these data are theory-laden measurements of regularities in human behaviour in the work-place, and they are aggregated so as to constitute particular types of agricultural organisation in the statistical correlation model. For example, 'measuring' ownership meant identifying the group or individual that controlled the land and the crops cultivated for at least 20 years. The kind of labour was ‘measured’ by observing whether those individuals performing the actual cultivation and physical work (labourers) were paid in cash, whether they were given rights to yields from a small part of the land (usufruct use). The degree of machinery in use was determined by 'measuring' whether the labourers in question had specialized power machinery like roller gins, centrifuges and mills at their disposal or not. Now some idealizations apply to these variables, for example, when more than one kind of labour was employed in an enterprise, the source that contributed the largest share of the labour was the labour kind used in the study.[222]

An example of a resulting type of agricultural organisation is the 'commercial manor' or 'hacienda': it is an enterprise owned by an individual which lacks power-driven machinery and is worked by usufructuaries, wage labourers dependent on nearby subsistence plots or on the residence itself. Another example is the 'plantation': this is an enterprise controlled by a commercial organisation or governmental body, or by an individual if the enterprise at issue employs power-driven machinery, and worked by wage workers resident for continuous terms.[223] Idealization applies in the sense that Paige deems it unlikely that combinations of types of agricultural organisations exist, since in economic terms the types are mutually exclusive, although three systems (cotton in Sudan, sugar in Indonesia, cotton in Mozambique) do not fit his typology.

Let us continue with the other variable, 'rural protest' or 'social movement'. Paige here relies on data of 'incidences of collective violence', aggregated in secondary newspaper archives, which are often reckoned by social scientists to be 'behavioural evidence' of such an abstract thing as a 'social movement'. While they may be envisaged as a theory-laden measurement of some unobservable entity, in our view the data obtained need not be understood as constituting evidence for any such thing existing beyond our data. They are just interpretations of observable behavioural regularities.

The data obtained through newspaper archives were smoothed out in the sense that the events detailed in the newspapers were considered social movements only if they were collective (counted more than ten persons), noninstitutional (illegal or not sanctioned by any official body), and involved solidary groups (groups with some sense of shared identity).[224] Now in addition, Paige established on the basis of the newspaper data a typology of these movements in terms of their qualitative characteristics.

[222] Paige, *Agrarian Revolution,* 78-79.
[223] Paige, *Agrarian Revolution,* 78-79.
[224] Paige, *Agrarian Revolution,* 86-92.

Characteristics include the organisation, tactics, actions and ideology of the participants. Ideology, for example, was determined by the specific demands of the individuals involved in the collective action, and the party ideology with which they were affiliated. Actions, for example, were determined by whether strikes, riots, warfare, and land seizures were the case. On the basis of these characteristics, the incidences of collective violence were categorized into five types of social movements. An example of resulting types is the 'agrarian event', in which participants demand expropriation and redistribution of land but do not make radical economic or political demands and are not affiliated with revolutionary parties. Another example is the 'labour event,' in which those involved in the action demand better working conditions, higher wages, or the right to organize, but do not demand land redistribution, nationalization or severe changes in government. This idealizing procedure yielded about 1500 events in 135 export sectors in 70 developing nations in the period from 1948 to 1970.[225]

To take stock: the theoretical model suggested a structure between particular kinds of agricultural organisation and social movement in the form of specific hypotheses; these structures should be reflected in the surface model that consists of idealized and smoothed out aggregations of data that we explained above. This is exactly what was the case. To stick with the examples just discussed, the 'commercial hacienda' is correlated positively and significantly with the 'agrarian event'. The 'plantation' is correlated positively and significantly with the 'labour event'. All other correlations were predicted to be negative or weak, and the statistical model indeed shows negative low correlations between the types of social movements and negative low correlations between the types of agricultural organisation, indicating that in the data the two constructs are more or less independent entities. Events seem to be randomly distributed across all agricultural organisational systems except for those systems they were hypothesized to be significantly related to. [226]

Now the scope of this study is interesting, since it includes many different countries from different parts of the world. The abstract theory and its statistical correlation model appear to describe the data correctly; their concrete hypotheses are empirically adequate. Empirical adequacy means true not about observable phenomena but true about the phenomena measured, that is, true about the

[225] Paige, *Agrarian Revolution,* 72, 92-100.
[226] Paige, *Agrarian Revolution,* 104-06.

(admittedly idealized and smoothed-out) data in the surface model. Paige's statistical surface model as submodel of his general theoretical conjectures about the relation between agricultural organisation and social movements is a good description of the data.

Yet, the root of Paige's work is the observable phenomena; it is the regular behaviour of individuals we see in the behaviour of people, whether they display behaviour that would generally be interpreted as 'incidences of collective violence', 'social movements,' a particular kind of 'agricultural organisation,' and the like. In my opinion, Paige's abstract theory and its interpretations in terms of submodels did allow prediction since they held cross-sectionally. It is part of the definition of empirical adequacy that when we believe that a theory is empirically adequate, we expect it to hold in the past, present, and future. Paige's theory predicted structures in a large data set, which turned out to be true of the data. The theoretical model was adequate in many different contexts, in countries from Asia to Latin-America, over a period of 22 years. Unfortunately it is not always possible to test the predictions of a social theory in the future, since the data needed to evaluate predictions are not easily obtained and transformed into surface models. Often we can only check whether a social theory predicted in retrospect, or whether it is empirically adequate in other previously unexamined fields.

The remarkable scope and empirical adequacy of Paige's study was taken by Harold Kincaid as an argument for both the possibility and actuality of ceteris paribus laws in the social sciences. A ceteris paribus law is a law that is strictly speaking inapplicable to the concrete world, for example since it presumes that only one cause is operating on some object, which is implausible. Hence it is applicable to the world only with ceteris paribus qualification(s): 'other things being equal' (see section 4.1 for an example). According to Kincaid, Paige disclosed a law that ties the specific economic and political organisation of different types of agrarian classes to specific social and political movements. Paige was able to derive and confirm numerous predictions from this law together with some auxiliary assumptions.

Kincaid shows that there are many ceteris paribus clauses that apply to this law. For example, whether cultivators dependent on wage will end up in revolutionary movements will also depend on whether production is centralized or decentralized. Still, this is a ceteris paribus clause that was dealt with adequately by Paige, according to Kincaid, because he subdivided his units of analysis according to their degree of centralization and showed that controlling for centralization renders the correlations between income sources and political behaviour becomes even stronger.[227] Another ceteris paribus qualification Paige countenanced is the influence of local socialist or reformist political parties on agrarian movements. Paige added them ad hoc to his model of commercial haciendas and agrarian movements. He found that combining the effects of the specific agricultural organisation of the commercial hacienda and political parties in the statistical model rendered the correlation with agrarian events even stronger, thereby refining his model. What is more, political parties in themselves had only a trivial and insignificant effect on agrarian events, so they served as a control factor in the model as well. [228] It is interesting to remark here that counterfactual or modal talk in Paige's surface model is even a possibility, for the statement 'The agrarian event in this export sector would have been less likely if no local socialist and reformist political parties were present' makes sense in the context of the surface model.

