Ladyman's Realism, van Fraassen's Anti-Realism and Fine's Middle-Way

by

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Abstract

The aim of this thesis is to critically analyse and evaluate the current debate between scientific realists and anti-realists. Some thinkers claim that the debate is a stalemate, with both parties appealing to self-justifying axioms. I investigate whether this is the case.

I identify James Ladyman and Bas van Fraassen as exemplars for realism and for anti-realism respectively. I also include a third category - notably represented by Arthur Fine - that I label the 'middle-way'. The debate in the current literature generally centres around epistemology. The question is whether we can have knowledge of scientific 'unobservables' (e.g. trilobites, blood cells and the Higgs boson). The realists generally answer 'yes', the anti-realists say 'no' and the middle-wayers are usually undecided. There is also a concomitant question about whether successful scientific theories are (at least approximately) *true*. The three parties concerned generally answer as before: yes, no and agnostic.

In chapter 1 I introduce the three pertinent positions by briefly narrating the genealogy of each. Chapter 2 involves a lengthy exposition of Ladyman's *ontic structural realism*, van Fraassen's *constructive empiricism* and also the deflationism and/or pluralism of the middle-way, with particular focus on Fine's *natural ontological attitude*.

Ontic structural realism holds that metaphysics should be strongly continuous with science. The methods of science grant epistemic access to relational structures only, and not to essences of particulars. Furthermore, since questions beyond the limits of science are meaningless, the limits of our epistemology reveal the limits of ontology. Therefore, successful scientific theories *truly* represent the ontic structure of the world.

Constructive empiricism holds that we cannot have epistemic access to things that lie beyond what is observable. Microscopes, and other 'magnifying' scientific instruments, *create* phenomena that are studied by scientists. Metaphysical speculation beyond the phenomena is superfluous; ontological agnosticism about unobservables is the proper attitude. Moreover, successful scientific theories are not true *simpliciter*, but are rather only 'empirically adequate'.

The natural ontological attitude offers a deflationary position in which we should generally remain silent about the epistemology and ontology of science, since there are no philosophical meta-criteria by which to judge these issues. The realism/anti-realism debate presents a false dichotomy, and attaching the 'truth' appendage to a scientific theory is redundant. I also discuss some relativist and feminist philosophers of science who can be grouped under the 'middle-way' umbrella.

In chapter 3 I conclude by considering whether the positions discussed above represent an epistemic *cul-de-sac* or whether any of them allow for a way forward. I conclude that, in fact, one of them - although incomplete - does offer promising prospects for further development.

Abstrak

Die doel van hierdie tesis is om die debat tussen wetenskaplike realiste en anti-realiste krities te analiseer en te evalueer. Party denkers betweer dat die debat ‘n doodloopstraat bereik het, waar beide partye hulself beroep op self-regverdigende aksiomas. Ek ondersoek of dit wel die geval is.

Ek identifiseer James Ladyman en Bas van Fraassen as eksemplare van realisme en anti-realisme. Ek identifiseer ook ‘n derde kategorie - verteenwoordig deur Arthur Fine - wat ek die ‘middeweg’ noem. Die huidige debat in die literatuur handel grootliks oor epistemologie. Die vraag is of ons kennis kan hê insake wetenskaplike onobserveerbare entiteite (bv.trilobeite, bloedselle en die Higgs boson. Die realiste antwoord gewoonlik ‘ja’, die anti-realiste sê ‘nee’ en die denkers wat die middeweg volg is gewoonlik agnosties. Daar is ook ‘n verwante vraag oor of suksesvolle wetenskaplike teorieë (ten minste meestal) *waar* is. Die drie partye vroeër genoem antwoord gewoonlik op ‘n soortgelyke manier; ‘ja’, ‘nee’ en agnosties.

In hoofstuk 1 verduidelik ek die drie posisies deur elkeen se genealogie kortliks te verduidelik. Hoofstuk 2 bevat ‘n gedetailleerde verduideliking van Ladyman se ontiese strukturele realisme, van Fraassen se konstruktiewe empirisisme, asook die deflationism en/of pluralisme van die middeweg, met ‘n spesifieke fokus op Fine se natuurlike ontologiese houding.

Ontiese strukturele realisme beweer dat metafikiska kontinu met die wetenskap moet wees. Wetenskaplike metodes bied ons slegs epistemiese toegang tot relasionele strukture, en nie tot essensies van partikuliere nie. Verder, gegewe dat vrae wat die perke van die wetenskap oorskry betekenisloos is, openbaar die perke van epistemologie die perke van ontologie. Suksesvolle wetenskaplike teorieë openbaar dus die ontiese struktuur van die wêreld.

Konstruktiewe empirisisme beweer dat ons nie epistemiese toegang tot entiteite wat ervaring transendeer kan kry nie. Mikroskope en soortgelyke instrumente skep die fenomene wat deur wetenskaplikes bestudeer word. Metafisiese spekulasie wat die fenomene transendeer is sinneloos; die korrekte houding jeens onobserveerbare entiteite is ontologiese agnostisisme. Verder, suksesvolle wetenskaplike toerieë is nie waar simpliciter nie, maar is eerder slegs ‘empiries genoegsaam’.

Die natuurlike ontologiese houding bied ‘n deflasionêre posisie wat huldig dat ons eerder moet swyg insake die epistemologie en ontologie van die wetenskap. Daar is geen filosofiese meta-kriteria wat ons kan gebruik om sulke kwessies te beoordeel nie. Die realisme/anti-realisme debat skep ‘n valse dichotomie, en om die term ‘waarheid’ aan ‘n wetenskaplike teorie te heg is onnodig. Ek bespreek ook sekere relativistiese en femisistiese filosowe wat onder die saambreelterm van die ‘middeweg’ gegroepeer kan word.

In hoofstuk 3 bespreek ek of, gegewe die bogenoemde bespreking, die bogenoemde posisies ‘n epistemiese doodloopstraat is en of enige van die posisies wel die pad vorentoe aandui. My konklusie is dat een van hulle wel belowend, alhoewel steeds onvoltooid, is.

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“Realism is dead” - Arthur Fine (1986)

"Metaphysics is dead" - Bas van Fraassen (2002)

"The debate about scientific realism has been pronounced dead many times only to come back to life" - James Ladyman (2018)

Introduction

The over-arching debate in philosophy of science is arguably the one between scientific realists and anti-realists:[1] does science point towards a metaphysical reality 'out there', or is it only a useful *tool*, aiding various human goals? Which side of the divide one falls on informs the rest of one's views regarding science and philosophy. Running between these two camps is a quietist or deflationary approach; a middle-way of either tolerance or indifference. All parties involved claim common-sense for themselves, yet it is not immediately obvious to an outsider where to peg one’s epistemic commitments. Schlagel (1991) puts it best, when he states that the:

> belief that by experimentally probing deeper levels of physical reality we can discover additional microstructures and interactions accounting for observable regularities is what distinguishes scientific realists from antirealists (309).

The most prominent discussions in the contemporary literature revolve around the notion of *structure, probability* theory and the philosophical implications of *quantum mechanics*. These three big themes will, therefore, dominate the content of my thesis throughout. There are, of course, various well-known figures in the debate. One could possibly choose as exemplars for realism Stathis Psillos, Richard Boyd or early Hilary Putnam; for anti-realism one could choose Carl Hempel, Thomas Kuhn or middle Putnam; for the middle-way deflationists one could choose W.V.O. Quine, Larry Laudan or, perhaps late Putnam. However, in the literature certain thinkers are regularly cited and increasingly influential. Also, given that a structural interpretation of the issue at hand has, it seems, become the ‘received view’, I delimit the debate as follows.

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[1] From here-on simply 'realists' and 'anti-realists'.

As I judge it, we have James Ladyman, with *ontic structural realism* (OSR)[2] (1999; Ladyman and Ross 2007), representing the realists. While Bas van Fraassen's *constructive empiricism* (CE) (1980a) dominates the anti-realist position. Standing in the middle, Arthur Fine with the *natural ontological attitude* (NOA) (1986), is usually considered the leading middle-way voice. These writers offer neat and elegant views that are representative of the themes I wish to discuss here. Hopefully, over the next hundred pages or so, this agenda will take us on an ultimately rewarding epistemic journey.

I will follow Chakravartty in giving brief introductory course-grained definitions of these three positions that will serve as a foundation upon which to build throughout the rest of the thesis:

> OSR - rejects doxastic commitment to the 'entities' of scientific theories. Proponents hold that we only have knowledge of structural aspects, both observable and unobservable, of reality. There is, in fact, nothing else to know: structure is all there is, epistemologically and ontologically speaking (2007a: 54).
>
> CE - agrees with realists that there is some mind-independent ontology, but "recommends belief in our best theories only insofar as they describe observable phenomena, and is satisfied with an agnostic attitude regarding anything unobservable" (2017b: n.p.).
>
> NOA - is a form of deflationary quietism concerning the unobservable, prescribing a policy of non-engagement: "all ontological claims are on a par. . . It is intended as a neutral position for those who find nothing to be gained in debates surrounding them" (2007a: 33).[3]

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[2] Ladyman is part of a 'team' - including at various times French, Ross, Spurrett, Collier and Berenstain - who argue for OSR (to different degrees of conviction, and with varied nuance). Ladyman is - however, on my reading - the dominant figure in the promotion of the position. For brevity, I will from here on, therefore, generally just refer to him alone when discussing OSR.

[3] Some thinkers see the debate as primarily about the aim of science. Thematically this thesis has more to do with scientific ontology, so although mentioning aims from time-to-time, I will mostly focus on ontology and closely related concepts like metaphysics, epistemology and representation. Moreover, it is not clear to me that a process (or institution) such as science, can have an aim. My inclination is to reserve anthropomorphic terms such as 'aim', 'goal' and 'purpose' for living organisms. Even though most of the participants in the broader debate seem to talk this way, I am concerned that there may be an equivocation here. Rosen (1994) agrees that the dispute over the aim of science is "clouded by an underlying obscurity in the impersonal idiom 'Science aims to. . . '" (146). Rowbottom (2014), likewise, argues that there is an ambiguity in the notion of 'the aim of science'. This has created so much confusion in the realism/anti-realism debate that he concludes it is best to avoid talk of the aim of science altogether. Therefore, despite there being extensive back-and-forth about this subject in the literature, I will generally underplay it in this thesis.

In this thesis I will firstly give a brief history of the debate, including preliminary definitions of the three relevant positions. Secondly, I will give a detailed discussion of each as they stand today, incorporating both positive and negative commentary from various thinkers around the discussion. I, however, give more weight to realism and anti-realism than to the middle-way. My interest, for now, is in substantive or robust positions, when it comes to understanding the big philosophical themes of *truth*, *knowledge*, *belief* and *reality* (as mediated by science), as opposed to sceptical, quietist or conciliatory views. Thorough investigation of competing, positive theories should surely be made before we adopt a middle-way position. Ladyman and van Fraassen's views are also significantly more detailed than Fine's, and, therefore, deserve lengthier exposition.

Lastly, I will conclude this thesis by briefly discussing whether one of the three views may offer a convincing case for further development. I do not aim to articulate an overarching argument here. My goal is to survey the debate at hand, and to give a detailed, nuanced appraisal of the three positions just introduced. I allow each representative - Ladyman, van Fraassen and Fine respectively - to argue their case, and then I present thorough arguments against each position as we go. My overall conclusion is minimal in that I suggest only a tentative way forward with regards to which of the three views is most promising for future development.

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1. Framing the debate

There is a fascinating and multi-facetted history to this three-way debate that is unfortunately, due to space constraints, mostly beyond the scope of this thesis. I will therefore only trace a brief narrative that leads from the respective origins of realism, anti-realism and the middle-way to the three positions at the core of the current literature.

1.1. Realism, from Galileo to Ladyman

1.1.1. History of realism

Following Liston (2018), I will start with the Copernican revolution and the resulting Galileo affair, since that was when science - in its approximately modern form - began its intellectual push into philosophy and theology. Copernicus had promoted his 16th century heliocentric model of the solar system as a formal tool that made better predictions, and was simpler to use, than the preceding convoluted Ptolemaic geocentric model. Galileo, a century later, though, risked the further 'metaphysical' claim that heliocentrism said something about the way things really are 'out there': it is more than just a convenient instrument for contemporaneous Catholic calendar forecasts and other such pragmatic concerns. This realism about scientific phenomena (as models, theories and data) was continued by enlightenment thinkers such as Descartes, Newton and Bacon who advocated that the world is more than it appears to our senses.[4] There is a world of objects, events and/or processes that are causally responsible for, and somehow 'beyond', the phenomena that we observe. The task of science for these *metaphysicians* is to reveal and map the objective ontology of the mind-independent world: "to strip reality of the appearances covering it like a veil, in order to see the bare reality itself" (Duhem 1906/1954: 7).

Liston (ibid.) notices a genuine realist/anti-realist divergence appearing in the 19th century debate among philosopher-physicists like Maxwell, Planck (for the realists), Duhem and Poincaré (for the anti-realists) on the nature of space and the ontology of forces and atoms. Realism as a 'movement' in the philosophy of science gets crystallized proper, though, in reaction to the austere instrumentalism of the logical positivism that dominated the philosophy of science through the mid 20th century. The logical positivists adhered to the verification theory of meaning, involving the idea that all that is meaningful in a statement "is a function of what empirical [read observational] results would verify or refute it" (van Fraassen 1980a: 35).

Russell (1948) pioneered the modern structural turn for realism while pursuing epistemological monism. For him, to "exhibit the structure of an object is to mention its parts and ways in which they are 'interrelated'" (267); "structure always involves relations" (271). What do we know of the world behind the phenomena? Russell (1912/1997) offered an analogy: listen to the radio and hear the sounds produced miles away. The radio waves between the source and the listener have none of the qualities of the sound. We infer that these radio waves must have the *structure* that encodes the structure of the sound. We know, therefore, based on observation, not the qualities of the waves in themselves, but their structure. This structure is what science describes, in the form of equations (in this case Maxwell's equations). Russell continued this motif in his (1927a): "whenever we infer from perceptions it is only structure that we can validly infer; and structure is what can be expressed by mathematical logic "(254). As such, the "only legitimate attitude about the physical world seems to be one of complete agnosticism as regards all but its mathematical properties" (270).

[4] There is some dispute over whether historical figures such as Newton and Bacon were realists or anti-realists. Nothing of substance in my thesis depends on how exactly they are classified. I generally follow Liston (2018) and Musgrave (2018) in this regard.

Later, Quine (1951) and Kuhn (1962/1996), although not realists, blurred the boundary between observation and theory, slowly dismantling the tenants of the logical positivists radical anti-realist position. Quine urged that no statement is immune from revision in the light of new experience. For him 'common sense' epistemology is our starting point. Both Cartesian certainty and the dualism of the logical positivists is unworkable. Quine offers instead 'holism': statements are tested as a whole. They form a 'web of belief' that "impinged on experience only along the edges" (1951: 35). Statements cannot be confirmed or disconfirmed in isolation. Pragmatic revisions to the web are informed by empirical concerns. Any statements can be revised, but should involve ‘minimal mutilation’ to our overall web. Quine calls this ‘methodological monism’ (i.e. science and philosophy have the same method). Philosophy should be 'continuous with' science - we should approach science obliquely - so to speak, rather than try to ground it. Kuhn, on the other hand, argued that statements depend on background assumptions for their meaning and conditions of application. He doubts the idea of correspondence between theory and reality, and sees no historical evidence for a scientific convergence on truth. There is no “neutral algorithm of theory choice” (Kuhn ibid: 198), and observation terms cannot be isolated in the way the logical positivists thought.

Also, logical positivism cannot be justified by its own tenets; a trust in the semantic and epistemic power of observation does not come from observation. The positivist’s central dogma is a metaphysical presupposition, and consequently, this kind of ‘naive’ empiricism falls within the scope of what it rules out: it reduces to apparent absurdity. Realists often say that their position should be accepted because it offers the best explanation for why the observable phenomena are as scientific theories predict them to be. Sellars (1963) concluded, matter of factly, that to “have a good reason for holding a theory is ipso facto to have good reasons for holding that the entities postulated by the theory exist” (97). Popper (1963; 1972), although arguing that it is impossible for us to justify truth-claims about scientific theories, held that we attempt in science to - as far as possible - explain reality. Although realism is, according to his own criteria of falsifiability, an irrefutable metaphysical hypothesis, he nonetheless favoured it over anti-realism (1972: 40-42). This is because he viewed realism as entailed by scientific theories, and as an extension of common sense. The preceding thinkers’ efforts made way for modern realism - of the sort we are interested in - to emerge.

1.1.2. Contemporary realism

I will now briefly discuss some of the relevant thinkers representing contemporary realism. This serves as a prelude to our detailed focus on Ladyman’s OSR later on. Sankey (2001), a staunch advocate of realism, argues that the success of science allows inference to both the approximate truth of mature scientific theories and to the truth conduciveness of the methods of science. He lists six principles, or characteristics, of scientific realism. In abridged form, they are:

(1) The aim of science is to discover the truth about the world.
(2) Scientific discourse about theoretical entities can be interpreted literally.
(3) The world investigated by science is an objective reality, independent of human agency.
(4) Truth consists in *correspondence* between a claim about the world and the world.
(5) Theoretical claims are made true or false by the way things are in the mind-independent reality investigated by science (truth is objective).
(6) Scientific inquiry yields genuine knowledge off the objective world (Sankey 2001: 35-38).

As we will see, Realists also generally give explanation, and - in particular - inference to the best explanation (IBE) or abduction, much weight. IBE, in this context, states that explanatory considerations play an evidential role in science and in the assessment and justification of scientific hypotheses. Furthermore:

> explanatory virtues - parsimony, unification, explanatory scope, and precision, for example - should be taken into account in assessing competing hypotheses' comparative likelihoods (Saatsi 2017: 203).

Scientific realism is often classified as falling into two camps (Psillos and Ruttkamp-Bloem 2017): explanationist realism and selective realism. I will introduce these briefly. The so-called explanationist (or 'no-miracles') account of science was cemented by Putnam (1975) and Boyd (1983), and is still approximately held by Psillos (1999, 2003). According to Psillos (2018), this view implies at least three theses: (1) theoretical terms refer to unobservable entities; (2) theories are (approximately) true; and (3) there is referential continuity in theory change.

Selective realism was developed mainly as a response to arguments against explanationist realism by Hesse (1976) and Laudan (1981). As the name suggests selective realism selects parts of scientific theories to be realist about; "even false theories, many of which employ terms that do not refer to anything, may still incorporate finer-grained truths and referring terms, which may then serve as the basis of continuities across theory change" (Chakravartty 2017a: 23). The components of theories that guide novel predictions are typically the part committed to. Ruttkamp-Bloem (2013) explains that:

> [d]efenders of this form of realism typically separate theories into components or aspects according to some criterion such as 'working posits' (Kitcher 1993), structure or what

> have you; and argue that only the selected components are eligible for realist claims (203).

Non-working parts "may be 'false' or 'nonreferring' idle parts of past theories", that have been rejected through theory change, "while truly success-generating features have been confirmed by further inquiry" (Stanford 2003: 913). One can, perhaps, think of the explanationist account as the traditional view, and of selective realism as the modern view. The former was the dominant version of realism in the 1970's and 80's; the latter has been dominant in the literature since then. My aim in this thesis is to discuss the most current views; it seems the debate has moved on, and the explanationist account may be somewhat outdated. Also, there are strong reasons why we have to be selective regarding which parts of theories we are realist about. To hold that theoretical terms refer to unobservable entities *tout court* appears untenable given recent arguments by Ladyman and van Fraassen that will be described later in this thesis. As we will see, scientists sometimes deliberately distort their models in order to gain some practical advantage. They also, at times, posit ideal entities in their theories – such as frictionless planes – that clearly do not deserve ontological commitment.

Due to these sentiments, I will focus on versions of selective realism in this thesis. There are two dominant versions of selective realism recognised in the literature; these are structural realism and entity realism. As mentioned on page 8, a survey of the current literature indicates that the primary one appears to be structural realism. Also, since van Fraassen (2008) – our anti-realist representative – has recently developed a structural version of empiricism, it seems apt to focus here on a juxtaposed structural form of realism. Structural realism is described by Chakravartty (2017a) as follows:

> This view identifies certain structures or relations as the aspects of the world described by scientific theories to which realists can commit, as opposed to the unobservable entities that putatively stand in these relations (30).

These structures (or forms) are retained across theory change, even when the ontological posits of many theories are doxastically discarded as science progresses. The structure of a theory 'reflects' reality in some way without reference to the observable objects of that reality. Modern structural realism, developed initially as epistemic structural realism (ESR) by Worrall (1989b) involves a rejection of natural necessity, and holds that what survive scientific revolutions are mathematical equations: taken to 'encode' the structure of the theory's subject matter. The preservation of equations through theory change, therefore, amounts to the preservation of structure. Worrall holds that this structure is a purely epistemic phenomena; ESR cannot access the noumenal ontology of the world.

Recently, Ladyman (Ladyman and Ross 2007) reifies these structures, thereby giving us ontic structural realism (OSR), in which it is claimed that we have epistemic access to only structure because, in fact, structure is all there really is. Motivated mostly by implications from quantum mechanics, Ladyman fuses the epistemic and the ontic; there is no gap between science and metaphysics. Relational structure is ontologically subsistent, and objects or particulars do not have *being* distinct from the relations in which they stand. Ladyman, though, insists that - *pace* Worrall - his structures are physical, not mathematical.[5]

Embracing 'modal objectivism', i.e. a non-Humean stance,[6] towards modal structure in the world, Ladyman emphasises that modality is the key to his account of ontology "which harmonizes entity realism and ontic structural realism, because featuring in projectable models and/or statements is taken to be the criterion of reality" (2018: 105). Adopting a naturalistic approach to metaphysics, Ladyman argues that there are no *real* objects, individuals or essences. Structural form, or pattern, is what is real; "the distinction between illata and abstracta has no scientific basis. . . its real patterns all the way down" (Ladyman and Ross 2007: 228).[7]

Ladyman understands the aim of science to be *the truth* and the aim of metaphysics to be the unification of science. He rejects reductionism and argues for a 'scale-relative' ontology in which "real patterns are entities of whatever ontological category that feature (non-redundantly) in projectable regularities" (2018: 103). Ladyman suggests that scientific realists should ultimately

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[5] Ladyman's structures are physical, rather than mathematical, for two reasons:

> (1) physical structures "can be related - via partial isomorphisms. . . to the (physical) 'phenomena'. This is how 'physical content' enters" (French and Ladyman 2003: 75).
>
> (2) physical structures are causal.

Ainsworth (2010), though, wants to know "[h]ow do we distinguish physical phemonena from mathematical structures?" (50-1). Surely mathematical structures can also be related via partial isomorphisms to physical phenomena. So, there is still no way to distinguish physical from mathematical structure.

[6] Hume, says Ladyman, "reduces singular causation to generic causation, and generic causation to laws, which are construed as mere regularities" (2005: 332). For Hume, (Newton's) laws merely summarize the data, they do not determine them.

[7] 'Illata' and 'abstracta' were introduced by Reichenbach (1957). Ladyman (Ladyman and Ross 2007) explains:

> The former are the things that exist at the fundamental level, the latter are those things that only exist because we conceptualize mereological sums of the illata as if they were genuine objects for pragmatic purposes (178).

focus on providing “an account of the relationship between the modal structures found in scientific theories at different ontological scales" (105). OSR accommodates a pluralistic stance towards the models and theories at the different scales within science (i.e. chemistry, biology, economics etc.) albeit with a special respect for physics, since physical theories always trump special science theories when the two conflict.

As such, OSR posits that all sciences answer, in some sense, to quantum field theory. This apparently allows the position to cope with - amongst other traditional dilemmas for realism - quantum entanglement, compositions and identity of entities over time, modality in scientific theories and the unity of science. More on this later; for now, though, suffice to say that Ladyman's OSR has given scholars a powerful, up-to-date realist theory to contemplate on. Kuhlmann (2015) has called OSR "the most fashionable ontological framework for modern physics" (n.p.). OSR has, understandably, attracted most sympathy among physicists and philosophers of physics. Maudlin (2007), for example, although not an OSRist, understands metaphysics similarly:

> Metaphysics is ontology. Ontology is the most generic study of what exists. Evidence for what exists, at least in the physical world, is provided solely by empirical research. Hence the proper object of most metaphysics is the careful analysis of our best scientific theories (and especially of fundamental physical theories) with the goal of determining what they imply about the constitution of the physical world (104).

Here Maudlin precisely states what I take to be the method of philosophy *qua* metaphysics. The purpose of this thesis, and all the associated research, has been to fill the gaps - so to speak - in the above passage.

The second form of realism identified on page 14 - entity realism - is (like structural realism) a form of selective realism involving an emphasis on 'real life' experiments rather than the semantics of theories. The most prominent advocate of entity realism is Hacking (1982; 1983), who is generally not concerned with the truth or falsity of scientific theories themselves, but only with the entities (observable or unobservable) that feature in theories. If these entities can be manipulated causally in physical experiments to affect independent empirical domains, and to produce new phenomena, then one can take them to exist. Manipulatable entities are tools for *doing* science, and something cannot be a tool unless it is *real*. “There are two kinds of scientific realism, one for theories, and one for entities” (Hacking 1983: 26); realism about the second need not be accompanied by realism about the first. Furthermore:

> The scientific-entities version of [realism] says we have good reason to suppose that electrons exist, although no full-fledged description of electrons has any likelihood of being true (ibid: 27).

Entity realism can be thought of as the view that if one can demonstrate causal knowledge of a putative entity "that facilitates the manipulation of the entity and its use so as to intervene in other phenomena", one has good reason to grant it ontological status (Chakravartty 2017b: n.p.).[8] Not all philosophers of science have followed the current realist trend though. Let us look now at the genealogical development of anti-realism, culminating in its 21st century fountainhead: CE.

1.2. Anti-Realism, from Bellarmine to van Fraassen

1.2.1. History of anti-realism

Empiricism, instrumentalism and fictionalism are positions towards science generally grouped under the anti-realist label. Musgrave (2018) understands the history of anti-realism as principally an ongoing reactionary theological rationalization against the encroachment of science on once protected divine premises. He traces anti-realism's origin to Saint Bellarmine's insistence on certainty regarding truth when responding to the inherently imprecise Copernican science of the 16th century. Bellarmine held that since Copernicus' heliocentric theory - like all scientific theories - could possibly be wrong, certainty should be reserved for divine axioms. Scientific theories are only useful 'instruments' declared the Catholic Church; so was anti-realism about science born.

The leading defender of anti-realism through the 19th century, according to Musgrave (ibid.), was the fictionalist Pierre Duhem: a Catholic philosopher-physicist. Duhem argued instrumentally against Newtonian and Baconian mechanistic realism, insisting that physical theories are interpreted mathematical artefacts, irrelevant to objective metaphysical truth or reality. Duhem rejected the ontology of theoretical laws. A physical theory, he asserts, "is an abstract system whose aim is to summarise and classify logically a group of experimental laws without claiming to explain these laws" (1906: 7).

Religious convictions are, of course, not the only motivation for anti-realist positions. The logical positivists, as mentioned on page 11, tied cognitive meaning to empirical confirmation. This they

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[8] Egg (2017), though, questions whether there "might be some real entities which we will never be able to manipulate" (121), such as black holes. The same goes for certain evolutionary biological events: mutation and speciation, for example. Conversely, he also worries that the entity realist may take some entities that we know to be unreal as real, since they appear to be manipulable. Examples include quasi-particles in solid-state physics and unoccupied electron states in semiconductors.

dubbed the verification theory of meaning, according to which there is no real difference between two statements or theories having the same empirical content. Pioneered by the early Carnap (1937), the logical positivists attempted to reduce all empirically significant statements to reports about sense-data, and thereby do away with 'useless' metaphysics. Analytic statements are reducible to basic logical tautologies, while synthetic statements are reducible to empirical atoms. This involved the elaborate, formal task of reducing all theoretical terms to observation terms via correspondence rules, or bridging principles. The project is generally considered to have imploded though; metaphysical implicits cannot be stripped out of semantics. Ladyman (Ladyman and Ross 2007), for example, points out that the logical positivist's notion of 'sense-data' is not supported by empirical science, and the "verificationist theory of meaning was likewise a piece of metaphysics they did not derive from science" (8).

Concurrently, in physics, the early 20th century debate over the interpretation of quantum mechanics turned philosophical. Bohr's anti-realism is generally considered to have won out over Einstein's realism - lending weight to the anti-realist cause (Fine 1986: 112). The anti-realist flag was carried through the mid 20th century by, among others, Mach. Being an instrumentalist, he held that there are no unobservable physical things for us to describe:

> Properly speaking the world is not composed of 'things' as its elements, but of colours, tones, pressures, spaces, times, in short what we ordinarily call individual sensations (1960: 579).

1.2.2. Contemporary anti-realism

Now enter van Fraassen (who is out-spokenly Catholic) and has almost single-handedly ushered anti-realism, in the form of his CE, into the 21st century.[9] He has been heralded for "'unwinding the linguistic turn': boldly talk[ing] about the world and the distinctions in it" (Cartwright 2007: 33). For him 'empirical adequacy', viz. accurate predictions, is what science aims for: "a theory is empirically adequate exactly if what it says about the observable things and events in the world is true" (1980a: 12). van Fraassen is, therefore, a realist when dealing with perceptions: observations about macroscopically visible objects only. There are good theories and bad theories and a theory need not be true to be good. The function of scientific theories is to 'save the phenomena', in other words: to give an account of what is *actual*. Driven by advances in technology, science *does* progress, in van Fraassen's view, but theories are never complete. Science does not get to, point to or proceed to truth *simpliciter*. It is only a useful instrument

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[9] van Fraassen doesn't, for understandable reasons, call himself a scientific anti-realist. He refers to himself as an empiricist, and calls scientific realists 'speculative metaphysicians'.

providing, firstly: empirically adequate theories codifying phenomena, and, secondly: contingent workable models having anthropic utility.

Successful theories have survived, “the ones which *in fact* latched on to actual regularities in nature" (ibid: 40),[10] but these regularities - if beyond the observable - are so ethereal, our only sensible doxastic position towards them should be agnosticism. Science, on van Fraassen's account, therefore has pragmatic, epistemic and heuristic import; the fundamental ontology of the world, however, remains nebulously beyond the reach of our probing instruments and mathematical musings. Observation-transcendent forces, or objects - as well as 'necessary' laws, or causes - are only features of scientific theories: convenient fictions. Metaphysics lacks any prospect of empirical testing; it involves additions to science that we have no reason to believe.

van Fraassen, although semantically committing to adequate scientific theories, adopts a sceptical stance regarding the ontological status of theoretical phenomena, and doesn't recognise objective modality. One may believe in claims about unobservables, if so inclined, but this would not be the proper, justified scientific attitude (Cartwright 2007). Scientific theories have truth-values - some might even be true - but no theory should be *accepted as true* (Musgrave 1989). Metaphysical questions are not meaningless, they are superfluous, tending to only make things murky instead of clear. The "illusory charm and glamour" of metaphysical speculation, beyond the proper realm of science, leads the philosopher into an "insidiously enchanted forest" (van Fraassen 2008: 259), when in fact "making sense of a subject need not consist in portraying it as telling a true story" (1980b: 665).

van Fraassen (2002) - in his later career - allows for pluralistic epistemic stances towards ontology, partly in order to avoid problems with his observable/unobservable demarcation. Recently (2008) he has also promoted a modernised version of CE: empiricist structuralism (ES), which is an effort to accommodate some kind of structuralism within his worldview. I will discuss these post-positivist developments later in my thesis; as well as - so far glossed-over - terms such as 'empirical', 'observable', 'pragmatic', 'stance' and 'belief' that all play an important role in van Fraassen's oeuvre. For now, though, let us take an introductory look at the attempted placatory middle-way between realism and anti-realism.

1.3. The middle-way, from Wittgenstein to Fine

1.3.1. History of the middle-way

[10] Italics in quotations are always the author’s own.

A ‘synthesis’ emerging out of the debate at hand is a quietist view, dominated by Fine and his NOA. Conventionally understood, quietism - as broadly relevant to the realism versus anti-realism stand-off - developed initially with the later Wittgenstein (1953/1997). He argued that the questions central to these types of debates are confused due to being foundational on unsupported premises. Once unmasked for what they are, these issues can be set aside, resulting in a freedom from worry. The later Carnap (1950), too, endorsed a kind of pluralistic quietism with his notion of internalist 'frameworks' for knowledge, outside of which ontological questions are meaningless.

1.3.2. The contemporary middle-way

Later 20th century advocates of this deflationary middle-way, most notably Fine, question whether a resolution to the realist versus anti-realist dispute at hand is even possible. Fine (1986) has argued that neither realism nor anti-realism is tenable, and so promotes NOA: a methodological compromise, involving commitment only to the evidence of one's senses, and acceptance of the confirmed results of science. Both realist and anti-realists share this dual commitment; what Fine rejects is the extra 'redundancy' proponents on each side add to this shared ‘core position’. Realists add speculative truth claims about an underlying reality and also essential correspondence between concepts and the world. Anti-realists add pragmatic or instrumentalist conceptions of truth; plus maybe an overlay of constructivism or empiricism (127-29).

There is no unitary account of - and no global aim of - scientific practice, says the NOAer. Anything a realist can do with a ‘true’ theory, an anti-realist can do also, just without the truth appendage. We should evaluate each scientific theory as it comes, adopting a deflationary methodological stance, rather than trying to construct overarching meta-arguments of the sort Ladyman, Hacking and van Fraassen do. This deflationary approach has been broadly influential amongst contemporary philosophers of science (prominent are Stein 1989 and Kulka 1994). We will look at it again later, along with affiliated pluralist, relativist and feminist views. For now, though, let us explore - in some detail - the status of the realist versus anti-realist philosophical tug-of-war, as one finds it today.[11]

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[11] All the relevant thinkers have refined their views to varying degrees over time. For the sake of parsimony, I will generally gloss over genealogical glitches in their worldviews. As such, this thesis is - in part (for better or worse) - a systematic exercise, attempting to distil each of the three's nuanced philosophies into three unitary summaries.

2. The current debate

In this chapter I will discuss firstly Ladyman (OSR), then van Fraassen (CE) and lastly Fine (NOA) at some length. In order to zoom in on where exactly the conflict plays out, let us follow Chakravartty's (2011) identification of three central commitments for scientific realism:

> Realism is often explicated in terms of three sorts of commitment: a metaphysical commitment to the existence of a mind-independent reality; a semantic commitment to interpret scientific claims literally. . . and an epistemological commitment to regard these claims as furnishing knowledge of both observable and unobservable entities and processes (157-158).

The realists and van Fraassen have no general disagreement over the metaphysical and the semantic components of realist commitment. The antagonism is to do with the knowledge of and/or belief in 'unobservables'. As such, the debate falls within the third of Chakravartty's realist commitments: epistemology. Keeping this in mind, let us now explore the realist position, as represented by Ladyman's OSR.

2.1. Realism, Ladyman's OSR

I take Ladyman as the exemplar for structural realism over other structural realists (e.g. Worrall, French or Esfeld) because his view is, on my reading, the strongest and the most detailed account. It is up-to-date and incorporates the most recent philosophical implications from physics – in particular quantum mechanics. OSR also appears to be the most cited version of structural realism in the current literature. Ladyman works out ontic structural realism (OSR) most thoroughly in his much-discussed 2007 book *Every Thing must Go: Metaphysics Naturalized* (Ladyman and Ross 2007). He attacks both analytic metaphysics, with its appeal to the intuitive *a priori*, and also CE which - although sharing OSR's disdain for analytic metaphysics - doesn't live up to its promise of offering a positive account of science with zero metaphysics.[12]

2.1.1. Naturalizing metaphysics

Ladyman criticises that much of analytic metaphysics is just dressed-up conceptual analysis. It assumes what he calls the 'containment metaphor', where the world is treated as a container of objects with properties that are ordered in hierarchical levels. These levels relate to, supervene on, or interact causally with one another by *a priori* laws. He argues that intuitive mereological

[12] Ladyman refers to the naturalized metaphysics that he endorses as 'weak metaphysics', and to the type of metaphysics that both he and van Fraassen frown upon as 'strong metaphysics' (Ladyman and Ross 2007: 65).

axioms - such as that parts make up wholes or that two objects can't occupy the same space - are violated by conclusions from quantum mechanics. CE - in turn, despite denying it - has implicit metaphysical commitments. This is because van Fraassen needs modal objectivity to draw his observable/unobservable distinction (due to the pertinent 'able' suffix). Ladyman begins from a naturalistic commitment, which he describes as follows:

> together with naturalized epistemology, we can envisage a naturalized metaphysics according to which all that exists is in space and time and is knowable only through the methods of the sciences with which philosophy (properly construed) is continuous (2000: 845).[13]

He adds that standard realism has no sensible account of the relationship between the different ontologies of the different sciences; OSR promises to deliver just that. This naturalized metaphysics is an attempt to unify science. Philosophers should develop a metaphysics that gives a unitary scale-relative account, connecting seemingly disparate scientific hypotheses and theories:[14] from fundamental physics all the way up to economics. Ladyman sees his metaphysics as a synthesis of CE and scientific realism based on a "non-positivist version of verificationism" (Ladyman and Ross 2007: 29, 67). He holds that there is no principled methodological distinction that can be drawn between physics and metaphysics. Naturalistic metaphysicians' claims differ only in a manner of degree with those of scientists. Ladyman points out, though, that his verificationism is about epistemic value, not meaning (as the Logical Positivists had it). His naturalized verifiability criterion is based on there being an information channel between an object and the perspective of a recipient of the information about the object, rather than a traditional verificationist/empiricist *a priori* criterion based on observation. Ladyman asserts two methodological principles that guide and constrain this method:

> (1) The principle of naturalistic closure (PNC) which can be summarized as: any metaphysical claim that goes beyond what science delineates as empirically *investigatable* should not be taken seriously. Also: any metaphysical claim (if true) should show how

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[13] Ladyman (Ladyman and Ross 2007: 309, fn. 7) points out that, as a naturalist, he embraces fallibilism. Any metaphysical posits are open to revision in the light of new empirical developments.