In Kincaid's view natural science suffers the same ceteris paribus predicament, which he illustrates with an example from empirical evolutionary biology. He discusses the work of Peter Grant, who conducted a longitudinal study of finch evolution on the Galapagos islands. He developed a detailed evolutionary picture of beaks adapted to seed sizes, active selection among some populations which favoured large beak and body size, and the availability of seeds as constraint on population size. However, Grant's work was subject to many ceteris paribus qualifications. For example, he acknowledged the role of sexual selection but was unable to assess its specific contribution; the same applied for mutation and gene flow. Disease and predation were other complicating factors, but the only thing Grant could do was giving some reasons based on field experience why they were probably insignificant factors. Kincaid does not intend to challenge the quality of empirical work done in evolutionary biology, but shows that Paige's method of adding clauses and trying to control them is comparable to work done in this branch of natural science.[229]

I think that Kincaid is right in pointing out the similarities between generalizations in the natural sciences and generalizations in the social sciences, especially their 'hedged' natures. In any case we would like to cash out the conjectures

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[227] Harold Kincaid, "Confirmation, Complexity and Social Laws," in *Proceedings of the Biennial Meeting of the Philosophy of Science Association* 2, (1988): 302-304.
[228] Paige, *Agrarian Revolutions,* 110-11.
[229] Kincaid, "Confirmation," 304-305.

of the two cultures in terms of abstract theory and their surface models as submodels. Yet, the existence of empirically adequate models can be no argument for the 'possibility and actuality' of ceteris paribus laws in the social sciences. Furthermore, there is no other option for these models than to just be 'mere' generalizations instead of 'real' laws, for the constructive empiricist is unwilling to appeal to a 'nomic force' that separates 'real generalizations' from the 'accidental ones' in terms of some physical necessity conferred on their empirical adequacy. Kincaid seems to think of a generalization as employing some kind of nomic force, instead of just a systematic summary of regularities in the phenomena.[230] But a generalization such as Paige's is acceptable if it is empirically adequate of the surface model; there is nothing beyond the observable world that confers necessity or likeliness on this description. The upshot of this section is that description and prediction in a constructive empiricist view of social science are very well possible. They are possible insofar some abstract theory is empirically adequate of the representations of observable phenomena in some surface model.

Peter Winch writes in *The Idea of a Social Science* that the idea that social phenomena can be predicted by social science makes no sense at all. The concepts central to our understanding of social life are incompatible with the possibility of scientific prediction. Even if we had a specific set of initial conditions, we can still not predict the outcome of any historical trend, because human decisions cannot be sensibly regarded as things that are determined by antecedent conditions. The idea of a decision is that one may 'do *X* but also *not-X*', which bars any form of prediction.[231]

While the latter claim seems apt of humans, it is manifestly not the standard assumption employed in the bulk of the social sciences. In the study by Paige discussed above, there is, as Kincaid notes, a kind of rationalist assumption at work: cultivators are well aware of their material conditions in terms of sources of income, which significantly shape their decisions to engage in social movements. Paige actually made concrete predictions about what humans will do given some antecedent conditions, which were empirically adequate in many different circumstances, on just that rationalist assumption.[232]

I think that contrary to what Winch supposes, at least some part of our concepts

[230] Kincaid, "Confirmation," 305-306.
[231] Peter Winch, *The Idea of a Social Science and its Relation to Philosophy* (London: Routledge, 1990), 91-94.
[232] Kincaid, "Confirmation," 304.

central to understanding social life actually encompass the idea that behaviour is determined by antecedent conditions. This is an innocuous assumption for the constructive empiricist. Ultimately the question whether behaviour is or is not ultimately 'determined', is not too important for the scientific enterprise. It may well be an assumption in some model, actually quite a useful one, but we are not in any problematic sense committed to its truth. I think that we employ the assumptions in our everyday lives as well, since it allows us to – to some extent – predict the behaviour of other individuals quite adequately.

As I have said earlier in footnote 167, I am in favour of the Quinean idea that science is the continuation of common sense, albeit in a more systematic and careful way. Science does better at prediction than common sense knowledge, but ultimately, it is experience that is the starting point for knowledge about the world.[233] With Van Fraassen, I hold that we should adopt a modest epistemic attitude towards that which is not observed. That does not mean that the assumption that human behaviour is determined by antecedent conditions is meaningless, metaphorical, or an assumption that is ultimately false. It is simply something that we will probably never know. But it is still an assumption that in the mundane social world is often not problematic or explicit at all. We commonly believe that behaviour of others is shaped by antecedent conditions since that is convenient in the social interactions in which we engage. I suppose that the assumption that behaviour depends on antecedent conditions, a conception of 'determinism' if you will, can very well be part of the concepts central to social life.

5.4 Explanation in natural and social science

Cartwright's solution to the problem that generalizations are context-sensitive (such as the law of universal generalization) is to postulate that the causal principles of an object are powers. Massive bodies have in principle the power to cause a force of a particular size but whether they do so depends on the context: on which other powers are at work, for example.[234] Cartwright believes that employing causal relations in a generalization provides a warrant to believe in the entities at issue. Moreover, the implication is that it renders one an entity-realist, on pains of being unable to make

[233] Willard V.O. Quine "The Scope and the Language of Science," in *The British Journal for the Philosophy of Science* 8, no. 29 (May 1957): 2.
[234] Cartwright, *How the Laws of Physics Lie,* 61.

sense of scientific explanation: she maintains that 'causal explanations have truth built in them' and that 'existence is an internal characteristic of causal claims'.[235]

In my view, this is a statement of a 'default' position that reinvigorates exactly the relation between explanation and description which the constructive empiricist discarded. Cartwright supposes that only if we assume that a concrete particular particle caused a vapour trail in a cloud chamber, our explanations make sense, since it 'brings about, causes, makes, produces, that very track'.[236] This view of explanation supposes that a causal chain necessitates the existence of a particular entity, that in turn renders an explanation true. However, we had many such entities in the history of science that were assumed to have particular causal properties, the existence of which was inferred on that basis, entities that even yielded correct predictions, but were later discarded anyway, because some other theory employing other entities proved empirically more adequate. The phlogiston theory of combustion and the caloric theory of heat are famous examples.[237]

Another reason for cutting the link between description and explanation is the argument that there are always many mutually inconsistent explanations of some observable regularity conceivable. This argument supposes that many inconsistent explanations are potentially equally capable of causing, rendering necessary or conferring likeliness on some regularity. This possibility is harmful for scientific entity realists (like Cartwright) who believe that a scientific theory aims to give us a truthful picture not only of the observable world, but also of the unobservable entities it postulates, for many mutually inconsistent unobservable entities may figure in different explanations of the same regularity. Constructive empiricists can hold multiple mutually inconsistent explanations of some observable regularity at the same time, hence, can accept theories that include different unobservable entities at the same time, since they are not committed to believe all these explanations to be true so there is no logical conflict. Realists like Cartwright are actually committed to believe that explanations are true, hence, risk believing in multiple mutually inconsistent unobservable entities at the same time.

A more logical point raised by Van Fraassen is that almost all claims that ask us to infer the truth of explanations, ask us to affirm the antecedent in the form '*P*

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[235] Cartwright, *How the Laws of Physics Lie,* 91-93.
[236] Cartwright, *How the Laws of Physics Lie,* 2.
[237] Laudan, "A refutation," 33.

explains *Q*, *Q* is true, so *P* is true as well'. However, from a logical point of view, this conclusion is invalid.

The philosopher Peter T. Manicas argues that in both the natural and social sciences, explaining phenomena consists in disclosing the mechanism or 'causal nexus' that produces the phenomenon of interest.[238] Causal mechanisms are 'productive powers': constellations of entities that bring about some change or state of affairs in the world.[239] In the natural sciences we may think of some molecular mechanism that describes the behaviour of fluids; the analogue for the social sciences is a social mechanism that consists of conscious individuals and their behaviour that produces social phenomena.[240]In science we often presume mechanisms to be 'closed systems', since we usually considering only a limited number of causes in mechanisms. Yet, in actual fact it is virtually impossible to know and control all causes for some phenomenon, hence, outcomes generated by mechanisms are to some extent contingent. This predicament is true for both the natural and the social sciences.[241]

The role causal powers occupy in Manicas' argument is similar to the role they have in Cartwright's argument. Cartwright asks us by appealing to causal powers to believe in the unobservable entities in physics that exhibit these powers. Manicas asks us to take the individual and the individual's behaviour as the fundamental unit of explanation, since behaviour causally produce social phenomena.[242] He believes that society is not some entity over and beyond individual behaviours but is only 'incarnate' in behaviour (we discuss this in more detail later).[243]

Still, behaviours are in turn produced causally by the individual's consciousness and 'intentional states' such as reasons, beliefs, desires, and intentions, and are explained in these terms.[244] Manicas is a realist about these intentional states in the sense that he thinks that it is 'next to impossible' to deny that humans possess them.[245] So he slides from being a realist about humans and behaviour, as point of departure for social science, to being a realist about reasons, beliefs, desires, and intentions.

There are two comments I would like to make. First of all, it is curious that

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[238] Peter T. Manicas, *A Realist Philosophy of Social Science. Explanation and Understanding* (New York: Cambridge University Press, 2006), 20.
[239] Manicas, *A Realist Understanding,* 16.
[240] Manicas, *A Realist Understanding,* 2, 74-75.
[241] Manicas, *A Realist Understanding,* 33-36.
[242] Manicas, *A Realist Understanding,* 75.
[243] Manicas, *A Realist Understanding,* 3, 59, 60, 61, 182.
[244] Manicas, *A Realist Understanding,* 49-50, 53, 54.
[245] Manicas, *A Realist Understanding,* 49.

Manicas takes scant notice of philosophical discussion about these intentional states. To take one example, Churchland famously argued that the pre-scientific common conception of such thing as beliefs (like Manicas' conception) is a mistaken posit in a false and outdated folk-psychological theory, like phlogiston once was in natural philosophy and is prone to replacement by neuroscientific concepts.[246] Manicas is vulnerable to this critique, since he does not provide anything but a thin common-sense description of beliefs, and a cartesian appeal to indubitability as argument.

The second comment is more significant for present purposes. Constructive empiricism has a lesson to teach (the philosophy of) social science. Being a realist about humans and observable behaviour is perfectly fine with constructive empiricism. Yet, there is no need to be committed to the existence of intentional states as an 'explanatory reality' behind the appearances and the regularities they exhibit. We may of course represent observable behaviour as if it is imbued with intentional states in our theories. In addition, statements about intentional states are in principle capable of being true, hence, are understood literally. Still, the aim of science is not to disclose the 'explanatory realities' behind the appearances and the regularities they exhibit, but to yield empirically adequate theories.[247] There *could* be but there *need* not be any such explanatory reality behind the appearances. In addition, although we couch explanation in causal terms, that does not imply that we are realists about causality as 'productive power' that is some kind of real property of things.

To be clear, this second comment does not mean that theories in social science do not explain at all. Let us briefly recall the essence of pragmatic explanation. For Van Fraassen, explanation is not a benchmark of scientific theory itself but is rather an application of scientific theory in some context.[248] An explanation is an answer to a specific request for information. What exactly is requested, usually in the form 'Why is it the case that observable state of affairs *P* obtains?', depends on the context: the interests and background knowledge of the inquirer. An inquirer may be interested in a specific causal factor leading to the event to be explained, for example, in the weather conditions leading to a plane crash but not in the human conditions in the sense of the pilot's actions.

Since different inquirers possess different interests and bodies of background

[246] Paul M. Churchland, "Eliminative Materialism and the Propositional Attitudes," in *The Journal of Philosophy* 78, no. 2 (February 1981): 67.
[247] Van Fraassen, *The Scientific Image,* 25, 31, 32.
[248] Van Fraassen, *The Scientific Image,* 156.

knowledge, a question may be valid for some inquirers but not for others. This context-dependence also applies for the evaluation of answers: the inquirer's background knowledge plus some additional data are the contextual factors that determine how good some proffered answer is for that inquirer. In principle every inquirer evaluates answers in a Bayesian way. A satisfactory explanatory answer must confer the greatest personal probability on the state of affairs implied by the question compared to other possible relevant answers; personal probabilities are calculated relative to a fixed body of background knowledge and some additional information. Since inquirers are different, what counts as a good answer for inquirer A may be a bad answer for inquirer B.

The message of this section is that explanations in the social sciences can freely exploit the language of probability, law-like generalizations, causality, and the like. That we use a certain language that employs various unobservable entities, events and processes, does not commit us to the full picture of the world it draws. Van Fraassen's sunrise example illustrates this: my statement that the sun will rise tomorrow, does not commit me to the Ptolemaic solar system.[249]

[249] Van Fraassen, *The Scientific Image,* 14.

Chapter 6. Concepts in the natural and social sciences

The next fundamental difference Kagan discusses concerns the concepts of different scientific cultures and their vocabulary. According to Kagan, natural scientists use 'semantic and mathematical concepts whose referents are the material entities of physics, chemistry, and biology, and are assumed to transcend particular settings'.[250] Social scientist, on the other hand, use 'constructs referring to psychological features, states, and behaviours of individuals or groups, with an acceptance of the constraints that the context of observation imposes on generality'.[251]

The challenges I take from these assertions are to explain how the constructive empiricist makes sense of the concepts in both natural and social science, in particular their alleged reference to particular entities and their generality. The danger is apparently that the nature of the concepts of the natural sciences, the entities they refer to, and their generality are so different from the nature of concepts in the social sciences that constructive empiricism is a fitting philosophy for the former but not for the latter. In section 6.1 I show that Kagan's idea about semantic reference and the generality of concepts in natural science is too simple. In section 6.2 I discuss social concepts as well as some philosophical approaches to address them that are vain from the perspective of constructive empiricism. In section 6.3 I give an illustration of the variety of social concepts that purport to explain more or less the same social phenomena, and in section 6.4 I consider this variety in the light of pragmatic explanation.

6.1 Concepts in the natural sciences

Van Fraassen's discussion of the nature of concepts (at least in the physical sciences) is clear enough. A concept such as 'gravity', for example, assumes a different theoretical form in a Newtonian context compared to an Einsteinian context. It is a concept that refers to some entity that may or may not exist but which we cannot observe in any case. That does not prevent us from interpreting the concept naturally and literally, in that it is a thing that potentially is true or false of the world. It is a concept that is interpreted in terms of general mathematical equations, although the equations are quite distinct in the two theoretical pictures mentioned. Gravity is

[250]Kagan, *The Three Cultures,* 4.
[251]Kagan, *The Three Cultures,* 4.

interpreted in different models that specify different structures that have to be true of the surface model if the theory is to be acceptable at all. There is the intuitive idea of a 'force' or 'curvature of spacetime' as things to which these concepts refer respectively, but in any case, I suppose that even regardless of whether one is a scientific realist or anti-realist, Kagan's claim that the referents of physico-mathematical concepts are 'material' entities is contentious. It is far from clear what exactly is meant by his use of 'material'. Do physical concepts such as 'space', 'time' or 'spacetime' refer to material things? With regard to biology, does the concept of 'natural selection' refer to anything material?

One point that Kagan overlooks is that the generality of physical concepts is to some extent always restricted, viewed from the theoretical context in which they were formulated. The kinetic conception of gases is not valid in cases of high pressures and/or low temperatures. Newton's mechanics - still in use today since it is considerably simpler than Einstein's theory of relativity and yields quite accurate predictions in our solar system - breaks down for objects moving at high velocities and/or objects that have large masses, and presumes that no other forces interfere in the system at issue. These examples concern the limits of the generality of models that scientists who are 'immersed in a certain theoretical programme' will accept.