[14] Ladyman gives a permissive, institutional definition of what a scientific hypothesis is: it is a hypothesis that a professional scientist could reasonably propose to a 'serious' funding source with some prospect of success (Ladyman and Ross 2007: 33).

two or more scientific hypotheses (at least one of which is from fundamental physics) jointly explain more than they do taken separately (Ladyman and Ross 2007: 37-8).[15]

(2) The primacy of physics constraint (PPC) which abridges as: special science hypotheses that conflict with fundamental physics should be rejected (44). This is taken to be a regulative principle in current science, and so should be respected by naturalistic metaphysicians.

Ladyman's two norms ground a self-termed ‘scientistic stance’, in which metaphysics is understood to be the "enterprise of critically elucidating consilience [or unificatory] networks across the sciences” (ibid: 28). The central aim is the attempted unification of scientific hypotheses 'on the basis of' physics. This construal of what metaphysics is supposed to be about has been controversial, since acceptance of OSR depends on one embracing a 'weak’ physicalism that is entailed in the PPC (special sciences must defer to physics). Many philosophers have a different conception of what metaphysics is, and will therefore dismiss the position out-of-hand. Also it is not clear how we are to get from this apparent aim of metaphysics to talking about ontology. Let us see if OSR can provide some answers as we explore the mostly nuanced arguments and themes that define this position.

2.1.2. The pessimistic meta-induction

According to Ladyman (2016), Worrall's ESR (introduced on page 14), upon which OSR is predicated, was introduced in its modern form solely as a realist response to the pessimistic meta-induction argument (PMI). First proposed by Hesse (1976), then Laudan (1981), PMI is a historical argument against realism. PMI notices that most scientific theories believed in the past have been shown to be false, and have been replaced by newer, better theories over time. By induction we can infer that our best theories of today will also be discarded, and replaced, in the future. Therefore the realist's claim that our best current scientific theories are true (or approximately true) is false. This is especially relevant, in the context of this thesis, *vis-à-vis* realist’s reference to unobservables when theorizing about scientific ontology.

Hesse (1976) goes even further; she argues that all scientific theories are false! She claims that it is the very ontologies of scientific theories that are most vulnerable to radical change during scientific revolutions. Scientific theories “cannot all be true in the same world, because they contain conflicting answers to the question ‘What is the world made of?’ Therefore they must all

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[15] Ladyman considers that "one metaphysical proposal constructed in accordance with the PNC is to be preferred to another to the extent that the first unifies more of current science in a more enlightening way" (Ladyman and Ross 2007: 66).

be false" (266). Ruttkamp-Bloem (2013) develops, as solution to PMI - a new historical criterion for establishing realism - which she calls 'evolutionary progressiveness'. This involves a notion of 'assembled', rather than approximate truth: a mid-way between subjective and objective epistemologies. Single theories are not true; truth is "a complex and dynamic notion which is the result of a network of reference relations all constantly 'revealing' aspects of the inaccessible domain at issue to various degrees of refinement" (227). If we follow Ruttkamp-Bloem, we never believe in the existence of unobservables *tout court*. Beliefs about inaccessible entities build up "over time, slowly, taking into account 'mistakes' as well as 'successes', piecing together aspects of these entities" (211-12).

Fine (1986) challenges the realist "to explain the *occasional success* of a strategy that *usually fails*" (119). The realist's generic fall-back on the notion of *approximate* truth in response (see Putnam 1975 and Boyd 1983), is unconvincing to Fine (and others). What this 'approximate' adjective is supposed to mean is never clearly articulated by realists. In reaction to PMI, realists must try to reconcile the historical record with some form of realism. They are, therefore, typically selective about what they are realist about - for example: only structure or entities - which are the bits carried through theory change.

Worrall (2000) understands PMI, not as an argument, but rather "a plausibility consideration, which in turn sets a *challenge*" (234). He has argued in response to PMI that, although there is theoretical discontinuity over time, what remains constant through theory change is the mathematical, or structural, content of our best theories. This epistemic structure reveals the way in which the entities in the domain of the relevant theory are related to each other even if the entities referred to by earlier theories are discarded over time. The equations of earlier theories are often 'carried over' in some way to later theories; "there is 'approximate continuity' of structure" (Worrall 1989b: 121). Realists can thereby, in Kantian fashion, have knowledge of, and commit doxastically to, this theoretical structure without any claims about ontology.[16] Ladyman adds that the:

> structuralist solution to [PMI] is to give up the attempt to learn about the nature of unobservable entities from science. The metaphysical import of successful scientific theories consists in their giving correct descriptions of the structure of the world (2016: n.p.).

Ladyman (2011) holds that part of the structure of the phlogiston theory of combustion, for example, survives into the modern theory of oxidation. Anti-realists, however, point out that the

[16] See appendix A for more on Kant as he relates to this thesis.

division between the content and structure of theories is never discernable beforehand. This is a poignant issue for structuralists who want to be naturalists, as most of them do. The structure discovered seems identifiable only in retrospect: the very part retained through scientific theory change . “The atoms are still there at some level, so that was structure. The ether is no longer there, at any level, so that was a mistake about content” remarks van Fraassen (2006: 290). Surely a naturalist’s general theory of epistemology or ontology shouldn’t have this postdictive character; we want bold, predictive theories. van Fraassen wonders if it isn’t:

> a little embarrassing to start with the thesis that what is preserved through scientific revolutions is the structure attributed to nature, and then to have to identify structure by noticing what has been preserved? (ibid: 303)

For van Fraassen, what is consistent through theory change is the body of empirical knowledge about the observable. It is knowledge that has been tested and retained - still accepted after a Kuhnian revolution as a triumph of past science. This empirical description is the stable, evolving surface structure of science - retained through scientific revolutions. Older theories were partially successful, continues van Fraassen, their representational models of observed phenomena were partially accurate. They got right the structure of the phenomena, where ‘structure’ is “a certain character, defined by certain measurable parameters both old and new theory use to describe those empirical successes” (ibid: 304). Science presents us with structure, and it is knowledge of this structure of the empirical phenomena that is consistent, and accumulating. Furthermore, there is:

> warrant for the assertion of an accumulation of empirical knowledge through theory change precisely if it can be demonstrated for phenomena counted among the empirical successes of earlier science that, if they are embeddable in the new models then they are ‘approximately’ embeddable in the old models (ibid: 305).

For Ladyman, PMI deconstructs all forms of non-structural realism. Embracing ESR, though, is only the beginning, Worrall's ontic agnosticism won't do. Ultimately, Ladyman will argue that ESR's epistemic/phenomenal structure is what it is because it is in fact the very ontic/noumenal structure of the world. For now, though, let us look at the other major objection to realism: the underdetermination (of theory by data) argument.

2.1.3. The underdetermination of theory by data

Developed out of work done by Duhem (1906/1954) and Quine (1953) this challenge to realism argues that, in the context of ontological theorizing, "any given set of empirical data is compatible with different theoretical accounts of underlying entities whose natures and behaviours might explain it" (Chakravartty 2017: 92). van Dyck (2007) understands the underdetermination argument as consisting of the following convenient syllogism:

P1. All theories have empirically equivalent rivals.

P2. Since empirically equivalent theories are equally supported by all possible evidence, they are all equally believable.

C. Therefore, belief in any theory must be arbitrary and unfounded (12-13).

We should, as such, withhold epistemic commitment to unobservables, and thereby withdraw into the anti-realist camp. A common realist response to underdetermination is to appeal to 'empirical virtues', such as: simplicity, novel predictive power, elegance, fruitfulness and explanatory power. This ostensibly offers a way to choose among empirically equivalent rivals. Ladyman considers the history of successful novel prediction in science to be the most compelling evidence for realism. He, though, worries that the empirical virtues sometimes pull in different directions, and there is no obvious way to rank them. Tulodziecki (2012) argues convincingly that even if realists had a complete, accepted list and ranking of these virtues, the details of epistemic equivalence are so complex that serious comparison between rival theories is impossible. This impasse gives rise to a new kind of underdetermination built on top of the original underdetermination.

Ladyman therefore acknowledges the strength of underdetermination, and recognises that it cannot be ignored by realists. He maintains, though, that it is the only positive (i.e. non-sceptical) argument the anti-realist has for preferring her position over realism. In fact, underdetermination equally applies to which theories are 'empirically adequate' (van Fraassen's criteria for theory success, intrduced on page 18).[17] Resultantly, underdetermination does "not seem unequivocally to support either inflationary realism nor sceptical antirealism", and therefore provides no "compelling grounds to abandon standard scientific realism" for now (Ladyman and Ross 2007: 82-83).

Inspired by Worrall's ability to cope with PMI, and the apparent ubiquity of underdetermination, Ladyman has been motivated to develop a robust version of structural realism. He also wants to use this position to deal with some perennial puzzles in the philosophy of physics, specifically to

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[17] For this reason, van Fraassen, himself, doesn't utilize either PMI or the underdetermination argument (although some of his anti-realist affiliates and supporters do). Referring to PMI, he is "proud never to have relied on [this] quite unacceptable argument" (2007: 347). van Fraassen does not think that realism is irrational, but rather "he rejects the 'inflationary metaphysics' which he thinks must accompany it, that is, an account of laws, causes, kinds, and so on" (Ladyman and Ross 2007: 98).

do with quantum mechanics (QM). Let us follow him as he slowly lays out his conciliatory account advancing OSR.

2.1.4. Quantum field theory

QM throws much of our intuitions, and therefore our analytic conceptual methods, upside-down. This is mostly due to what Fine (1986), inspired by Bohr (1958), labels ‘contamination’. It is impossible to measure the values of incompatible quantities (e.g. position and momentum) to arbitrary accuracy in the same QM experiment. Our interaction with the system disturbs it, giving results that are unavoidably contaminated. QM tells us how to apply a mathematical rule to calculate experimental outcome probabilities from a model, anything more seems to be metaphysical conjecture layered on top of the theory. Ruetsche (2017), using the simple case of a single particle of some mass moving in a straight line, explicates as follows:

> Classical mechanics assigns the particle a state by equipping it with precise values for its *position* on the line and its *momentum* along the line. All of the particle’s other properties are determined by its position and momentum. . . Thus, given the particle’s classical state, we can predict with certainty the values of all its other physical properties. . . By contrast, the quantum theory of our particle attributes it a state which is a *vector* in a vector space and associates position, momentum, and other properties (aka *observables)* with mathematical objects called *operators* on that vector space. Typically, the state vector does not fix the values of these observables but instead offers a probability distribution over possible values. Given a pair of quantum observables, there is usually a trade-off in the informativeness of the probability distributions the state vector defines over their possible values (294-95).

Ladyman urges that we should take seriously the revisionary metaphysical implications of QM, in that it "has shown us that the universe is very strange to our inherited conception of what it is like" (Ladyman and Ross 2007: 10). QM is our most fundamental and most successful theory. Classical physics cannot even explain the stability of macroscopic structures. QM uniquely explains and predicts these ‘bound states’: why atoms are stable, and why collections of atoms couple to one another. Any non-fundamental science uses and requires these stable structures, provided by QM, as a foundation. Ladyman has been particularly interested in two related landmark problems from the philosophy of QM, a conciliatory answer to which would be strong support for OSR:

> (1) The nature of identity and individuality of quantum particles and space-time points, and entanglement.[18]
>
> (2) The relationship between scientific representation and the world, notably the role of models and idealisations in physics (2016: n.p.).

Quantum field theory (the theoretical framework for constructing quantum mechanical models) is our most basic scientific theory, and so - as per the PPC (the primacy of physics constraint, introduced on page 23) - all other sciences answer to it in some way. Metaphysics naturalized, i.e. ontological theorizing continuous with science, must therefore also operate within this constraint. As the name implies, quantum field theory treats ‘the field’ as basic. Particles, *qua* individuals, are excitations in this field. Esfeld (2013) posits that if we embrace quantum field theory - as we should given its predictive success - then we must somehow make room for particle creation and annihilation. If we retain a commitment to particles, and if particles are substance, then we have to somehow allow for substance to come into and go out of existence.

Ontological commitment to unobservable entities - such as deterministic micro-particles, mereological atoms, relata (prior to relations) and space-time points - is problematic in the face of the well-known 'weird' implications of QM, *viz.* entities being in two places at the same time and light being both particle and wave for example. This weirdness results specifically from quantum entanglement and superposition.[19] McKenzie (2017) explains that:

> the generic state of a set of quantum particles is an entangled state, and the properties ascribable to an object in such a state are shared by all with which it is entangled. As such, no property - either intrinsic or extrinsic - can differentiate them (6).

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[18] Discovered (or created, if you like) by Schrödinger, quantum mechanically entangled states are “quantum states of a composite system that cannot be expressed as direct products of quantum states of the individual components” (Myrvold 2018: n.p.). Entanglement is a correlation between different subsystems of a system. Information about the system will be lost if the subsystems are separately analysed. “Entangled particles act as a unit. Properties of the entangled state. . . are non-supervenient on the properties of the parts and their spatiotemporal arrangement” add Briceño and Mumford (2016); “the very notion of discrete particular things seems to break down at this level” (199).

[19] See Esfeld (2004), who argues that the superposition of states implied by QM indicates entangled quantum particles have relations to each other, but no properties. A quantum superposition is a sum of two possible solutions to equations in QM - each corresponding to a different measurement outcome - which is itself a valid solution to the equations of QM, but never measured. The superposition of solutions gives a probability for the two single solution measurement outcomes. Quantum ‘collapse’ is when the probabilistic superposition of solutions becomes a single solution upon measurement.

The only way, according to OSR, to discern an object's distinctness is by the relevant relations in which it stands to other objects. Esfeld (2013) adds that, as per Bell's theorem,[20] the relations of quantum entanglement between objects cannot be reduced to anything other than a fundamental dynamical relation.[21] Ladyman concludes that QM "denotes a set of mathematically specified structures without self-individuating objects" (Ladyman and Ross 2007: 44) or classical locality. However, these abstract structures of modern physical theories furnish us with explanations and allow for novel predictions, and so must surely have some grip on reality.[22] This 'grip on reality' goes beyond a mere van Fraassian-style "description of the actual phenomena to the representation of modal relations between them" (Ladyman 1998: 418).[23] This theoretical move away from objects and properties towards an emphasis on structure and modality encourages, for Ladyman, a collateral metaphysical *gestalt* switch. We should shift towards a commitment to the notion of structure as basic, and also collapse the abstract/concrete distinction. At the level of fundamental physics there are no objects or properties; "the structural dissolution of physical objects leads to a blurring of the line between the mathematical and the physical'' (French and Ladyman, 2003: 41).

Some physicists view particles as peculiar kinds of individuals that do not obey Leibniz's principle of the identity of the indiscernible (see Forrest 2016 for a summary of Leibniz's influential principle). Others view particles as non-individual excitation events in the quantum field. Ladyman (1998) cites work by French and Redhead (1988) that appears to demonstrate how fundamental particles are, due to quantum entanglement, metaphysically underdetermined with regards to individuality versus non-individuality. Ladyman claims that "elementary particles are just sets of quantities that are invariant under the symmetry groups of particle physics". "[W]e have an invariant state under such transformations which represents the objective state of affairs" (421), viz. the structural ontology of fundamental physics. "Such sets of transformations may be

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[20] See appendix B for an explication of what Bell's theorem entails.

[21] Worrall (1989b) argues that the "enormous empirical success" of QM indicates that "the universe is (probably) something like quantum mechanical" (123), but since the nature of the quantum state is beyond our classical understanding, ontological agnosticism is the only sensible option. We should commit only to the epistemic, structural content of a scientific theory.

[22] Or, as Worrall puts it: "must somehow have latched on to the blueprint of the universe" (1989: 99). Saatsi (2017) adds that "[e]xplanations and explanatory reasoning are at the heart of the sciences" (201). The standard model of particle physics, for instance, "appeals to the Higgs boson and spontaneous symmetry breaking in order to explain why some particles have mass" (ibid.).

[23] Ladyman cites the Everettian, or many-worlds, interpretation of QM (Everett 1973) as a particularly salient example of this representation of modality in modern physical theories.

shown to form a group in the mathematical sense, and the group(s) under which a given equation remains invariant intuitively represents a structural feature of it", adds McKenzie (2017: 7). According to OSR, individuality does obtain, in some sense, at the classical 'level', described by the special sciences. It is, though, a manifestation of the 'lower' structure at the quantum level. Ladyman cites Bohr's 'correspondence principle' as justification: QM models ought to be mathematically isomorphic to classical models, providing a "structural continuity between classical and quantum mechanics" (Ladyman and Ross 2007: 95, fn. 24). In the same way, too, the indeterminacy (incompleteness in the description of a physical system) implied by QM at the microscopic level 'infects' the macroscopic (25).

QM demonstrates "the failure of our best theories to determine even the most fundamental ontological characteristic of the purported entities they feature", submits Ladyman, it would be "an ersatz form of realism that recommends belief in the existence of entities that have such ambiguous metaphysical status" (1998: 420). Quantum field theory cannot be described without recourse to idealized mathematical structure, and represents 'the quantum state', or 'the field', as ontologically basic. What is needed as a naturalistic response is an ontology that that has no notion of individuality: no particles or space-time points; i.e. structure.[24] Esfeld (2013) agrees that QM lends support to OSR. His interpretation holds that:

> whatever the distribution of matter in physical space may be, there is no possibility to account for its temporal development on the basis of local and hence intrinsic properties, given Bell's theorem (229).

The temporal development of matter in physical space is fixed in QM by a holistic, modal property "instantiated by the matter distribution as a whole and represented by the universal wave function" (ibid.).[25] This holistic property is a structure relating everything that makes up the distribution of matter in space. Ladyman (Ladyman and Ross 2007: 136, fn. 15) cites Brown et al. (1996) who have argued that the dynamics of quantum field theory are such that properties - such as mass and charge - normally associated with particles, are in fact inherent in the field. Chakravartty adds that "talk of 'particles' in this context is loose - it is a placeholder for whatever has the properties associated with them by the theory, like mass, charge, and spin" (2017a: 138). We can, however, perhaps maintain a particle centred view if we follow Bachelard in conceiving

[24] In reply, Ainsworth (2010) offers institutional criticism. He doubts that a coherent interpretation of QM, with relations as primitive, is possible. Even the most deflationary interpretations are committed to objects and properties in some way. While it may be philosophically elegant to "interpret quantum field theories using an object-free ontology. . . no one has even sketched a coherent interpretation using a property-free ontology" (53).

[25] See appendix C for an extended discussion on the wave function.

of instantiated matter as sudden transformations of energy. See his (1934/1968: 61-63). Bitbol (2007) understands there to be two options available to those who want to preserve a particulate worldview:

> (1) Embrace a pragmatist attitude - like most physicists, who talk of 'particles', but with many qualifications.
>
> (2) Try to argue for a hidden variables interpretation of QM. This option "has some value as a prop for intuition [but] it is 'metaphysical' in the most speculative sense. . . its 'surplus structure' is immune to empirical test" (246).

Einstein wrote that physics "is the attempt at the conceptual construction of a model of the *real world*, as well as its lawful structure" (Letter from Einstein to Schlick, November 28, 1930. In Fine 1986: 97). General relativity, which identifies the fundamental gravitational field with the metrical structure of space-time, also suggests to Ladyman a foundational role for his structure concept (Ladyman and Ross 2007: 141). Since ontologically basic space-time points, i.e. mereological simples, are ruled out by the arguments on pages 28 to 30, the entities of physical theories cannot supervene on them. The identity and individuality of entities and their properties must be secondary to, and embedded somehow in, the fundamental relational structure of space-time. In quantum gravity, background space and time are themselves treated as quantum objects, instead of *fixed*, as in standard QM.[26] If quantum gravity can be coherently formalised, as many physicists believe it will be, then Ladyman's argument would apply here too.

Halvorson (2016), points out that Einstein's general theory of relativity isn't just a list of claims. Instead, it provides a collection of models describing, for example: the expansion of the universe, the collapse of stars and the orbits of satellites around the Earth. For Halvorson:

> general relativity shows that these situations have common structural elements; it systematizes these diverse phenomena, providing us with an efficient means of generating predictions (138).

[26] Standard QM, or what is institutionally termed 'the operational point of view', is not an interpretation of QM. It is the text book standard, concerned only with the pragmatics of quantum systems (measurements, predictions and so on). In popular books the 'Copenhagen interpretation' is often stated to be the standard view, but professional scientists generally dismiss this notion. Firstly, because the standard view does not involve *interpretation*, and, secondly, because Bohr's so called Copenhagen interpretation - as it was originally formulated - is not taught or used in current physics. Ladyman prefers the many-worlds interpretation of QM, since it is the interpretation that is most consistent with his structural/modal ontology. van Fraassen - being an empiricist, without need for interpretations - works with the standard view, as formulated by Born (1926/1983).

Repeatedly, while probing the implications of QM and general relativity, Ladyman comes to a bed-rock of non-individuality, non-determinism and non-locality. French (2006) adds that, in OSR, quantum structure "can be considered to be 'causally empowered'" (98), i.e. inherently modal. All the pathways conciliate towards a notion of structure - specifically modal structure - as basic, even primitive.[27] Modality is, according to Ladyman (*and* van Fraassen), the crux of scientific realism. Modal (and thereby causal) realism is architectural to taking Ladyman's OSR argument to the next stage. Let us, therefore, take a closer look at how it facilitates his audacious 'everything is structure' ontology.

2.1.5. Modal objectivity

Ladyman claims that, as per QM, "[i]ndividual things are locally focused abstractions from modal structure" (Ladyman and Ross 2007: 153-54). Modal structure is to be understood as the relationships among phenomena that pertain to causation, possibility, potentiality, probability and most importantly: natural necessity *qua* the laws of nature (over and above regularities). Ladyman's structure is, in other words, naturalized, modal, nomological structure. This juxtaposes to Humean generalization-over-regularities, which "focuses on generic causal relations between types of entities and events", rather than singular causation (Berenstain and Ladyman 2012: 5). Ladyman argues that Humeanism cannot be merged with realism as some have tried to do (see Psillos' 2003 hybrid). Even the best account of Humeanism - the so-called Mill-Ramsey-Lewis view - is:

> plagued with problems. . . it cannot be formulated non circularly, it cannot explain why inductive inference ought to be successful, and it cannot offer a satisfactory account of laws in the special sciences (Berenstain and Ladyman 2012: 17).

A structure's existence consists in its playing a certain generalizable, nomological role. For Ladyman, our metaphysics must account for how novel predictive success can occur in science. The explanation he favours is that it is because the objective modal structure of the world is being tracked and projected forward. Objective in the sense that facts about this modal structure do not depend on our doxastic or epistemic states. For Ladyman (Berenstain and Ladyman 2012), "the laws of nature are what they are because of how the world is independently of the way we as conscious subjects describe it" (2). Objectivity consists of finding "prediction-licensing and counterfactual supporting" generalizations (Ladyman and Ross 2007: 229). Ladyman argues that

[27] Ladyman (Ladyman and Ross 2007) also makes the institutional observation that practicing physicists routinely entertain the idea of modal structure, ungrounded in the nature of fundamental entities. This is *prima facie* "PNC-compatible evidence that OSR should be taken seriously" (188).

'existence' has traditionally been understood to explain the phenomena, but - in fact - what does the explanatory work is stability as part of the world's modal structure, plus our capacity to make ampliative inferences and projections there from.

"Scientists always look for theories of the observable, not the observed", notices Ladyman (2000: 852). Scientific theories are ubiquitously modalized, allowing for "a variety of different initial conditions or background assumptions rather than just the actual ones, and so describe counterfactual states of affairs" (Ladyman and Ross 2007: 110).[28] Different formulations of the correspondence between empirical data and theory may lead naturally to new empirical discoveries of modal relations in nature. The mathematical modality in scientific theories represents the objective modal structure *qua* the fundamental nature of reality. It is manifested in patterns in empirical phenomena (both possible and actual), going beyond what is 'observable', in the van Fraassian sense.[29] Take, for example, the cosmic speed of light. Physicists state that material bodies *cannot* go faster than light, not just that they do not go faster than light (Berenstain and Ladyman 2012: 2). Central to OSR is the hypothesis that science provides a unified account of the world by representing structures that modally constrain our inferences from phenomena. As per the PPC: "The modal structure studied by fundamental physics constrains the modal structure studied by the special sciences" (Ladyman and Ross 2007: 299). See also French (2010), who understands structure to be a "'web of relations' [that is] inherently modal and, in particular, causal" (92-3).

Ladyman denies that *a priori* inquiry can give an account of metaphysical modality. He argues that realists - as naturalized metaphysicians - are, *contra* Hume, committed to modal objectivism because they care about (non-miraculous) explanation. Realists want scientific theories to explain the regularities in phenomena, while van Fraassen - only wanting to commit to what is observable, and treating regularities at brute - takes a nominalistic stance towards modal

[28] The burden of proof is, therefore, on the sceptic about objective modality and natural necessity. She "must offer some positive account that makes sense of the ubiquity of modal notions in science" (Berenstain and Ladyman 2012: 4).

[29] van Fraassen insists that contra OSR: "[t]he locus of possibility is the model, not a reality behind the phenomenon" (1980a: 202). Although he is agnostic about realism, van Fraassen is an atheist about modality. There is no objective causality or necessity in the world; these are artefacts of scientific representations. There is, therefore, a *pima facie* tension in van Fraassen's view. He holds that unobservable entities may exist, but that the laws relevant to them certainly do not.

notions.[30] Only when scientific hypotheses 'latch onto' modal structure are they genuinely explanatory, insists Ladyman. Hypotheses have explanatory power when they predict what behaviour is *possible* for entities (both observable and unobservable) and what unobservable behaviour will *necessarily* cause what observable phenomena (Berenstain and Ladyman 2012).[31] Also, realists who incorporate laws, causes or possibilities in their explanations of science are committed to a metaphysics of modality. OSR, for example, entails a commitment that there are mind-independent modal relations between phenomena, but these "relations are not supervenient on the properties of unobservable objects and the external relations between them. Rather, this structure is ontologically basic" (Ladyman and Ross 2007:128), and "relata are constructed as abstractions from relations" (154). This promotion of relata as secondary to, or dependant on, relations is a crucial move that Ladyman makes in his book (Ladyman and Ross 2007). Before this book OSRists had generally argued for the conceptually problematic (and much criticised) idea of relations *without* relata.

To sum up, OSR involves a commitment to structural, physical modality: most notably natural necessity *qua* the laws of nature. Ladyman asserts both descriptive and normative claims when it comes to his account of modality. He states: "[w]e may know little else about the nature of reality but we are warranted in supposing that it has a modal structure" (ibid: 106). Furthermore, the focus of the scientific realism debate *should* be on providing "an account of the relationship between the modal structures found in scientific theories at different ontological scales" (2018: 105). This spirited attitude leads us conveniently to our next topic: Ladyman's attempt to develop a unified metaphysical picture that supports both a scale-relative ontology, as well as an emergence of the special sciences' ontology of individuals, causation and laws from within the OSR framework in fundamental physics.

2.1.6. Real patterns/rainforest realism

Ladyman has been cited a number of times in this thesis as insisting on a deference of the special sciences to fundamental physics. He gives an operational definition of a special science as follows:

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[30] Ladyman muses that, all things considered, perhaps OSR "ought to be understood as modal structural empiricism" (2007: 99).

[31] Ladyman (Berenstain and Ladyman 2012) also notices that cosmologists regard models of GR's field equations, such as Minkowski space-time, as *possible* ways some universe could be (17).

> a science is special iff it aims at generalization such that measurements taken only from a restricted area of the universe, and/or at restricted scales are potential sources of confirmation and/or falsification of those generalizations (Ladyman and Ross 2007: 195).

This contrasts to fundamental physics, which aspires to completeness and to making predictions that could, in principle, be falsified from anywhere in the universe. Ladyman calls this deference of the special sciences to physics a 'weak physicalism'. He doesn't believe that the laws of the special sciences are in practice reducible to those of fundamental physics, since such derivations are computationally intractable. Special science laws sacrifice some exactness by being simplifications of fundamental laws and allow for relatively accurate predictions at the macro-scale.

Ross (2000) adds institutional support for 'weak physicalism', by observing that "no special science has ever sanctioned generalizations that violate the fundamental generalizations of the physics of its day" (154). While the laws of the special sciences may in principle be derivable from the laws of fundamental physics (being quantum field theory), Ladyman argues that naturalists should respect the special sciences capacity to succeed in discovering laws of nature, offering good explanations, and making successful predictions without direct reference to physics (Berenstain and Ladyman 2012). Let us take a look at how Ladyman tries to give a unitary, yet scale-relative,[32] structural account of scientific ontology, somehow 'constrained' by quantum field theory.

Ladyman extends OSR - as we've understood it up to now - out of fundamental physics and into the special sciences by embracing the twin concepts of Dennett's 'real patterns' and Ross' 'rainforest realism'. Dennett's (1987) 'real patterns' hypothesis was originally formulated in the context of an argument against eliminativism about the mind. It centres around propositional attitudes, involving beliefs and desires, which are neither concrete objects, nor merely nominalistic notions. They are abstract patterns of, in principle, *compressible* information visible from the 'intentional stance'.

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[32] By scale relativity Ladyman means that "terms of description and principles of individuation we use to track the world vary with the scale at which the world is measured" (Ladyman and Ross 2007: 199): with coarser versus finer degrees of resolution. Epistemic scale-relativity is not controversial, says Ladyman, the various pragmatic stances adopted by special scientists, working with their relevant theories, involve descriptions and principles that track the world as a function of varying scales of measurement. However "[s]cale relativity of ontology is the more daring hypothesis that claims about what . . . exists should be relativized to. . . scales at which nature is measurable" (Ladyman and Ross 2007: 200).

Dennett's famous intentional stance involves taking a deliberate epistemic perspective towards an object, in which one treats it as a rational agent. This allows us to predict how "this rational agent will act to further its goals in the light of its beliefs" (1987: 17). He also talks of a scientist adopting a 'predictive strategy/stance' when recognising, then using, real patterns to make successful, novel predictions. Dennett holds that a "pattern exists in some data - is real - if *there is* a description of the data [i.e. an algorithm] that is more efficient that the bit map[33] whether or not anyone can concoct it" (1991: 34). In other words, there are real patterns that no-one has discovered, and may never be discovered. "These patterns are objective - they are *there* to be detected" (39), and our capacity to recognize, then use, them to make successful predictions from the relevant stance motivated Dennett to tentatively embrace what he calls 'mild realism'.

Ross (2000), in turn, modifies Dennett's theory by reading it as a generalized exercise in fundamental ontology. Propositional attitudes are only a special case of existence more generally, and the intentional stance is only a special case of a broader perspectivism. Dennett thinks of real patterns as abstracta, distinct from illata, and his theory has therefore often been interpreted as an instrumentalist one. Ross, though, collapses Dennett's abstracta/illata distinction by arguing that both reductionism and supervenience are untenable. So, if patterns are real but not dependent on anything more basic, then there are just patterns and more patterns, and all real patterns are ontologically equal. As such, consistent with both naturalism and realism, Ross leaves us with the 'lush' universal ontology of *rainforest realism*: consisting of encodable real information patterns in the world; patterns that are tracked and projected by the various sciences to make novel predictions.

For Ladyman, a real pattern is "something that makes for a simplified description relative to some background ontology" (2018: 103). At a glance we can see why this real pattern idea would appeal to him. He sees something special about the notion of real patterns as embedded in rainforest realism: something that looks potentially convergent with his own modal structures from QM. He sets out to collate the two: are theoretical/mathematical structures identifying real patterns in the modalities of nature?

We have already followed Ladyman in thinking about scientific theories and phenomenological data as fundamentally structural, but how will he fuse structure with real patterns? Let us now follow his consilience argument in some detail. Consistent with his naturalism, Ladyman thinks of Dennett's intentional stance as a special case of scale-relative perspectives in science. These

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[33] A bit-map is a direct encoding mapping (one-to-one) each bit of information in some initial data to a bit in the encoding, without compression. In other words, a bit-map is a replica of what it is a map of.

express metaphysical facts about the way in which reality is organized. He labels this the *scale relativity of ontology* (Ladyman and Ross 2007: 199). Ladyman further interprets real patterns as relatively stable and enduring macroscopic objects emerging from within the structure of the quantum state of the world (2010: slide 43). Reality does not 'bottom out' at some fundamental level of individuals (such as quarks or leptons), in fact there are no 'levels of reality' in OSR.

Rainforest realism accommodates this scale relativity of ontology. The various sciences track and project the mind and theory independent real patterns that are their subject matter. This allows for the kind of non-compositional unity of science (i.e. without reduction or supervenience) that Ladyman is looking for. Real patterns are also carried over from old to new theories, says Ladyman, making the apparent problem of reference over theory change (a.k.a. PMI) a red herring. That said, and even though all Dennettian-style patterns are real and autonomous on Ladyman's interpretation, there is an asymmetry among the real patterns across the scales of the sciences - as per the weak physicalism of the PPC. Fundamental physics has 'universal' validity, in that it studies real patterns "about which measurements taken *anywhere in spacetime at any scale of measurement* carry information" (Ladyman and Ross 2007: 251). The special sciences, on the other hand, discover and refine predictive and explanatory generalizations, such that measurements taken only from a restricted area or scale of the universe are relevant.

Special science generalizations, made from the relevant scale-relative stances, must respect negative implications of physical theory. If fundamental physics indicates that there are no individuals, then the special sciences must - for the sake of unification - admit of "an ontological interpretation that is compatible with a non-atomistic metaphysics" (ibid: 190). It is acceptable for the special sciences to invoke entities and processes from the more fundamental sciences in explanation, but not vice versa. Dennett's real patterns allows Ladyman a plausibly unified account (within the constraints of the PPC) of scientific ontology that can support both scale relativity, and a principled distinction between the special sciences and fundamental physics. For Ladyman, Ross' 'universal real patterns' are the modal structure of the universe, since they are there to be discovered (i.e. are *discoverable*) and are used by scientists to make novel predictions from their scale-relative stances. There is the physics stance, the chemistry stance, etc. all the way 'up' to economics. Also included is the 'everyday' or 'common sense' stance: "[t]ables are particularly robust real patterns because they allow us to make predictions about many other real patterns, for example, about light distributions and about mass distributions" (Ladyman and Ross 2007: 256). Empirical theories are successful when their structure represents real patterns in the modalities of nature. We can commit to particulars, essences, kinds and to causation as perspectival devices: heuristics used by observers to track macro-level phenomena.

Real patterns - of which individuals, essences etc. are properties - determine what kinds of objects are legitimate subject matter for the relevant special sciences. Some real patterns “behave like things, traditionally conceived, while others behave like traditional instances of events and processes" (Ladyman and Ross 2007: 121). Some patterns may be conceptualized as individuals from the perspective of one special science, while simultaneously not from another.[34] Ontology is scale-relative for Ladyman, in the sense that “different energy levels and regimes, as well as different length and times scales, feature different emergent structures of causation and law” (2018: 103). Some real patterns are mathematically represented as individuals, others as perhaps a run of data, depending on the stance of the scientist. Certain of these patterns in the world are only visible at the scale of the relevant special science stance.[35] If "you don't see them you are missing something about reality and that is good enough to allow us to say that the objects, properties and processes described by the special sciences are real" (2010: slide 45). Real patterns, tracked over time by scientific representation, are used to make novel predictions - becoming individuated as ontological distinctions in terms of objects, events, properties and processes - scale-relative to the various sciences.