But there are also more serious issues with regard to the generality of physical concepts identifiable from the general history and philosophy of science, in the sense that already within a single scientific discipline, those involved could not agree on the validity of some concepts, let alone on their generality, and on Kagan's idea that concepts always transcend their particular research settings.

About around the turn of the 20th century, Ernst Mach was sceptical about the validity and generality of the concept of 'atoms', which for him entailed the view that essential properties of matter are the product of the interactions of fundamental particles such as atoms or molecules.[252] He thought that in the study of nature, we only deal with the connections between appearances, and what we imagine to be behind these appearances exists only as crutches of our understanding.[253] Molecules or atoms were for him just mental tools for thought. Mach, and others like Wilhelm Ostwald and Pierre Duhem thought that a unified phenomenological description of all physical phenomena was instead possible and that phenomenological concepts should be

[252] Stephen G. Brush, "Mach and Atomism," in *Synthese* 18, no. 2/3 (April 1968): 198.
[253] Brush, "Mach and Atomism," 200.

preferred in physics.[254] For example, the concept of time was understood by Mach as observable change in events occuring in a definite direction - in other words, it consisted of mere relations between objects.

Thomas S. Kuhn captured well the differences in the scopes of concepts in his famous description of paradigms in scientific disciplines, in which analogously the concepts of one scientific subculture (in this case within physics) are deemed invalid and not applicable in different contexts by another scientific subculture.[255] One may in this context also think of the shifts in the conceptions of 'locality' or 'simultaneity' in classical mechanics, relativity theory and quantum mechanics, that are relevant and applicable only in quite different and relatively limited contexts, and assume different meanings. Problems of generality in the natural sciences aside, in the previous chapter I argued by appealing to Paige's study that there definitely are sociological theories that have some generality. Let us now focus on concepts in the social sciences.

6.2 Concepts in the social sciences

It is virtually impossible to give a characterization of the concepts of social science (even under a narrow idea of social science as comprising sociology and psychology) that is satisfying to scientists in the field and philosophers, and that in addition will cover most of its countless facets, but I shall try here nonetheless.

My quick common-sense definition of social research is that it is the business of examining social phenomena by means of scientific methods, with the epistemic goal of constructing models that are empirically adequate, and with the pragmatic goals of explanation and understanding. Social research is characterized by a variety of concepts that are best understood as social constructs. These constructs are theoretical terms, that refer to unobservable entities, processes and structures. As examples of constructs we can think of norms, interests, cohesion, religion, social pressure, social integration, socioeconomic classes, psychological disorders, personality, intelligence, and so on. By the lights of constructive empiricism, these constructs are to be understood somewhat like the concepts in natural science, such as 'electron', 'spin', 'space', 'time' and 'spacetime'; they are also abstract constructs that must receive some interpretation in a model so as to be empirically adequate. Most of

[254] Brush, "Mach and Atomism," 197, 201-02.
[255] Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: The University of Chicago Press, 1970), 10-11, 102.

the constructs of the social sciences should be understood as unobservable entities. There is a clear sense why. We do not observe norms, interests, cohesion, religion, social pressure, social integration, social economic classes, psychological disorders, personality, intelligence, and the like and it is difficult to imagine in what circumstances we as humans could observe them or whether it even makes sense to conceive of this at all. We can measure regularities in behaviour, however. We observe regular social practices, but we do not observe constructs. This goes some way towards Giddens' idea that we do not see society itself but rather observable patterns of individuals doing things.[256]

Now there are some possible issues that must be faced. Do I imply that these constructs are reducible to individuals and their behaviour? This is a thorny issue in the philosophy of social science. In a strong reductionist view, talk about norms, interests, cohesion, and so on is redundant and can be dispensed with, since these concepts are, with respect to their meaning and validity, completely exhausted by reference to observations of behaviour. This train of thought effectively originally stems from operationalism and behaviourism as its psychological offshoot.[257]

The strong reductionist position is not the path the constructive empiricist wishes to venture. What is vital for the constructive empiricist is that the language in which these concepts are couched is interpreted naturally and literally. It must not remove implications of existence or imply ontological reductions to some other language that is purportedly more 'objective' or neutral (e.g. observable behaviour), for that would amount to positivism. We are barred from holding that social constructs are only meaningful through their connection with the observable world.[258] If, for example, a social study discusses the concept of 'norms' as a tacit system of social rules (e.g. help persons in public that ask for help) that sanctions behaviour by punishments and rewards in social interactions, then the question is whether this abstract theory implies structure that is empirically adequate of the data which are smoothed-out in a

[256] Manicas, *A Realist Philosophy,* 58,59.
[257] Operationalism is the view that 'the meaning of a concept is synonymous with the corresponding set of operations' or measurements. E.g. the meaning of the concept of length is just the act of measuring. See Percy W. Bridgman, *The Logic of Modern Physics* (New York: Macmillan, 1927) 5, 28. Bridgman thought that operationalism could be fruitful in other fields of inquiry as well (pp. 30-32). Various psychologists embraced operationalism as a modern and acceptable form of positivism, contra psychological introspection. See Malia L. Walter, *Science and Cultural Crisis: An Intellectual Biography of Percy Williams Bridgman (1882-1961)* (Stanford: Stanford University Press, 1990), 178-80.
[258] Van Fraassen, *The Scientific Image,* 10,11.

surface model. 'Norms' in this model are not intended as a metaphor, neither as a 'fiction', nor as a mere instrument, but on the contrary are couched in a language that respects the point of departure that the entity is 'capable of being true or false'. A norm is an entity that in the exemplary model may well be intelligible and meaningful in some scientific, but also more mundane contexts, since for some scientists and laymen this may fit the (scientific) background knowledge of the groups quite well. For some scientists and laymen, a model can on this count 'explain' behaviour.

There is also a weaker form of reductionism that is potentially applicable to my account. The earlier discussed 'realist' philosophy of Manicas provides an example. He ascribes to what he calls the 'half-truth' of methodological individualism - which is the Weberian doctrine that social phenomena must be explained in terms of individual actions, which are in turn explained by the individual's intentional states.[259] According to Manicas, the regularities in the social realm (social phenomena) are the products of 'generative mechanisms', which are social structures that shape actions. These social structures, however, have no independent but rather a 'virtual' existence in the actions of individuals.[260] Consider an illustration: if someone grows up in a country in which English is the main language, probably English will be that person's native language and not some other possible language. The social structure that enables and constrains that person's behaviour consists of the behaviours of others: the person's parents, siblings, teachers, and friends speaking and teaching English, thereby enabling that person to learn and speak English, and at the same time restricting that person's opportunity to become a native speaker of some other language.[261] The social structure at work here consists solely of behaviours of individuals and is no entity, event, or process with causal powers over and above individuals.

Hence Manicas' theory aims to be a way out of the problem of the opposition between structuralists and reductionists. He attempts to pick out the fruitful aspects of both doctrines. The structuralists erected social structures that causally shaped behaviour. Emile Durkheim is arguably one proponent of this kind of theory. One vital property of Durkheimian social facts, as we have seen, is that they are general ideas that exert influence over many individuals' lives in a society. Since they do so they cannot be reduced to organic or physical entities, but rather enjoy an independent

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[259] Max Weber. *Economy and Society. An Outline of Interpretative Sociology*, ed. Guenther Roth and Claus Wittich, trans. Ephraim Fischhoff et al. (Los Angeles: University of California Press, 1978), 13-14.
[260] Manicas, *A Realist Philosophy,* 61, 84-85.
[261] Manicas, *A Realist Philosophy,* 60.

existence as a constraining 'force' in a society.[262] This idea of something that causally shapes behaviour is intuitively compelling, hence Manicas' statement that 'we want to preserve the Durkheimian insight that society influences behaviour'.[263]

However, social facts are things over and beyond individuals *because* they have causal powers; an unpalatable risk implicit in this view is that social facts or social structure are completely beyond control which implies a one-way causal relation and loss of individual agency.[264] As a result, Manicas advocates a third way between the two poles, by holding that social structure or social facts are grounded in individual behaviour and are 'virtual', which means that social structure is not real over and beyond humans, but is only incarnate in their actions and interactions.[265] Social structures and social forces do not exactly cause behaviour, since that would render social structure an independent entity – thereby violating the virtuality requirement - but only 'enable' and 'constrain' it.[266] We should say something about this moderate reductionist view and what the constructive empiricist's remarks are.

With regard to this second, weaker variant of reductionism, it is definitely instructive to see how intermediate positions between social structuralism and reductionism have been formulated, yet these are mainly theoretical issues. The main question underlying them is what is ultimately responsible for the regularities we observe in the social world. But for the constructs the constructive empiricist social scientist brings to the table, there is really no issue of whether social structures or virtual structures, or even rational choice theory or profit maximization are the accounts that explain how we come to have the social regularities we observe, if the models developed from these conceptions are empirically adequate. The constructive empiricist does not assert that our concepts as explanations have any reality or bearing upon the observable regularities in order to be acceptable as social science; they just have to fit the surface model in order to be acceptable at all. Many different conceptual schemes and models of the same regularities are possible. Theories employing different conceptual schemes can even be mutually inconsistent. Yet, mutually inconsistent theories that bring distinct concepts - in the sense of distinct unobservable entities, processes and mechanisms - to the table can still be empirically

[262] Durkheim, *Suicide,* xxxvi-xxxvii, 59, 60-83.
[263] Manicas, *A Realist Philosophy,* 60.
[264] Manicas, *A Realist Philosophy,* 73-74.
[265] Manicas, *A Realist Philosophy*, 82.
[266] Manicas, *A Realist Philosophy,* 115.

adequate. Concepts in the natural sciences need not explain observable regularities, concepts in the social sciences need not either.

We said that regularities are presupposed and need not be explained by any concept in a constructive empiricist view of natural science. That is no different for the social sciences. This is quite counterintuitive, since we are inclined to think that the explanations we have for the social regularities we experience every day, go some way towards telling why the regularities we observe obtain.

According to a scholar like Andrew Sawyer, we explain social regularities causally since these regularities depend on internally stable objects that are invariant in some conditions. Accounts that involve causality usually end up in a metaphysical argument, that, just like Sawyer, attempt to demonstrate that some special entities in the social world have causal powers, that are sometimes efficacious in yielding a particular regularity.[267] This account is in essence no different from Cartwright's causal powers account. Both are attempts to demonstrate how a particular concept necessitates or makes more probably a correct description of the world. But as we have seen, no such assumption is necessary for the constructive empiricist to believe, for we already assumed regularities to be brute and autonomous in the first place.

6.3 Concepts in constructive empiricism: an implication

I have tried to show in the previous two sections that with regard to concepts, all that matters for the constructive empiricist is that they receive interpretations in models and that the observable consequences they entail are true of the surface models. This strategy is essentially no different for the social and natural sciences. There are other pragmatic - but not epistemic - reasons why concepts are, in the orthodox view, regarded as good explanations and as providing understanding.

Now this kind of 'deflationary' take on concepts is useful in an interesting way. We see a bewildering variety of concepts and their interpretations in models already in the social sciences themselves. For the orthodox view of concepts and their role this is a potential source of puzzlement. Many conceptual approaches in the social sciences purport to explain more or less the same phenomena, and naturally scholars argue that the nature of the concepts they bring to the table warrants an explanation in the

[267] Andrew Sawyer, "Reductionism in Social Science," in *Questioning nineteenth century assumptions about knowledge, II: reductionism,* ed. Richard E. Lee (New York: State University of New York Press, 2010), 9-10.

theoretical terms they postulate instead of some other alternative approach, like Durkheim did with a social explanation of suicide instead of an individual, psychological explanation. How to deal with many distinct explanations of the same phenomenon? These different conceptual approaches model the social regularities in different ways. Possibilities to do so abound since many regularities can be found in the social world. I maintain that there is no single correct or best way to explain these social phenomena from a constructive empiricist viewpoint of social science. There are only explanations that are more relevant and fitting in one context of inquiry compared to another. This is a claim I alluded to earlier, but that I will consider now with reference to some concrete examples of different conceptual explanations of the social phenomenon of suicide.

Durkheim famously thought that each society has a kind of propensity or 'definite aptitude' to cause suicide among members, which can basically be analysed and explained from the examination of demographic data. The impact of the society's propensity or predisposition towards suicide could be read off from the proportion of suicides in the total population.[268] Durkheim understood suicide as applicable to all cases of death 'resulting directly or indirectly from a positive or negative act of the victim himself, which he knows will produce this result'.[269] Suicide was for Durkheim a social phenomenon that manifested itself in observable, individual cases, which led to observable regularities in official demographic data. Since it was a social phenomenon it was susceptible to a social explanation that could not be reduced to individual, psychological characteristics of the individuals in a society, such as insanity.[270]

Durkheim constructed what we could - admittedly anachronistically - describe as a 'model' that employed various assumptions and empirical consequences that ultimately were to establish that his concept of 'social integration' - a Durkheimian 'social fact' - could explain suicide rates in a particular society. His assumption that suicide rates were not the result of individual anomalous psychologies but rather cohered with social activities, was borne out by the empirical data models that showed that occurrences of suicide were parallel to what he thought were moments where social 'collective' life was most active peaked. Suicides were more common on

[268] Durkheim, *Suicide,* xiv.
[269] Durkheim, *Suicide*, xlii.
[270] Durkheim, *Suicide*, 23-24.

weekdays than weekends, for example, and more common in the morning and afternoon than around midday.[271]

More importantly, his main concept of 'social integration' was a kind of mechanism that understood integration as the degree to which a community had shared beliefs and practices, in short, a collective credo. For Durkheim, Protestants where a less socially integrated community compared to Catholics, since the latter was a more organized, hierarchical community with a more shared orthodox acceptance of common beliefs and practices.[272] Jews were even more integrated in terms of a collective credo, due to historical hostility towards them, among other reasons.[273] This model implied obvious empirical consequences and indeed, in various societies under consideration (Bavaria, Italy, Austria, France, Switzerland, Spain, Prussia) suicide rates among Catholics were lower compared to Protestants, but higher compared to Jews who had the lowest rates.[274]

Durkheim 'hedged' his model step by step by adding assumptions that are reflected in the empirical substructures. For example that, since Switzerland had both German and French speaking areas wherein largely Catholic and Protestant enclaves existed, and since suicides were still proportional to the number of Protestants and inversely proportional to the number of Catholics, effects of nationality or race could be neglected, while the effect of religion on suicide still held.[275] These are just two examples of structure following from Durkheim's 'models'; he hypothesized 'social integration' to be relevant in small societies such as the family as well.[276]

The upshot of Durkheim's explanation of suicide was therefore that as a society or the groups within a society disintegrate and social ties become weaker, the individual becomes less dependent on the shared values of a group and more on his private interests, which may ultimately result in self-inflicted death.[277] The concept of social integration clearly specified some empirical consequences, and the theory was empirically adequate for the data. Many different correlations in demographic data

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[271] Durkheim, *Suicide*, 67-68.
[272] Durkheim, *Suicide*, 111-112.
[273] Durkheim, *Suicide*, 114.
[274] Durkheim, *Suicide,* 105-25.
[275] Durkheim, *Suicide,* 107-08.
[276] Durkheim, *Suicide,* 126-74.
[277] I am well aware of the simplified presentation of Durkheim's theory I provide here. I omit the difference between social integration and social regulation and their relations with different kinds of suicide (altruistic, fatalistic, anomic, egoistic) here for the sake of brevity. Durkheim himself thought for example, that a high level of social integration can lead to 'altruistic' suicide, as may be the case among followers of a sect or kamikaze pilots who literally 'sacrifice' themselves.

were appealed to by Durkheim, in order to argue that suicide varies inversely with the concept of 'social integration'. Durkheim did not appeal to advanced statistical operations since there were simply no multivariate analyses or correlation models available yet; he simply used data models in the form of official demographics.