Ladyman concludes his promotion of rainforest realism by declaring that from:

> the metaphysical point of view, what exist are just real patterns. To put the point another way, to define ‘real pattern’. . . is to say everything there is to be said about the criteria for existence. Science motivates no separate metaphysical theories about objects, events, and processes" (Ladyman and Ross 2007: 121).

Rainforest realism, in the form of the real patterns account of ontology - merging now with OSR - offers Ladyman a potentially unified solution to standard problems for realism, such as the relationship between:

> (1) Scientific realism and common sense realism: everyday objects, tables and chairs, are real patterns too.
>
> (2) Past and current theories: real patterns are consistent across theory change, as structures carried over.
>
> (3) The sciences of different scales: special science stances track and use their relevant real patterns to make novel predictions.

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[34] Ladyman emphasises, though, that these special science real patterns have nothing to do with the traditional notion of epiphenomena, such as qualia. For him, qualia are not even mere patterns - never mind real ones.

[35] Ladyman defines a pattern as "just any relations among data" (Ladyman and Ross 2007: 228).

> (4) The ontologies of the special sciences and fundamental physics: real patterns ontologies of the special sciences are constrained by the real patterns ontology of fundamental physics, via the regulatory principle of naturalistic closure (PNC) and the primacy of physics constraint (PPC). (Ladyman 2018: 100).[36]

Ladyman's representational structures (as mathematical relationships) and Ross' rainforest realism (entailing real patterns) are starting to look conceptually convergent. Why, though, isn't all of this merely an instrumental, pragmatic or epistemic method, as van Fraassen might suggest? We turn now to information theory in order to answer this question, and to complete Ladyman's over-arching abduction.

2.1.7. Information, compressibility, projectibilty and perspective

The laws of the special sciences can be understood as tools of data compression, claims Ladyman, and the ontology of macro-objects as a function of the necessary theoretical coarse-graining in the special sciences: "the recovery of statistical properties of low-level entities when tracking high level ones" (2010: slide 44). This course-graining is a form of algorithmic data compression that allows for information projection, *qua* novel prediction, though with some loss of fidelity. Real pattern is fundamentally a computational notion, submits Ladyman, and the special sciences are possible because the world is to some extent algorithmically compressible. In other words, the real patterns idea is:

> based on the compression of data and the reduction of information processing made possible by a high level description of a system that could in principle be described at a fine-grained level but at a much greater computational cost (Ladyman 2010: slide 47).

As mentioned above (pages 35 to 37), Dennett and Ross, conceive of real patterns as compressed information regularities described by an algorithm more efficient than the bit-map representation of the data from which the pattern could be computed. Structural information can be attributed directly to real patterns, as well as to representations of them. Let us follow Ladyman now as he lays out his argument for a coalescence of the OSR and rainforest realism themes into what he calls *information-theoretic structural realism* (ITSR):

> OSR is the hypothesis that science provides a unified account of the world by modelling structures that modally constrain inferences from measurements. [Rainforest realism] is the metaphysical theory of the relationship between these structures and extra

[36] Ladyman also submits that the perennial philosophical problems of 'vagueness of composition' and 'identity over time' are also dissolved by rainforest realism. The "real-patterns account of ontology offers a unified solution for both problems in all cases" (Ladyman and Ross 2007: 104).

> representational real patterns. ITSR is their conjunction (Ladyman and Ross 2007: 251-52).

ITSR is stated in terms of the interrelation of four core concepts we will explore as we go along: information, compressibility, projectibilty and perspective. Ladyman sees a synchrony between the formalism of QM and the computational motif in rainforest realism, in that QM makes use of non-epistemic, irreducible probabilities, in the same way that information theory does.[37] Information is apt for characterizing the modal nature of QM because it is itself a modal concept; "information is a fundamental concept for understanding the objective modality of the world, for example, laws, causation, and kinds" (Ladyman and Ross 2007: 189).[38] Consistent with his attempts thus far to show a fusion between the ontic and the epistemic, Ladyman tentatively endorses what he calls Zeilinger's (2004) ‘information-theoretic fundamentalism’: "it is impossible to distinguish operationally in any way reality and information. . . the notion of the two being distinct should be abandoned" (Zeilinger 2004: 219). To represent the ontology of the world, according to scientific theory, is nothing more than to present the equations, and to explain the model that is thought to describe the actual world. Reality has no *nature* underlying its represented structure. There is nothing more to be said than:

> mathematical structures are used for the representation of physical structure and relations, and this kind of representation is eliminable and irreducible in science (Ladyman and Ross 2007:159).

Matter, as substance, has become increasingly nebulous in modern physics, and the dependence of physics on ideal entities and models argues against the abstract/concrete distinction. Ladyman, though, rejects that this may involve the postulation of some new basic substance: some ‘info-stuff’. The PNC and PPC normatively regulate that one's metaphysics remain naturalized. Ladyman admits that there are many interpretations of 'information', but settles on the notion of algorithmic compressibility, or logical depth, as appropriate for structural models of real patterns in the context of metaphysics. Certain regularities in the world, manifested over time as compressible data sets (the phenomena), are represented in algorithms (laws, models or theories)

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[37] Ladyman stipulates that the relevant sense of information for his theory is the Shannon-Weaver notion from communication theory, but modified to incorporate the non-thermodynamic abstract informational content of the sort relevant to his view. The Shannon-Weaver model consists of five elements: information source, transmitter, channel/s, receiver and destination. (Noise, is a dysfunctional sixth factor.)

[38] Information-processing always requires some physical system to run whatever computations, and is therefore subject to environmental entropy. In other words, this system will necessarily be constrained by the 2nd law of thermodynamics, implying that a Ladymanian universe is time asymmetric.

by scientists (or computers) who try to find a suitable compression that expresses the projectable information-carrying regularities involved. The right amount of compression of data from a relevant stance, or scale of resolution, will lead to maximum novel predictive power. If further compression of data is possible, without losing predictive power, then the stance can be discarded, or modified. Indispensability of a stance is equivalent to the impossibility of further data compression. Pattern reality, for Ladyman, is conditional on the indispensability of a stance, viz. the impossibility of further compression, by any possible physical agent (person, alien or computer).

Ladyman endorses Ross' (2000) two necessary and sufficient conditions for pattern-reality: to be is to be a real pattern, and a pattern is real iff:

> (1) It is projectable.[39]
>
> (2) It has a model that carries information about at least one pattern in an encoding that has logical depth less than the bit-map encoding of the pattern,[40] and where the pattern is not projectable by a physically possible device computing information about another real pattern of lower logical depth than the one at hand (233).

Regarding condition 1, any correlation of phenomenological data points is a pattern, explains Ladyman, but real patterns are those that can be reliably projected forward, and counterfactually generalized to unobserved cases (from the relevant perspective). Real patterns are "entities of whatever ontological category that feature (non-redundantly) in projectable regularities" (Ladyman 2018: 103-4). Featuring in projectable models and/or theories is taken to be the criterion of reality. Projectability, in this computational modal context, is a special kind of information-carrying possibility, or 'information flow', from the past into the future. It is information-carrying possibility "relevant to properties of information structures that warrant inference to real patterns" (Ladyman and Ross 2007: 224). Projectability can be recursively applied to - not only models of real patterns - but to real patterns themselves. Real patterns carry information about real patterns, carry information about real patterns, and so on.

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[39] Ladyman gives the following definition of 'projectibility': "just better-than-chance estimatability by a physically possible computer running a non-trivial program" (Ladyman and Ross 2007: 224).

[40] 'Logical depth' is a quantitative measure of informational non-redundancy in a model, while the bit-map is a maximally redundant representation of the model. Bennett (1990) the originator of the concept, defines logical depth as a quantitative index of the execution time required to generate the model of a real pattern "by a near incompressible universal computer program [a Turing machine], that is, one not itself computable as the output of a significantly more concise program" (142).

Condition 2, stripped of the technical formalism, expresses the requirement that conditions for pattern reality respect parsimony. Ontological commitment is restricted to "what is *required* for a maximally empirically adequate science" (ibid: 234). Real patterns must be generalizable; further compression of a *real* pattern should be impossible for any physically possible computer (at any location/from any perspective), without sacrificing projectability. Redundant patterns that bring no informational gain, or are just arbitrary combinations of other patterns, are excluded by condition 2. Ladyman, with a nod to Occam, regards as real only those patterns that "minimize the overall logical depth we attribute to the world while nevertheless acknowledging all projectable correlations" (ibid: 231).[41] To "be real a pattern must be informationally non-redundant, [if] counterfactual-generalization-supporting information is not to be lost" (ibid.). Condition 2 is stated in a way that allows for a real pattern to encode itself. This boot-strapping obviates the need for grounding/supervenience, and thereby avoids talk of a fundamental level metaphysically prior to real patterns. Fundamental real patterns encode the whole universe, according to ITSR.

Existence claims, in the context of ITSR-style metaphysical theorizing about scientific ontology, are a function of informational logical depth/algorithmic compressibility, as a property of mathematico-structural representations of inferred, and projectable, real patterns *simpliciter*. There is, consequently, a distinction between *mere* patterns of correlation and ontologically *real* patterns (as delineated by rainforest realism's condition 1). ITSR denies that real patterns are grounded in, or supervenient on, self-subsistent individuals. From an ontological point of view, mere correlations (i.e. non-projectable non-real patterns) exactly resemble individuals. The notion of individuals is a practical convenience understood by reference to the properties of real patterns. The world is not made of any *thing*, "individuals are resolved out of patterns. . . it's real patterns all the way down" (228-29). Phenomenal regularities are mathematical structures, are real patterns, are modal information, are existence *qua* being.

Summarily, concludes Ladyman, as per the verificationist PNC: "nothing else about existence in general should be said" (ibid: 228). Metaphysical speculation about reality into realms beyond what is, in principle, *investgatable* by science are ruled out. Alternatively, in information theoretical terms: inquirers should remain silent about domains of inquiry from which they are informationally disconnected (309). Ladyman believes that his fusion of OSR (from fundamental physics) with rainforest realism (entailing real patterns), via analysis of projectability as

[41] A pattern assembled arbitrarily out of some other real patterns, for example "my left nostril and the capital of Namibia and Miles Davis's last trumpet solo", is not a real pattern because "it increases logical depth in exchange for no gain in projectibility." (Ladyman and Ross 2007: 232).

facilitated by logical depth, incorporated into a scale-relative ontology, gives a robust account of the unity of science. ITSR, thereby, completes the grand conciliatory, metaphysical project with which we begun this section of my thesis. Since we've taken the aim of metaphysics - as ontological theorizing - to be the unification of scientific hypotheses, and since information-theoretic structures do this, that's the end of the story so to speak.

2.1.8. Conclusion - OSR

Although Ladyman's position is often referred to as *ontic* structural realism, the most recent incarnation is the above argued for, *information-theoretic* structural realism.[42] I will, however, as is the norm in the literature, continue to refer to the view as OSR. Ladyman (Ladyman and Ross 2007) states that his single most important idea is "that to take the conventional philosophical model of an individual as being equivalent to the model of an existent mistakes practical convenience for metaphysical generalization" (229). In other words: the non-existence of individuals and essences stipulates a generalization to regularities, patterns, information or just *structure*. On my reading, North (2009) gives the best summation of Ladyman's project:

> Take the mathematical formulation of a given theory. Figure out what structure is required by that formulation. This will be given by the dynamical laws and their invariant quantities. . . Make sure there is no other formulation getting away with less structure. Infer that this is the fundamental structure of the theory. Go on to infer that this is the fundamental structure of the world (80).

Ladyman rejects the traditional conceptual dichotomies between science and metaphysics, observable and unobservable, abstract and concrete, formal and physical, relata and relations, epistemic and ontic, phenomenal and noumenal and between substance and structure. He insists on explanatory and predictive utility before admitting something into his ontology. Ultimately he views his OSR as, at heart, a commitment to verificationism, rather than to one or the other side of the traditional realist versus anti-realist stand-off.[43] At any rate, one may challenge his emphasis on explanation, projection, and perspectives in the context of metaphysics. One may ask: yes, but what is really *really there*, independent of representation? Ladyman replies that:

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[42] Ladyman's philosophy has been carefully cobbled together over two decades involving many texts and collaborations. His method has not followed the reader-friendly step-by-step dialectic I have narrated here; some nuance has therefore surely been lost. I am though confident that I have captured the gist of his worldview and hopefully offered a fair and respectful representation. (The same goes for van Fraassen and Fine to follow.)

[43] Ladyman is, though, ubiquitously interpreted, by other thinkers in this debate, as falling on the realist side of the fence.

> What makes. . . structure physical and not mathematical? That is a question that we refuse to answer. In our view, there is nothing more to be said about this that doesn't amount to empty words and venture beyond what the PNC allows (Ladyman and Ross 2007: 158).[44]

In the next section I will present some common criticisms and apparent problems with the OSR position.

2.1.9. Standard problems with OSR

Ladyman's theory of scientific ontology has received much perplexed, and often dismissive, attention from other philosophers of science. He has thematically offended both analytic metaphysicians and empiricists. Ladyman (2016) recognises seven common complaints against OSR (most are also relevant to Worrall's epistemic structural realism). I present this as a descriptive list, rather than an evaluation. In the next section I will isolate two further issues for more detailed commentary. (I will also follow this format at the end of the sections on CE and NOA.)

2.1.9.a. Structural realism collapses into standard realism

Psillos (1995; 2017) and van Fraassen (2006) have highlighted, and explicated, this problem most saliently. Psillos argues that, despite its declaration of 'everything is structure', OSR - in order to be coherent - must presuppose a distinction between the form (structure) and the content of a scientific theory. Otherwise, what is the structure of a theory contrasted to? If there is no demarcation between structure and non-structure, then how can belief be restricted to structural claims? Also, "mathematical equations alone - devoid of their physical content - [cannot] give rise to any predictions" (Psillos 2017: 30). If the OSRist claims that it is only mathematical structure that is retained over theory-change, then she cannot make sense of the predictive success of science. Psillos concludes:

> The only way out is for structural realism to abandon pure structuralism and to treat structure as being defined by real or natural *relations*. Having first specified these natural relations, one may abstract away their content and study their structure. But if one begins with the structure, then one is in no position to tell *which* relations one studies and *whether* they are natural or not (ibid: 31).

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[44] Einstein, in a letter to Schrödinger, wrote:

> the real problem is that physics is a kind of metaphysics; physics describes 'reality'. But we do not know what 'reality' is. We know it only through physical description (Letter from Einstein to Schrödinger, June 19, 1935. In Fine 1986: 125).

Similarly, van Fraassen (2006) complains that if structure is all there is to nature then any difference between OSR and standard scientific realism also disappears. “It seems then that, *once adopted*, it is not to be called structuralism at all! For if there is no non-structure, there is no structure either”, he declares, “nature needs to be entirely re-conceived” (292-3). Furthermore, for van Fraassen, the OSRist’s primitive concept of ‘structure’ is very vague. What exactly is the difference between matter, content and structure? Is there “really an objective difference in nature, as opposed to merely in our representations of nature?” (303).

2.1.9.b. Isn't structure also lost in theory change?

If OSR won't delineate structure, then it cannot claim - as it does - that only the structural parts of theories are continuous over theory change. Psillos (ibid.) adds that OSR gains no advantage over standard realism against PMI, because it fails to make any distinction between structural and non-structural parts of theories. In the same vein, Chakravartty (2004) asks of OSR: why aren't mathematical structures, like entities, also lost in theory change?

2.1.9.c. Structural realism is too metaphysically revisionary

Many have questioned whether OSR's apparently naturalized metaphysics doesn't, in fact, entail implicit *a priori* posits. Furthermore, can Ladyman abandon core axiomatic mereological concepts (such as identity and individuality), the way he does, when they are so central to our intuitions? A *prima facie* tension seems to exist between a naturalist’s general deference to science on ontological questions and her embrace of metaphysics as a method by which we gain knowledge of the fundamental nature of reality. Naturalistic metaphysicians, like Ladyman - when convenient - appeal to intuition, thereby sometimes sneaking analytic metaphysics into their discourse (see Chakravartty 2017a and Mumford 2017). Also, points out van Fraassen (2002), the failure of OSR-style metaphysics to be properly falsifiable is problematic given Ladyman’s naturalism.

2.1.9.d. Structuralists can't account for causation

Chakravartty (2003), among others, has argued that Ladyman can't account for change: how the world gets from one concrete instance of some set of relations to some other state of affairs. OSR doesn't have the ontological 'clout' to explain what constitutes the active principle or mechanism that transforms one set of relations into another. Traditionally individual objects are considered to be the locus of causal or dispositional 'oomph', but if OSR treats these 'nodes' as mere heuristic devices, then it has no explanatory resources for change in the world. In order to locate causal efficacy, OSRists posit a dubious new primitive: spacio-temporal relations-in-themselves.

2.1.9.e. Why do certain properties and relations tend to cohere?

According to this objection, again from Chakravartty (2003), OSR cannot account for why certain properties have the disposition to regularly clump together - why they are ‘sociable’. He considers this *empirical* discovery to be the best reason for thinking of objects as ontologically significant, and concludes that OSR can't give an explanation for this sociability of properties, since it doesn't have a robust notion of dispositionality in its framework.

2.1.9.f. Structural realism only applies to physics

Newman (2005) has pushed against the scale-relativity of theoretical structure Ladyman promotes by arguing that the mathematical nature of the modal structure in OSR only applies to the highly abstract models from physics. The nonmathematical theories of the special sciences have surely retained certain features through theory change, but OSR only allows for mathematical structure to be carried over; it "fails precisely because it is limited to only the mathematical sciences" (1377). Votsis (2017) agrees that many social science theories are not mathematised and cannot be said to posit unobservable entities. For him, these facts raise serious doubts, and can be added “to the long list of issues that need to be resolved before any concrete answers can be given” about structural realism (117).

2.1.9.g. Collapse of the mathematical/physical distinction

The last criticism is the most often repeated in the literature (see, for example, van Fraassen 2006 and Esfeld 2013), and generally considered to be the most serious for Ladyman’s view.[45] Much like the charge that structural realism collapses into standard realism (page 44), this objection targets an apparently missing demarcation; this time between mathematical (or abstract) and physical (or concrete) structure. If only mathematical structure is relevant to the ontology of mathematics, and only mathematical structure is relevant to the ontology of physical science, then there appears to be no distinction between mathematical and physical structure. OSR's fusion of mathematical structures (as representations) with real patterns (constituting the physical world) is grossly counter-intuitive. Where's the physical 'oomph'? Ladyman's Wittgensteinian ‘remain silent’ response has been unsatisfying to most critics.

2.1.10. Further problems with OSR

I will finish this section expressing some further concerns with OSR. I highlight two specific issues deserving further investigation. As may already be apparent, I generally find structural

[45] Ladyman himself, though, considers the biggest challenge for his project to be the measurement problem in QM, "just because it is the hardest challenge for the philosophy of physics" (Ladyman and Ross 2007: 180).

realists' arguments persuasive. Therefore, these issues are presented as incomplete puzzles needing further work, rather than as stumbling-block objections. On my reading, any future developments built upon Ladyman's themes will have to involve overcoming the following two obstacles:

2.1.10.a. Underdetermination of structure

If objects aren't fundamental, why is Ladyman's structure the only alternative? The fundamental nature of the world may be inconceivable to human minds; perhaps suspension of belief is a more appropriate ontological stance. Eliminating objects, in favour of structure, because of the underdetermination implied by QM may be too radical. Frigg and Votsis (2011) agree that it is "not clear why this is not just a case of burying one's head in the sand. . . It seems that at most underdetermination lends support to agnosticism between the two" (60).

The nature of OSR's structure is underdetermined by the available evidence, and Ladyman's pointing to the fact that the 'other side' has the same problem isn't a reassuring response. Chakravartty (2007a) concurs that since the "natures of structures are underdetermined by physics, they are no less metaphysically ambiguous than objects" (82). Structural realists attempt to attribute causal 'oomph' to basic structural relations, rather than to the particles of fundamental physics (as entity realism does). This inversion, continues Chakravartty(2017a), cannot elude the destabilising effects of underdetermination, thereby rendering structural realism's claims inconclusive. In fact, he submits that any realist theory making assertive claims about the fine-grained ontology of the world faces the same inevitable irresolution. Different ontologists will come to different - yet rational and possibly useful - conclusions, and there is no sensible 'deeper' algorithm to appeal to in these arguments (151). This impasse presents an epistemic stalemate. The OSRist says there is *structure* all the way down, but instead it is a case of *underdetermination* all the way down.[46]

Or, is it? What can we say about ontology in spite of underdetermination and without loose speculation involving questionable epistemic *virtues*? I dub this Chakravartty's dilemma (van der Merwe 2019): theorists inquiring into fundamental ontology face an unsavoury choice between ontologies that require the acceptance of a contentious ontological primitive - *à la* OSR - on the

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[46] Chakravartty (2017a) goes on to argue for a kind of 'sceptical relativism': a conclusion of agnostic quietism reached by repeated invocations of undecidability due to underdetermination. While I don't agree with him (see van der Merwe 2019) that underdetermination leads to broad-based agnosticism, I do recognise that our knowledge of fundamental ontology is severely limited. Ladyman's claim to have solved the grand problem of the nature of the ontology of the world is overconfident, to say the least.

one hand, and scepticism on the other (Chakravartty 2017a: 137). This dilemma presents a thought-provoking puzzle for some possible future project.

2.1.10.b. OSR is incomplete

This last issue is what I take to be the most pressing challenge for OSR. Despite Ladyman's insistence otherwise, OSR sounds very much like a Kantian theory of the phenomena, rather than one about objective ontology to me. Resultantly, it is not clear that he has improved on the preceding structural theories of Russell, Worrall and van Fraassen. Let me explain.

Ladyman (Ladyman and Ross 2007) recognises that "a fundamental question about structural realism [is:] is it metaphysics or is it epistemology?" (123). He devotes a section of his book (ch. 6.1.) to explaining why his theory isn't just Kant redux. He asks how, in OSR:

> is it that special-science objects of generalization don't stand to the fundamental physical structures as phenomena stand to noumena in Kant's system? After all, it is the essence of our view that we resist substantivalizing the fundamental physical structures. We *say* these structures describe real patterns, but since we can only represent the real patterns in question in terms of mathematical relationships, in what sense are these real patterns 'real' other that in which, according to Kant, noumena are real? (ibid: 299).

For Ladyman, fundamental physics studies extra-representational real patterns directly, while the special sciences study these indirectly through representational notions like 'object', 'kind' and 'causation' (ibid: 298).[47] These "resources of the manifest image cannot be (directly) used for satisfactory representation in physics. Hence, mathematics has an ineliminable role to play in theories" (158). Likewise, for Kant, matters of contingent scientific fact are presuppositional on certain *a priori*, conceptual principles. Because the truths of these principles are presupposed before empirical enquiry, they, naturally, cannot be proven empirically. For example: "the principle that Euclidean geometry is the correct geometry for describing spatial relations is presuppositional for Newtonian physics" on Kant's account (Chakravartty 2017a: 72). These kinds of *a priori* principles allow us to describe empirical phenomena, design experiments and then forge the results into a system of knowledge. In other words, through "the process of 'fit'

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[47] Ladyman declares as false the "idea that people think only about 'phenomena' while what really exist are 'noumena'" (Ladyman and Ross 2007: 243). According to him, "people *can* think and communicate about extra-representational real patterns but usually don't try to" (ibid.).

between concept and experience, Kant argues, the whole of scientific knowledge is generated" (Scruton 202: 146).[48]

Ladyman goes on to argue that special science real patterns are not approximations of fundamental physics real patterns, however. Particular special-science subject matter relations can also be real patterns since "there are physically possible (actual) perspectives from which they non-redundantly compress information" (Ladyman and Ross 2007: 250, fn. 64). Even though the ontology of the special sciences bears information about the ontology of fundamental physics, Ladyman doesn't know - beyond the primacy of physics constraint (PPC) - what the positive relationship between the two might be exactly, "or even if there is any such general relation" (299).[49] He recognises that this sounds like Kantianism: with special science real patterns as phenomena and fundamental physics real patterns as noumena[50] Moreover, Ladyman suggests that the concepts of 'cohesion' and 'causation' are, in some sense, principled subjective constructs:

> Prices, neurons, peptides, gold, and Napoleon are all real patterns, existing in the same unqualified sense as quarks, bosons, and the weak force. We use concepts of cohesion and causation to keep track of the former but not the latter (ibid: 300).

Furthermore: "we *need* to organize the local domain of reality by means of the notional-world book-keeping principles of cohesion and causation" (ibid: 299). Aren't these just Kantian categories - principles of the understanding - that organize the phenomena? No, answers Ladyman. He insists "that science can discover fundamental structures of reality that are in no way constructions of our own cognitive dispositions" (300). Also, since - as per his verificationist

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[48] Einstein (1949) objected to Kant's doctrine of science, and, in particular, to neo-Kantian interpretations of general relativity. Kant's obvious error, he says, is "contained in the sentence: 'The real is not given (*gegeben*) to us, but put to us [aufgegeben]' (by way of a riddle)'' (680). Einstein understands Kant's apparently vacuous claim to be that there:

> is such a thing as a conceptual construction for the grasping of the interpersonal, the authority of which lies purely in its validation. This conceptual construction refers precisely to the 'real' (by definition), and every further question concerning the 'nature of the real' appears empty (ibid.).

[49] Ladyman seems to want to have his cake and eat it here. He needs the ontology of the special sciences and the ontology of physics to both be understood as real patterns for the sake of unification. However, he also wants the two to be somehow different, thereby necessitating the special sciences' deference to physics.

[50] Chakravartty (2007a) argues persuasively that in Kantian-style theories "the noumena are robbed of the ontological and explanatory roles they might otherwise fill. Lacking a role, things-in-themselves are prime targets for scepticism" (95).

naturalism - real patterns are determined empirically, and not by 'pure reason'; his theory is open to modification at any time. Both Kant and Ladyman "distinguish the propositions to be taken seriously in science and metaphysics by reference to human constructions" (ibid.). Ladyman urges, however, that OSR's constructions are "literally constructions: our scientific institutions" (ibid.). Scientific institutions, he continues, are not merely extensions of human cognition:

> They have shown themselves to have a truth-tracking power - partly thanks to mathematics - that bootstraps the process of scientific learning beyond the capacities of individual minds (ibid.).[51]

Ladyman concludes that OSR is, therefore, not a mind-centred position in the way Kant's is, and so the two are sufficiently different from one another.[52] This may be so, but although the two theories differ in this methodological institutional way, epistemologically and metaphysically speaking, they appear suspiciously similar.

We turn, now, to the structuralist theories of Russell, Worrall and van Fraassen. Kant is generally interpreted as saying "that we have knowledge not of the world 'as it is in itself', but only of the world as it appears ('the world of appearance')" or phenomena (Scruton 2002: 148). Scientific knowledge is understood in subjective, psychological terms as something generated by human understanding, through a synthesis of concept and intuition. Knowledge does not reach beyond this synthesis to an independent world of things-in-themselves. In other words, the "world of appearances marks a limit which we cannot in the nature of things transcend" (ibid.). Russell developed a version of structuralism that can be considered Kantian in this way. For Russell 'events' are the fundamental constituents of the world. Ryckman (2005) understands Russell's structuralism as follows:

> we can know the *intrinsic nature* or *quality* only of those [events] occurring in regions where there is a brain ('percept events'), whereas our knowledge of nonpercept events is limited to knowledge of their *structure* (214).

Worrall's (1989b) original formulation of epistemic structural realism (ESR) holds that there is an epistemic constraint on realism. We only commit to the structural content of scientific theories, while remaining agnostic about what lies beyond. "ESR allows for the possibility that there is some underlying noumenal nature that instantiates the structure", suggest Briceño and

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[51] It is not at all clear to me what it means for scientific learning to be *bootstrapped* beyond the capacities of individual minds. This kind of talk echoes the 'aim of science' anthropomorphism I objected to previously.

[52] Ladyman goes on to ask that someone left unconvinced by his insistence that OSR isn't a Kantian position "should at least grant that our view is realist where fundamental physics is concerned" (Ladyman and Ross 2007: 306).

Mumford, while "[t]he OSR. . . version, does not" (2016: 199). ESR has a distinctly Kantian flavour, and although tagged as a version of 'realism', I interpret Worrall's ESR as philosophically closer to van Fraassen's CE that to Ladyman's OSR.

CE has obvious Kantian features; perhaps one can think of ESR as 'Kantian realism' and CE as 'Kantian anti-realism'.[53] van Fraassen, according to Ladyman, "identifies himself with the Kantian critique of metaphysics construed as theorizing about the nature of the world as it is in itself (while not identifying himself with Kant's transcendental turn)" (Ladyman and Ross 2007: 74). As we will see, the core of van Fraassen's theory is that knowledge is limited to phenomenal appearances and we - as theorizers - *will* anthropic presuppositions onto science.

The syntax may be different, but thematically Russell, Worrall and van Fraassen all offer quasi-Kantian - self-admittedly limited - views that urge an agnostic attitude towards the primitive nature of reality. Ladyman realises that the burden of proof is on him "to show that ontic structural realism (OSR) as motivated by current physics is intelligible without any Kantian residue" (Ladyman and Ross 2007: 131). If he hasn't sufficiently demonstrated that his theory either facilitates direct access to the noumena, or makes the noumenal entirely superfluous, then it is not clear he has improved on Kant, Russell, Worrall or van Fraassen's more modest theories. Ladyman seems to want to have it both ways: he claims that physicists study noumenal patterns directly while special scientists study them indirectly, yet he also claims that both physical and special science patterns are equally real. This is odd given that his entire theory is predicated on the idea that the world ontology is revealed by scale-relative scientific epistemologizing.

As cited on page 42, Ladyman ends his argument for structure-as-ontology by asking: what makes structure physical and not mathematical? His response:

> That is a question that we refuse to answer. . . The 'world-structure' just is and exists independently of us and we represent it mathematico-physically via our theories. (This may sound suspiciously Kantian. . .) (Ladyman and Ross 2007: 158).[54]

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[53] Poincaré, mentioned above as a pioneer of anti-realism, can be thought of as a 'structural neo-Kantian empiricist'. He claims that "Nature will hide forever from our eyes" the 'true relationships' between 'real objects' (1905: 161). His structuralism was combined with Kantian views about the nature of arithmetic as providing new *a priori* knowledge, "and with conventionalism about the geometry of space and time" (Ladyman and Ross 2007: 123).

[54] The term 'mathematico-physically' is also worrisome. It is only used once in the book, and appears to have been invented - *ad hoc*, without conceptual support - purely to encourage the reader to think in terms of a blurred abstract/concrete distinction.

It sure does. Ladyman's overarching abductive argument amounts to the claim that when there's nothing left to say in epistemology, we have reached ontology. Epistemically basic real patterns just are the real patterns of the world ontology. Epistemology collapses into ontology at the limit. Ladyman believes that the abstract/concrete distinction, the epistemic/ontic distinction, as well as the illata/abstracta distinction, should be collapsed. He finds this suggestive "of a rapprochement between the objects of physics and of mathematics" (ibid: 160), and thereby - *mutatis mutandis* - between the phenomenal and the noumenal. "OSR simply collapses or ignores any distinction between the concrete (physical) and the abstract (mathematical). To do that is to flirt with forms of Platonism or Pythagoreanism", argue Briceño and Mumford (2016: 208) convincingly.

My claim here is that Ladyman has not demonstrated his bold phenomenal/noumenal merger in a way that is sufficiently convincing. Mumford (2017) presents a sharp critique along the same lines. Science, and in particular physics, offers a largely mathematical representation of the world, and we have "to be careful of mistaking the representation for the world itself" (n.p.). In other words, confusing the abstract for the concrete, and thereby the phenomena for the noumena. Ladyman is just putting "relations into the place in our ontology that particularlity formerly occupied. . . trying to get relations to do the same job that substance used to do", objects Mumford, however "it still looks like a kind of 'thingy ontology'" (ibid.). Likewise, the notion of structure as basic to substance, or relations prior to relata, defies mereological common sense. "It cannot be that the relation <__ is taller than __> is real, for instance, if there are no things it relates: namely, things having heights" notice Briceño and Mumford (2016: 200). Our basic ontology should concretize the world and have the capacity for independent existence, which relations don't seem to be able to do.

Plus, an intuition persists - for most - that the nature of things-in-themselves remains out of epistemic reach. Is the world ontology really revealed at the limit of *human* epistemology? OSR is beginning to sound like an anti-realist theory of the phenomena. Ladyman appears to faces a trilemma; either he must:

(1) detail the relationship between representational and extra-representational patterns, thereby accounting for the noumenal. This would be a standard realist account, involving some-or-other correspondence relationship,

(2) or he must assert that, and articulate how, the two kinds of patterns are the same, thereby making the noumena superfluous. This would, however, push him into an anti-realist position and/or Pythagoreanism.

(3) Alternatively, admit that his is a Kantian theory that attempts to merge options (1) and (2); then explain the necessary intricate synthesis of subjective presuppositions and objective patterns.

Further work is needed to escape the trilemma. Chakravartty (2007a) argues convincingly that - for a realist - if there is no grip on the noumena, then "Kant's transcendental idealism runs the risk of collapsing into idealism *simpliciter*" (95). Collaterally, van Fraassen (2006; 2008) notices that, even if objects have only structural properties, they would have to have at least one non-structural property: *existence*. There appears to be some Kantian residue, after all. So, option (2) of the trilemma is out. This leaves either option (1) or (3); both of which would require an overhaul of OSR as it stands. A fourth option would be to offer some breakthrough schema unlike anything anyone has seen before. This revolutionary theory, though, wouldn't look anything like what we've been discussing. OSR has not met the strong burden of proof carried by any ontological theory that radically overturns our most basic metaphysical intuitions (Chakravartty 2004).[55] Ladyman needs to do more to account for the apparent noumenal aspects of the world.

Concurrently, Esfeld (2012) challenges Ladyman's lack of an appropriate account of the dynamics and mechanisms involved in his structures, in that the claim that structure is all there is in the world begs the question of how this structure is implemented, instantiated or realized:

> OSR puts forward an ontology by telling us what there is in the world, namely certain physical structures, but not a dynamics - that is, it remains silent on the question of how the structures to which it is committed develop in time (5).[56]

Moreover, Ladyman's structure concept is too loose to be metaphysically meaningful, i.e. too good to be true, on my reading. Mumford (2017) too, complains that he has far less grip on

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[55] Ritchie (2009) offers a spritely thought experiment (discussed originally in the context of arguing against reductionism about mind, but conveniently relevant to our purposes):

> If God creates a world in which there is only Adam and Eve, he can also create a world in which let us say Adam is on Eve's left. He didn't have to create Adam, Eve and the relation of Adam being on Eve's left (124).

Creating all the physical properties is enough to create all the relations. Mumford (2017), in turn, asks us to suppose that God wants to make a world according to an OSR recipe. How "would we, and how would He, be able to tell the difference between his plan for the world and the world itself?" (n.p.).

[56] Furthermore, committing to objective modality begs the question as to where the laws of nature came from, and also why the laws are as they are, rather than some other way. See Smolin (2013) for a thorough analysis of, and apparent solution to, this dilemma.

OSR's notion of structure than on conventional accounts of particulars; the concept needs 'tidying up'. Stanford (2010) expresses similar reservations that there is no unequivocal notion of structure that can do all the jobs Ladyman wants, and needs, it to do. More specifically, he doubts:

> there is a single kind of structure that is simultaneously recommended to us by fundamental physical theorizing, preserved in the transitions between all or most suitably successful past scientific theories, and sufficient to answer the realist demand to genuinely explain the success of our scientific theories (164).

Ladyman puts "on relations more weight than they can bear. . . if relations are to do so much work in OSR, we need a clear and defensible account of their metaphysics" (Briceño and Mumford 2016: 206-08).

In conclusion then, OSR has some problems. Further refinement is needed; firstly to do with the phenomenal/noumenal distinction (or lack thereof) and, secondly, a more robust, clearer articulation of the notion of 'structure'. As such, "OSR is an incomplete description of nature" (ibid: 215). Ladyman recognises as much in at least one aspect, since he admits that he does not know the relationship between special science patterns and fundamental physical patterns.[57] They are somehow the same, yet different also. Having explored the details of Ladyman's worldview - with van Fraassen, Fine and others commentating from the sideline - let us now make a thorough investigation of CE.