This kind of research, which appeals to 'social' concepts as explanations of observed regularities of suicide, is still prevalent today, albeit with modifications to the concept of social integration and with different models and empirical consequences. Some examples are studies that attempt to demonstrate that those who die by suicide experienced social isolation before their death, and studies that attempt to demonstrate that those adolescents with a history of suicide-attempts usually do not seek support in their social environments, and did not share their thoughts during their period of contemplating suicide. There are also studies that attempt to demonstrate that the presence of social networks is a protective factor against suicide.[278]

At the same time, there are also other conceptual approaches that purport to explain suicide but now with concepts that are more directed towards the psychology of the individual instead of the social. To provide an illustration, Edwin Shneidman famously suggested that we should conceptualize suicide in terms of 'psychaches'. Psychaches are intense and unbearable emotional pains that are different from mental states as depression and hopelessness. Factors leading to psychaches are psychological needs that are frustrated, and are among others, a sense of control, love and belonging, meaningful relationships, and a positive self-image. The individual seeks relief for these pains and often there is only an extreme option left, namely self-inflicted death, that will halt consciousness and therefore stop the experience of psychological pain.[279] The underlying idea that a suicidal act is an instrumental act to fulfil a need is rooted in 'psychodynamic' theories of suicide, which contend that suicide is an instrument to enhance self-worth, join a loved one, or gain love.[280]

Talia Troister et al. attempted an evaluation of Shneidman's abstract theoretical conjecture that suicide is caused by psychache and that other psychological factors, such as depression and hopelessness, are only relevant for suicide insofar they relate

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[278] For an overview, see Thomas E. Joiner, Jessica S. Brown and Laricka R. Wingate, "The Psychology and Neurobiology of Suicidal Behaviour," in *Annual Review of Psychology,* 56 (February 2005).
[279] Edwin S. Shneidman. *Suicide as Psychache: A Clinical Approach to Self-Destructive Behavior* (Lanham: Rowman & Littlefield Publishers, 1993), 1-15.
[280] Shila Barzilay and Alan Apter, "Psychological Models of Suicide," in *Archives of Suicide Research* 18, no. 4 (February 2014): 297-98.

to psychache. Their study is special because it is longitudinal in the sense that it covers two analyses separated by five months. In addition, it employs a normal group and a suicide risk-group, to enable a quasi-experimental comparison of the significance of the concept of 'psychache' for groups at different suicide risk-levels (those at risk had a history of suicide attempt or endorsed an active or passive suicide desire). The study involved 683 'normal' undergraduates and 262 'high-risk' undergraduates.[281] Troister et al. obviously did not rely on demographical statistics since they took themselves to be measuring psychological states, so they could not focus on completed suicide, but focused on suicidal behaviour. They think of suicidal behaviour in three dimensions: suicide ideation, suicide motivation and suicide preparation. The first refers to the extent to which suicide is ideated, the second refers to the degree to which one is ambiguous about living or dying, and the third refers to the planning of the suicidal act.

These constructs are measured by scales on which subjects report scores. The same procedure applies for the constructs psychache, hopelessness and depression. These 'appearances' were aggregated in data models or variables. What the scientist takes herself to be doing in this situation is analogous to what we commonly do when we observe something and infer unobservable entities, but in this case, we infer carefully defined scientific constructs. We are theoretically-laden (from Shneidman's perspective) measuring phenomena; we obtain data that we take as evidence for some unobservable entity that figures in our theory, but is also supposed to be part of the world. Measurement in this particular study is indeed quite indirect, but ultimately it is the quantified precipitation of some observable behaviour we understand as suicidal behaviour. Still, indirect measurements of some alleged entity are no problem in science, as the case of the solar-neutrino experiments purported to demonstrate. Be that as it may, in any case we are not ontologically committed to a realist interpretation of any construct or concept.

The structure hypothesized to be revealed in the statistical surface model was as follows. The first hypothesis was that psychache scores correlate significantly with suicidal behaviour scores. The second hypothesis was that this also holds when controlling for hopelessness and depression. The third hypothesis was that follow-up

[281] Talia Troister et al. "A Five-Month Longitudinal Study of Psychache and Suicide Ideation: Replication in General and High-Risk University Students," in *Suicide and Life-Threatening Behavior* 43, no. 6 (December 2013): 611-612.

scores on psychache and suicidal behaviour scores correlate significantly when baseline scores on psychache and suicide were statistically covaried out. The fourth hypothesis was that follow-up scores on psychache and suicidal behaviour correlate significantly when follow-up scores on hopelessness, depression, baseline scores on psychache, suicidal behaviour, depression and hopelessness are statistically controlled. The fifth hypothesis was that these findings are consistent for all risk-levels.[282] The constructed statistical surface model smooths out and idealizes the data to some extent. For example, regarding the testing of the third, fourth and fifth hypothesis, since the data in the data models (constructed variables) were not distributed randomly, a bootstrap procedure was applied that estimated sample parameters by drawing random samples repeatedly (10.000 times) from the sample itself. This is a common procedure in case of small samples or samples with skewed distributions.

The structure hypothesized was similar to the structure of the surface model, with one small exception for the last hypothesis. Psychache was, for both risk levels, positively significantly correlated with all dimensions of suicidal behaviour, and this was also the case for the follow-up analysis. These results remained robust when controlling for depression and hopelessness. The first two hypotheses were confirmed. In order to test the third hypothesis, the follow-up scores on suicidal behaviour were regressed in a regression model on the follow-up scores on psychache, while covarying out the corresponding baseline scores. The obtained standardized regression coefficients were tested for significance. For both risk levels, change in psychache was significantly related to change in all dimensions of suicidal behaviour, thereby confirming hypothesis three. The procedure to test the fourth hypothesis was again regressing the follow-up scores of suicidal behaviours on the follow-up scores of psychache, while statistically covarying out the follow-up scores on hopelessness, depression, and all corresponding baseline scores. The obtained standardized regression coefficients were tested for significance. The last hypothesized structure was partially borne out by the data in that indeed, change in suicidal behaviour was significantly related to change in psychache except for the motivational dimension in suicide. This was similar for both risk levels.[283]

Shneidman hypothesized some abstract structures in his theory; conceptually,

[282] Troister et al., “A Five-Month Longitudinal Study,” 612-613.
[283] Troister et al., “A Five-Month Longitudinal Study,” 614-617.

that psychache is an essential component of suicide, is not a psychiatric disorder – perhaps like depression or hopelessness is – but an escape from unbearable pain caused by the frustration of needs. The surface model exhibited the structures of the abstract theory to a large extent, showing that indeed, psychache is an essential component of suicidal behaviour even when other 'competing' factors were present, thereby attempting to distinguishing the construct of psychache from other competing constructs. The model was empirically robust over five months and for groups associated with different risk levels, with the exception of the motivational dimension of suicide in the fourth hypothesis. One important limitation, among others, is that Shneidman thought that psychache was the psychological determinant of suicide par excellence; he held that other factors are only relevant for suicide in their connection with psychache. In the study by Troister, this is not hypothesized. This would amount to the claim that psychache in the surface model is the only factor significantly correlated with and significantly predicting (changes in) suicidal behaviour. The surface model rather indicates that changes in depression and hopelessness are also significantly predicting changes in suicidal behaviour.[284]

Is this a problem for constructive empiricism? Many competing theoretical models employing different constructs are possibly empirically adequate. A theory that competes with Shneidman's, for example by hypothesising that the relation between depression and suicidal behaviour is fundamental instead of the relation between psychache and suicidal behaviour, can be empirically adequate for the same surface model. This points to the fact that theoretically speaking, the world can be represented in many different ways. This is more of a predicament of science rather than some consequence of it that should and can be solved.