2.2. Anti-realism, van Fraassen's CE

van Fraassen is the obvious choice to represent the anti-realists in this thesis. His position is by far the most cited by other contemporary anti-realists and the most criticised by realists in the literature. Consequently, although there are, of course, a variety of versions of empiricism, instrumentalism, fictionalism and so on that fall under the anti-realist umbrella, I will focus here almost exclusively on van Fraassen's constructive empiricism (CE). He has developed a number of modifications to his view in his long and highly impactful career. All fall under the umbrella of

[57] Ladyman also appears to recognise that there are (noumenal?) patterns outside of his schema. Yet these don't really matter because they beyond the (epistemic?) reach of science. He says:

> A pattern that could not be tracked from any perspective that current physics tells us is possible is a pattern whose existence cannot be empirically verified as far as we now believe. Such patterns are therefore not possibilities that empirical scientists should take seriously. By the PNC, these are therefore also patterns that metaphysicians should not take seriously (Ladyman and Ross 2007: 235)..;

empiricism, but involve various alterations that track his adaptation to the changing intellectual landscape.

Okruhlik (2014) identifies three stages in van Fraassen's philosophical maturation, each centred around one of his books.[58] First is (CE), as described and defended in *The Scientific Image* (1980a). This position grants much to realism, but holds onto a notion of epistemic commitment towards the *observable* only, thereby purporting to offer an account of empiricism less austere than those that came before. Second is stance empiricism, as articulated in a series of published lectures: *The Empirical Stance* (2002). This involves a change of mind for van Fraassen. As we will see, he abandons his earlier strict empiricism, with its dogmatic claim that experience is the only legitimate source of information about the world, for a view that highlights pluralistic themes such as 'stances' and 'attitudes'. Third is empirical structuralism, developed in *Scientific Representation: Paradoxes of perspective* (2008). This reactionary position is a response to the rise in popularity of structural realism (of the Ladymanian sort), and claims to offer an empirical, anti-realist alternative. It is substantially an extension of CE, but includes some influences from stance empiricism as well. I will explore these three themes in turn. I will pay the most attention to the last theme, since - being a structural account - it stands nicely juxtaposed to Ladyman's OSR.

2.2.1. Constructive empiricism

van Fraassen does not defend empiricism as such in his (1980a); instead, he wants to know how those inclined to the view can sensibly be empiricists.[59] CE contains both a negative and a positive component. One the one hand, it is a sceptical method: questioning metaphysical postulates such as unobservable ontologies, real modalities, universals and possible worlds. On the other hand, CE is a project advancing a doctrine about the aim of science as a pursuit of 'empirical adequacy',[60] and of giving an account of our knowledge of the world. A theory is

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[58] Okruhlik also dedicates a section in her paper to a fourth stage: van Fraassen's anti-realist account of the 'laws' of nature, as presented in *Laws and Symmetry* (1989). The basic idea is that there are no laws of nature: there are only symmetries in our theoretical models. Laws systematize the phenomena; they do no more than describe how things behave. This forth stage is somewhat orthogonal to the debate at hand, and some of its content is covered in van Fraassen's other work. As such, I will focus on the other three stages in his career.

[59] Ladyman (2007) therefore worries that CE only "offers the appropriate view of science for someone with a prior commitment to empiricism" (48). van Fraassen presents no reasons compelling us to be empiricists in the first place.

[60] van Fraassen asserts that the "aim of science is of course not to be identified with individual scientists' motives. . . What the aim is determines what counts as success in the enterprise" (1980a: 8). He uses an analogy to chess: the aim of the game *itself* is checkmate, even if the individual players are motivated by fame and fortune.

empirically adequate "exactly if what it says about the *observable* things and events in this world is true - exactly if it 'saves the phenomena'" (van Fraassen 1980a: 12). Alternatively, an empirically adequate theory "has at least one model that all the actual phenomenon fit inside" (van Fraassen 1980a: 12). van Fraassen also understands formal logic to be the construction of a series of models. Logicians aim to save the phenomenon of "the surface grammar of our assertions and the inference patterns detectable in our arguments" (134). Their models are adequate if they fit the phenomena to be saved. These trademark van Fraassian slogans 'empirically adequate' and 'saves the phenomena' will be repeated throughout this section of my thesis. In the end, though, we will notice that much of CE's weight hangs on some principled - yet conceptually problematic - distinctions, which may be arbitrary, having no clear epistemic import constraining belief.

In his (1980a), van Fraassen distances himself from empiricist efforts of the past. He thinks of logical positivism as having suffered a rather spectacular crash, mostly due to its linguistic orientation. Not all problems of science and philosophy are problems of language. According to Cartwright (2018) CEists read the claims of theory literally, but need not count them as true; a "theory is acceptable just in case its empirical consequences are correct" (166). Our language cannot be cleansed of theory-laden terms. As such, there is no talk of the verification criterion of meaning, or Ramsey sentence-style reduction of theoretical sentences to observation language, in van Fraassen's discourse. The meaning of observation terms depends on their role in a larger conceptual scheme. Scientific theories are not mere instruments or metaphors, and the statements of science are, in principle, capable of being true or false. van Fraassen's gripe with realists is not semantic, but rather epistemic: over what is the "correct understanding of *what a theory says*" (1980a: 11).[61] Realism, as a metaphysical doctrine, asserts the existence of a real, external world; CE makes "that world disappear from the debate" quips Fine (2001: 120). van Fraassen has stressed that CE is not telling us what to believe. He wants to know what science is, and what

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[61] This focus on epistemology in CE is similar to traditional instrumentalism (some writers label van Fraassen an instrumentalist). Traditional instrumentalism holds, like CE, that there is a mind independent world, but that we cannot have knowledge or belief about unobservables. Where the two positions differ is over the semantic interpretation of scientific theories. CE says that we can literally construe scientific theories, while traditional instrumentalism says that theories are merely instruments: "claims involving unobservable entities and processes have no meaning at all. [They] do not have truth values." Explains Chakravartty (2007: 10-11). Some also see an affinity between CE and fictionalism (often associated with Duhem, mentioned above). Fictionalism holds that things in the world behave as if our best theories are true. However the statements about the world in these successful theories are not literally true - they are 'useful fictions' (see Fine 1993 for an explication).

counts as success in science. What sort of account can a contemporary, aspirant empiricist give of science that avoids the mistakes of the forefathers?

2.2.1.a. Defining terms

van Fraassen presupposes that scientific theories:

> account for the phenomena (which means, the observable processes and structures) by postulating other processes and structures not directly accessible to observation; and that a system of any sort is described by a theory in terms of its possible states (1980a: 3).

Scientific activity is not about discovery of *truth*, rather it involves the *construction* "of models that must be adequate to the phenomena" (ibid: 5). Empiricism, for van Fraassen, requires that theories give a true account of the observable only. Unobservable structures or entities are eschewed, along with modal realist notions of genuine possibility and necessity. There is no need to articulate an account of laws, causes and essential properties, since these notions carry with them modal implicits. Modality is relegated to a linguistic or semantic role: facilitating the description of what is actual. Modal assertions only have a pragmatic value, depending on what Humean regularities we find significant in a given context (Monton and van Fraassen 2003). In other words "[b]elief in the truth of some claim of the form 'x is observable' amounts simply to belief in the truth of. . . a context-fixed, non-modal conditional" (Monton and Mohler 2017: n.p.).

This introduces a nominalism, of sorts, into CE: if we want to examine the truth of a modal statement, we can only do so by investigating related non-modal ones (see van Fraassen 1985). Empirical adequacy concerns actual, not counterfactual, phenomena. *Pace* Ladyman, van Fraassen holds that hypotheses about what *would* happen are merely background theories, having no basis in fact. "The term 'observable' classifies putative entities (entities which may or may not exist)" (van Fraassen 1980a: 15); 'observable' and 'exists' do not imply each other.[62] 'Observable' means observable by people, not *possible* aliens or 'sentient' computers: "the 'able' in 'observable' refers [to] our limitations, *qua* human beings" (17). Although van Fraassen recognizes that the term 'observable' is conceptually vague, he implores that this is the case for all meaningful concepts. What matters is that:

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[62] There is, however, a difference between 'observed' and 'observable' on van Fraassen's account. Jupiter is not observed without artificial aid, but it would be observable if we moved nearer, while a water molecule is neither observed without artificial aid, nor observable by adjusting our position to it. Another important difference is between 'detection' and 'observation'. A particle in a cloud chamber, for example, is detected by observation of its vapour trail, but the particle itself is not observed.

> even if observability has nothing to do with existence (it is, indeed, too anthropocentric for that), it may still have much to do with the proper epistemic attitude to science (ibid: 19).

Observation is a special subclass of *measurement*, and measurement interactions are a special subclass of physical interactions: interactions between *us* and the world. The *structures* (or patterns, if you like) discernable in empirical data are a subclass of the physical structures described by the data, intimates van Fraassen. Therefore, in Quinean fashion:

> the empirical import of the theory is now defined from within science, by means of a distinction between what is observable and what is not observable drawn by science itself (van Fraassen 1980a: 81).[63]

What counts as observable is, therefore, the subject of scientific output and not something that can be delimited *a priori*. Science is the arbiter of what is observable and what is not. van Fraassen is, of course, highly critical of the realist's demand that theories provide some *deeper explanation* of phenomena. This realist tendency leads to the dubious reification of metaphysical 'hidden variables' (read ontic structure) by abduction. IBE, and other ampliative rules, can only lead to conclusions about the observable in CE. The phenomena exhibit certain regularities that fit a theory; there may or may not be:

> an explanation in terms of unobservable facts 'behind the phenomena' - it really doesn't matter to the goodness of the theory, nor to our understanding of the world (ibid: 24).

The realist's 'leap of faith' into the metaphysical yonder is unwarranted. An empiricist does not yearn for explanation; she is satisfied with theories that are useful. When we 'explain', we are merely describing or organizing the detailed knowledge we have of the phenomena.

Another important term, needing axiomatic definition, is the concept of 'belief'. For van Fraassen the belief in accepting a scientific theory is only that it "'saves the phenomena', that is, correctly describes what is observable" (ibid: 4). Doxastic commitment to a theory *tout court* is underdetermined by the evidence, so we only pragmatically commit to some Carnapian

[63] Kincaid (2017) objects that identifying observables, in the CE sense, in economics is problematic. "It is not at all obvious what should count as observable", he says, "at least if the goal is to view some significant part of economics as successfully saving the phenomena" (369). To my knowledge van Fraassen does not mention economics, in a formal way, in any of his books, so it is not clear what his take is here. Kincaid speculates that, on a CE account, economic observables (perceived by the unaided senses) must be "marks on paper and computer screens. . . the constructive empiricist would be restricted to believing only that economics tells us that such marks exist" (373).

framework. What is rational to believe about the world, in CE, is restricted by the data. In other words, the CE framework is self-constrained by empirical output.

2.2.1.b. The no-miracles argument

van Fraassen has little patience for the realist's most resilient counter to empiricism: the no-miracles argument (usually credited to Boyd 1971 or Putnam 1975). This challenge is more of an intuition than an argument *per se*. It can be thought of as a grand IBE, seeking to defend the reliability of scientific methodology. Putnam (1975) states that the "positive argument for realism is that it is the only philosophy that does not make the success of science a miracle" (73). Either the success of science is explainable in terms of the truth of scientific theories or the success of science is a miracle. We would have to suppose that the regularities in the phenomena, seemingly "brought about by the non-existent things ostensibly talked about in the theoretical vocabulary", were a series of "innumerable lucky accidents", or cosmic coincidences (Smart 1968: 150). The argument then says that, since these approximately true theories are arrived at by means of IBE, such inference is reliable. It, therefore, defends both the claim that scientific theories are approximately true, as well as that IBE is reliable.[64] Worrall (1989b) sums up nicely:

> The 'no miracles' argument cannot *establish* scientific realism; the claim is only that, other things being equal, a theory's predictive success supplies a *prima facie* plausibility argument in favour of its somehow or other having latched onto the truth (118).

The most famous response to the no-miracles argument arguably comes from Laudan (1981). Inspired by Kuhn, he holds that science is a problem-solving, rather than a truth-seeking activity. Scientific progress for Laudan, says Niiniluoto (2017), "can be defined by the *problem-solving effectiveness* of a theory (the number of solved problems minus the anomalies and generated conceptual problems)" (196). Laudan presents a list of historical cases of scientific theories that were considered to be successful, but that are now thought not to have been true. Examples are

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[64] Musgrave (1999) syllogises IBE as follows:

> P1. *F* is the fact to be explained.
>
> P2. Hypothesis *H* explains *F*.
>
> P3. Hypothesis *H* satisfactorily explains *F*.
>
> P4. No available competing hypothesis explains *F* as well as *H* does.
>
> C. Therefore, it is reasonable to accept *H* as true.

He points out, though, that the argument is invalid. It is missing an implicit premise - the contentious epistemic principle: "[i]t is reasonable to accept a satisfactory explanation of any fact, which is also the best explanation of that fact, as true" (ibid: 284-85).

the luminiferous ether and phlogiston theories. Laudan's counterexamples aim to show that there is no connection between truth (or approximate truth) and success of a theory.

Laudan (1981) also insists that inferring the truth of a theory from its success is an instance of the fallacy of affirming the consequent. Just because true theories are successful, doesn't mean that successful theories are true. Others, such as Howson (2013), argue that the no-miracles argument commits the base-rate fallacy. The fallacy consists in neglecting the dependence of the posterior probability on the prior probability (206). Brad Wray (2017) explains:

> The prior probability of a successful theory being true is quite low if in fact there are many successful but false theories. For success to be a reliable indicator, it would have to be the case that most successful theories are true (39).

The no-miracles argument is also frequently accused by critics of being circular. Saatsi (2017), for instance, charges that there "is clearly an air of circularity in the way the realists aim to justify scientific IBEs as well grounded with this (metalevel) IBE about science" (204). Realists have sometimes responded, somewhat unconvincingly, that this circularity is not pernicious. "Instead of being viciously *premise* circular", the no-miracles argument apparently "involves a kind of *rule* circularity, in that the rule of inference employed - namely, IBE - also appears in the conclusion of the inference" (ibid.). See also Psillos (1999).

Moving on, van Fraassen (1980a) targets the implicit demand for explanation in the no-miracles argument. The realist unjustifiably requires that science "provide a theoretical elimination of coincidences, or accidental correlations" (25). Sometimes coincidence may be a legitimate explanation of some happening. Realists seek the completeness of theory, deterministic universal laws,[65] or hidden common-cause of observable events, and they declare this to be the ideal aim of science. They see science as incomplete, and search for explanatory realities behind the phenomena. But this demand for explanation brings no gain in successful prediction. Science, in contrast to scientific realism, "does not place an overriding value on explanation in the absence of any gain for empirical results" (34). Science is perfectly intelligible without the realist's extra metaphysical baggage, insists van Fraassen.[66]

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[65] van Fraassen explains that putative laws are, in fact, "heavily subject to *ceteris paribus* qualifications". For example, "water boils at 100°C - provided atmospheric pressure is normal; a falling body accelerates - provided it is not intercepted", perhaps we have no genuinely empirical laws (1980a: 32).

[66] Metaphysical baggage can be useful as one explores avenues for future empirical possibilities, but must be discarded when some detour pays off, adds van Fraassen. Hidden variables, in QM for example, are extra structure added to certain models that are empirically equivalent to no-hidden variables models. To an anti-realist such extra

The demand on science should be "for imaginative pictures which have a hope of suggesting new statements of observable regularities and of correcting old ones" (ibid.). The overall success of science is to be expected, given that theories are thrown into Darwinian competition with other theories. Only the successful, well-adapted theories survive - the ones adequate to the tasks to which we put them. Successful theories are the ones whose novel predictions are borne out in the course of observation and experimentation. Competition between theories hinges on which theory accurately represents the observable phenomena, not on which theory is literally true. There is nothing miraculous, or even surprising, about it (40). Science is, according to CE, a "biological phenomenon, an activity by one kind of organism which facilitates its interaction with the environment" (39). "All scientific activity is ultimately aimed at greater knowledge of what is observable" *to us* (31).

van Fraassen develops CE by exploring three main issues where realists and empiricists (i.e. anti-realists) conflict:

> The first concerns the relation of a [scientific] theory to the world, and especially what may be called its empirical import. The second is a theory of scientific explanation, in which the explanatory power of a theory is held to be a feature which does indeed go beyond its empirical import, but which is radically context-dependent. And the third is an explication of probability as it occurs within physical theory (ibid.).

I will discuss these three themes of CE in turn.

2.2.1.c. First theme - scientific theory

van Fraassen favours the semantic, rather than the syntactic, approach to theories. The syntactic interpretation identifies a theory as a set of sentences, in a specified language, that can be divided into observational and theoretical terms (as in Logical Positivism). A conceptual divide is made between the phenomena and the extra-phenomenal by means of a distinction in vocabulary. The theoretical terms are then supposedly reducible to the observational terms. van Fraassen (along with many contemporary philosophers) sees the syntactic approach as deeply flawed, creating more well-known problems than it solves (see particularly Quine 1953, ch. 2). The empirical content of theories cannot be linguistically isolated in the way the logical positivists wanted, rather the phenomenon "are saved when they are exhibited as fragments of a larger unity" (van Fraassen 1980a: 56). van Fraassen goes as far as to say that the "main lesson of twentieth-century philosophy of science may well be this: no concept which is essentially language-dependent has

baggage is superfluous, as far as empirical commitment goes, but is "capable of being mobilized should radically new phenomenon come to light" (1980a: 68).

any philosophical importance at all" (ibid.). Empirical science ultimately decides what the limits of observability are; it is a theory-independent question, to do with us as biological and psychological agents.

The semantic interpretation, on the other hand, understands a theory as a collection of structural models that can be matched, in some way, to some phenomenon. The semantic approach is generally preferred by contemporary theorists such as van Fraassen and Ladyman, because it avoids the problem of how to linguistically delineate epsitemically warranted from unwarranted content. Furthermore, any way of rendering the equations and principles of theories as propositions "either makes them false or undersells their usefulness" (Cartwright 2018: 165). In CE, to "believe a theory is to believe that one of its models correctly represents the world" (van Fraassen 1980a: 47), i.e. stands in an isomorphic relationship to it (reminiscent of Dennett and Ladyman's bit map representation). van Fraassen (2002) considers that:

> to say that structure A is isomorphic to structure B means that there is a one-to-one correspondence between them that preserves certain operations and relations. That is an explication of the vaguer phrase 'A and B have the same structure' (22-3).

Models are abstract mathematical structures, "not distinguished beyond isomorphism - to know the structure of a mathematical object is to know all there is to know" (van Fraassen 2008: 238). In other words, "it makes no sense there to speak of differences between isomorphic structures - and that is why it makes perfect sense to say that here we are dealing solely with structure" (van Fraassen 2006: 304). Theories that are empirically equivalent, but varying in content, have the same epistemic import. Such theories cannot be distinguished, other than pragmatically in CE. They do not have 'hidden virtues' independent of experiment that allow successful extensions to new kinds of phenomena. What matters to scientists is that a theory guide experimental design, not that it offer a descriptive simplification of the world. Pragmatic superiority of one theory over another, rather than truth or falsity, helps facilitate the progress of science: from theory construction to experimental design, to theory construction, to experimental design etc. aiming ultimately for empirical adequacy.

As such, inspired in part by Suppes (1967), van Fraassen presents a new picture of theories. A scientific theory is to be understood as a 'family of structures' (i.e. models), and certain parts of these models (the empirical substructures) are possible direct (isomorphic) representations of observable phenomena. One cannot make general pronouncements about empirical adequacy, only contextual claims linked to specific classes of models; "the question of empirical adequacy can only be raised *with reference to a specific theory*" *qua* family of models made up of minimodels (van Fraassen 1980a: 84). A theory is "empirically adequate if it has some model such that all appearances are isomorphic to empirical substructures of that model" (ibid: 64).[67]

van Fraassen introduces the virtue of 'empirical strength': the fewer the empirical substructures of a theory, the more empirical strength it has. Also important is the criterion of 'consistency': it is a virtue for a theory to be internally consistent, in terms of its rational structure.

Belief is not necessary for the constructive empiricist in the way it is for the realist, concludes van Fraassen. We can never be sure that a model is a faithful replica of the world in every detail. Pragmatic *acceptance*, rather than belief, of a theory is the weaker restraint for one concerned with empirical adequacy instead of truth *simpliciter* (with its metaphysical burden). Okruhlik (2014) argues that when we "accept a theory, we are committed to *using* it, but the only belief required by theory acceptance is belief that the *observational consequences* of the theory are true" (655). Alternatively, Jones (2003) understands van Fraassen's account of doxasticity as follows:

> theory-commitment to a theory or group of theories T is not doxastic commitment to the truth status of T *per se*, but rather doxastic commitment to a proposition of the form 'T possesses a property P' (329-30).

This is a safer, more readily justifiable, approach. Theories with the right kind of epistemic virtues - logical consistency, adequacy to the phenomena and empirical strength - justify belief. Theories with the right kind of pragmatic virtues - simplicity, explanatory power and other realist favourites - justify only acceptance. Pragmatic virtues give us no "reason over and above the evidence of the empirical data, for thinking that a theory is true" (van Fraassen 1980a: 4). In other words: "we are allowed to believe that the scientific story about observables is true, and no more than that" (Fine 1986: 143).

2.2.1.d. Second theme - scientific explanation

van Fraassen (1994) notices that for realists "the question *Why?* has absolute primacy; and they presuppose that it must have an answer always" (114). They look deeper than the facts - happy to give 'answers by postulate'. By abduction, realists go beyond logical consistency, empirical

[67] For van Fraassen, what are traditionally taken to be the propositional axioms of QM are, in fact, a description of a family of models, plus specification of the empirical substructures of these models (1980a: 55). Rosen (1994) elucidates van Fraassen's interpretation as follows:

> to say that [theory] *T* is empirically adequate is not to say that some subset of the sentences that express *T* (the observation sentences) is true, but rather that there exists a function *f* which embeds a model of the phenomena within one of the models that constitute *T*" (166).

adequacy and strength when advocating theories, says van Fraassen. They over-utilize extra-empirical devices, such as: mathematical elegance, simplicity, unificatory ability (*à la* Ladyman) and most notably explanatory power. van Fraassen wants to present a constructive account of explanation without the realist's central notion of interference to the best explanation. For him, the main criterion for theoretical success is consistency with the phenomena, not explanation.

Importantly, van Fraassen accepts that we make successful ampliative extrapolations beyond the evidence in daily life, and wants to avoid the radical scepticism that can follow from too strict a doctrine of empiricism. He, though, sees inference to empirical adequacy as the lesser of two evils (the other being inference to the truth of the best explanation). He admits that he is sticking his neck out here, and that a complete epistemology "must carefully investigate the conditions of rationality for acceptance of conclusions that go beyond one's evidence" (1980a: 72). He holds that epistemic decisions are constrained by a permissive rationality, but not rationally compelled - we have *choice* when making inferences.[68] Immersion in a "theoretical world-picture does not preclude 'bracketing' its ontological implications"; we inhabit a changing conceptual framework through which we "perceive and conceive the world" (81). If one is interested in some contextual question (dependent on personal and cultural criteria), then these virtues can aid one's inquiry within that framework. This, though, involves pragmatic theory *acceptance*, and has nothing to do with belief in the truth *tout court*. The only belief involved is the belief that the theory is empirically adequate. To accept a theory is to:

> make a commitment, a commitment to the further confrontation of new phenomena within the framework of that theory, a commitment to a research programme, and a wager that all relevant phenomena can be accounted for without giving up that theory (van Fraassen 1980a: 88).

Furthermore, commitments are "not true or false; they are vindicated or not vindicated in the course of human history" (ibid.). The only virtues, which go beyond empirical adequacy, empirical strength and internal consistency, are pragmatic ones. A scientific theory gives a good *explanation* if it has these three virtues, and it thereby serves the aim of science. The term

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[68] Ladyman (2007) objects that van Fraassen's voluntarism commits him to 'inductive libertarianism', which cannot:

> impugn the rationality of deviant induction and so wishful thinking is as rational as critical realism, and someone who capriciously disregards all the evidence and counter-inducts cannot be criticized for irrationality so long as their synchronic degrees of belief remain consistent (105).

van Fraassen's loose rationality, and his rejection of induction, may be technically plausible but rhetorically impossible. It would, for example, be rational for one to "counter- induct to the degree that I believe that every week I will win the lottery, even though I never buy a ticket, because I keep expecting to find one" (Ladyman 2004: 142).

'explains', though, is radically context-dependent to some pragmatically chosen linguistic framework, concludes van Fraassen. When scientists value explanation it is because it aids in the contextual search for empirically adequate (i.e. acceptable) theories. Explanation is anyway, though, not an overriding virtue for scientists; it is subservient to "rock-bottom criteria of minimal acceptability: consistency, internally and with facts" (94). If a theory is taken to explain some fact, then there is a relationship between theory and fact, "which is independent of the question whether the real world, as a whole, fits that theory" (98). Although a theory does not offer a true explanation on van Fraassen's account, but it can be true to say that a theory is used to explain something.

Explanation is "not the same as a proposition, or an argument, or a list of propositions" (ibid: 134). When we ask for an explanation of some phenomenon - when we ask 'why questions' - we are usually asking for a causal-historical story, continues van Fraassen.[69] Explanation is, therefore, closely linked to causation. Causation, however, introduces notions of modality, which an empiricist generally wants to avoid. As we saw in the section on Ladyman, the consensus view is that realists have to be modal realists. How, though, can van Fraassen deal with the modal implications that seem to come with talk of explanation? He replies that "conditionals carry a tacit *ceteris paribus* clause": if *A* (and all else had been the same) then *B* (115). But how can all else be the same? How can the environment of whatever modal event be kept constant? This is done, explains van Fraassen, in the mind of whoever is contemplating the 'if *A* then *B*' clause. There is a contextual variable determining the content of the *ceteris paribus* clause - a subjective judgement. There appears to be no objective determinant keeping one thing rather than another constant, and therefore no clear truth conditions for the relevant conditional. In other words, until the:

> context that fixes the ceteris paribus clause is specified, we cannot say what the truth value of the counterfactual in question is. Only once the context is determined does the counterfactual admit of an objective truth value (Monton and Mohler 2017: n.p.).

Science cannot dictate what *we* decide to 'keep constant'. Science, *pace* Ladyman, contains no counterfactuals, insists van Fraassen. It then follows that *science* contains no explanations (van Fraassen 1977: 149). Modality appears in science "only in that the language naturally used once a theory has been accepted, is modal language" (1980a: 198). Accounting for the use and structure

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[69] van Fraassen (2002) recognizes that QM presents a potential problem for this expectation, in that quantum processes don't seem to "correspond to a continuous spatio-temporal trajectory" (122). Yet explanation in QM should be no different to explanation in other areas.

of modal language is a problem for the philosophy of language. If, as seems to be the case, explanation requires counterfactual language, and if van Fraassen is right about modality, then explanation is significantly context-dependent. Since scientific propositions aren't context-dependent, but explanatory counterfactuals are, van Fraassen concludes that explanation involves something more that the descriptive information science gives us: namely, the volitional interests of the individual seeking an explanation in answer to some question. Science describes a net of structured causal relations according to CE. An explanation says something about the pragmatically salient features of these relations. This allows for the fact that an event seems to have multiple causes and explanations, depending on one's preferred orientation. The meaning of '*A* caused *B*' is not dependent on the phenomena, but rather on the context in which the phrase is uttered: the pragmatics of the situation.

In conclusion then, an explanation is an answer to: 'why?'. Accordingly, "a theory of explanation must be a theory of why-questions", in turn based on a theory of propositions (van Fraassen 1980a: 134). On van Fraassen's account of explanation we first determine what why-question is being asked by examining the contextual environment *K*: background propositions and information judged to be relevant. We thereby establish what can count as an explanation. There is a range of possible answers $A_1 . . . A_n$ to the why-question. Some possible answer *A* is evaluated by (1) judging whether *A* is consistent with *K*; if so, (2) asking what probability *K* bestows on *A*; and (3) comparing this probability with the probability *K* bestows on other possible answers in the 'contrast-class' (147).[70] As for laws of nature, with their implicit modality, van Fraassen inverts realist intuitions by noticing that if laws imply context-dependent counterfactuals, "then the concept of law does not point to any objective distinction in nature" (118). The same with explanation: if laws of nature have to be understood in a counterfactual way, then the counterfactuals' context-dependence implies that those laws, too, go beyond what science reveals to us.

Duhem (1906/1954) argued that only metaphysical theories explain, and since metaphysics is alien to science properly conceived, explanation is not the aim of science. van Fraassen, inspired by this empiricist spirit, surmises that explanation is not some irreducible, mystical ideal - as realists seem to have it. There is no evidence for the idea that explanation is a *sui generis* link between theory and fact. Rather, there is a *three-way* relationship, between theory, fact, and

[70] van Fraassen (1980a) recognizes that his account of scientific explanation is neither complete nor precise, but recons that this shortcoming is shared by all philosophical theories of explanation.

context. A scientific explanation draws on science to get data from scientific theories in order to offer a contextual description of some interesting phenomenon. "Being an explanation is essentially relative, for an explanation in an *answer*" to a question, which is a request for information in a specific context (van Fraassen 1980a: 156). Scientific explanation is an application of science, urges van Fraassen, there is no such thing as 'explanatory power' as such: a successful explanation is merely an informative description. It can, however, be rational to value explanatory power, because it aids the search for empirical adequacy/strength. Explanation is a pragmatic affair, and explanatory power is a pragmatic virtue.[71] Although we seek explanation, the value of this search is in finding empirically adequate, strong theories.[72]

2.2.1.e. Third theme - probability

Being an empiricist, van Fraassen rejects objective modality when it comes to physical occurrences. He, though, includes notions of probability - or 'possibility-with-degrees' - in his CE schema, and therefore needs an account of apparent modality. As we saw with Ladyman, realists reify certain modalities. Realist philosophers of space and time think of Einstein's space-time as a concrete entity, and of geodesic curves as real light pathways in that substance. Furthermore, some philosophers of QM - convinced by the Everettian interpretation - take the many worlds, implied by the equations, to be real 'universes' out there somewhere. Others even hold probability to be a real, physical magnitude, measuring the chance of some event occurring. van Fraassen sets out, instead, to argue for a constructive account of probability loyal to the empiricist tradition. As he recognizes, probability models in scientific theories undeniably utilize notions of possibilities making the task a challenging one. How can apparently inescapable modal elements be incorporated into CE?

The mathematical theory of probability provides the foundation for the statistical methods used across the sciences. van Fraassen (1980a) explicates the standard, simple account of probabilities in scientific theorizing as follows: a "*probability space S* consists of three parts: the sample space *K*, the family of events *F*, and the probability measure *P*" (179). The possible outcomes of an experiment are represented by the elements of *K*. The family *F* is the specified experimentally significant events, corresponding to subsets of *K*. The probability measure *P* is a function assigning probabilities between 0 and 1 to events. *K*, though, is treated as continuous (i.e. infinite)

[71] Recall that pragmatic virtues "do not concern the relation between the theory and the word, but rather. . . provide reasons to prefer the theory independently of questions of truth" (van Fraassen 1980a: 88).

[72] Stanford (2009), however, argues convincingly that if we underplay the value of explanatory success, then we should have trouble committing to evolutionary theory, which relies predominantly on explanatory achievements for its success as a scientific hypothesis.

in standard scientific probability theory. Some experiments are concerned with a range of possible outcomes over time. So, for example, if one is interested in the outcome of coin tosses (heads or tails), then the number of possible tosses is $n = 1, 2, \ldots$ i.e. temporally infinite. Also, scientific measurements - such as volume, temperature and marginal income - are generally made on continuous scales involving infinitely many possible values. This introduces problems when it comes to application in physics because of the discrete, granular - rather than continuous - structure of matter. For example, if we have some quantity of radium, half of the radium atoms will transmute into radon in 1600 years - the half-life of radium. van Fraassen, though, worries what if:

> there were only a few radium atoms in the world, or an odd number? If we change the above to 'approximately half of all radium atoms', should we also say that the probability is only approximately half? And if so, does that mean that there is a real but unknown probability, which is a real number very close to ½, but cannot be further determined by us? (ibid: 169)

Further problems arise in the application of probability in QM. On the standard view, as per Born's (1926/1983) formalism, *K* is represented by means of microstate vectors in Hilbert space, typically the mysterious Schrödinger wave function *Ψ*.[73] *F* is some interesting macroscopic state, such as electrons scattering in some direction. The physical properties of *F*: quantities such as position, momentum, spin etc. have a range of possible values determinable from *K* (or *Ψ*). For each measurable set of these values we can calculate - using Born's rules (serving the role of *P*) - a number between zero and one.[74] This standard formalism, though, runs into the notorious 'measurement problem'. van Fraassen explains:

> a measurement is itself a physical interaction, and hence, a process in the domain of applicability of quantum theory. So there is a serious consistency problem: does what

[73] The Schrödinger equation, "often thought of as the basic dynamical law of quantum mechanics. . . [specifies] the time development of a system's quantum state *Ψ*" (Healey 2017). Alternatively, it "governs the behaviour of a particle or the evolution of a system by encoding how its wave function varies with time" (Kumar 2009: 383). It can be understood, following Born, as describing an abstract probability wave.

[74] Some of the 'weirdness' of QM is due to an inversion of the classical micro/macro-state relationship, in terms of our knowledge. van Fraassen considers that:

> in classical physics, it was the theory of the underlying microstates that was best understood, whereas in quantum physics, the laws governing the basic physical quantities (position, momentum) as opposed to the laws governing the probabilities about them (as summarized in the quantum-mechanical state) [are] totally unknown (1980a: 173).

> quantum theory says about such processes cohere with the role they play in the Born rules linking states with measurement outcomes? This is called the *measurement problem* (1980a: 177).[75]

In other words, when we perform a measurement on a quantum system we get out classical probabilities displayed on, for example, the dial of a measuring apparatus. However the formalism of QM is inconsistent with classical probability (more on the measurement problem later). In order to develop an alternative empiricist interpretation of probability, submits van Fraassen, we need to properly understand the structure of probabilistic scientific models, and also how these models are meant to fit the data. We will look at this issue in detail when we examine his (2008) later. For now, though, he suggests that if we look into a typical probabilistic model, "we see a picture of various different configurations in outcome sequences in an infinitely repeated experiment" (1980a: 194). The probability function *P* of the model is determined by the common features of the outcome sequences. To claim that a specific model is the *correct* one, "means that the actual series of experimental outcomes will display these common features" (ibid.). The radium decay example mentioned earlier can be rephrased in van Fraassian terms as: the events of spontaneous decay of radium atoms *F* are represented by certain elements of a model *K*, and there is a probability function *P* defined on those elements.

van Fraassen proposes an interpretation of probability in science that involves models of a probabilistic theory as, or part of, probability spaces. Each model is reconstructed as consisting of elements, each representing an alternative possible sequence of outcome-events. "At most one of these sequences can correspond to the sequence of events (such as experimental outcomes) which actually happens in our world" (ibid: 196); this is the most economical reconstruction possible of what goes on in a probabilistic physical theory, he concludes.[76] He dubs this view the 'modal frequency interpretation'; it allows him to remain agnostic on whether models, or their substructures, correspond to *real* parts of the world. Commitment to non-actual real entities, states or trajectories simply because one is committed to the relevant model introduces metaphysical realism about unsavoury notions, such as concrete possible worlds.

2.2.1.f. Conclusion - CE

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[75] van Fraassen (2008) explains that the Born rules involve "a *prediction of outcomes, conditional on the performance of a measurement*" (297). Upon measurement the probability of finding a particle at some point is proportional to the square of the magnitude of the particle's wave function at that point.

[76] "A reconstruction of a model of such a theory, in which every part corresponds to something actual, cannot be had", adds van Fraassen (1980a: 196).

In CE "all that is *both* actual *and* observable finds a place in some model of the theory" via some empirical substructure (van Fraassen 1980a: 197). An empirically adequate theory is indifferent as to the existence of unobservables. Scientific truth is not primarily a function of linguistic devices. A theory is 'true' if "there is an exact correspondence between reality and one of its models" (ibid.). Defining the logical relations between theories and propositions in terms of truth is, though, allowed in CE. Acceptance of the theory, however, does not require truth; it requires only empirical adequacy.[77] This world of empiricism is a world of opportunity and chance; there is no cause and effect - no necessity. Anything is possible: "whatever happens merely happens and not because something greater is making it happen" (van Fraassen 1994: 123).