To wrap up, we have considered two seminal approaches to suicide, and the research stemming from these conceptual paradigms that one could perhaps interpret as Kuhnian 'normal' science - much of the research is directed towards developing and testing the implications of the main concepts 'social integration' and 'psychache'. Each approach has its own explanatory concepts that latch onto different surface models of the world. It is hard to integrate these very different conceptual strategies. While this is sometimes seen as an indication of the sorry state of social science, that due to its variety of distinct explanatory models it is unable to demonstrate how social

[284] Troister et al., " A Five-Month Longitudinal Study," 614-617.

regularities come about, that the complexity of the social world apparently is insurmountable for social science, all these claims boil down to the idea that social science needs to demonstrate or explain how regularities in the social world are brought about. Social scientists need not believe this, since they only devise abstract structures that have to be adequate for the surface model and make no statement about the unobservable social world. The concepts that are brought to the table make sense as explanations, if one considers the particular context of inquiry and the theoretical background knowledge of the inquirer in question. To give an illustration of this point, we reconstruct the explanations of suicide discussed in this section pragmatically in the next section.

6.4 Two illustrations of pragmatic explanation

To start with Durkheim, his question was: 'Why are some societies more disposed to suicide than other societies?' The topic of this question was implied by demographical data that showed that in some societies suicide was more prevalent than others. Van Fraassen holds, as we have seen, that contrast classes depend on the interests and background knowledge of the inquirer. Contrast classes also anticipate different answers.

It is in this light interesting to mention Durkheim's background knowledge. In his (1885) work on political economy, he inquired what constituted national cohesion, but also examined the relations between the macrosocial and the individual, for example, between political organisations and professional corporations.[285] Thereafter, the concept of 'degeneration' became a controversial but prevalent term among philosophers, physicians, and psychiatrists. Around the turn of the twentieth century, 'degeneration' referred to the 'double crises' in both social and biological health of the French nation. The idea was that low birth rates, syphilis, alcoholism, suicide, mental illnesses, and the like were the effects of 'hereditary degeneration', or the biological effects of living in a social unhealthy milieu. These 'metrics' of the nation were compared with other countries such as Germany.[286] As Marcel Fournier shows, much of Durkheim's significant work around 1890 was concerned with pathology, heredity and psychology, for example, the article 'Crime and Social Health'. From about the

[285] Marcel Fournier, "Durkheim's life and context: something new about Durkheim?" in *The Cambridge Companion to Durkheim,* ed. Jeffrey C. Alexander and Philip Smith (Cambridge : Cambridge University Press, 2005), 50-52.
[286] Fournier, "Durkheim's life," 57-58.

1893s he introduced the notion of 'collective consciousness', with which he aimed to establish a collective psychology that objectifies psychic problems by locating them causally not in the individual but in the 'base of social life'.[287]

From this constellation of interests, prevalent ideas and knowledge of that time, possible contrast classes and answers arose, contrast classes such as: 'Why are some societies more disposed to suicide than others instead of being equally disposed to suicide?'. Durkheim observed that some of the societies at issue were at least similar in some respects, for instance because they all had geographical homogeneous diffusions of suicide without central nuclei.[288] Another contrast class was "Why are some *societies* more disposed to suicide than other societies instead of the individuals they comprise?" As we have seen, Durkheim was more interested in locating psychic problems in the macrosocial than in the individual.

Possible answers to Durkheim's question were: "Because the psychological inclination of individuals to suicide, for example in the form of insanity, varies from society to society," or: "Because in some societies people tend to imitate suicidal behaviour more strongly than others," or: "Because some societies are more tightly socially integrated than others." I will not repeat the details concerning social integration here, but we know that the latter answer was the answer employing a reason that Durkheim advocated. As we have seen, Durkheim was at pains to show, by arguments of analogy, by reassessing definitions, by 'hedging' and invoking ad-hoc assumptions, and by appeal to official statistics, that his answer was the best explanation to be had, and that his answer was not 'shielded off' by extra-social causal factors such as geographical influences or psychological individual influences.

In short, Durkheim's background knowledge together with some additional data implied for him that the answer had to be sought in differences in social integration (although he did not oppose psychological explanation per se).[289] In this story it is not imperative to assert that the explanation confers some degree of necessity or likeliness on the social regularity (suicide rates) observed, although of course, Durkheim himself did so. Durkheim's explanation is reconstructed in a pragmatic sense in the sense that it is a fitting account of how the social regularity in question may have been brought about by such a thing as 'social integration', in the

[287] Fournier, "Durkheim's life," 58.
[288] Durkheim, *Suicide,* 84, 97.
[289] Durkheim, *Suicide,* xxxvi, 276.

context of his personal interests in 'social facts', additional data at hand and ideas prevalent at his time. Durkheim himself, however, believed that social facts were real because they were entities that established uniform effects.

The same can be done for the contemporary research by Troister et al. on psychaches. The question is 'Why are some individuals more likely to engage in suicidal behaviour than others?'. Again, the topic is suicidal behaviour, which we take to be a social phenomenon, the occurrence of which differs among individuals. An example of a contrast class may be 'Why are some individuals more likely than others to engage in suicidal behaviour instead of equally likely?', since one premise in the background knowledge is that the participants and their concomitant properties are distributed normally, so that most of the participants will not differ too much in various respects from others.

The background knowledge of the researchers in question is, of course, Shneidman's theory of psychaches, the details of which I will not repeat here. Possible answers we can anticipate, given that we know that Troister et al. were committed to Shneidman's abstract theory, are: 'Because some individuals are characterized by depression and depressed people are more likely to commit suicidal behaviour than others', or 'Because some individuals are characterized by hopelessness and are more likely to commit suicidal behaviour than others', or 'Because some individuals are characterized by psychaches and are more likely to commit suicidal behaviour than others'. The last answer, as we know, is for Troister et al. the answer that employs the reason they advocate. Note that other possible answers are not ruled out by the study but that the theoretical context from which the researchers work also bear on the favoured answer. With their background knowledge comprising Shneidman's theory, previous relevant research on the relation between psychaches and suicide, and the additional data gathered from the participants from their research, they hold that the answer that employs psychaches as the reason for the topic in the question is simply the best answer.

In my view this take on explanation saves us from discussions about the ontological commitments we make in employing the concepts, and from discussions about how the explanations in question confer necessity or likelihood on the description of the observable world instead of the data. We are not committed to a realist interpretation of this statistical explanation. We have yet no access to knowing whether, indeed, psychaches make individuals more likely to commit suicidal behaviour. We do not have to argue, as Durkheim did, that social constructs refer to irreducible emergent phenomena (the combination of individual consciousnesses establishes an entity that cannot be reduced to their constituents), nor do we have to assert that one may infer the existence of an entity if it causes uniform effects (a more or less stable degree of social integration explains the stability of suicide rates in any particular society). This latter claim resonates nowadays in a slightly different form in Cartwright's argument we considered earlier.

I take it that the concepts coined by Durkheim and Shneidman are not strictly limited to their 'contexts of observation', as Kagan contends. For Durkheim that was not the case since he applied his model to data models stemming from many different countries and the structure predicted by his theoretical model was similar to the structure in the surface models. Although there were some exceptions, this should not upset us too much, as I tried to explain in the previous chapter. Shneidman's concept of psychache is valid beyond a single context, not only in university students, but also in Brazilian patients suffering mood disorders.[290]

[290] Marcelo T. Berlim et al., "Psychache and Suicidity in Adult Mood Disordered Outpatients in Brazil," in *Suicide and Life-Threatening Behavior* 33, no. 3 (Fall 2003).

Conclusion

The aim of this thesis was to reinvigorate empiricism in the social sciences. My strategy to accomplish this was by extending constructive empiricism - a sophisticated and critically acclaimed variant of empiricism that was developed in the context of physics in the 1980s - to the social sciences. Of course, what I understood as social science in this thesis was inevitably somewhat of an aberration, since only the social sciences that employ quantitative methods were eligible to a constructive empiricist treatment; (sub)disciplines as anthropology and historical sociology were left out of the picture. On the other hand, many disciplines that actually use quantitative methods were for the sake of brevity excluded from discussion; political science and economics are examples.

I started this thesis with articulating constructive empiricism in relation to classical forms of empiricism and laying out the constructive empiricist world-view, with the observable-unobservable distinction as one of its key aspects. This distinction is crucial in the aim of science according to constructive empiricism: science aims to give us theories that are empirically adequate, that is, are correct with respect to what is observable. The constructive empiricist is not committed to any stronger claim about the aims of science. This, in a nutshell, was the message of the first two chapters.

I was convinced that it is fruitful to put this distinction to work in social science. This had been attempted in a different time and form before. Behaviourism already insisted on the reduction of the meaning of entities that were not observable, like mental states, to observable behaviour. Behaviourism, in turn, was inspired by logical positivist thought that in the same vein advocated the reduction of non-observable entities, like electrons, to theory-neutral sentences that only referred to observations. Van Fraassen ingeniously saved empiricism from this reductionism and maintained that our statements about the unobservable realm are meaningful, because we must understand them literally and naturally as being true or false of the world; we simply lack the means to verify them.[291]

In my opinion this was a welcome and fresh perspective on unobservables. I

[291] I believe that this is the constructive empiricist's answer to Abma's critical exposition of the problems of a behaviourist view of social science in his (2017): constructive empiricism respects humans as objects of study by respecting the statements about their unobservable properties. See Ruud Abma, *Het verdrongen curriculum. Over onderwijs in de sociale wetenschappen. Afscheidscollege Utrecht, 22 juni 2017* (Amersfoort: Drukkerij Wilco, 2017), 28-30.

imagined that, in the light of constructive empiricism, talking about a thing as gravity would in at least one sense not be fundamentally different from talking about a thing as a mental state, since they are both unobservable entities. In addition, I imagined that this would close the alleged fundamental gap between the natural and social sciences a bit, since this would go some way towards levelling the imposing unobservables found in the natural sciences and the unobservables found in the social sciences. Social unobservables have a rather mundane status since they occupy a central role in the vernacular; since they are part of our everyday lives, we take ourselves to be experts on them.

The new perspective on observables led to a different situation. The idea of brute non-human observables behaving in a regular way is one thing, the idea that human actions are also observable regular phenomena is quite another. Many scholars in (the philosophy of) social science think that behaviour is imbued with meaning, goals and intentions, norms, values, and that these things are the metaphysical clothespins of social science. The crucial point of constructive empiricism is that we do not need to adopt these claims to have good scientific theories.

In chapter four Van Fraassen's amended model view was expounded. I showed that it offers the tools needed to represent social phenomena quantitatively by transforming observable social phenomena into values on a mathematical scale. It is social theory that infects the measurements that translate phenomena to appearances. After smoothing out and idealizing the data, usually we have variables that we take to represent some aspects of social theory. These variables can be put to work in statistical analyses of correlation and regression; I proposed to see these analyses as data or surface models, that must implement the same structure as the structure hypothesized in the abstract social theory. In part II of this thesis I interpreted some concrete studies in the light of this research methodology, among others, contemporary studies of Durkheim's abstract theory of social integration, Paige's abstract theory of agricultural organisation and social movements, and Shneidman's abstract theory of suicidal behaviour.

Since, as I said above, constructive empiricism is a philosophy of science that was developed in the context of physics, I provided a brief sketch of the history of the perceived chasm between natural and social science in chapter three. The chasm reappears in a contemporary account of the alleged fundamental differences between natural and social science, which I interpreted as a series of challenges a constructive empiricist outlook on social science has to address. This was done in the chapters four through six.

I argued in chapter four that Van Fraassen's amended model view is able to accommodate many aspects of both natural and social research that are a potential source of distinctiveness. Moreover, I argued that cashing out the difference between the cultures in terms of the degree of control over the sources of evidence experiments is an oversimplification of actual scientific practice.

I argued in chapter five that fleshing out differences in the natural and social sciences with regard to the extent they have description, prediction, and explanation as 'products of inquiry', is unsuccessful in the light of constructive empiricism. A social theory yields a satisfying description if it is empirically adequate; a theory predicts if the hypothesized similarity of structures is true in past, present, future; a theory explains pragmatically by offering scientific answers to context-specific inquiries. This is the same for the natural and social sciences. Nothing in this picture commits us to stronger ontological commitments regarding the social realm.

In chapter six I criticized the idea that the concepts of the two cultures are essentially different in terms of their generality and semantic references. The generality of concepts is already a critical issue within cultures themselves, and concepts may but need not refer to any unobservable reality in good scientific theories at all. This is the same for the natural and social sciences. I discussed different philosophical approaches to concepts in (the philosophy of) the social sciences, and I addressed the bewildering variety of conceptual approaches to social phenomena from the viewpoint of pragmatic explanation.

Let us now address the inevitable question latent in every concluding section of academic writing: where does all this leave us? My thoughts on this are as follows. Although constructive empiricism was not conceived of as a normative account of science, I think that it teaches us a different attitude towards both inflationary and deflationary metaphysics in the (philosophy of) social science. Discussions about which social entities, events and processes exist and which not, which of them can and should be reduced to others, discussions about how we can be sure that we really measure the social entities, events and processes of interest in the world, and discussions about which social entities are the true explanations for some social phenomenon, are misguided.

In a constructive empiricist outlook on science, we need not know and probably will never know whether social entities exist; we need not know and probably will never know whether we actually measure social unobservables as beliefs, desires, and social integration as things in the world; we need not know and probably never will know what the true explanations of social phenomenon are, if they employ unobservables. In fact, we should not focus exclusively on the relation theory-world since it is incomplete. We must invoke in both scientific representation and explanation the user, who carries specific goals, interests, and background knowledge; it is this user who represents social phenomena by some particular theoretical framework; it is this user who decides what counts as a proper question and a satisfying answer to it. Nothing in this picture commits us to any ontological commitments about the non-observable world.

I further believe that when we are talking about scientific cultures, we should not soothe ourselves to sleep too easily with contemporary accounts of alleged fundamental differences. The account we considered drew on oversimplifications of, among other things, actual scientific practice with regard to the degree of control in experiments, and the generality of concepts in the natural sciences. It is time to develop an approach that focuses not exclusively on alleged insuperable differences between the different sciences, but on the parallels between them instead. I believe and hope to have showed that this approach is already available and waiting for us, although we have just begun reaping all of its fruits in new areas. In that spirit, a promising path future inquiry may venture is the development of constructive empiricism in other (quantitative) areas of social research, for example, economics, neuroscience, and political science.

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