To conclude, van Dyck (2007) understands van Fraassen's positive argument for CE to have four components:

> (1) It is possible to isolate the empirical content of a theory, thereby allowing a distinction between acceptance of, and belief in, the theory.
>
> (2) There are no epistemic rules that provide compelling reasons to have full belief in theories.
>
> (3) One can understand all aspects of scientific methodology perfectly well if the criterion for scientific success is empirical adequacy, rather than truth.
>
> (4) CE is the best view of, and makes the most sense of, science (22-3).

We have explored van Fraassen's major contribution to the philosophy of science: his megalithic anti-realist theory of CE. He later developed two secondary modifications, though, and I will discuss these in turn. Thereafter I will conclude this section with a critical analysis of van Fraassen's empiricism, about which there has been extensive commentary and much disapproval.

2.2.2. Stance empiricism

Twenty two years after taking charge of the anti-realist cause, van Fraassen repudiates his earlier classical empiricist slogan of *sola experientia*. In fact, "he expresses amazement that he could ever have said such a thing" (Okruhlik 2014: 656). CE and other traditional 'naive' empiricist accounts embarrassingly ignore the role of the subject, and therefore *the will*. If the supposedly objective scientific 'world picture' is taken to be the entire world picture, then we ourselves - as

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[77] Philosophy of science involves two separate practices, says van Fraassen. One the one hand, there is the study of models, actual phenomenon and the relationships between them. On the other, there is the subsidiary task of analysing "the structure of the language used in a context where a scientific theory has been accepted", for example, an explication of modal locutions without doxastic metaphysics (1980a: 198).

subjective agents - don't fit into our own world picture (van Fraassen 2002: 189). van Fraassen also regrets that in his (1980a) he was mistakenly trying to straightforwardly adapt his "notion of acceptance designed for deterministic theories to [cases] where probability is involved" (2007: 338).[78] More on probability theory later.

The empiricist cannot make dogmatic *a priori* statements about what we should believe, such as: believe only what observation tells you. This leads to self-refutation, since that statement itself is not gleaned from observation. van Fraassen's 'new empiricism' is, therefore, not a doctrine to be believed, but rather a stance to be held and participated in. He now endeavours only to defend the reasonableness of agnosticism about unobservables, and allows that realism can be a rational position for those inclined to it. One can be a scientific gnostic and an empiricist, but one would then be *choosing* beliefs that go beyond what science aims at or requires (i.e. saving the phenomena).

Experience of some phenomenon is not a passive activity, says van Fraassen. We engage with our experience, making interpretations and choices, as we "approach the world and relate to our experience" (2002: 194). CE focused on the fairly narrow question: what should an empiricist think about science? While stance empiricism tackles the broader, more challenging, question: what is it to be an empiricist? In his (2002), van Fraassen now thinks in perspectivist and voluntarist terms, thereby introducing an explicitly existentialist flavour to his philosophy.[79] Adopting the empiricist stance involves displaying certain *attitudes* that include "taking the empirical sciences as a paradigm of rationality and resisting demands for further explanations (i.e. demands that would lead to metaphysics)" (Okruhlik ibid.). This new stance schema can, as we will see, accommodate Kuhnian revolutionary change.[80] By taking a deflationary approach to rationality, it can ostensibly show how a rational agent can willingly move from one paradigm to another, and also how rational disagreement among theorizers is possible. Let us take a closer

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[78] In 1980 van Fraassen had subscribed to an objectivist interpretation of probability, in which 'belief' is the operative term. After 2002, though, he changes to a subjectivist interpretation, centred around the notion of 'opinion', rather than belief (van Fraassen 2008: 318).

[79] van Fraassen's (2002) is based on his series of Terry Lectures, and incorporates many theological - as well as scientific and philosophical - themes.

[80] van Fraassen gives a definitive 'yes!' answer to the question of whether there really are radical Kuhnian scientific revolutions. See Psillos (1999), though, for the counter-argument, in which he endeavours to separate essential from idle parts of theories. When this is done, the history of science looks like a relatively stable evolutionary process, from which core theoretical principles and explanatory hypotheses emerge (see also Ruttkamp-Bloem 2013; 2015 for a similar synthesis).

look at van Fraassen's stance empiricism, in which he argues "for a view of philosophy as stance, as existential" (2002: xviii).

2.2.2.a. The stance *stance*

As one may expect, van Fraassen sets his theory up in opposition to scientific realism, or what he calls the "metaphysical enterprise [that] subverts our understanding both of our own humanity and of the divine" (2002: 4).[81] He insists that ontology cannot be read off of scientific theories. When the realist offers an ontology that is an apparent continuation of science, she utilizes *values* and *desires* as well as theoretical and empirical criterion. The realist's materialistic naturalism is "nothing more than a certain attitude. . . [a] deference to the content of physics" (190). At best, those inclined towards realism can pursue:

> a sense of understanding in which truth is bracketed, in order to give us a handle on the conceptual structure of a theory, seen from various perspectives, and. . . give us some assurance of its coherence (van Fraassen 2007: 358).

In this form metaphysics is tolerable to van Fraassen: if it is reconceptualised as 'interpretation' attempting to aid intelligibility, rather than a search for capital 'T' truth. The stance empiricist makes no definitive factual claims, nor forms conclusive beliefs, about what the world is *really* like. The realist, on the other hand, claims to infer to the *best* explanation, but van Fraassen want to know "[w]hat is good, better, best? What values are slipped in here. . . and where do they come from?" (2008: 14). Contra both realism and CE van Fraassen proposes that a suitable philosophical position can:

> consist in a stance (attitude, commitment, approach, a cluster of such - possibly including some propositional attitudes such as beliefs as well). Such a stance can of course be expressed. . . but cannot be simply equated with having beliefs or making assertions about what there is (2002: 47-8).[82]

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[81] Chakravartty has argued convincingly that van Fraassen, despite valiant effort, cannot escape metaphysical posits. Stances or paradigms are "unobservable, cognitive, cultural, heuristic entities, underlying the phenomenon of observation" (Chakravartty 2007b: 204). They are speculatively posited to *explain* the relevant social phenomena. It seems that van Fraassen is declaring unobservable entities to exist, in order to explain things! In fact, concludes Chakravartty, all of us "will make recourse to the unobservable in fashioning a coherent picture of empirical enquiry" (205). In similar vein, Baumann (2011) complains about the vagueness of van Fraassen's stance notion: how many "of them are there and which ones are there?" (28).

[82] Dennett (1987), of 'real patterns' fame, agrees that while belief is a perfectly objective phenomenon, it can "be discerned only from the point of view of one who adopts a certain predictive strategy, and its existence can be confirmed only by an assessment of the success of that strategy" (15).

Stances are not identical to the factual claims they incorporate at any given time. A position like metaphysical realism is not a theory about what there is; it has no precise definition, nor any empirical support. It is only an attitude or cluster of attitudes - a research program: incorporating opinions, inclinations, personal values etc. Bitbol (2007) sums up what a stance is nicely: it is "an 'epistemic policy'. . . primarily a way of behaving, an interpretative orientation; a commitment to act and understand events along with a certain outlook" (230-1). Empiricism must, of course, also be a stance rather than a thesis, thereby avoiding the self-refutation problem that hurt the logical positivists.[83] On van Fraassen's account, rational discourse about values, attitudes and commitments is also possible between realists and anti-realists. Rationality is independent of ontological assertions, and "does [not] require any commitment to follow a rule devised beforehand" (van Fraassen 1989: 174). He understands science now as "a paradigm of rational inquiry", teaching us "how to give up our beliefs" given that they may well be overturned by future evidence (2002: 63).

2.2.2.b. How does stance empiricism work?

Recall that van Fraassen is concerned with epistemology, rather than semantics or ontology. In developing his stance *stance*, "the focal question is this: How are we to understand scientific and conceptual revolutions?" (2002: 64). This is "an unsolvable problem for objectifying epistemologies" (81) of the sort endorsed by Ladyman and Hacking (as we saw when discussing PMI). The realist therefore faces a dilemma, says van Fraassen: either become an empiricist, or else be content with an epistemology that fails to allow for scientific and conceptual revolutions. Stances, on the other hand, are adaptable; they can persist through theory change and, *mutatis mutandis*, through changes in belief (62). A stance, being an attitude or approach, can remain constant, even while specific variables (beliefs, propositions or models) within the stance are shifting through time.

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[83] It is debatable whether this move really escapes the positivist's self-refutation problem. Ladyman (2004), then Mohler (2007), for example, argue persuasively that the empiricist must hold certain beliefs constant in order to coherently maintain the empiricist stance: in order to anchor the stance, as it were. The van Fraassian must subscribe to certain core beliefs about "the adequacy of various methods of belief acquisition to the achievement of the goals/policies/intentions associated with those attitudes" (Mohler 2007: 214). For example: a belief in the adequacy of the scientific method, and the inadequacy of explanatory metaphysics. van Fraassen (2004) replies that any beliefs attending the empiricist stance do not form the basis for the stance. Instead, they 'come along for the ride', so to speak, as a natural outgrowth of empiricist attitudes.

Stance empiricism takes inspiration from James (1896/1956), who proposed that, epistemically, we aim to believe truth and avoid error.[84] This presents another dilemma. If we want truth, then we should believe everything, thereby ensuring that all truths are encompassed within our overarching set of beliefs. On the other hand, if we want to avoid error, we should "cut our opinions down to the bare bone, perhaps end up believing tautologies only" (van Fraassen 2002: 86). The pragmatist/empiricist cannot both gain informativeness and expect accuracy. She cannot infallibly choose only beliefs that are guaranteed truth-apt. James' two values pull in opposite directions, they cannot be jointly maximized: to some extent, each is gotten at the expense of the other. Some balance between the two extremes is required. This is a third ingredient: "the balance. . . the measure. . . of truth believed as against error avoided" (van Fraassen 2002: 87).[85] Truth and error are objective categories, but the measure of balance is not. The very notion of *balance* between two conflicting aims introduces a *value judgement*, according to stance empiricism.

If we follow this method, then we measure and balance the value of increasing our information content against the risk of false beliefs. Finding a point of equilibrium on this continuum between truth and error is not determined by logic or empirical output. We supply it ourselves through a contextual value judgement, and then a choice: a 'leap of faith'. Consequently, the "element of personal decision, values, and volition has entered and received a legitimate place in our epistemic life" (2002: 91). Belief, says van Fraassen, "is a matter of the will" (1984: 256). Change of belief is not forced on us; "what it is rational to believe includes anything that one is not rationally compelled to disbelieve" (1989: 172). Consequently, he sums up with his much quoted slogan: "rationality is but bridled irrationality" (2002: 92).

Chakravartty (2007b) understands van Fraassen's stance schema as consisting of three 'levels' of epistemic analysis. The first consists of factual claims: propositions that are potential objects of belief. The second is the stance level: stances are clusters of policies or strategies, adopted methodologically for generating factual beliefs. Third is the level of 'meta-stances', where stance choice takes place, and where voluntarism - constrained by permissive rationality - enters into the picture.

2.2.2.c. Emotions

[84] For van Fraassen, this is a pragmatic kind of truth, of course, referring to observables only.

[85] A fourth ingredient to van Fraassen's schema is 'relevance': "we want information about what concerns us", about what is relevant to our purposes (2002: 87).

How can we exercise *the will* in this way if the future is unclear? How can the process of (theory) choice be intelligible if it "takes us into options that we could not see as genuine options beforehand?" (van Fraassen 2002: 93-4). van Fraassen again turns to James, whose account of decision-making he sees as the paradigm for modern probability theory. He asserts that each agent - when hoping, judging and deciding - "must consider the value of each possible outcome and also its probability" (95). There is no *rational* recipe for this epistemic 'gamble' though; we each carry the weight of responsibility for our probabilistic assertions. Enter values again, and choice of values, which "we can only understand under the heading of emotion, not as factual or theoretical deliberation" (143). As James (ibid.) famously declared:

> Our passional nature not only lawfully may, but must, decide an option between propositions, whenever it is a genuine option that cannot by its nature be decided on intellectual grounds (558).

van Fraassen turns next to Sartre's (1948) essay on emotions:

> on Sartre's account, the central factor in emotion is a certain kind of change in view that transforms our subjective situation. This gives emotion a definite cognitive and volitional function. . . The values of the possible outcomes of various actions are changed, in a way that changes the action itself into something different (van Fraassen 2002: 104-6).

Epistemology is thereby apparently transformed by subjective hopes, passions and interpretations. Changing from one paradigm (or stance or perspective) to another, during revolutionary theory change, is akin to a "traumatic conversion experience" (van Fraassen 2002: 66).[86] This change is, though, not rationally compelled - it is rationally *permitted* - surmises van Fraassen. Also, the underdetermination of theory by data doesn't necessitate speculative abduction, *à la* Ladyman, but rather allows space for emotions to facilitate the progression of our epistemic commitments.[87] No dogmatic, absolutist theory can be sustained in the face of

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[86] Kuhn, himself, likened change of paradigm, in the face of scientific revolutions, to a religious conversion. See his (1962/1996: 149).

[87] Some have criticized that van Fraassen's disdain for IBE should lead him to doubt the existence of other minds, since other minds are not ostensibly known through direct experience (see Ladyman 2007: 47). Magnus (2005), similarly, notices that the problem of other minds is arguably underdetermined. Two rival theories - one that others have minds, and the other that everyone else is a zombie - "would lead us to expect exactly the same course of events in the world" (30). There is therefore *prima facie* reason to allow, at least, some abduction in the face of underdetermination, on pain of radical Cartesian doubt or anything-goes permissivism. Musgrave (2018) sums up nicely, that if you "deny scientific realism you will find it hard to avoid slipping down into Berkeley's denial of commonsense realism as well" (58).

underdetermination, "only non- or antifoundational positions can survive" (130). van Fraassen (2000; 2007), in fact, dismisses as 'left behind' the traditional foundationalist versus coherentist (as well as the externalist versus internalist) debate in epistemology. In favour of what exactly though?

2.2.2.d. Isn't this just relativism?

Chakravartty (2007b) submits that stance empiricism is "subject to a form of relativism that renders it effective only to the ears of empiricists" (195). He argues that the realist's 'metaphysical stance' cannot be shown to be internally incoherent. Metaphysicians simply have different values to the empiricist. Accordingly, the stance empiricist - in order to be consistent - must embrace relativism, since she cannot step outside of her stance to take a God's eye view. It seems that no amount of philosophizing can bridge the divides between internally coherent stances, and no stance can establish a non-question begging demonstration of its own justification. van Fraassen recognises that he is inviting the usual dilemmas of sceptical relativism: "how to respect the coexistence. . . of alternative beliefs and attitudes without giving up one's own" (2002: 133). He surmises that non-foundationalism is the human condition: our *actual* situation. Philosophical activity cannot rescue us, but should rather account for how "we can live and function epistemically perfectly well (as we sometimes do) and how changing one's mind remains rationally possible" (ibid.).

van Fraassen's view is therefore, as I understand it, a deflationary or negative position opposed to any kind of foundationalism. He insists that this 'perspectivism', or 'contextual epistemology', is - *pace* Chakravartty - not a debilitating form of relativism however. It cannot be, continues van Fraassen, since we somehow function coherently in the world as it is. We can clearly gain knowledge and form reliable beliefs without any foundation.[88] van Fraassen's voluntarism is purportedly not a crude, anything-goes sort. Requirements of reason, rationality, and/or internal coherence of a stance (loosely) constrain his voluntarist epistemology:

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[88] Consistent with his empiricism, van Fraassen often employs this demonstrative tactic throughout his writings. He argues against some-or-other typically metaphysical claim by pointing to the world and saying, in effect: 'no, *look* there - at what is happening before our eyes - *there* is the fact of the matter, even we if can't explain it'. He feels no obligation to justify worldly facts by *a priori* unification: by deductive simplification to some deeper principle. This applicative, no-nonsense approach is a great strength of empiricism, in my view. There are no invisible mechanisms, so liberally posited by non-empiricists, which make sense only to the individual making the ontological declaration. (The archetypal example of this ontological liberalism would be Derrida's (1963) bizarre concept - *qua* non-concept - *différance*.)

> *nothing more* than staying within the bounds of reason is needed for [rationality], nothing is needed above and beyond coherence. Thus any truly coherent position is rational (van Fraassen 2000: 277).

The 'rules' of coherence in stance empiricism are mostly concerned with how a body of beliefs - i.e. a stance - 'hang together', not the content of those beliefs. van Fraassen acknowledges that his constraints of coherence "are really empty, because they don't limit the factual content of belief at all" (2001: 168). Nevertheless, the rationality just mentioned is bounded by (1) one's prior commitments and opinions, (2) consistency with probabilistic logic and (3) 'no self-sabotage', meaning: a "decision is unreasonable if vindication is *a priori* precluded" (van Fraassen 1983: 297).[89] Rationality is not an anchor for theorizing, but rather consists in "how well we criticize, amend, and update our given condition" (2002: 139).[90] We supply:

> our own opinion, with nothing to ground it, and no method to give us any extra source of knowledge. Only the 'empty' techniques of logic and pure math are available either to refine and improve or expose the defects of this opinion. That is the human condition. But it is enough (van Fraassen 2000: 279).

Given the permissive nature of stance constraints, we are also able to "stand back sufficiently to see how we may need to change and can change" (van Fraassen 2002: 138). During scientific revolutions, we may find ourselves stuck in a dying paradigm "burdened by more and more blatant anomalies" (102): in "a situation of severe epistemic hardship and increasing despair" (92). Bold new ideas, such as Galileo's heliocentrism or Einstein's warped space-time, are a "call to radical conversion" (71). We are faced with an existentialist moment, requiring an escape - a leap!

van Fraassen's process involves changes in values, and values can only be made sense of in terms of emotions (as explicated on pages 74 and 75). Our definition of rationality must admit "a place for emotion, or analogues to emotion" (2002: 139). We cannot "theorize about the world without making decisions and choices about how to theorize" says van Fraassen (224). There is, however, vagueness and ambiguity is our scientific discourse. The word 'science' itself, "displays [an]

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[89] Self-sabotage would include (1) logical incoherence, such as: believing contradictions (*P* and ~*P*) or non-conformity to the probability calculus (probabilities not totalling to 1); (2) pragmatic incoherence: holding attitudes or methods that undermine or conflict with each other.

[90] Ladyman has complained that van Fraassen's permissive, epistemic voluntarism "according to which ampliative inferences are permitted but never mandatory, amounts to a manifesto for epistemological anarchy" (Ladyman 2007: 47). He holds that, despite van Fraassen's protestations, anything-goes if we can choose what to know and what to believe.

ambiguity between activity and product" (155). Interpretation is always needed when we try to make sense of science. van Fraassen concludes that one can, perhaps, think of scientists as artists: portrait painters interpreting their subject matter. Science, "like art, interprets the phenomena, and not in a uniquely compelled way" (149). We, the viewers, must interpret the scientist's art, and thereby hopefully "see nature and humanity in a new way" (ibid.).

I have summarized van Fraassen's stance empiricism, as developed in his (2002).[91] We have seen how he abandons his previous empirical foundation for a new emotivist pluralism, in which empiricism is but one of many internally coherent stances. Next we will explore his most recent position: empiricist structuralism (ES). Although this theory is consistent with CE, it does not depend on it. What both have in common is a rejection of any appeal for capital 'E' explanation by reference to an underlying reality. The search for understanding "would not be aided but hindered by insistence that every regularity must have a reason" (van Fraassen 1991: 374). Empiricist structuralism "is *not a view of what nature is like but a view of what science is*" (Okruhlik 2014: 658).

2.2.3. Empiricist structuralism (ES)

The structural realists, a.k.a. Worrall (ESR) and Ladyman (OSR), declare that all we know is structure. In van Fraassian terms, this just means that science:

> represents empirical phenomena as embeddable in certain abstract structures (i.e. theoretical models), and those structures are describable only up to structural isomorphism (Okruhlik ibid.).

Considering that van Fraassen subscribes to the semantic interpretation of scientific theories (discussed on page 61) the challenge for him in developing an empiricist alternative to structural realism is how to give an account of the connection between the actual empirical phenomena and the mathematical structures in *representational* artefacts. Artefacts that are both 'concrete physical' (graphs, scale models, computer displays etc.) and 'abstract mathematical' (theoretical models).

Scientific representation is, on an ES account, not purely a function of theoretical models. In order to understand scientific representation, we must appreciate that it is mediated by measurement and experimentation. How is the represented like its representation? Just how does something playing a representational role play that role? In order to answer these questions, suggests van Fraassen, it is necessary to understand the *use* of representational artefacts, and also

[91] Based on his 2000-2001 Locke Lectures.

what characteristics are germane to the role they play in this use (2008: 2). Also important, as we will see, is the role of 'non-objectifying' notions such as 'perspective', 'indexicality', 'intentionality' and 'intensionality'. Let us now follow van Fraassen's argument for ES.

2.2.3.a. What is representation?

van Fraassen once again takes as his starting point a duty to empiricism. This he defines as 'common sense realism', predicated on reference, and thereby commitment, to *observable* phenomena. As already mentioned, van Fraassian empiricism involves a certain attitude that takes "empirical science as a paradigm of rational inquiry" (2008: 3). No explanations or extensions of science are demanded, and no belief in overall truth. The only aim is empirical adequacy - as defined in the section on CE.

Representation cannot simply be equated with 'copy', 'likeness' or 'resemblance', begins van Fraassen, other than in the simplest of cases. There is "an asymmetry in representation that resemblance does not have" (ibid: 17). If *X* represents *Y*, then *Y* does not necessarily represent *X*, but a relationship of resemblance goes both ways. The standard minimal locution for representation takes the form of: *X* represents *Y* as *F*. "Here X is a representation, Y its referent, and F a predicate that X depicts Y as instantiating", explains van Fraassen (20). A representation may carry 'surplus structure', in that sometimes "the best representation [is] one that embeds its target in a larger structure" (30). Sometimes an accurate representation requires deliberate distortion: misrepresentation, due to considering the perspective from which the representation will be viewed. Consequently, "*misrepresentation is a species of representation*" (15), and traditional attempts to explain representation as a structural relation between representation and represented usually fail on this issue. Intuitively, we can recognise a misrepresentation as being about the represented, but also recognise that something is wrong: the misrepresentation is a caricature of sorts.

Realists take scientific representation to be representation of reality. For van Fraassen, experimental data is nothing more than a representation of an observable fragment of a fundamentally unobservable universe. Also, naturalistic or referential accounts of representation don't consider the perspectival, interpretative attributes that representation includes. What counts as an accurate representation is highly context-dependent, insists van Fraassen. It depends on what criteria of success are used, i.e. what the *purpose* of the representation is.[92] A representation has no semantic content independent of *use*. The question 'what does *X* represent?' is elliptical

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[92] Resultantly, van Fraassen does not advocate a 'theory of representation' as such; "this could not possibly be offered. . . since that would be circular" (2008: 23).

for 'what is *X* being used to represent?' This introduces a (deflationary/pragmatic) triadic relationship between symbol, user, and thing, instead of a (realist/substantive) dyadic relationship between only symbol and thing (Knuuttila 2005). As such, our minimal locution in the previous paragraph needs to be extended to: *Z* (being some agent) uses *X* to depict *Y* as *F*.

The system of symbols that are used in a community (of scientists) to form representations must, naturally, be commonly held and understood conventions. Representations are therefore relative to pragmatic, indexical systems.[93] This emphasis on *use* excludes representations being mental, since we do not put brain states to use in the relevant sense, according to van Fraassen. Also, representations cannot be natural - as in 'naturally produced' - independent of intentional activity. Representations must have a *role* bestowed on them by an agent. Inert structures or patterns are not representations if they merely happen to resemble something or other. They are only representational artefacts if they have a representational function - an *aboutness* (*qua meaning*) - bestowed on them by an intentional agent. So, for example, to ask 'what is in the picture?' - while looking at a photograph - is to ask 'what is the picture about?' A coherent answer requires consideration of the pragmatics of the situation, specifically, the intent of any agents concerned. If one asks 'what is in the picture?' "while taking the picture simply to be [a] physical object. . . the answer would have to be 'Nothing!'" (van Fraassen 2008: 25).

Consistent with his empiricism, van Fraassen reminds us that "there is no universally valid inference from what the best representation is like to what the represented is like" (ibid: 30). A representational artefact is both a natural and a cultural object, relying on use, context and meaning for its character.

2.2.3.b. Imaging, picturing and occlusion

Everything resembles everything else in some way or another, and so scientific representation must be a *selective* resemblance. Resemblance may consist:

> in having properties in common, or instead in having properties that have properties in common with relevant properties in what is represented. That is, the representor may have properties which form a structure resembling a structure formed by the properties of the represented, and so on up the hierarchy of types (van Fraassen 2008: 34).

[93] van Fraassen adds that although representation is indexical, "it is not 'subjective' in contrast to 'public' or 'inter-subjective'" (2008: 182). Representations typically refer to a particular vantage point, but this vantage point is a publically ascertainable feature of the representational set-up.

Resemblance is therefore a special case of representation that van Fraassen calls an 'image'. "Imaging. . . is representation that is effected through resemblance" (ibid: 56), i.e. when a resemblance contributes to the success of a representation. Continuing the terms of art, he introduces the concept of 'picturing'. This involves the formation of a 'picture' by the addition of 'perspective' or 'distortion' to an image. Picturing may diminish, or even exclude, certain aspects of what is to be represented, since the introduction of perspective can artificially distort or hide some parameters for the benefit of others; this is 'occlusion'.[94] A picture can be multi-dimensional, and may be varied in different ways so that some objects come to the fore, and other objects are *occluded*.

2.2.3.c. Scientific and mathematical modelling

Scientific representations are typically *used* to convey information, and the application of the representation is successful if the viewer of the representation receives that information. Scientific models, like maps, provide "input for an application, where conditional predictions made *on the basis* of that model feed into planning and action" (van Fraassen 2008: 76). A scientific *scale* model can be thought of as a three-dimensional pictorial representation, continues van Fraassen. Scaling is not simply reduction or increase in size, and it involves - not just resemblance - but also 'suitable' approximation, distortion and occlusion, according to the purpose at hand.[95] In other words, "a scale model of X is an object which is structurally similar to X but suitably smaller. . . The goal implicit in 'suitable' will determine the contextual parameters for" terms such as 'structurally', 'similar' and 'smaller' (51).

In mathematical modelling the representor is a mathematical object that can be distorted in various ways. In fact, mathematical modelling, *qua* abstraction, unfailingly includes distortions, submits van Fraassen. Although this cannot be demonstrated *a priori*, he takes it to be obvious that every imperfect detail of some part the world can never be captured in a representation. There is always idealisation; mathematical representation of nature invariably involves features

[94] Other notable varieties of perspective and/or distortion are 'marginal distortion', 'texture-fading' or 'graining' and 'angle' (van Fraassen 2008: 34).

[95] Testing a scientific hypothesis on a scale model, and drawing conclusions there from, should be practiced with caution, adds van Fraassen. For example, a scaled down model of an airplane will maintain proportionate shape, but lose the relevant relation to its environment (since the environment hasn't been scaled down). So, if one wants to study air resistance with respect to an airplane by tests on the scale model, one will have to have a suitably distorted mini-airplane; one that mimics the features relevant to the larger scale phenomenon one is interested in. This is because the air molecules interacting with the model cannot be scaled down. The scale model must *selectively resemble* the real object (2008: 50).

that are failures in resemblance. The scientific image, as much as the manifest image, "harbours vagueness and ambiguity" (ibid: 45). The common assumption that "if certain conditions follow from the ideal case, then approximately those conditions will follow from an approximation to the ideal case" is questionable; "even small departures in approximation can have widely divergent consequences" (52-3). Humility is called for when constructing models, urges van Fraassen, since - although representations can be useful - "the idea of 'perfect' modelling is so far from realistic that it can certainly not be maintained" (57).

Einstein created a new mathematical object: space-time. Light paths are curves in this object, but we are not to conclude that the predictive success of Einstein's theory indicates that space-time is a *real thing*. On van Fraassen's reading, special relativity says merely that "we are to use *this* mathematical object *in that way* to represent the natural phenomena in this domain" (ibid: 71). We cannot know how things are, only how they *appear* to an observer. An account of 'appearance' must look into what is *invariant* under projection when moving from one visual perspective to another (more on 'appearances' later). The invariant content of a variety of perspectival images is their 'common structure'. The content of the images is 'latching onto' a common geometric structure. The invariant content "carries the information in the perspectival image that is independent of the choice of origin and orientation" (73). By analogy to art again, one can perhaps think of the 'proportions' of the subject matter being retained across the variety of images.

van Fraassen concludes that "scientific models trade for their success on resemblance with respect to structure alone" (ibid: 199). As per CE, a "model's structure must be taken to reveal structure in the observable phenomenon, while the rest of the model must be serving that purpose indirectly" (87). Thinking of the phenomenon as embedded in a 'larger' *unobservable* structure can be *useful*, nothing more.

2.2.3.d. Measurement

Measurement ascertains, in certain respects, the invariant structure common to different perspectives. In van Fraassen's schema, one can think of measurement as locating or orienting an agent with respect to the public information about some object of interest. Alternatively, measurements are the instrumental means for arriving at indexical judgements needed to use representations. However, if two objects are under study, they may incorrectly appear to differ in some property due to a difference in some other property. For example, is someone wants to measure the velocity of two motorcars from the side of the road, the car nearer to her will appear to be going faster than the car further away. In other words, the content of her "measurement would be *what the motions look like* and not *what they are*" (van Fraassen 2008: 69). This variance must be artificially adjusted for according to prior knowledge of, for example, the geometry of space and the laws of projection.

There is, as in CE, no experimental science without assumptions, presuppositions and interpretations: "all our reading, even in the general sense of taking notice of measurement results of any sort, is always theory-laden" (ibid: 75). "[*E*]*xperimentation is the continuation of theory construction by other means*" (112); in fact, even just making a single observation statement about the most simple phenomenon is to *locate oneself* in the logical space provided by the relevant background theoretical framework. A measurement parameter relates to a set of possible values, continues van Fraassen, which is itself nomologically linked to a defined region in a much larger logical state space specified by a theory. In other words: the outcome of "a measurement operation is a representation of the target, but it represents the object as it appears in that measurement set-up" (176). Important to note is that the outcome is typically not at an exact point of the state space, but in a region. Measurement is imprecise; it:

> assigns not numbers but intervals, hence takes its assignments from a partial rather than linear ordering. . . what is measured is usually only some aspect of [a] 'field of possibilities' (ibid: 165).

As to the question: what is measured? ES holds that it is: a physical system characterized by certain parameters. A measurement of the system is a measurement of one of those parameters.[96] When the evolution of scientific theory and experimental practice is stable, there is a "theoretical *classification* in place, of the objects that can be measured, before those measurements are made" (ibid: 142). Measurement always involves a physical interaction between 'object' and 'apparatus'. This is 'the physical correlate of the measurement', says van Fraassen: a physical precondition for meaning. A meaningful outcome to a measurement is the information gathered "at the end of a suitably structured physical interaction" (143). A measurement interaction is represented in a theory if:

> the theoretical characterization of the measurement situations is. . . coherent with the claims about the existence of measurement outcomes, their relation to what is measured, and their function as sources of information (ibid: 145).

Measurement is not simply matching numbers to data. There must be a 'coherent story', viz. coherence conditions, about how outcomes of measurements provide information to us about

[96] van Fraassen understands the terms 'parameter', 'property', 'quantity' and 'observable' (that pertain to an object) to be interchangeable in the context of measurement.

what is being measured (bearing in mind that the measured object is already classified theoretically).

2.2.3.e. Observation

Crucially, ES distinguishes detection by means of instruments from ‘observation’: “observation is perception, and perception is something possible for us, if at all, without instruments” (van Fraassen 2008: 93). It is commonly thought that instruments - whether microscopes, MRI scanners or particle accelerators - open a ‘window’ to a world beyond the appearances. This metaphor may be a helpful heuristic at times, but it ignores the wilful, active role played by the experimenter. van Fraassen prefers to think of experimentation in terms of “a literal *enlargement of the observable world,* by the creation of new observable phenomena, rather than a metaphorical *extension of our senses*” (99). As such, we never see *beyond* appearances; by definition appearances are what appear to us. Instruments, taken by realists to represent the invisible mimetically are, in fact, experimental arrangements expanding and enriching the observable world. For example, Faraday - using an electromagnet - created the familiar phenomenon of elliptically patterned iron filings on a sheet of paper (which theory must account for). He did not, on an ES account, discover waves in the electro-magnetic field, or anything like that.

van Fraassen focuses on the humble optical microscope as he elaborates his theory of measurement, experiment and instrumentation. When we ‘view’ the world through instruments we are not having an experience of seeing objects, we are having an experience of seeing images of something/s. The phenomena that microscopes create are optical phenomena, such as reflections and mirages. We refer to these phenomena using count nouns, but this is a mistake, according to ES. Phenomena are not independent ‘things’ or properties of things in the conventional sense. They are not located in some specific place when viewed from different perspectives, yet they are also not hallucinations or dreams. As such, “*[n]ature creates public hallucinations*” that can even be captured by a camera, and are scientifically significant (ibid: 103). Instruments mimic nature: they are ‘engines’ creating *artificial* public hallucinations.

van Fraassen argues that what is observable is on ‘our side’ of the optical microscope, and therefore worthy of epistemic commitment. Images of unobservables, created by instruments - such as spectroscopes, x-ray cameras and cloud chambers - are not granting epistemic access to a hidden-from-view ontology. They are, instead, latching onto *significant* regularities in the phenomena.[97] We will see later how this observable/unobservable distinction becomes the primary target for critics of van Fraassen's empiricism.

2.2.3.f. The evolution of theory and measurement

One cannot, of course, identify what is measured solely from within theory. What counts as a measurement of an observable, and what an observable is, are intertwined questions.[98] This hints of circularity; is van Fraassen trapped in the hermeneutic circle?[99] No, he answers, there is no moment outside history: we are sailors in Neurath's boat (Neurath 1921). This kind of apparent circularity is a perennial problem in physics. Physicists assume that the world is made up of relata and relations, and can be represented mathematically. A starting point (boundary conditions) not amenable to empirical verification needs to be chosen, and there appears to be no privileged point of entry for the introduction of such a choice. Instead, continues van Fraassen, as the evolution of measurement and theory progresses, these choices (or conventions, if you like) are either disproved or vindicated. They are disproved when encountering resistance or are vindicated when the evolution of measurement and theory proceeds smoothly. Measurement and theory evolve together in an entangled way, "and can come to a stable resting point" (van Fraassen 2008: 123). What is measured is identified in this historical process by the "*envisioned eventual* stable measuring practice, while it is differently identified *in retrospect* by the theory that draws on that history for its credentials" (139).

Once this stability has been achieved, we can speak meaningfully of an instrument accurately gauging an observable. Once the observable has, in this way, found a place within a theory, it is no longer defined by measurement, but by its role in the theory. This empiricist interpretation of measurement does not assume a God's eye view, lauds van Fraassen. It respects that we are

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[97] van Fraassen does, though, allow that the observable/unobservable distinction can be drawn in places other than the optical microscope. It is his *preference* to make the demarcation where he does. What matters is that *some* line is drawn when trying to understand science (2008: 110).

[98] van Fraassen uses the notion of temperature as an example. He analogizes that: "an item has temperature *T* if the stable procedure that we now designate as temperature measurement, when applied to this item, has outcome value *T*" (2008: 122).

[99] Physicists may want to know, for example, "of the equality of successive time intervals (equality of duration of successive processes)" (van Fraassen 2008: 130). The only way, however, to compare two successive processes with respect to duration is with a clock, "but clocks present successive processes that are meant to be equal in duration" (ibid.). A historical example van Fraassen gives is that of the stipulation of the length of a meter. At the Convention of the Meter in 1875 it was decided that a meter is precisely the distance between the midpoints of the *mètre des archives* (a metal bar housed in the French State Archives) at the temperature of melting ice. Temperature, though, is measured in terms of length intervals on a thermometer. Hence, an ostensive hermeneutic circle results.

intimately involved within the Quinean historical process of science. However, once the measurement procedures and theories have stabilized, we can momentarily view things from 'above', so to speak. This is the communal, or public, "world-picture of currently accepted physical theory" (ibid: 141). The criterion for what sorts of interactions can be measurements is that the representation must represent the represented in a *certain* fashion; that is, the outcome must *selectively* resemble the target "at a certain level of abstraction, according to the theory" (ibid.). We will see later what this 'certain selectivity' might be.

2.2.3.g. Traditional structuralism

van Fraassen understands structuralism to be "roughly speaking, the contention that scientific representation is of structure only" (2008: 190), and scientific models are successful if they *structurally* resemble what is being modelled. Alternatively: science represents the phenomena as embeddable in certain abstract structures, *qua* theoretical models, that are describable only up to structural isomorphism. He wants to know, however, what is structure and what is not*?* Traditional attempts to answer this question tend "to dissolve into vacuity or inconsistency when pressed to precision" (ibid.). Structuralists, from Russell to Ladyman,[100] have either reified scientific models or regarded "them as delineating the objective structure of a hidden qualitative content" (van Fraassen 2006: 305). van Fraassen, though, believes that "structuralist views of science are right, at heart" (2008: 190), and therefore endeavours to develop an empiricist version of structuralism centred on pragmatics.[101]

Structural realists hold that nature is a relational structure, in the same way that a mathematical object is a structure. This simplistic view has models as pictures or copies of the structure of reality, complains van Fraassen. A physical system then appears, counter-intuitively, to be an abstract entity, with concrete parts or elements. On an ES account, scientific representation must "allow us to go reliably from what we know to what we will or can encounter further on. This is the empirical constraint on scientific theorizing" (van Fraassen 2008: 197). As per CE, unobservables implicit in scientific equations help provide us with useful representations that

[100] In his (2008), van Fraassen only mentions Ladyman in some of the footnotes. This is presumably because Ladyman's main text (Ladyman and Ross 2007) was published at around the same time as van Fraassen's, and only had a major influence on the philosophy of science after some lag. Ladyman's OSR is, though, on my reading, a paradigm case of the kind of structuralism that van Fraassen positions his theory against. As such, in this thesis I take van Fraassen to be implicitly arguing against Ladyman.

[101] van Fraassen defines pragmatics as: "the study of language in which word-thing relations are seen as abstractions from word-thing-user relationships" (2008: 189).

need to fit observed regularities, nothing more. van Fraassen cites Heisenberg (1945) who expressed similar scepticism regarding unobservables in the context of atomic theory:

> The atom of modern physics can be symbolized only through a partial differential equation in an abstract space of many dimensions. . . *All* its qualities are inferential; no material properties can be directly attributed to it. . . any picture of the atom that our imagination is able to invent is for that reason defective (36).

For the empiricist, scientific theorizing is free from "the entire game of metaphysics [with its] illusionary charm and glamour" (van Fraassen 2008: 259). This allows us to exercise the imagination, and draw qualitative consequences from theory without formal calculation. van Fraassen understands his project as the attempt to answer this foundational philosophical question: "*How can an abstract entity, such as a mathematical structure, represent something that is not abstract*, something in nature?" (240). In CE terms, this translates as: "*how, or in what sense, can such an abstract entity as a model 'save' or fail to 'save' this concrete phenomenon?"* (245). In other words, what is the relationship between the mathematical structure described by our equations and some naturally, or artificially, produced phenomenon?

If the target of some function (facilitating a correspondence role between model and world) is a non-mathematical object, then we do not have a well-defined range for that function. A function must have a set as its range, and physical processes, events or objects are not sets. For example, Boyle's ideal gas law - stating that if the volume of a gas is kept constant, a change in pressure is proportionate to a change in temperature - only has meaning if its terms are coordinated with the relevant physical quantities. Pressure, volume and temperature, though, all change with time and vary from one body to the next. The phenomena are arbitrary and chaotic, intimates van Fraassen. There is no epistemic access to a parameter to be measured independent of measurement itself. Insisting that there is leads to a regress, since a new relationship - between oneself and the measurement set-up - would now need to be explained along with the old relationships, and so on.[102] There is no Archimedean point from which to view experiment: "measurement practice and theory evolve together in a thoroughly entangled way". The measured parameter "is *constituted* in the course of this historical development" (ibid: 138).

2.2.3.h. van Fraassen's structuralism

[102] van Fraassen states that we cannot answer the question "'*how can an abstract mathematical structure represent a concrete physical entity?'* by saying '*this is possible if we assume the latter is represented by some other mathematical object'"* (2008: 243).

Having detailed his understanding of the relevant key terms, and of alternative structural accounts, van Fraassen lays out his own empirical structuralism (ES). He agrees that the steady accumulation of knowledge in the sciences is knowledge of structure. However, there are only two sorts of things we deal with directly:

> the concrete, observable things, events, and processes in nature on the one hand and on the other hand, the abstract structures studied in mathematics. We characterize the structure of the former in terms of the latter (2006: 297).

In ES, a phenomenon is embeddable in a model if a representation of that phenomenon is isomorphic to a part of the model. The phenomenon then has the same structure as that part of the model. A theory offers a range of structural models "as candidates for the representation of the phenomena in its domain" (van Fraassen 2008: 250). If the range of structural models contains a candidate within which the phenomenon is embeddable, then the theory is empirically adequate.

van Fraassen understands theory construction as the step-by-step process of increasingly abstract representation: "[s]cience abstracts, it presents us with the structural skeleton of nature only" (2006: 280). The phenomena appear first in the outcome of measurement, then - at slight remove - in data models constructed there from, then in surface models constructed by fine graining patterns in the data models. This final highly abstract construct can then properly confront the pertinent theory. If the former can be suitably embedded in the latter, then a successful representation has taken place. The matching of abstract models to theory can be an unproblematic matching of mathematical structure to mathematical structure. However, the metaphysical realist may insist on an objective explanation of the correspondence between a data model and the physical phenomenon. As discussed on pages 86 and 87, this is mistaken on an ES account. Realists miss the point "exactly because they focus on content rather than function" (van Fraassen 1994: 131). Precisely what relates models to the phenomena is the *user* of the theory.[103] Importantly (and controversially), van Fraassen introduces a new intermediary between the phenomena and an observer - 'appearances':[104]

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[103] Scientific knowledge, in van Fraassen's view, is "objective, in a sense that it implies maximal inter-subjectivity" (2008: 266). The traditional subjective versus objective (or appearance versus reality, or manifest versus scientific image) distinction, and the resultant dilemma of how to relate the two without a regress, is a red herring for the empiricist. The so called 'problem of the external world' is "unsolvable, hence not one that makes sense at all" to even bother with (276).

[104] This marks a change from the original formulation of CE, in which van Fraassen did not "distinguish clearly the observable phenomena from the appearances" (2008: 317). The notion of 'appearances' was used by Kant (1933),

> Phenomena are observable, but their appearance, that is to say, *what they look like in given measurement or observation set-ups*, is to be distinguished from them as much as any person's appearance is to be distinguished from that person (2008: 284-5).

Appearances - the content of possible measurement outcomes - are not subjective impressions. They are public: as per the scientific dictate of experimental repeatability. How an observable object or process, *qua* phenomenon, appears in a measurement outcome is a public, intersubjectively accessible fact; in other words: an *objective* fact. 'Reality' does not refer to the Kantian thing-in-itself, or to a Cartesian external world. A person is an observable phenomenon, but can *appear* quite differently depending on the angle from which one views her. van Fraassen (2006), in typically poetic fashion, explains: "[*a*]*ll we ever get are appearances*, all we ever get are more and novel observable phenomena to save, even if conjured up by previously unimagined instruments" (284). Data and inferences from appearances are used to attempt an accurate description of the phenomenon. In ES, the phenomena admit of objective description by purely theoretical models, independent of a context of observation or act of measurement. Appearances, on the other hand, are how the phenomena look to us in the context of measurement set-ups. Appearances can change in time, varying from one occasion to another; "[h]ow are those appearances saved? By fitting them into the postulated theoretical world" (285). They provide perspectives on the phenomena, and are therefore always limited: a measurement set-up only displays what is inside its range. Scientific representation, as such, consists of three ostensibly different domains:

(1) Theoretically postulated reality

— *Micro structure, forces, fields, global space-time structures*

(2) The observable phenomena

— *Macro objects, motions, tangible and visible bodies*

(3) The appearances

— *Measurement outcomes, 'how things look' in observational context* (van Fraassen 2008: 289).

Appearances are perspectival representations of phenomena. van Fraassen is drawing an important distinction here between perspectival and aperspectival representations. He talks of viewing science 'from within' versus viewing science 'from above'. This echoes the stance

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which he - like van Fraassen - distinguished from 'phenomena'. However van Fraassen makes sure to point out that nothing in his "usage of these two terms refers to [Kant's] factors, unless very indirectly" (2008: 284).

empiricism schema from his (2002), as well as Carnap's internal versus external frameworks idea. Okruhlik (2014) explains that, according to ES, we view science from within when we pragmatically adopt a given perspective "while developing and using theories within a particular historical context" (658). The view from above is aperspectival; it is "the 'official formulation' of general scientific theories" in the public domain (ibid.). One can, perhaps, think of van Fraassen's original CE as the formulation of an empiricist meta-perspective on science. Then, with stance empiricism he proposes that there is, in fact, only a plurality of contextual perspectives. Now, in ES, he attempts to merge the two by developing this dual 'view from above' (CE) and 'view from within' (stance empiricism) synthesis.

Experimental practice involves the gathering of data in a certain way on a certain occasion, so as to construct models according to certain criteria, in order to conform to a created theory. A selective agent must match the relevant steps in the sequence to each other. Nothing in the theory, model or phenomena themselves implies what represents what, and asking for an objective correspondence leads to the regress mentioned on page 87. Representation is a three-way relation; nothing "represents anything except in the sense of being used or taken to do that job or play that role for us" (van Fraassen 2008: 253). To say that a given data model represents a phenomenon, is akin to expressing the indexical judgement 'I am here' on the map: 'I am here' in some logical space of possibilities. The correctness of this judgement pertains not to semantics, but to the pragmatics involved in how the relevant structure is being used. In order to "use a theory or model, to base predictions on it, we have to locate ourselves with respect to it" (261). To claim that a theory is adequate to the phenomena is to claim that it is adequate to the phenomena *as represented by me/us*. van Fraassen thereby establishes a connection between his theory of representation and the world by invoking indexicality. Okruhlik (ibid.) explicates the idea as follows: for *us* the claims:

> (A) that the theory is adequate to the phenomena and the claim
>
> (B) that it is adequate to the phenomena as represented, i.e. *as represented by us* are indeed the same.
>
> *That (A) and (B) are the same for us is a pragmatic tautology* (659).

To hold that the theory is adequate to the appearances, but not to the phenomena, is like saying '*P*, but I don't believe *P*'. It would be Moore-paradoxical to make such as assertion in ES.[105]

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[105] Moore (1942) presented the following sentence:

> I went to the pictures last Tuesday, but I don't believe that I did (543).

2.2.3.i. Language in theories

We have looked in some detail at van Fraassen's conditions for the possibility of scientific representation. These conditions "can be brought under one heading: the crucial role of use and practice" viz. *praxis* (2008: 189). Consistent with the general theme so far, he states that we cannot think in newly introduced theoretical terms except in a context where it is already possible to rely on old terms as meaningful. New terms must be meaningful and useful, but:

> this makes sense only if we think of them as introduced into an already extant language, into *our own language in use*. . . The distinction between what is newly introduced theory, and what is language in which the instruments and measurements are already described, is historically conditioned. (ibid.).

It does not seem that scientific theories can be presented in pure mathematics involving only isomorphic matching. Despite his earlier preference for the semantic interpretation, van Fraassen admits that theories must be regarded, in part at least, as sets of propositions in a scientific language incorporated into mathematical symbolism. Scientific representation, therefore, cannot just be about picturing, observation, measurement etc. This presents a, possibly insurmountable, problem for structuralist interpretations of scientific representation. For traditional structuralists, if there is isomorphism between two entities preserving all definable structural aspects, then the entities are mathematically indiscernible. This understanding of representation is clean and neat, but excludes the unavoidable linguistic terms that come along for the ride.[106]

The structuralist holds that there is a model, the world and a function that maps the model to the world. We, though, have to define or identify this function in order to interpret the language of the relevant theory, explains van Fraassen. We have the complicated task of describing both the function's domain and its range - i.e. "both the syntax of that language and [the world] as well as

The speaker of the sentence appears to have contradicted herself, but there is no logical contradiction involved. These paradoxical 'Moorean sentences' entail a pragmatic contradiction despite being logically consistent. Fine (1991) understands Putnam's no-miracles argument to be "a kind of Moore-type argument in support of realism" (2). It would be Moore-paradoxical to recognise the success of scientific theorizing, yet claim that the entities central to successful scientific theories don't exist.

[106] Also, adds van Fraassen, we can sometimes intuitively discern differences in objects that may have the same structure. For example, a colour space (used for representing the colour capabilities of digital devices) can be isomorphic to some geometric object, but the colour qualia will not be represented in the latter. One could then add more mathematical equations to encompass this difference within a larger structure, but one can never be sure that all the excess variation has been captured (2008: 164-65).

the way in which the former gets mapped onto the latter" (ibid. 233). We can only grasp an interpretation - i.e. a function linking words to parts of the world - if:

> we can identify and describe that function. But we cannot do that unless we can independently describe [the world]. . . A theory says nothing to us unless we can locate ourselves, in our own language, with respect to its content (ibid: 235).

That is to say, in order to identify a correspondence between two entities, we need access to the entities that is independent of the correspondence itself. The traditional structuralist's austere formulation excludes any "direct reference to what we encounter in experience, let alone indexicality, self-reference, or self-location" (ibid: 223). If we cannot describe both a language of a theory and the world, then we cannot have an interpretation of it.

2.2.3.j. Indexicality

The context-sensitivity of representation is expressed in theoretical language by indexicals, such as 'here' and 'there'. This is especially important in scientific theories that include talk of 'frames of reference' in terms of observers, such as Einstein's special theory of relativity.[107] Analogously, in order to use a map, one must introduce what van Fraassen calls 'self-location', a judgement to the effect: 'I am now here on the map'. Indexicality "is sometimes hidden, not apparent in the surface structure" of a description (2008: 86), but it *is* there, and any attempt to 'step outside' of an indexical perspective leads to an infinite regress of attempted meta-perspectives. In fact, in all philosophy, we always start from 'here'; "we can't step out of where we are into a presuppositionless discourse any more that into a view from nowhere" (van Fraassen 2007: 358).

A perspectival view, incorporating indexicals, also includes the metric outcome (having a certain value) of a measuring procedure on an object. It trades "on selective resemblance in just the same way that perspectival picturing does" (van Fraassen 2008: 60). van Fraassen thinks of representation as a three-stage process: theory, data model and reality. It is "the last link [between data model and reality] is the one that is expressed in indexical judgements" (2008: 257). The experimental set-up:

> produces or lends itself to a phenomenon that is meant to provide information about the character of some target object, event or process. The artificially produced or isolated

[107] Talk of 'perspective' and talk of 'frames of reference' are - although similar, crucially different - on an ES account. Perspective assumes a Cartesian God's eye view, while a frame of reference is "identified with an observer equipped with clock and measuring stick" (van Fraassen 2008; 70). van Fraassen complains that the conflation of these two themes has led to much confusion in the philosophy of science.

> phenomenon is treated as providing data about the target, to provide us with a 'view' of it [from 'here'] (ibid: 66).

Indexicality ensures that van Fraassen's theory of representation keeps contact with reality. If the semantic interpretation of theories involves just matching one mathematical model to another, then there appears to be nothing 'attaching' the representational schema to the world.

2.2.3.k. QM and the measurement problem

Lastly, given its central role in OSR, let us now look at how the important topic of QM interpretation is articulated in ES. Bohr - whose instrumentalism (1934) famously clashed with Einstein's realism - expressed that the discovery of QM called into question the "objective existence of phenomena independently of our observation" (115). The formulation of QM imposes a limit on "the possibility of speaking about phenomena as existing objectively" (ibid.).[108] When trying to interpret what QM is saying about the world, we have a puzzle, in that on the one hand the theory offers a:

> precise formalism for the calculation of probabilities for measurement outcomes [but] on the other hand this formalism does not wear an ontology on its sleeves (Esfeld 2013: 224).

QM involves physicals systems "characterized by states that change over time and physical quantities ('observables') which are represented by operators on the state space" (van Fraassen 2008: 297). The Born rule, discussed in the section on CE, specifies the *probability* of possible instrumental outcomes conditional on the performance of a measurement. The quantum state of an isolated system, though, evolves *deterministically* according to quantum dynamics. van Fraassen suggests that we face a dilemma. If 'outcome' and 'measurement' do not refer to quantum states - only to instruments - then QM is obviously incomplete.[109] If, however, 'outcome' and 'measurement' refer to quantum states, then they must include the measuring instruments (as well as the quantum object under study) in that state. The dynamics of system and

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[108] Einstein (1954), though, objected to this kind of sentiment. He held steadfastly to the realist conviction that "belief in an external world independent of the perceiving subject is the basis of all natural science" (266).

[109] Einstein famously argued that the 'spookiness' of QM demonstrates that the theory is incomplete. He insisted that in a:

> complete theory there is an element corresponding to each element of reality. A sufficient condition for the reality of a physical quantity is the possibility of predicting it with certainty, without disturbing the system. (Einstein, Podolsky and Rosen 1935/1970: 123).

He went on to spend the last few decades of his life fruitlessly striving to build a 'theory of everything' from the principles of relativity theory.

instruments would now be complete, but then the Born rule would be superfluous, since "'outcome' and 'measurement' are supposed to be in the domain of this dynamics" (299). "The Born Rule cannot be deduced from the dynamics, so it is not superfluous", but its probabilistic nature is inconsistent with the determinism of the dynamics (ibid.). We are left with a dilemma between incompleteness and inconsistency, viz. the notorious measurement problem. Another way to think of the measurement problem is as follows:

> [m]acrophysical objects, including the devices that are used as measurement apparatuses, can be localized in physical space if and only if the microphysical objects that compose them are also localized (Esfeld 2013: 226).

However, the formalism of QM indicates that microphysical objects are not localized in this way. On Ladyman's interpretation, measurement apparartuses are not "the mereological sums of quantum particles. Rather, they are real patterns and their states are legitimate posits of science in so far as they enable us to keep track of the phenomena" (Ladyman and Ross 2007: 182). Bell (2004) understands the measurement problem to imply that either the "wavefunction, as given by the Schrödinger equation, is not everything, or it is not right" (201).

We can translate ES's three-domain schema, described on page 89, into the language of QM:

(1) Reality - the quantum states are the theoretically described *reality*.
(2) Phenomena - "observable objects on which measurements are made, or any observable events and processes targeted for quantum-mechanical explanation" (van Fraassen 2008: 299), are the *phenomena*.[110]
(3) Appearances - the contents of probabilistic, instrumental outcomes (following the Born rule) are the *appearances*.

The Born rule predicts what the appearances will be, "with specified probabilities - under certain conditions" (ibid.). The question - for van Fraassen - is, though: can QM specify a process (whether deterministic or stochastic) by which the appearances are produced? In other words: is there an interpretation of QM that can escape the measurement problem?[111] Measurement is information gathering, and a measurement outcome is both something physical and something that has meaning, according to ES. Standard foundational literature on QM often - for reasons of

[110] Including the entire experimental set-up, viz. "the instruments involved in measurement [and] the physical events that are the final states of such apparatus (which are meaningful as measurement outcomes)" (van Fraassen 2008: 299).

[111] Alternatively, in realist jargon: can the outcome of the production of the appearances - by the measurement of observables - be *explained* (in quantum theoretical terms) by recourse to some *deeper* reality?

parsimony - ignores the indexical complexity involved. A measurement is considered to be a single physical interaction with the object to be measured. The convention in physics is that a measurement procedure requires that a measuring apparatus be 'coupled' to that which is to be measured, in a way that a process will occur that counts as a measurement. That is to say, there must be "a transfer of some character of the object's initial state to the apparatus' final state" (ibid: 150-51).

Experiments, however, have a probabilistic nature, continues van Fraassen (2008). The above intuitive understanding of measurement means that probabilities pertaining to an observable on the final state of the apparatus (a dial, or pointer, or what have you) will reflect the probabilities pertaining to a measured observable in the initial state of the measured object. In practice though, theory is not developed from the 'pure' data of individual measurement outcomes. As detailed on page 88, theory is developed from data models *constructed* (or created) on the basis of data. Data models summarize the relative frequencies found in the data; then surface models 'smooth over', or idealize, this summary. This is done by, for example, replacing discrete variables with continuous functions, or depicting finite data sets as graphs. The highly abstract surface models must then be isomorphically embeddable in theoretical models; only then is theory construction complete.[112]

An experimenter also conventionally assumes that (1) the parameter to be measured in an experiment has a definite value that can be revealed in the measurement outcome, and (2) that measurement faithfully reveals that value. QM violates these two conditions on measurement. QM utilizes - in part - Lagrangian dynamics, "which deals with mechanical systems whose internal constitution is not fully specified" (van Fraassen 2008: 202). Representation in QM is also subject to the kind of distortion discussed from pages 79 to 81. An object may be in a state that implies its location to be probabilistically 'smeared out' over the whole of space, but whatever measuring apparatus we use to study it will only be able to probe a small, finite region. This means that the relevant information gathered will be indefinite beyond a narrow range. Concomitantly, occlusion is also at play in QM: if one observable is measured, explains van Fraassen, "the information that can be gathered with respect to incompatible observables is drastically reined in" (184).

While these issues present a tough challenge to the realist, the measurement problem in QM is "not a problem from an empiricist point of view", says van Fraassen (ibid: 291). Instead it marks

[112] Schlagel (1991) demurs that the belief in a reality that "can be understood to some extent, relative to certain contexts and instruments of investigation, is the *inexpugnable realism* underlying all scientific enquiry!" (315).

a methodological rejection of the realist's deterministic "demand for explanation which is satisfiable only by connections deeper than brute or factual regularity" (283).[113] Bitbol (2007) agrees that the empiricist view of QM consists of a "probabilistic formalism bearing on experimental events, with no need of any remnant of the materialistic ontological [read metaphysical] hierarchy-type" (264). Empiricism provides us with a dissolution, rather than a solution to the measurement problem. According to ES, adequacy of a theory is simply when the relevant surface models fit *properly* into the theoretical models (bearing in mind the linguistic/indexical proviso articulated from pages 90 to 92). This, though, does not simply involve matching between "individual events summarized in the surface model and the states represented in the theoretical model" (van Fraassen 2008: 305). The relevant *frequencies* of events:

> must have a good fit to probability functions, extrapolated from them in surface models, which are identifiable as parts of corresponding probability functions in the theoretical models (ibid.).

In other words, surface models provide probability functions for events that are the outcomes of measurements of phenomena. These probability functions must be matched to "the theoretically specified Born probabilities for the same situation as theoretically represented in terms of possible states and evolutions" (ibid: 305). As such, the relevant *matching* is between two families of probability functions: those in the surface models and those in the theory. When this demand is met (whether strictly or approximately), then "the theory is borne out by the experimental results, and can be used to make predictions" (ibid.). That's all there is to it. ES need not explain the *meaning* of reference, nor the *nature* of correspondence, concludes van Fraassen. Theoretical postulates in QM do not identify the *real* underlying ontology of the world. Attempts to interpret QM in terms of many worlds, hidden variables (*à la* Hacking) or basic *structure* (*à la* Ladyman) suggest a dogmatic allegiance to the metaphysics of 'deeper explanation'. QM need only:

> predict what [the] appearances will be like, and that it can do via the Born Rule to the extent needed in empirical applications: by providing probabilities and expectation values (ibid: 308).

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[113] In his (1991) van Fraassen does concede that realism can sometimes be useful to fundamental physics. Speculative interpretations of QM can suggest how the world *might* be, and thereby stimulate future research. Ladyman (2004), though, wants to know why this conciliatory sentiment can't be extended to metaphysics. Various metaphysical projects - such as atomism, Cartesian mechanism and materialism - have facilitated the development of science.

Our theoretical science is only accountable to the observable part of the world, and:

> that implies for us that what it is in practice directly accountable to are the appearances - the outcomes of the measurements and observations that are actually made (ibid.).

2.2.3.I. Conclusion - ES

We have seen how van Fraassen develops an empiricist version of structuralism in the philosophy of science. He tackles the crucial issue of how to connect the phenomena (or reality) with the mathematical structures in theoretical models, mainly by introducing two distinctions:

(1) Between the phenomena and the appearances; “phenomena possess a kind of reality, where as appearances are representations of those phenomena” (Okruhlik 2014: 658).
(2) Between perspectival and aperspectival representations. These are the view from within and the view from above science. They evolve together in an intertwined way, “combined in a single synoptic vision, while denying. . . a ‘God’s eye view’” (ibid.).

Only perspectival representations of phenomena derived from appearances are embeddable in theory. This involves a fairly straightforward isomorphic mapping of representational mathematical structure to theoretical mathematical structure. The aim of theory is to save the phenomena, though, not save the appearances. In order to explicate the more tricky relationship of physical phenomena to mathematical models, van Fraassen invokes indexicality:

> construction of a data model is precisely the selective relevant depiction of the phenomena *by the user of the theory* required for the possibility of representation of the phenomena (2008: 253).

2.2.4. Standard problems with CE

We have explored the three-part evolution of van Fraassen’s career in some detail. As we have seen throughout, the view has been attacked from many angles. Each aspiring scientific metaphysician cuts her teeth attacking some element of van Fraassen’s complex position. I have highlighted (mostly in the footnotes) a number of criticisms, levelled against the various incarnations of CE. I have, however, saved what I judge to be the major flaws in his theory for last. Despite the *attitude* of stance empiricism being important for understanding van Fraassen’s overarching method, I will generally focus on CE, and its later modification ES now, since these views are more relevant to the theme of my thesis. As seems to be the norm in the literature, from hereon I will simply refer to the combined positions of CE and ES as CE.

Fine (2001) understands there to be two dubious distinctions at the core of CE: (a) between what is observable and what is unobservable, and (b) “between belief and a strong notion of acceptance-with-commitment" (107). These two distinctions play a central supporting role in CE. The first allows van Fraassen to demarcate where genuine empirical science ends, and unwanted metaphysics begins. The second withholds crucial notions of 'belief' and 'truth' from scientific theorizing. I agree with Fine that these two distinctions are saliently problematic elements of CE. I will discuss them in turn.

2.2.4.a. The observable/unobservable distinction

The later Carnap (1966), despite his early positivism, held that 'observable' and 'unobservable' are vague concepts:

> There is a continuum which starts with direct sensory observations and proceeds to enormously complex, indirect methods of observation. Obviously no sharp line can be drawn across this continuum; it is a matter of degree (226).[114]

van Fraassen notoriously draws his observable/unobservable demarcation at the microscope. Anything viewed with the naked eye, through a window or through spectacles is real and can be believed to exist. Anything viewed through a microscope, particle detector or FMRI scanner could be real - but since we can never know that it is - we should be agnostic about whether it exists. He is therefore, counter-intuitively, agnostic about the ontology of blood cells. These unobservable entities are posited in order to account for the phenomena created by the microscope through which they are 'viewed'. As described on page 84, a phenomenon created by an instrument is the image that is a public hallucination. Public hallucinations are the observable phenomena that need to be saved by science. Maxwell (1962), like Carnap, called the observable/unobservable distinction into question. Maxwell, sensibly I think, understood there to be a continuum of vision: looking through a window pane, through glasses, binoculars, a low powered microscope, a high powered microscope etc. Observability is a vague notion, he argued, all entities are observable under suitable conditions, when observability is properly understood as *detectability*. There are continuous degrees of detectability, and therefore no non-arbitrary way to draw a line between observable and unobservable. Hacking (1981), in turn, asks: 'what's so great about 20-20 human vision?' Observation, he insists, "is not passive seeing. Observation is a skill. Any skilled artisan cares for new tools" (135).

Recall that, according to CE, we do not see objects through a microscope - only an image, i.e. a phenomenon. We posit the existence of an unobservable - in this context - in order to account for

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[114] Analogously, Quine (1953; Quine and Ullian 1970) argues that, when it comes to the web of belief, there is no sharp distinction between our core beliefs and our belief at the periphery of the web. Typically, core beliefs deal with unobservable entities and peripheral belief with observable ones.

the phenomena created by the microscope.[115] So, we can have knowledge of observable mites, but must be agnostic about smaller mites, and also - as mentioned - blood cells. The inference from the existence of a phenomenal image to the existence of a blood cell is taboo metaphysical adduction for the CEist. According to Blackburn (2002) van Fraassen must also be agnostic about whether there once existed trilobites. This is because what is observ*able* can only be observed in the future; we cannot travel back into the past.[116] Blackburn enquires snippily:

> What can we do but disdain the fake modesty: 'I don't really believe in trilobites; it is just that I structure all my thoughts about the fossil record by accepting that they existed'? (128).

This agnosticism about blood cells and trilobites strikes many - including myself - as grossly counter-intuitive, and perhaps even disingenuous. Also, Musgrave (2018) wants to know why van Fraassen allows things viewed through spectacles to count as observables, but things viewed through a microscope are somehow unobservables. He sees, as do I, a slippery slope. Once we allow glasses in, why not telescopes or microscopes:

> or any of the many other fancy detection devices that scientists have invented? The observable/unobservable distinction is vague, species-specific and shifting. And yet, idealists and antirealists give it crucial philosophical significance (60).

van Fraassen's observable/unobservable distinction is the most criticised aspect to his CE in the literature. It is *the* central supporting lever in his theory of empiricism, yet appears to all who study the position - except anti-realists themselves - to be arbitrary and *ad hoc*. Bueno (2017) confirms that "for van Fraassen, the role played by the observable/unobservable divide is crucial". After all, "it's in terms of this distinction that the key notion of empirical adequacy is characterized" (100). Musgrave (1985) asks whether a distinction between observable and unobservable entities which is "admitted to be rough-and-ready, species-specific, and of no ontological significance really bear such an epistemological burden?" (205). Critics want to know why exactly the observable/unobservable distinction defines the border between what is

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[115] Hacking (1983) - of entity realism fame - argues that, since microscopists interfere with the entity on the microscope slide, we can take that entity to exist. He suggests that the "conviction that a particular part of the cell is there as imaged is, to say the least, reinforced when, using straightforward physical means, you microinject a fluid into just that part of the cell" (189-90). Furthermore, to attribute the success of modern interventionist microscopy to coincidence (as van Fraassen might want to do) would be to invoke a "Cartesian demon of the microscope" (203).

[116] Putting aside bizarre sci-fi thought experiments involving shrinking people in size, time-travel machines or aliens with electron microscopes for eyes (see Psillos 1999: 190-91 and Churchland 1985: 44-45 for examples).

epistemically accessible and what is not? Why is human observation the benchmark? (Are galaxies millions of light years away observable?)

Theory is "wholly blind to the idiosyncratic distinction between what is and what is not humanly observable, and so should our own ontological commitments" be, asserts Churchland (1985: 35). CE's distinction "is only very feebly principled and is wholly inadequate to bear the great weight that van Fraassen puts on it" (40). Similarly, Ladyman (2002) holds that "any act of perception may be an observation or not, but this does not amount to showing that the *objects* of perception can be classified as *observable* or not" (188). The CEist "hasn't provided an account of what is epistemically special about observation. The closest we get is a discussion of what can be called the *empiricist dogma*" (Bueno 2017: 102). As with any thinker who promotes some universal demarcation, and then uses this distinction as a central support for their worldview, van Fraassen ought to present a robust defence. Instead, he generally shrugs the problem off, wondering 'why all the fuss?'

2.2.4.b. The belief/acceptance distinction

Fine (2001) notices that one of the functions played by the belief/acceptance distinction is to differentiate CE from Ramsey-style instrumentalism. CE is, he says, at heart a pragmatist theory, in which "acceptance-with-commitment for empirically adequate theories. . . is just general reliability" (113). Horwich (1991) - another deflationist - concurs that scientific enquiry aims at generating reliable beliefs, rather than true beliefs, and "we merely *use* theories, believing their verifiable predictions but nothing more" (2). However, "believing a theory reliable amounts to trusting it in all our practical and intellectual endeavours" (Fine 2001: 112). In other words, it appears that van Fraassen's *strong* notion of acceptance is indistinguishable from belief. Horwich further argues that van Fraassen's sceptical notion of 'acceptance' is equivalent to a disposition to use a theory. However, believing a theory "is nothing over and above the mental state responsible for using it" (ibid.). Beliefs are mental states with a certain causal role, consisting of features such as: inferring, deliberating, making utterances and generating predictions. This, though, is to define belief in exactly the way CEists characterize acceptance. So, it seems "belief and acceptance are one and the same state of mind" (ibid.). It is impossible to accept a theory, in the van Fraassian sense, without being in a believing mental state, or without taking an attitude of belief.[117]

[117] See also Jones (2003), who argues that we cannot hold a doxastic commitment to a theory, while collaterally thinking that we hold that commitment because of considerations having nothing to do with the truth of that to which we are committed.

Fine (1991), in typical deflationary style, suggests that scientific inquiry has not suffered at all because of the apparent difficulty over whether belief is entailed in acceptance. Acceptance is "nicely ambiguous, allowing for various specifications (accept as true, as useful, as expedient. . . etc.)" (6). The "ambiguity over the character of acceptance in science that results from not raising the realism/[anti-realism] question seems to be an ambiguity we can quite well live with" (11). Worrall (1984) gives sharp institutional criticism. He points out that success in science is sometimes when:

> one theory, introduced to deal with one set of phenomena, has turned out to predict in a straightforward and uncontrived way, a completely different (and perhaps hitherto unsuspected) phenomenon (68).

This is a sticky problem for van Fraassen's view of scientific theory building and representation, in which presently accepted theories have evolved in, and been formed by, the environment of known empirical results. Newly predicted results are not from within the environment, or stance, where they were first proposed: they could not have been consciously built into the theories. Worrall (ibid.) considers this to be the issue that carries the most pro-realist persuasive power, threatening even the most strident empiricist with a 'creeping realism'. Fine (1986) agrees that CE is, in fact, a restricted version of realism. The position holds that the truth of a theory is identified with "the condition that there is [a] correspondence between reality and one of its models" (van Fraassen 1980: 197). As such, CE contains elements of realism as well as the correspondence theory of truth (but only applicable to what is observable).

Further objections to CE appearing repeatedly in the literature, besides the primary two just discussed, are: (c) the truth versus empirical adequacy distinction, (d) van Fraassen's refusal of metaphysics and (e) the seemingly modal suffix in CE's central notion of 'observ*able*'. I will discuss these three issues in turn.

2.2.4.c. The truth/empirically adequate distinction

In the same way that many - including myself - consider the realist's 'belief' and CE's 'acceptance' to be indistinguishable, so too may 'truth' and 'empirical adequacy' be. Blackburn (2002) argues that once we are immersed in our best theories, there is no room for a distinction between regarding theories merely as useful instruments versus believing them true. There is no difference between using a theory as a point of reference to predict and control the future and, on the other hand, believing in the unobservable ontology of the theory. Anti-realism thereby threatens to collapse into realism.

Musgrave (2018) complains that anti-realists conflate truth with certainty. They rightly point out that scientific explanation can never be certain, but then conclude that science can never achieve truth. A "genuine explanation need not be an ultimate explanation" however (54). Also, the empirical adequacy of any theory is - as with truth - "itself something that is radically underdetermined by any evidence conceivably available to us" (Churchland 1985: 38). Concomitantly, Schlagel (1991) presents an interesting inversion. Anti-realists generally claim - as per PMI - that realists can't explain how some theories are successful. This poses a reverse challenge to anti-realists; can they:

> explain the *falsification* of theories while denying their approximate truth - that is, the possibility of continually refining the theory and/or depicting its structures and properties more accurately - along with any reality to the experimentally discovered entities that contribute to the falsification? (319).

van Fraassen also seems to be guilty of the performative fallacy.[118] He takes only claims about observables to be true, but he arrives at CE by some other method. As per Habermas (1987), if he doesn't take his own position - or the stance *stance* in which it is situated - to be true then there is no reason for us to pay any attention to it. Bueno (2017) asks how "can the observable/unobservable demarcation be correctly drawn", if the CEist cannot believe "it to be *true* that anything is *unobservable*, given that belief in the truth is restricted to the observable" (100). van Fraassen insists that CE is merely one of many rational stances, but then why does he defend only that one stance so passionately and so consistently?

2.2.4.d. CE's rejection of metaphysics

A fourth problem for van Fraassen is his general refusal to ostensibly accept any metaphysics into his theorizing. Countless writers have pointed out moments when he apparently slips into metaphysical inference in his discourse. I have highlighted some of these objections in the footnotes throughout my coverage of CE, and so will not elaborate at length here. Also, van Fraassen has recently allowed that some *soft* metaphysics could perhaps be tolerable, if it is the appropriately 'good' variety. At the end of his (2007), he is willing to concede that his empiricism may involve "metaphysics as well, if we can see this project as the good way to engage in metaphysics" (381). I take this to be a concession.[119]

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[118] See Austin (1962) or Habermas (1987) for a discussion on what the performative fallacy entails. (However, Habermas' interpretation is - on my reading - more applicable to the debate at hand).

[119] Of the very tentative sort, typical of philosophers giving up a previously hard-held stand.

Also, we all engage in some kind of metaphysics as we go about our daily lives making abductive inferences in order to navigate the world. For example, hunter-gatherers infer to the best explanation when tracking wounded animals, says Crisp (2016). They postulate hypotheses about:

> the animals' injuries and locations from appearances on the trail. Such attempts to get behind the appearances and understand the nature and structure of the reality underlying them are. . . a form of metaphysics (62).

It seems van Fraassen must, therefore somehow, allow IBE about observables, while adopting an arbitrary scepticism about inference to unobservables. Putnam (1975) points out that the abduction van Fraassen dismisses is used by scientists themselves to form and justify their beliefs in unobservable entities. In other words, if only observables are actual, and no existing domains of unobservables can be discovered experimentally, then "there is really nothing to influence and guide the construction of theories" (Schlagel 1988: 807). Realism can be seen as a methodological, empirical meta-hypothesis supported by the fact that it offers the best explanation of the success of science. A grand IBE, if you like.

Rosen (1994), notably, has argued that van Fraassen's triadic representational schema - involving theories, models and functions - commits the CEist to the existence of, at least, three sorts of abstract, unobservable objects: "models of the phenomena (data structures), the models that comprise [a theory] and functions from the one to the other" (166). To be agnostic about unobservables means to be agnostic about the empirical adequacy of any theory. CE, "consistently applied thus collapses into radical Pyrrhonism" (167). Also, despite trying to escape the self-justification problem that dogged the logical positivists, van Fraassen seems to have invented a new one. Fine (1986) points out that the property of 'being observable' must somehow itself be observable *according to science*. However we do not know which parts of a theory are the observables, unless we have some criteria of identification. The observability of observables should, as per van Fraassen's empiricism, come out of science. It does not; instead it is "something forced on us a priori by [the] empiricist philosophical stance" (144).

2.2.4.e. The '-able' in 'observable'

The '-able' suffix, which gets so much attention in CE, "refers to us and our limitations according to science" (Fine 1986: 143). Why not merely the 'observed', rather than the modally loaded 'observable'? Because then there would be no reason for scientists to perform experiments that generate new phenomena never observed before. Science would grind to a halt, but one of the hallmarks of good science is how scientists push beyond the limit of what has been observed so far (Monton and van Fraassen 2003: 407).

Ladyman (2000; 2004; 2007; Ladyman and Ross 2007) has been particularly vocal about this apparent modality right at the centre of CE's discourse; '-able' implies an 'if. . . then. . .' clause, and therefore counterfactuals. For van Fraassen a scientific theory must save *all* the actual phenomena - past, present and future - not just the observed phenomena (1980: 12). As such, to even "accept a theory as empirically adequate is to believe something more than is logically implied by the data" (Ladyman 2000: 839). Also, it seems, we can never know for certain that a theory is empirically adequate if future observable phenomena are to be saved (Worrall 1984: 66). If we try to assess whether a theory is empirically adequate, we have to make futuristic guesses about its performance: about what future successful, novel predictions it might make. Lipton (2007) adds that it is:

> implausible to claim that we always have more reason to believe what a theory says about some very distant, though observable, aspect of space-time than what it says about something right in front of our microscope that is just slightly too small to be seen by the naked eye (118).

Ladyman (2000) argues that, although the CEist need not "be committed to the full truth of theories. . . she is committed to belief in some of their modal implications" (851). In order to demarcate the observable from the merely observed, she must believe "in more than just what theories say about what is observable and actual" (ibid.). The notion of empirical adequacy implies modal commitment, in that it applies to the observable, and not merely the observed (Berenstain and Ladyman 2012: 152). Either CE or modal anti-realism must be given up.[120]

Rosen (1994) has given prolonged and thoughtful criticism. He argues convincingly that if CEists must suspend judgement about modality, but 'observability' entails modality, then CEists must suspend judgement about what is observable (171). Also, when believing that a theory is empirically adequate, unless CEists have beliefs about the unobservable objects involved in the theory, then the belief involved in acceptance of the theory is non-specific in content. The CEist cannot use her theories to form inductions about the future, i.e. make predictions. Even worse - unless she forms clear, fixed opinions about observables - how can she ever have grounds for believing that one theory, rather than another, is:

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[120] van Fraassen has replied to Ladyman's criticisms by presenting a *possible* combination of CE and objective modality (see Monton and van Fraassen 2003). Ladyman (2004), though, responds that this hybrid is tantamount to a form of structural realism.

> empirically adequate in the first place? The whole idea of a phenomenon - an observable part or aspect of the world - becomes entirely idle if the empiricist is never in a position to identify the phenomena as such. (Rosen 1994: 173).

Fine (1986) agrees that van Fraassen's shift from 'belief' to 'acceptance' doesn't escape modality. The truth-as-acceptance formula has a 'would accept' clause built into it. In order to accept *X* we must get a sense of what it would mean to not accept *X* in altered circumstances. In other words, how things *would* "have been where they different in certain respects from what they are now" (141).

2.2.5. Further problems with CE

Empiricism is a position that ostensibly respects scientists[121] and also the scientific method: our most powerful and noble epistemic tool. CE says 'here is the limit to our understanding' as biological organisms, and doesn't allow grandiose armchair speculations beyond experimentation and novel predictive success. We have seen, however, that formulating a coherent theory of empiricism can be surprisingly difficult.

I have covered the major problems with CE in some detail. To my judgement, these are not just puzzles that can possibly be solved - of the sort Ladyman faces - but are, in fact, claims that make CE appear irredeemably beyond the pale. As such, I do not have much to add to the extended criticism that has preceded this conclusion. I will, however, expand upon one issue in van Fraassen's philosophy that stand out as particularly problematic to me. It relates to van Fraassen's rejection of 'strong' inference, but tolerance of the 'weak' form. A debilitating dualism seems to lurk behind this distinction.

2.2.5.a. Strong inference versus weak inference

van Fraassen rejects the strong form of inference to some ultimate reality, of the sort utilized by Ladyman and co. However, he does allow everyday inference about observables and a weak pragmatic inference about unobservables (as inference to empirical adequacy). He waxes, in a much quoted phrase, that "it is not an epistemological principle that one might as well hang for a sheep as for a lamb" (1980: 72). In other words, both inferring to the best explanation and

[121] I agree with van Fraassen that most professional physicists are generally anti-realists. In my experience, they are mostly concerned with getting hard results, rather than with metaphysical musings (when they are at work, anyway). At most, physicists are fallibilistic epistemologists: they will claim that science gives us the best knowledge there is at any given time. They will, however, generally smirk when asked about certainty or about primitive ontology. Niiniluoto (2017) understands the biggest "challenge to scientific realism [to be the combination of] fallibilism with the view that truth is defined by a relation of correspondence with reality" (189).

inferring to empirical adequacy go beyond the evidence, but this doesn't mean that we have to go the whole hog. We should, modestly, only slip in a small dose of metaphysics, if we want to be good van Fraassians. This move introduces yet another distinction into CE needing articulation. Also, van Fraassen doesn't - yet ought to - explain when exactly we are to use modest metaphysics versus full-blown metaphysics.

In his (2007), van Fraassen writes: "I do not think that there is such a thing as Induction, in any form" (343). Note the capital letter 'I'. So, he allows *i*nduction to observable phenomena, but disallows *I*nduction beyond.[122] He (n.d.) explains the acceptable kind of *i*nduction as follows:

> Certainly there is ampliative 'reasoning', in the sense of *moving from evidence to opinions that go beyond that evidence*. We do that all the time. We may call this practice 'induction' with a small letter 'i' (n.p.).

*I*nduction , though, is the pretentious forbidden kind, and involves:

> the idea that there is a logic to such 'reasoning', a recipe, a set of rules of inference, which is not only humanly or even mechanically followable, but also objectively reliable, even compelled by standards of rationality. (Whatever that is, let us call it 'Induction' with a capital 'I'. . .) (ibid.)

Presumably, it follows that *a*bduction (to the best observable explanation), *m*etaphysics (as inference to the phenomena) and *t*ruth (as empirical adequacy) plus *b*elief (as pragmatic acceptance) are allowed; while *A*bduction, *M*etaphysics, *T*ruth and *B*elief are disallowed. The associated adjectives, verbs and so on, forming the linguistic structure around these core concepts, would also need dual meanings, since they are semantically intertwined with the concepts.[123] It seems we now need two entire vocabularies: *v*ocabulary and *V*ocabulary. One semantic structure is for the permitted weak *v*ocabulary; the other for the illicit strong *V*ocabulary (only used by CEists when talking theory or when attacking realist excesses).

This sounds just like the logical positivists' impossible observation language/theoretical language dichotomy. But wait, van Fraassen does not have a semantic gripe with realists. He allows that talk of unobservables can be literally construed: there aren't two languages that we speak. His

[122] van Fraassen uses the same capitalization when referring to the limitations of '*I*nsight' and '*R*eason':

> The philosopher's only equipment is Insight and Reason. . . [But] Insight and Reason deliver nothing except the forms of our thinking [about science, mathematics etc.] (van Fraassen in Chakravartty and van Fraassen 2018: 20).

[123] As per de Saussure's (1916/1977) semantic structuralism, or Quine's semantic holism (1951; 1953).

claim is epistemic: we cannot abduce to, know about, or believe in the existence of unobservables. The dichotomy must, therefore, be in the head not in the language. [124] This may be even worse than the logical positivist's dilemma. We all speak the same vocabulary now, but have two cognitive frameworks between which we must alternate; let's call them *t*hinking and *T*hinking. However, we *t*hink/*T*hink in concepts and relations among concepts. We will, therefore, need two epistemic frameworks: a *s*emiotics of *t*hinking and a *S*emiotics of *T*hinking.

This scenario presents a number of problems for van Fraassen:

(1) He faces the overwhelmingly difficult challenge of demarcating the *s*emiotics of *t*hinking from the *S*emiotics of *T*hinking. This would be an even more baroque task than the unsuccessful reduction project attempted by the logical positivists.[125] It would involve articulating two consistent ways of thinking about every concept that could possibly be used in scientific discourse, plus the structural relations between these concepts.

(2) Applicatively, this weak/strong dualism won't work either. van Fraassians somehow need to be able to fluidly shift from the *s*emiotics of *t*hinking to the *S*emiotics of *T*hinking. The pragmatic challenge is how to know when to adopt the two different epistemic frameworks. When exactly should one jump from a weak framework to a strong framework? And how exactly does this framework jumping work? How does one cognitively adjust from framework-to-framework? Is there a meta-viewpoint or meta-principle that determines when and how one positions oneself within different frameworks, and then discards one for adoption of the next?

(3) Also, how would two CEists - engaged in discourse about science - coordinate their epistemic states? How would the one know which of the two frameworks the other is in?

[124] van Fraassen may protest that these conceptual differences exist on a continuum, not a dichotomy. I cannot make sense of such an idea. Properly articulating a sliding scale of epistemic states about a concept would, I assume, be an infinite project. The same would be the case for a continuum of meanings about a concept. Although our cognition may actually function on a continuum, there is no way to articulate it as such without either (1) positing a contentious dualism or (2) embracing either reductionism or idealism. I also take it that one cannot speak of two kinds of things, and then say that there is no demarcation between them (see Bueno 2017 for a convincing articulation of this sentiment). Nevertheless, since the CEist is positing that weak concepts (e.g. observables) are okay, while strong concepts (e.g. unobservables) are not, I take the burden to be on her to say when the former is the case, and when the latter.

[125] Alternatively, the project is akin to an attempt to demarcate science from non-science. All past attempts to do so are generally considered to have failed, so prospects are bleak. Furthermore, each concept (in particular 'existence', 'truth', 'knowledge' and 'belief') would need two articulated ways to think about it.

> If the spoken language is indistinguishable - as per van Fraassen's literal construal criteria - there seems to be no way to communicate what framework one is in, or that one has shifted frameworks and desires the other to do so too. The two dualists would have no way to know whether they agree, or whether the discussion may have crossed over from the permitted into the taboo.

Reichenbach (1951) presented 'the dilemma of the empiricist': either be an honest empiricist, accepting only what can be derived from experience, or admit the inductive inference. On the first horn of the dilemma, empiricists "cannot make inductions and must renounce any statement about the future" (89); on the second horn they have "admitted a nonanalytic principle not derivable from experience and [have] abandoned empiricism" (ibid.). In terms of our current discussion, this means that if van Fraassen accepts inference - as abduction or induction - *tout court*, then his view is substantially indistinguishable from realism (as we have seen a number of critics suggest). If, on the other hand, he rejects inference, then his position slides into a debilitating scepticism. As solution, he slowly introduces - over the course of his career - various tolerable weak versions of inference. I have suggested, however, the he thereby introduces an unsustainable epistemic dualism. This is because the conceptual and structural details of van Fraassian dualism are unspecified, and probably unspecifi*able*. Furthermore, this dual schema appears to have no applicative potential.[126] Russell (1927b) foreshadowed my sentiments in this regard, when he asserted that:

> the whole structure of science, as well as the world of common sense, demands the use of induction and analogy if it is to be believed. These forms of inference, therefore, rather than deduction, are those that must be examined if we are to accept the world of science or any world outside our own dreams (278).[127]

We now move on to our next topic. This is the vast, densely populated, arena situated between the bulwarks of the debate: the arena of the 'middle-wayers'. There is a rumour in the academy that an irresolvable stalemate has been reached between the two sides. Many philosophers don't pick a side in this realism versus anti-realism debate. They argue that the disagreement is either

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[126] Any dualistic epistemology - even if only implicitly so - will have this problem, I suppose.

[127] Russell (1927b) goes on to add that scientific knowledge is always a matter of degree:

> It is important to realise the fundamental position of probability in science. At the very best, induction and analogy only give probability. Every inference worthy of the name is inductive, therefore all inferred knowledge is at best probable (285).

pointless or irresolvable. Often they emphasise the pedagogy of cognitive, social and historical aspects of science, rather than ontology, epistemology and semantics. Forbes (2017) opines that:

> the generally accepted analysis of the situation suggests that no *non-circular* argument will ever be able to demonstrate the unqualified superiority or rational preferability of either scientific realism or constructive empiricism as a philosophy of science (3328).

2.3. The middle-way

This chapter of my thesis is significantly shorter than the sections on realism and anti-realism. Although the middle-way is regularly represented in the literature, my overarching concern here is with the debate between realists as represented by Ladyman and anti-realists as represented by van Fraassen. As mentioned in the introduction, my general interest is in positions that aim to give positive accounts of truth, knowledge, belief and reality, rather than in sceptical, quietist or conciliatory views.

Generally - as we will see, and as one would expect - the middle-wayers attack realism when promoting their views, and tend to have a soft spot for anti-realism. Pragmatists, relativists, deflationists, sceptics, agnostics, social-constructivists, post-modernists and certain feminist philosophers of science[128] are clumped together in this section of my thesis. I will, though, let Fine, and his influential natural ontological attitude (NOA), carry the flag for the middle-way 'pacifists'. NOA appears to be the most thoroughly worked out non-committal position, formulated by someone who has been actively commenting on the dispute for decades. As such, I take Fine to be representative of the middle-way.

I will, however, also briefly discuss a few other subsidiary views that can be grouped into this mixed-bag category. These positions generally either promote agnosticism or pluralism given the apparent undecidability of the debate at hand, or they consider the debate to be irrelevant because some other ethico-political motivation is what really matters for science. For the sake of parsimony, I will divide this cluttered group into three: the deflationists, the relativists and the feminists. Regarding the deflationists I will - as just mentioned - look at primarily Fine, but also Maddy and Ruttkamp-Bloem. Representing the relativists are Chakravartty and Forbes. The feminists, in this section of my thesis, will be Longino and Sveinsdóttir. I choose these individuals because, as with the previous selections, they offer thoughtful and elegant views.

[128] I am referring here specifically to a strand of philosophers of science who actively attempt to inject feminist norms into the epistemology and ontology of science, and not - of course - to philosophers of science who also happen to be feminists.

Their writings are well represented in the literature and their arguments embody the relevant positions nicely.

2.3.1. Fine and the deflationists

Fine articulates his view most thoroughly in his *The Shaky Game: Einstein, Realism, and the Quantum Theory* (1986). He is sceptical of the global approach taken by both realists and anti-realists. They try to impose a limit on science that goes beyond practice, and they treat science as if it is a single enterprise with universal goals. Both realism and anti-realism, in their generic forms, approach science from the top down.[129] Both share:

> the legacy of logical positivism, which is to set for philosophy an agenda of topics to be treated in a perfectly general way: theories, laws, explanation, probability, confirmation, and so on (Fine 2001: 119).

It is time to "acknowledge that the idea of a general, explanatory theory of confirmation has turned out to be a philosophical dead end" (Fine 1996: 235). There is no global algorithm or single theoretical schema that can account for science. Fine opts instead for his neutral particularist position - NOA. There is no empirical support for either realism or anti-realism, and the epistemic value of scientific theories is to be judged on a case-by-case basis. Universal 'meta attitudes' - like those of Ladyman, Hacking and van Fraassen - unreasonably increase 'epistemic risk'. Claims, such as 'electrons exist' or 'the unit of heredity is the gene' are only meaningful within the discourse of the relevant Carnapian framework.

2.3.1.a. The core position

Fine's quietism strips away all superfluous epistemology and metaphysics. He encourages doxastic commitment to only the *core position.* The core position, recall, involves commitment

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[129] See also Crisp (2016), who suggests that even deciding between the realist's and the anti-realist's explanation for theoretical success is an exercise in highly abstract metaphysics. He concludes that the mental, the moral *and* the physical are, therefore, all beyond the scope of the assumption of naturalism that anchors the debate. Rouse (2018) argues that both realists and anti-realists assume two suspect anti-naturalist dualisms: (1) a separation between mind and world that needs to be made sense of, and (2) the independence of meaning and truth. Both sides are committed to a distinction between claims about the world and our causal entanglement with the world. Rouse concludes that there is "no gap between how the world appears to us and how it 'really' is for realists to overcome, or for anti-realists to remain safely on the side of those appearances" (49). Therefore the questions that realists and anti-realists grapple with are outside the purview of naturalism. They should either stop asking these questions, or give up naturalism.

only to the evidence of one's senses, and acceptance of the confirmed results of science.[130] Both realists and anti-realists view science as needing philosophical interpretation, while NOA recognises that science operates on its own terms and has its own goals. Interpretation brings with it the unwanted "correlative idea of invariance (or essence)" (Fine 1986: 149). Fine (1998a) states matter-of-factly that NOA is "simply an attitude that one can take to science" (583). It is "minimal, deflationary [and] rejects any general theories or interpretations of scientific truth" (ibid.). In other words, Fine offers no overarching theory of truth; he does not seek the 'essence' of truth. For him, "NOA recognizes in 'truth' a concept already in use and agrees to abide by the standard rules of usage" (1986: 133). NOAers refuse to amplify their referential semantic concept of truth (Musgrave 1989: 386).

The realist searches for truth, while the anti-realist searches for reliability (or empirical adequacy). However, says Fine, there is "no practice motivated by a search for truth that could not be motivated just as strongly in a quest for reliability" (2018: 45). Regarding motives, research, aims, progress etc. in science, there is simply no difference. Science is not in need of interpretation. Its "history and current practice constitute a rich and meaningful setting. In that setting, questions of goals or aims or purposes occur spontaneously and *locally*" (Fine 1986: 148).

The 'humanness' of science, its social nature, makes it open. This openness "blocks any 'deep' characterizations of the constitution of scientific concepts, activities, and products" (Fine 1996: 247). No metaphysical distortions of science and no ideological constructivism please. Fine's, now highly fashionable, position consists in five premises:

(1) Bracket truth as an explanatory concept.
(2) Recognize the openness of science at every level, especially the pervasive activities of choice and judgement.
(3) Concentrate on local practices without any presupposition as to how they fit together globally, or even as to whether they do fit together.
(4) Remember that science is a human activity, so that its understanding involves frameworks and modalities for social action.
(5) Finally, on the basis of all of the above, try to understand the phenomena of opinion formation and dissolution in science in all its particularity (Fine 1996: 249).

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[130] Musgrave (1989) criticises that there is no 'core position'. Realists and anti-realists have different conceptions of what 'truth' is, while Fine dispenses with truth altogether. Therefore, with no "sense yet attached to the term 'true', neither the realist nor the antirealist knows what it is to assent to the so-called 'core position'" (385).

Fine wants to stay as close to the details of science as possible, and he trusts scientists' claims about the ontology of the world. If working scientists say that quarks exists, then we "must accept that there really are such things with their attendant properties and relations" (1986: 127).[131] As per van Fraassen, few physicists, however, give credence to the realist's *existence* claims. Most take a deflationary attitude; they consider theories to be organizing tools for attending to certain problems, claims Fine. It is precisely this non-realism that has brought about the amazing predictive success through the history of science.[132] Progress is stalled by fussing over the nature or essence of things; "the road to scientific progress [is often] blocked by realism" (ibid: 125).

Fine is, however, not committed to the idea of progress in science. NOA is consistent with the Kuhnian alternative, which counts revolutionary changes in science as wholesale changes of reference. There need not be any aspects invariant throughout history: "no necessary uniformity in the overall development of science (including projections for the future)" (ibid: 149). The particulars of each case will decide the issue. Furthermore, Bell's theorem (explicated in appendix B) as well as the renowned incompatibility between QM and general relativity are "living refutation of the realist's claim that only his view of science explains its progress" (123).

2.3.1.b. QM and probability

In classical physics the state of a system is an assignment of exact values of position and linear momentum. In QM the state of a system is represented by the nebulous wave function. Many realists argue that contemporary physics lends support to their preferred interpretation of the unobservable nature of reality, while anti-realists argue that contemporary physics indicates there can be no unitary account of the unobservable. By a case study of QM, Fine sets out to demonstrate that "realism is a metaphysical doctrine that finds neither support nor refutation in scientific theories or investigations" (1986: 151). He explains that in QM there is a complicated balance between experiment (skilful generation of laboratory effects and intricate data analysis) and theory (complex and varied considerations of probability calculations). Every stage in the process, from experimental design to "theoretical reconciliation, involves significant matters of judgement" (ibid.). These judgements do not follow realism's rigid dictates of unification, modality or correspondence. Instead, they express mostly transient norms for pursuing science.

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[131] Schlagel (1991) can't understand how, after all Fine's remonstrations against the realist's identification of a reality underlying theory, he avows to take scientist's word that there is such a reality. As such, "these conflicting assertions are evidence that Fine has not resolved the issues in his own thinking" (316).

[132] Fine speculates that, as per Bohr and Born, it was Einstein's realism that led him into a dead end. Einstein infamously 'wasted' the latter half of his career chasing an impossible dream of a unified field, while other instrumentalist physicists made progress (Fine 1986: 124-25).

To "accept as true a particular existence claim", insists Fine, is to "accept the complex network of judgements that ground it" (ibid.). Tentative truth claims emerge locally, and are scrutinized by locally constrained reasons and judgements. The physics community accepts a particular claim if it 'sits the right way' within an array of overlapping theories and norms generally subscribed to by the community.[133] This method sounds more like NOA than like realism or anti-realism, thinks Fine. According to QM, a variety of objects seem to *exist*: various particles and fields, with their associated properties and relations. The realist tries to sort these into a coherent, observer-independent 'world picture' using her metaphysical toolkit. However the probability formulas of QM all seem to make reference to an *observer*. These are probabilities for measurement outcomes, and so presuppose a *measurer* of some sort.[134]

Fine explains the task involved in making QM realist as involving, at least, the following minimal structuralist method. The realist must somehow correspond quantum systems to real objects, whether entities or structures. The observables in the theory - such as spin, position and momentum - must correspond to some generic feature of the real objects. This generic feature can take various particular forms, which are definite properties of the objects. On a realist interpretation, the wave function is the way particular forms of generic features are attributed to a real object. A system S is in a state Ψ "just in case the real object corresponding to S has the array of particular forms of the generic features that Ψ attributes to that object" (ibid.). Suppose A is an observable of S, the possible values of which are associated with clustered probability measures. Then, if system S is in state Ψ, "Ψ attributes to the real object corresponding to S a particular form of the generic feature that corresponds to A" (ibid.). If that form is associated with the probability distribution QM assigns for finding values of A, then these probabilistic assertions are true for system S in state Ψ. In other words, for the realist, a probabilistic assertion holds "just in case the object has the right forms of the features; that is, the right properties" (161). A theory corresponds to the world structure if the probabilities in the theory match the probabilities associated with the properties of the relevant real object.

[133] Fine is impressed that Einstein appeared to endorse a non-correspondence conception of truth. Einstein writes that truth is a quality we assign to propositions. Deduction and reasoning are our tools "to bring cohesion to a world of perceptions. The label 'true' is used in such a way that this purpose is served best" (Letter from Einstein to Candido, November 4, 1951. In Fine 1986: 90). For Fine, these remarks "call our attention to structural features in the use of the concept 'true', primarily the role of truth in logical inference" (91). A structural notion of truth *prima facie* supports Fine's deflationism.

[134] The so-called 'collapse of the wave function', central to the Copenhagen interpretation of QM, also appears to involve an observer/measurer driven notion of empirical inquiry. Furthermore, the theory doesn't work when observers themselves are treated quantum mechanically.

Fine hereby presents a possible structural realism: a minimalist framework into which details could be added in various ways. The problem, though, is that this framework for making QM realist requires treating probability in a uniform way. It requires treating "the probabilities as mere averages over a single well-determined ensemble" (ibid: 163),[135] which then provides the definite world structure 'underlying' the mechanics. This deterministic approach, though, conflicts with Bell's theorem. In Bell-type experiments the values of observables (e.g. polarization angles) associated with a single system (e.g. a photon), entangled with a spatially separated second system (a second photon), are measured and analysed. The realist requires that the observables in a single system must have values signifying properties of the underlying real objects: these "properties ground the probabilities for the various measurement outcomes" (162). There must also be a well-defined joint probability distribution for their values, relative to the observables of the other system. Realism requires that the joint probability for each system pair arises by averaging over the product of the probability for the outcomes on each system separately. However, this is not what we see in the empirical data from Bell-type experiments.

Moreover, QM determines "a single distribution for the outcomes of each measurement separately, and a joint distribution for the simultaneous outcomes of each [system] pair" (ibid.). The formalism of QM assigns no joint probability to any pair of observables in a single system.[136] The joint outcomes of measurements of observables in a single system are generally held not to be well defined. These observables are *incompatible*, according to QM; they are not "assigned values in *any* state, nor do they have joint distribution" (164).

2.3.1.c. Against realism

Fine is convinced that "no epistemic argument supports a robust form of realism in general as against an equally robust form of antirealism" (2018: 42). Bell's theorem shows that the realist cannot ground the truth of all the probabilistic assertions of QM. There is a mismatch, a numerical inconsistency, between realism and QM; the realist's interpretation is incomplete.[137] It fails on two accounts, according to Fine:

(1) it fails to describe the values of incompatible observables.

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[135] The 'ensemble' in this case would be hidden variables (Fine 1986: 165).

[136] To use van Fraassen's picture language, we can analogise that, according to QM, one cannot combine two photographs, taken from different angles of the same object, to reconstruct that object.

[137] Alternatively, QM is incomplete, but the extraordinary predictive success of the theory indicates - to Fine anyway - that realism should rather go (1986: 165-66).

> (2) it fails to specify the well-defined joint distributions for incompatible observables (1986: 164).

Nevertheless, ultimately there is no way to refute realism using QM, or any scientific practice for that matter, shrugs Fine. Realism is a metaphysical thesis that "transcends human experience and rational support, in a manner similar to that, say, of a religious doctrine" (1986: 156). When the realist interprets QM, she constructs her own "'real' world according to personal (or social) constraints" (170-71). The realist tries (but fails) to stand outside the *game* of science. She also wants to know what is *really* 'out there' in the external world, and demands justification of science's ontological claims. She thinks that scientific models or theories can 'reach out' somehow to the very stuff of the world. Sadly, even though we yearn "for just such a comforting grip on reality", there is "no possibility of justifying the kind of externalism that realism requires" (132). Objectivity is - as per van Fraassen - a function of our collaborative agency: our intersubjectivity. The objectivity of the process of inquiry does not 'attach' (is not logically connected) to the products of that inquiry (Fine 1998b: 18). Fine suggests that instead, with regards to existence claims, we should follow scientific practice. "[L]et the ontological chips fall where they may" (1996: 250) is his often cited slogan. A better conception of objectivity is that which in the process of inquiry "makes for trust in the outcome of inquiry. . . objectivity is fundamentally trust-making not real-making" (1998b: 18).

Fine also sees realism as entertaining a debilitating circularity. The realist says that a good explanation is, at least, approximately true. The realist claims to have good explanation for the success of science. Therefore scientific realism is approximately true. However:

> no support accrues to realism by showing that realism is a good hypothesis for explaining scientific practice [since] such a demonstration (even if successful) merely begs the question that we have left open ('need we take good explanatory hypotheses as true?') (Fine 1986: 129).

Such a question-begging methodology would never be allowed in science itself, says Fine. A scientist cannot use data explained by a theory in the derivation of that same theory. Also, we cannot possibly know if any given theory is approximately true, since this would require a comparison with the complete 'blueprint' of the world - something we can never know we have actually discovered.

'Reference', 'explanation', 'belief' and 'existence' are relegated to pragmatics in ultra-minimalist NOA. When it comes to the 'big' questions, the position has only this to say: if you believe that "guessing based on some truths is more likely to succeed than guessing pure and simple. . . then guessing on this basis has some relative likelihood of success" (ibid: 132). NOA refuses to "amplify the concept of truth by providing a theory or analysis (or even a metaphorical picture)" (133). The death of realism is without question, and NOA is the "suitable successor for these postrealist times" (136).

2.3.1.d. Against anti-realism

At any rate, anti-realism doesn't do much better, asserts Fine. CE's notion of truth-as-acceptance - truth as an interpersonal construction - "is just as metaphysical and idle as the realism expressed by a correspondence theory" (1986: 140). If judgements of truth are about what people will *accept*, asks Fine, then what rules are being used to arrive at those judgements? What are the rules for applying, and working with, these judgements? It seems that the anti-realist faces an infinite regress. The 'acceptance' project never gets off the ground; just like the realist's 'correspondence' idea, it is incoherent. With "respect to warrant and intelligibility, the acceptance picture emerges as quite on par with the correspondence picture", continues Fine (141). Both views try to offer a completed concept of truth, which acts as a limit for human aspirations. Both '-isms' "derive from a philosophical program in the context of which they seek to place science" (147). When science is put "in that context its significance, rationality, and purpose. . . just click into place" apparently (ibid.).

2.3.1.e. Conclusion - NOA

Fine's modest NOA (or core position) mediates between realism and anti-realism. It "sanctions reference and existence claims, but does not force the history of science into prefit moulds" (1986: 131).[138] We can be committed to some sort of 'truth talk', but truth is not a substantial essence or a natural kind. Neither is truth a relative, indexical or contextual concept (1988: 8). Truth is open-ended, growing with the growth of science, concludes Fine, and perhaps some truths will never be known. To:

> pursue NOA's third way means to situate humanistic concerns about the sciences within the context of ongoing scientific concerns, to reach out with our questions and interests to scientists' questions and interests (1986: 174).

Interpretation, intelligibility, goals, aims and purposes emerge locally and spontaneously from within science. These norms "call for an empathetic analysis to get at the cognitive (and temperamental) sources of the question, and then a program of therapy to help change all that"

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[138] By "placing science in a context, supplying it with an aim, attempting to make better sense of it, and so forth" (Fine 1986: 147).

(ibid: 148). Science is a social enterprise; concepts like 'method', 'objectivity' and 'truth' are institutional, and have shifted throughout history (1998b: 12). Science cannot be given a universal account without the regress or circularity problems discussed above. Simply put: "we build models and theories. . . and act on them without necessarily settling or even addressing the interpretive questions that realism or instrumentalism raises" (1991: 11).[139] Fine thinks of NOA as fundamentally a heuristic attitude: our ontological views should be "governed by the very same standards of evidence and inference that are employed by science itself" (1986: 150). This attitude tolerates varieties of opinion, but - *pace* Ladyman and van Fraassen - not overarching doctrines that try to force restrictions on science. When all is said and done, the consequence of Fine's view is that "NOA is the only defensible position in *philosophy* of science - the rest is *up to science*" (Musgrave 1989: 397).

2.3.1.f. Other deflationists

Another prominent deflationist is Maddy, who promotes a kind of sceptical contextualism she calls *second philosophy*. This involves ignoring big questions, universal philosophical arguments and system building in favour of the use, and reference to, the results of "what we typically describe with our rough and ready term 'scientific method'" (2007: 2). She specializes in the philosophy of mathematics, and is the most well-known thinker involved in the effort to naturalize mathematics. Her approach is broadly Quinean, however she understands mathematical concepts to be useful heuristic devices internal to mathematics, rather than an indispensible part of science (see Quine 1960). Mathematics is itself a science and also deserves the respect naturalism affords to the sciences, she claims. Also, we can take mathematical theories to be true, since mathematics - like a science - has its own internal standards of confirmation.

Maddy sympathizes with the later Wittgenstein's (1953/1997; 1956/1978) claim that pure mathematics is senseless: only applied mathematics says anything.[140] Mathematical problems cannot be solved by the philosophy of mathematics; instead we should look towards "the needs

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[139] Horwich (2006), arguably the most prominent contemporary deflationist about truth, offers that "we can devise a coherent and attractive perspective combining the most plausible contentions of the self-styled 'realists' with the most plausible contentions of the self-styled 'anti-realists'" (195); thereby bypassing all unnecessary concerns about metaphysics.

[140] In his later writings, Wittgenstein held that "the only meaningful mathematics is applied mathematics; unapplied mathematics is just a meaningless sign-game" explains Maddy (1998:168). Pure mathematics is a piece of "architecture which hangs in the air, and looks as if it were, let us say, an architrave, but not supported by anything and supporting nothing" (Wittgenstein 1956/1978, II, §35). Furthermore, "[c]oncepts which occur in 'necessary' propositions must also occur and have a meaning in non-necessary ones" (ibid, V, §41).

and goals of mathematics itself" (Maddy 1998: 191). Maddy notices that scientists use whatever mathematics is available and convenient, without concern for the philosophy of reference or the ontology of numbers. The notion of 'exists' comes out of science, and may not be relevant to mathematics. As Leng (2017) puts it:

> if Maddy is right that this notion [of existence] is not sufficiently specified to determine its own application in the case of mathematics, then the right naturalist conclusion might be that fictionalism, modal structuralism, and 'thin' realism are not in the end in competition (417).

Nothing in the world makes mathematical realism or anti-realism right or wrong, they are "alternative ways of expressing the very same account of the objective facts that underlie mathematical practice" (Maddy 2011: 112). As per Carnap (1950), the choice between the two positions rest ultimately on "a matter of decision, rather than assertion" (26).

For the purposes of this thesis, though, Maddy's contextualist views on science are more relevant. In Finean spirit, she holds to the pre-eminence of science (and mathematics) over philosophy. When science and philosophy clash, philosophy must give way. Philosophy can neither prescribe nor restrict scientific (or mathematical) practice. She dubs her own view as 'anti-philosophical', and recommends we ditch sterile conceptual analysis (ibid.). Instead epistemology should become a thoroughly empirical investigation into "how we manage to acquire reliable information about the world" (2017: 214). For Maddy, 'truth' is only a useful expressive device. Liston (2007) explains that for Maddy the definition of "the reference clauses needed for truth amounts to a trivial list that only philosophical prejudices about physicalism could persuade us to see as a causal relation" (n.p.). Successful explanations can be handled without truth or reference.[141] Theories of science or philosophy cannot be founded on shaky intuitions, we should begin with individual case studies, then work from there to possible general principles (Maddy 2007: 403).

Similarly, Ruttkamp-Bloem, in her (2015), aims to 're-position realism'. She suggests that the realism versus anti-realism dichotomy is a false one. Much like van Fraassen (2002) and Chakravartty (2017a), she sees the two positions as being at the end points on a continuum of epistemic stances. Either realism or anti-realism can be adopted, depending on the degree of evidence for an individual theory. In pragmatist/deflationist spirit, she holds that "both of these attitudes are reasonable towards different parts of science at different times in the history of

[141] Laudan (1981) adds that realists have "no explanation whatever for the fact that many theories which are not approximately true and whose 'theoretical' terms seemingly do not refer are, nonetheless, often successful" (45).

science" (Ruttkamp-Bloem 2015: 86). Our position on the continuum, along the spectrum of stances, shifts as scientific knowledge unfolds in a self-correcting manner. Like CE's co-evolution of theory and experiment, Ruttkamp-Bloem has science and reality in constant causal contact. This ongoing 'evidence gathering' activity propels our fallibilist stance-choices forward in time, as "truth is assembled and re-assembled" (90). Truth is equated with method, rather than correspondence or representation. The epistemic status of scientific theories is determined, and constantly adjusted, on a progressive "continuum between traditional instrumentalism and full blown realism" (Ruttkamp-Bloem 2013: 202). This offers an account of scientific knowledge that is intended to mimic the evolving, dynamic, self-correcting nature of science itself.

2.3.2. Chakravartty and the relativists

There are a number of commentators in and around the debate at hand who are not ostensibly concerned with the fine-grained details of the epistemology entailed in scientific theorizing. These are scholars whose primary intent is to advise normative sentiments - such as humanism, multiculturalism, tolerance and cooperation - in the sciences and in the philosophy of science.[142]

Neurath (1983) expressed early relative social-constructivist ideas.[143] Howard (2006) interprets Neurath as arguing that logic and experience must first be used to evaluate scientific theories; after that, if we are stuck with empirically equivalent theories, we ask which are:

> most conducive to the achievement of our social and political ends. . . Value considerations are not intended to trump considerations of logic and experience; they are intended to respect them (14).

Nuerath urged that we should be honest about how these non-epistemic values play a role in theory choice; we should discuss and even use them as we go about doing science. Norms can play a role in the 'space' allowed by the underdetermination of theory by data. Such a 'politically

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[142] See, for example, Latour and Woolgar (1979), who use post-modernist rhetorical devices to urge that it is impossible to differentiate the theoretical and empirical practices of science from the social and cultural influences that shape them. Science is about human activity and not about nature *per se*. Scientists *construct*, rather than discover - not just theories - but reality itself.

[143] Even Quine could be interpreted as flirting with relativism at times. He spoke of, for example, the "myth of physical objects" (1951: 36) and encouraged tolerance towards alternative epistemologies. van Fraassen, too, has expressed relativistic sentiments. In his (1994) he takes inspiration from Nietzscheanism by likening science to myth. There are many myths: Christian myths, Jewish myths, Pagan myths and - of course - scienctific myths. Both traditional myths and scientific myths are apparently 'cosmological', 'narrative' and 'explanatory'. Myths are not "interpretable into non-myths; 'myths interpret each other [and] demythologizing the language of science is impossible'" (van Fraassen 1994: 129-30).

engaged' pursuit will best serve the *progress* of science and philosophy. Howard (ibid.) concludes, therefore, that we should collapse the distinction between epistemic and non-epistemic values: its "values all the way down" (20)!

Some contemporary thinkers pushing this agenda see van Fraassen's stance empiricism as an entry point for their relativistic norms.[144] This is because the perspectivist/pluralist motifs in stance empiricism mirror a relativistic approach in crucial ways. Forbes (2017), for example, while analysing stances, agrees with van Fraassen that it is rational to choose a stance that best serves one's epistemic values. However, it is not always obvious which stance best does so. Therefore, in order to additionally ground stance choice, it is legitimate "to admit. . . the influence of non-epistemic values on one's choice of epistemic stance" (3334). For Forbes, these are not universal human norms, but rather the individual considerations of, for example: wealth, liberty and happiness. That scientists and philosophers introduce some degree of ethical, social and thereby political norms into their theorizing is probably uncontroversial. The relativist - here tokened by Forbes - however, takes things to whole new level by promoting 'normative norms' in science: we *should* inject ethics into our science.

Chakravartty, our flagship relativist, embraces both epistemic and ontological pluralism. He holds that a sceptical analysis of the debate at hand leads to "an inescapable relativity" (2011: 158). He concludes with a counter-intuitive ontology, which he calls *taxonomic pluralism*: "[t]here is more than one structure of mind-independent entities and processes" (163). He posits dispositional, or 'oomphy', properties as basic, and it is these properties that manifest in different ways, depending on which stance we approach them from. Chakravartty thereby claims to fuse entity realism and structural realism through dispositional realism. The properties of scientific entities and their structural relations are intimately connected via dispositions. He argues that the kind of knowledge that is required in order to interact with unobservable entities is structural knowledge:

> Dispositions are dispositions *for* relations. Structures are 'encoded' in the properties of entities, because these properties confer dispositions for precisely those relations that the sciences describe in terms of structure. Dispositional realism thus facilitates a

[144] Most of the authors I will discuss in this section naturally deny being relativists. I label them as such based on the general attitude taken towards them in the literature, and on my own judgements. I take a relativist to be anyone who (1) claims to be one, or (2) who argues urgently and repeatedly against the notion that truth, reality and knowledge can be objective, or (3) promotes an 'ethics first' philosophy, in which ontology or epistemology should answer to social or political norms. (This last criterion may be contentious. Space limitations, however, don't allow for an extended argument.)

> rapprochement between the best insights of entity realism and structural realism (2017: 108).

The perspectivism, which has come up while discussing both Ladyman and van Fraassen, can apparently also be accommodated into Chakravartty's dispositionalism. He reasons that:

> putatively perspectival facts may be straightforwardly understood as non-perspectival facts regarding how behavioural dispositions are manifested under different stimulus conditions. Let us label these 'dispositional facts' (2010: 410).

Similar to Ruttkamp-Bloem, Chakravartty takes his inspiration from van Fraassen's stance empiricism, and tries to place realism and anti-realism on a continuum of epistemic commitment. He (2009) argues that we should not think of deflationary or pragmatic accounts of scientific representation (e.g. CE) as alternatives to substantive accounts (e.g. OSR), but rather as complements. He argues from within the framework of van Fraassen's stance *stance*, aiming to build a bridge of cooperation and tolerance between realists and anti-realists.

Chakravartty's pacific relativism is explicated most recently in his (2017). In the book he argues that, given underdetermination, metaphysics is inevitable when interpreting scientific output: "science itself underdetermines scientific ontology" (6). Different agents make different ontological commitments, as per their different prior epistemic commitments. These presuppositions are the Kuhnian (Kuhn 1977) attitudes and values that fine-tune one's choice of stance along a "continuum [of] magnitudes of metaphysical inference" (52). CE and OSR are stances along the continuum. Chakravartty's pluralism involves, not just a plurality of perspectives, but a plurality of ontologies. Contrasting descriptions of the world are not incompatible descriptions of the same thing, they are "*compatible* descriptions of *different* entities - compatible precisely because they describe different things" (190).

How does one choose a stance though? As we saw with stance empiricism, Chakravartty is motivated by James' (1896/1956) notion that the path one walks when making these choices is largely determined by one's temperament, *qua* values.[145] As such, *the will* permeates our metaphysical methodology in the form of van Fraassian voluntarism. The realist's metaphysics and van Fraassen's voluntarism are thereby incorporated into a single schema. The two positions are not juxtaposing bulwarks; they can be merged into one. These two major themes can be co-operative partners. They should be the two central working heuristics in a dual-core

[145] Chakravartty talks of these as epistemic values but refrains from expressly excluding other more 'humane' values. This implicitly leaves open the possibility for political or ethical influence into conclusions about scientific ontology. See his (2015: 179-80), in which he tentatively encourages this opportunity.

"collaborative" method: an instigative project - both institutionally transformative and personally inspiring (Chakravartty 2017: 251-2). If we follow Chakravartty, disputes over stance choice - when interpreting scientific ontology - should involve modest, liberal queries into the coherence and values of rival stances, and not fruitless, hard-nosed squabbles over the nature of ontology *simpliciter*. We cannot do otherwise. Our conclusion should be an attitude of Pyrrhonian "*ataraxia* — peace of mind, calmness, or freedom from worry in the face of previously pressing questions" (244-5).[146]

At the end of the book (ibid: 249-50) Chakravartty hints that his project is, in some sense, an attempt to further social and political transformation in the sciences. He nods to feminist philosophers who are working to improve the lives of those working in, and also those affected by, scientific practice. Chakravartty can be thought of as audaciously pushing transformative norms into the descriptive-explanatory philosophizing that happens around the ontology of the hard sciences.[147]

2.3.3. Longino and the feminists

Schrödinger (1954) complained that science literally construed (*à la* OSR's scientism) offers a world picture devoid of what we really care about. It is silent on emotions, aesthetics and ethics. It tells us nothing about "red and blue, bitter and sweet, physical pain and physical delight; it knows nothing of beautiful and ugly, good or bad, God and eternity" (95).[148] Since science can't regulate its own norms, some feel that science needs an ethical shake-up from philosophy. Patriarchy, power and privilege are infecting and dictating conclusions in scientific epistemology, and even scientific ontology. The perspectives of women and minorities are being systematically prejudiced against; something needs to be done.[149]

The pertinent strands of relativism and feminism are often clumped together in the literature, as I have done here. Both view science as primarily a social enterprise in need of normative instruction from outside. Both appear to compromise objectivity and truth in the name of ethico-

[146] James (1896/1956) agreed that in a world where we are so certain to incur errors "in spite of all our caution, a certain lightness of heart seems healthier than. . . excessive nervousness." (17-19).

[147] Chakravartty (2015) contrasts descriptive-explanatory philosophical projects to his preferred *transformative* philosophical projects in the sciences.

[148] van Fraassen (1994), too, has little patience for scientism. He scoffs that this dogma "is not science - it is superstition [requiring] the sacrifice of the intellect" (133).

[149] The reaction by scientists (particularly males) to this 'interference' has naturally been unwelcoming. Cf. Wilson (1975), who declares that science can offer its own ethic. The time has come, he says, "for ethics to be temporarily removed from the hands of the philosophers and biologized" (562)!

political agendas. Chakravartty (2018), for example, notices the "near universality with which feminist approaches [to science] are normative, offering corrective prescriptions for understanding concepts such as objectivity and knowledge" (608). However Longino (1990) argues that, rather than subverting objectivity, the 'socializing' of science promotes it. Public debate in science about personal values (epistemic or non-epistemic) leads to maximum objectivity, if objectivity is understood in the van Fraassian sense as intersubjectivity. Longino (2002) thereby promotes what she calls a 'modest epistemology'. Since complete knowledge is underdetermined, we must allow entry for social distinctions and normative judgements.

Sveinsdóttir (2016) argues that feminists should jettison any position that banishes normativity. Naturalists, whether realist or anti-realist, generally depict a scientific world without injustice and oppression. Causal explanations are presented to undercut normative claims. Domestic violence, for example, is often explained in terms of evolutionary genetic factors. If "the social is causally determined by the biological, there is no such thing as justifications" (55). A world without justifications is a world without norms. Since standard naturalism doesn't cater for the feminist ethic, "we have reason to reject this worldview" (ibid.). The feminist, on Sveinsdóttir's account, takes the lived experience of millions of oppressed people to be the starting point for philosophical theorizing. A feminist naturalist may pick and choose which ontological structures or entities she endorses, according to whether they are "central to her case against gender injustice" (56).

These philosophers generally reject any notion of 'scientific method' as a masculine ideal. This ideal functions to exclude feminist perspectives, and thereby degrade the openness and democracy of science (Fine 1998b: 11). Longino (2002) offers contextualism instead: justification depends on rules and procedures relative to the context of inquiry. She (1990) argues that democracy in science has epistemological weight, since evidence only emerges against a background of values, beliefs etc. If alternative views are excluded, then the range of possibly fruitful hypotheses is restricted, and science thereby impoverished. Even if this is not a relativistic case of anything-goes, the "lesson [is] that many things go" (Fine ibid.).

2.3.4. Conclusion - the middle-way

According to the middle-wayers, realists cannot show conclusively that belief in the capital 'T' truth of scientific theories is warranted, while anti-realists cannot sufficiently demonstrate that the aim of science is limited to instrumental ends. Blackburn (2002), for example, muses that on the one hand surely the objects of core scientific theory are real, but this demands that we somehow get 'behind' explanation, adopting a mythical God's eye view. On the one view "realism seems almost indisputably true" and on the other "equally obviously false or indiscussable" (112). Accordingly, a "surprising 'quietism' or pessimism about. . . metatheoretical position[s] begins to seem attractive" (111). Perhaps the debate is not entirely well-formed. The disputants are talking past one another, thumping the table due to a lack of any shared presuppositions.[150] Rosen (1994) gives a sardonic summation:

> We *sense* that there is a heady metaphysical thesis at stake in these debates over realism. . . But after a point, when every attempt to say just what the issue is has come up empty, we have no real choice but to conclude that despite all the wonderful, suggestive imagery, there is ultimately nothing in the neighbourhood to discuss (279).

We have looked at some prominent deflationists, relativists and feminists involved in the debate. On my reading, these thinkers serve as good-enough representative tokens of the varied positions within this broad middle-way stance. The majority of authors, touching in any way on the philosophy of science, fall into this mixed-bag category. What they generally have in common, though, is that they use scepticism and/or moral motives to 'deconstruct' positive, unitary positions; thereby reaching a conclusion of broad-based agnosticism and/or liberal pluralism.

2.3.5. Standard problems with the middle-way

As with the sections on OSR and CE, I will offer a brief descriptive list of the standard problems with the middle-way, then evaluate a few particularly troublesome issues thereafter. Despite its popularity, there are some (including myself) who see the middle-way approach as wrong-headed. Ladyman (2018), for example, criticises that, although there are multiple models in science, "there is often one theory choice that is right - oxygen over phlogiston being a good example" (105). Also deflationism, relativism and/or pluralism don't:

> do justice to the unity of science, nor [do they] take account of the special status of physics. The need for theories to be compatible where they overlap is a methodologically productive driver of scientific advancement suggestive of a non-pluralist metaphysics (ibid.).

As before, I will focus on Fine and his minimalist NOA in this section of my thesis. Ritchie (2008) identifies three possible problems with NOA: (1) it seems philosophically bland - we are told only about science, (2) it doesn't tell us why we should value science in the first place, and

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[150] See also Wylie (1986), who concurs that the most sophisticated positions on either side of the realism versus anti-realism debate "now incorporate self-justifying conceptions of the aim of philosophy and of the standards of adequacy appropriate for judging philosophical theories of science" (287).

(3) if there is no scientific method, how are we to demarcate science from pseudo-science? I will discuss these objections each briefly.

2.3.4.a. What about philosophy?

Ritchie is disappointed that philosophy has virtually no role to play in the NOA stance. If a philosopher finds Fine's arguments convincing, then she has to accept a deflated role for her own enterprise; *c'est la vie*. The NOAer must be happy with a job merely describing how scientists interact and conduct their research. The general strategy appears to be to only "pursue any philosophical project through a detailed investigation of the sciences" (Ritchie 2008: 197).

At face value, Fine's relegation of philosophy to a sociological role has some appeal. Mind-body problem, is-ought gap, problem of reference - not problem at all for the NOAer. If we just describe science, and take just whatever scientists say as deserving epistemic commitment, then we can sidestep a slew of perennial philosophical paradoxes and puzzles. I'm not sure what kind of philosophical *position* deliberately undermines is own status. Is Fine even doing philosophy? Musgrave (1989) agrees that science without philosophy is impotent. "Science unaided by philosophy could not overthrow common-sense realism" he suggests (397). How can the NOAer accept a claim, such as: 'electron are negatively charged', if she doesn't know what she has accepted? She doesn't know how the claim it is to be interpreted or what the ontological commitments are. The unphilosophical NOAer "does not just know nothing philosophical - [she] knows nothing at all" (398).

2.3.4.b. Why should we value science?

Why should we value science in the first place? This is a real problem for Fine, observes Ritchie, the only option seems to be a constructivist posit: *we* decide. The best answer NOAers can give is that "the aims of science overlap with basic pre-scientific aims that we all share to some extent" (Ritchie 2008: 107). This sentiment echoes van Fraassen and Chakravartty's voluntarism. Chakravartty's overarching intellectual motivation is a norm of social tolerance in science and philosophy of science. In his (2015), for example, he explicitly endorses what he calls 'transformative philosophical projects' in the sciences, "the ultimate aim [of which] is to. . . promote or enable some form of human flourishing" (169). He cultivates an argument that apparently includes claims about scientific ontology periodically answering to ethics (and thereby presumably to politics). If the answer to a question like 'do electrons exist?' *must* be subservient to our moral agendas, then surely something has gone wrong. Whatever an enterprise predicated on this norm would be it could not be called science. Furthermore, perhaps science would grind to a halt if relativistic ethical concerns dominated over an ideal of genuine correspondence between scientist and world. Schlagel (1991) agrees that:

> experimentally detectable magnitudes, the essential bridges between our theories and an independent physical world, comprise the *indispensable realist element* guiding scientific inquiry (314).

2.3.4.c. What make science different from pseudo-science?

NOA is "basically at odds with the temperament that looks for definite boundaries demarcating science from pseudoscience" (Fine 1986: 149). We cannot award the badge 'scientific' to this or that enterprise. Ritchie (2008) suggests that perhaps NOAers can - consistent with their general deflationary approach - say that science has made progress by its own self-critical standards.[151] Pseudo-scientific activities, such as astrology and creationism, either "fail by their own lights or they are undermined by the results and methods of successful sciences" (108). Stanford (2003), however, objects to minimalist, permissive theories of the sort Fine and Chakravartty promote. He demonstrates that even creationist biology or caloric thermodynamics are - at least - accurate to some degree: "none of these theories is wrong or misleading about absolutely everything" (567). [152]

2.3.6. Further problems with the middle-way

Of the three positions I have explored in this thesis the middle-way appears the least tenable to me. I am concerned that the middle-way is firstly indistinguishable from relativism and, secondly - *mutatis mutandis* - promotes a dubious norm of ethics first. I will discuss these two objections in turn.

2.3.6.a. Isn't this just relativism?

[151] Ritchie (2009), by the way, develops his own stance: *deflationary methodological naturalism*. Even more deflated than NOA, this position urges universal agnosticism - even apathy. Ritchie says we should suspend belief about any positive theory that cannot answer Descartes' problem of scepticism and Hume's problem of induction. Since no theory can do this, Ritchie's radical scepticism knocks down every argument in its way, and we are led straight to his ultra-minimalism. Philosophy is relegated to the purely descriptive role of writing down what scientists do. Ritchie, however, cannot tell us why we should care about, or believe, his theory (if it is a theory) since doing so would break his own rules.

[152] Schlagel (1991) considers the middle-way - or what he calls 'nonrealism' - to be an oxymoron. He asks how "can one be neither a realist nor an antirealist regarding the physical reality described by theories? Could *nonreal* entities play an effective role in scientific inquiry?" (316). He challenges Fine to give an account of how scientists can "*derive empirical implications and predictions essential for the testing of theories from nonactual structures and mechanism*[s]" (319).

I cannot discern how the various middle-way views don’t - when pressed - just dissipate into full-blown relativism. Both NOA and relativism conclude with a plurality of equitable indeterminacies or uncertainties. This kind of synchrony offers an apparently oxymoronic position Worrall (1989a) dubs ‘sceptical relativism’. Fine’s broad-based agnosticism (or disinterest) towards any non-particularist judgements about science surely allows aberrant views. It has no criteria by which to evaluate and discern scientific claims. He (1998b) recognizes that this is a difficulty for middle-way positions emphasising the social nature of science. Is science “constrained by general norms built into the very conception of objective inquiry”, or is it the case that “local practice (some would say, mob rule) governs” (12). The worry is “that unless there are universal principles governing the procedures that make for objectivity, then. . . relativism and irrationalism” follows (ibid.).[153] In his (2015), Chakravartty agrees that the stance voluntarism many middle-wayers endorse is a form of relativism. He is, in fact, impressed how a relativistic:

> assessment that no one belief is more compelling than its rivals *qua* truth maps nicely onto the stance voluntarist’s assessment that no one coherent epistemic stance is more compelling than its rivals *qua* rationality (190).

Moving on, are these pluralistic, deflationary positions serious methodological accounts of science or just relativism in disguise? If ethical (and political) norms can influence conclusions about scientific ontology and epistemology, then surely we have lost our grip on irrelativism. After the above inspection, pluralism and deflationism appear - for all intents and purposes - indistinguishable from anything-goes. Fine (1998b) identifies this objectivism versus relativism divide as a false dichotomy: “there is a middle ground, and lots of it” (13). His solution is that there are:

> intermediate standards of all sorts that one can explore as we examine answers. Depending on the subject and context, we can propose temporary rules for the discussion. . . we can agree to accommodate some local customs. We can set up feedback procedures. . . we can negotiate compromises. . . and then renegotiate (ibid.).

He urges that we move beyond heated debates over objectivity, truth and reality to a new idyllic collaborative science, in which “the operative attitude is that of trust” (ibid: 17). Schlagel (1991), however, is critical of this ‘trust’ motif. For him, Fine’s acceptance of science on trust is because

[153] It seems that we cannot adopt a:

> non realist attitude toward the doctrines and ontologies of novel theoretical frameworks unless we are willing to give up truth, falsity and real existence across the board (Churchland 1979: 2)

NOA precludes any justificatory arguments; "Fine has no recourse except mere 'trust' or 'acceptance' in accounting for any form of knowledge" (320). As per Laudan (1984) NOA:

> seems to go too far in the direction of 'de-epistemologizing' meta-science, because it fails to show us how to adjudicate rival claims about what there is and about how we know (66).[154]

This sums up the central problem for the middle-wayer nicely. She cannot appeal to a 'higher' principle to decide disagreements, since this would introduce a forbidden 'meta' offensive to her pluralism/deflationism.[155] She therefore has to appeal to ethics in order to avoid a slide into anything-goes.

2.3.6.b. Ethics first

If norms of social goodwill in science and philosophy are one's overarching intellectual motivation, then appeal to extra-epistemic principles seems inevitable. This liberal approach will, at times, apparently encourage 'ethics first'. The middle-wayer's 'socio-ethicism', however, seems to require an ideal Rousseauian conception of human nature. We are to think of persons (scientists, in this case) as epistemic altruists perfectly willing to give up their views in the name of tolerance and cooperation, if only the oppressive leviathan of metaphysics or patriarchy - or whatever domineering weight - were lifted.

This moral introduces an applicative, institutional obstacle, however. I don't have space in this text to get into a discussion about deep psychology, but I take it to be highly doubtful that scientists do, or will, hand over their favourite theories and associated opportunities for prestige in order to further humanistic moral ideals, such as trust. I will let Putnam (1985) have the final word to this chapter:

> if all notions of rightness, both epistemic and (metaphysically) realist are eliminated, then what are our statements but noisemaking? What are our thoughts but *mere* subvocalizations?. . . Let us recognize that one of our fundamental self-

[154] Laudan (1984) continues:

> Fine's formula, 'let science speak for itself,' ignores the fact that science often speaks in tongues or in paradoxes. What is needed, but not provided by NOA, is an account of the diverse cognitive axiology of science and an informal logic - limited to that axiology - of how best to secure the diverse ends of science (66).

[155] Fine (1986) criticises CE for being prone to a regress of meta-justifications, but NOA faces the very same slippery slope as far as I can tell.

> conceptualizations. . . is that we are *thinkers,* and that *as* thinkers we are committed to there being *some* kind of truth, some kind or correctness which is substantial (246).

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3. Concluding thoughts

We have explored in some detail the three positions that make up the title of this thesis: Ladyman's realism, van Fraassen's anti-realism and Fine's middle-way. I conclude by offering some final thoughts on these three views. I include suggestions on a possible way forward for those - like myself - who want to know what the limits of our epistemology are, and ultimately what we can say about the ontology of the world. In this last chapter I will discuss the three positions in reverse order to how they appeared in the main body of the text. As stated in the introduction, I offer a minimal conclusion here. Further investigation and argumentation is needed in order to make a definitive conclusion about this debate. Both realism and anti-realism have strengths in certain areas of philosophical concern. I tentatively suggest, however, that – when deciding between OSR, CE and NOA as presented here – OSR offers the most promising prospects for further development.

3.1. Final thoughts on the middle-way

I have simplified a plurality of views into what I termed the 'middle-way'. This position has intuitive appeal; we are encouraged towards respect for, and tolerance of, many different stances. This equanimity may partly explain the popularity of middle-way views in the academy. We saw, however, that these various positions - despite first appearances - have much in common. Deflationary, pluralist, relativist and ethically motivated theories can be understood as entailing a semantic diversity of equitable indeterminacies or uncertainties about scientific ontology. It is, therefore, not clear to me how middle-wayers can tell an astrologer, for example, that she is wrong. The 'astrological stance' can likely be contrived in such a way so as to appear grounded in empirical output, vaguely rational, successful by its own standards etc. The same goes for the 'scientology stance' and the 'solipsist stance'. The only way to rule out these obviously aberrant positions would be to adopt some meta-criterion of judgement, but this is generally forbidden by middle-way views, for the reasons discussed above.

As we have seen, the absence of a meta-criterion tends to usher in an ethics-first heuristic. Fine's acceptance of science on the norm of *trust* is because NOA disallows any overarching, stance-transcendent epistemologizing about scientific output. The NOAer cannot appeal to a meta-principle; this would contradict her epistemic diversity. The same goes for Chakravartty's relativism and Longino's feminism. I suggest that the middle-way position - *qua* non-position - entails either anything-goes or nothing-goes. As such, the middle-way does not appear to 'hang together' as proponents try to argue it does. Instead it collapses into either relativism or nihilism (which, for all intents and purposes, presents the same outcome). The middle-way, therefore, presents an epistemic impasse as far as ontological theorizing about the stuff of science goes.

3.2. Final thoughts on anti-realism

I began my research for this thesis wanting to be an empiricist. However, while writing the text, I have come to the realization that metaphysics is unavoidable, and that anti-realism - in the form of CE anyway - appears to be untenable.

Above I narrated the evolution of van Fraassen's thought by exposition of his three main texts. We saw how CE - in its initial incarnation (van Fraassen 1980) - encouraged a fairly strong empiricist conviction of *sola experientia*. van Fraassen is primarily concerned with two issues. Firstly, with giving an account of scientific ontology without epistemic access to unobservables. Secondly, with giving an account of scientific theorizing without rigid alethic or doxastic commitment. The first task is undertaken by an argument for the epistemic reliability of regularities in 'the phenomena', and also on a sceptical analysis of metaphysical theories making ontological posits beyond the phenomena. The second task involves conceptualization of successful scientific theories as *empirically adequate* - i.e. *acceptable* theories 'saving the phenomena' - rather than as *true* theories warranting *belief*. We should only commit to what can be made sense of in terms of observable phenomena and remain agnostic about the rest.

In the second book (2002), we looked at van Fraassen's introduction of 'stance' related themes such as pluralism, interpretation, values, emotion, *the will* and voluntarism. He now aims only to defend that it is *reasonable* to be agnostic about unobservables, instead of that we *should* be. Empiricism is only one of many internally coherent stances (realism also being one of these). This presents an anti-foundational position with an existential flavour and an emphasis on *the human*. It is a contextual epistemology that relies for its rigor on both rationality - as internal coherence of stances - and on voluntarist epistemology informed by personal values. A stance is a non-dogmatic attitude that can remain constant through Kuhnian revolutions, even while scientific propositions, models and ontologies are undergoing radical change.

In his (2008) van Fraassen promoted an up-to-date version of CE: empiricist structuralism, which involves incorporation of structuralist motifs into his brand of anti-realism. He is mostly concerned with giving an account of scientific measurement, representation, modelling and theory-building in structuralist terms. Successful scientific theories are understood as representing empirical phenomena isomorphically embedded in abstract structural models. In terms of explaining this connection between actual phenomena and representational mathematical structures, van Fraassen turns to the indexicality of the subject *qua* experimenter. Scientific representation is mediated by the *user* of the relevant instruments, models and/or theories, and the objectivity of science is understood in term of intersubjectivity. van Fraassen also introduces the notion of 'appearances'. While the phenomena are in some sense *real*, the appearances are perspectival representations of the phenomena. This distinction allows for there to be perspectival representation of the appearances from within stances (the physical stance, the biological stance, the economic stance etc.). It also allows, though, for aperspectival representation of the phenomena. This is the objective, overarching scientific 'view from above' - reached by the careful intersubjective collation of perspectival content. The view from within and the view from above then evolve together - intertwined and progressive - evidently neither collapsing into relativism nor implying ascension to a God's eye view.

I described some of the often repeated criticisms of van Fraassen's empiricism. His various distinctions - observable/unobservable, belief/acceptance and truth/empirical adequacy - have been extensively discussed in the literature. I went on to suggest that these various counter-intuitive distinctions, in fact, introduce a universal distinction no different from the nominalism that disabled the logical positivists. van Fraassen's theory involves an awkward juggling act implying an unsustainable universal dualism. This dualism introduces semantic, epistemic and practical difficulties that - as far as I can discern - irredeemably cripple anti-realism (in the form of CE anyway).

3.3. Final thoughts on realism

We also looked at ontic structural realism, as articulated by Ladyman. He presents a version of structural realism that claims to go further than its predecessors. Conventional wisdom has it that the world is made up of various objects standing in certain relationships. OSR holds that upon closer analysis there are only relationships. Objects are just relations behaving like objects. Objects, individuals, particulars or substances are - in fact - forms, patterns, information or *structure*. There is nothing that is *structured*.

By an elaborate inductive argument involving naturalized metaphysics, quantum mechanics, modal objectivism, Dennett's real patterns, Ross' rainforest realism and information theory Ladyman fuses the epistemic and the ontic. We *know* only, and there *is* only, 'mathematico-physical' structure. Epistemically basic structure just is the world ontology. Epistemology merges into ontology at the limit. There is no clear distinction between abstracta and illata, between mathematics and physics, and *mutatis mutandis*, no Kantian phenomenal/nounemal distinction. Ladyman sums up that:

> The tentative metaphysical hypothesis of [OSR], which is open to empirical falsification, is that there is no fundamental level, that the real patterns criterion of reality is the last word in ontology, and there is nothing more to the existence of a structure than what it takes for it to be a real pattern. (Ladyman and Ross 2007: 178)

I, however, articulated that Ladyman has not demonstrated his bold merger of epistemology and ontology in a way that is sufficiently convincing. There appear to be some loose ends - some Kantian residue - the 'thing in itself' still eludes us. I have suggested that Ladyman's realism is incomplete; that he doesn't improve on Kant, Russell, Worrall and van Fraassen's humbler offerings. More work is needed; particularly with regards to the phenomenal/noumenal distinction (or lack thereof). Nonetheless, I believe the view is pointing in the right direction. It offers a sturdy scaffold for a possible systematic unitary account of scientific ontology.

All told, I generally side with structural realists. Therefore, on my analysis, the issues with OSR present puzzles with possible solutions, rather than debilitating dilemmas or insurmountable obstacles as was the case with NOA and CE. As such, I conclude that anti-realism and the middle-way are dead ends. The task ahead, therefore, is to formulate a robust and sustainable version of scientific realism, possibly predicated on Ladyman's ideas.

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Appendices

Appendix A. Kant's structuralism

Structuralism was anticipated by Kant (1933), who held that the 'agreement' between a cognitive representation and the constitution of the represented can achieve only 'the *form* of truth', rather than unconditional truth. 'Form' meant 'space and time' to Kant, but we see in his philosophy the genesis of modern epistemic and alethic structuralism. Kant (1933) also famously argued that knowledge cannot transcend experience, yet is also not induced directly from experience. Human knowledge is part *a priori*, part *a posteriori*. This involves Kant's perennially discussed phenomal/noumal distinction. Scruton (2002) explains that, for Kant, certain fundamental principles of science, "such as the principle of the conservation of substance, the principle that every event has a cause, the principle that objects exist in space and time, can be established a priori" (141).

For Kant, logic, mathematics, space, time and causation are part of our basic psychological apparatus, required for the very possibility of *a posteriori* experience. These psychological *constructs*, or *categories*, are preconditions for investigating empirical reality. They are the basic 'forms of intuition' that allow us to construct *a priori* concepts by which we make sense of our sensations. As such "the outer world causes only the matter of sensation, but our own mental apparatus orders this matter in space and time, and supplies the concepts by means of which we understand experience" (Russell 1945/1967: 642).The mind independent, insubstantial things-in-themselves, however, are outside of space and time: beyond the boundary of what one makes intelligible by means of the categories. In other words, "our knowledge of the world is limited to the phenomenal world, the world of our experience; it is not knowledge of the world in itself (the noumena)" (Chakravartty 2017a: 185). By this dual schema we can gain insight into the sciences, mathematics, religion, politics, and the arts because we:

> engage [and] participate in these, and through this participation constitute what they are. But when it comes to the natural world, whether observable or hidden, Insight and Reason deliver nothing except the forms of our thinking about them (van Fraassen in Chakravartty and van Fraassen 2018: 20).

For Kant, our *a priori* conventions - space, time and causation - provide ontological scope for *a posteriori* scientific theorizing and experimentation. For later neo-Kantians "the world is, in part, a product of our ways of understanding it, which includes substantive shaping by frameworks of a priori principles" (ibid: fn. 3).

Appendix B. Bell's theorem

Bell's theorem has, perhaps rightly, been declared "the most profound discovery of science" (Stapp 1975: 271). It "discriminates between quantum mechanics and all theories where probabilities in measurement results arise from the ignorance of pre-existing local properties" (Maccone 2013: 854). An example of the latter is classical thermodynamics, where probabilities arise from our ignorance of the microscopic states of individual molecules.

Bell's theorem tells us that "any adequate successor to QM. . . cannot be both local and posit possessed values for all measurable observables" (Ladyman and Ross 2007: 175). According to Ruetsche (2017):

> Bell showed that any *local hidden variable theory* - that is, any theory attributing the correlations to common causes propagating non-superluminally - is committed to a set of inequalities not predicted by standard QM. Subsequent experiments reveal nature to violate these Bell inequalities and uphold the quantum predictions. Distant quantum correlations can't be understood in terms of local common causes (295).

No deterministic local (typically hidden variables) theory can reproduce all the predictions of QM (if the wave function is taken to be a complete description of the relevant system). QM probabilities cannot ostensively arise from our ignorance of *local* pre-existing variables. The non-superluminal causality in Einstein's general relativity must be violated if we want hidden variables in QM. Either refuse the idea that measurement uncovers pre-existing values - Copenhagen interpretation - or make use of nonlocal properties - de Broglie-Bohm interpretation. The many-worlds interpretation leaves the formalism of QM unchanged. It takes the infinite set of all possible measurement outcomes - Hilbert space - to be real.

A simplified conceptual version of Bell's theorem, which will suffice for our purposes here, is presented by Maccone (ibid.) as follows. Suppose we have two identical objects, having all the same properties with all the same values. Suppose local realism, and also that the values of the properties are predetermined. We take three of these properties *A*, *B* and *C*, each taking a value of either 0 or 1. We do not know any of the property values because we are separated from the two objects until we make a measurement. However, we do know that the values can take certain combinations (1, 1, 0), (1, 0, 0) etc. - eight possible combinations in total. Our ignorance is expressed through probabilities of these combinations over repeated measurements. Bell's famous inequality concerns the correlation among measurement outcomes of the property values.

Call $P(A, B)$ the "probability that the properties *A* of the first object and *B* of the second have the same value: *A* and *B* are both 0. . . or they are both 1" (Maccone ibid: 855). Obviously $P(A, A) = P(B, B) = P(C, C) = 1$ (given that the objects are identical). Also $P(X, X) = 1$, where $X = A, B, C$ (given that the two objects have the same property values). Under these classical conditions, Bell's inequality states that $P(A, B) = P(A, C) = P(B, C) =$ at least 0.333..., or:

$$P(A, B) + P(A, C) + P(B, C) \geq 1.$$

This is so even if we don't know which of the eight scenarios is actually occurring. Even if we randomly select any of the three pairs (A, B), (A, C), (B, C) enough times, we expect to get matching results at least one third of the time. The sum of the probabilities that two properties have the same value must be equal to, or more than, 1. In other words, since the two objects have the same properties, the sum of the probabilities "must be greater than 1 because all eight possible three-value combinations have been counted, some more than once" (ibid.). The objects here are intended to represent quantum entities, such as two photons, emitted from a common source, and having three properties (e.g. measurable polarization orientation along its axis). Both experiment and theory in QM violate Bell's inequality. $P(A, B) = P(A, C) = P(B, C) = 0.25$ according to QM, or:

$$P(A, B) + P(A, C) + P(B, C) = ¾ < 1.$$

More simply, a metaphorical illustration of this probabilistic asymmetry involves imagining two fair six-sided dice. Classically - using Maxwell-Boltzmann statistics - there are 6*6 = 36 different outcomes when we toss the two dice. We can track each individual die through time, since they are classically distinct individuals. Die one showing 3 and die two showing 5 is a different combination from that of die one showing 5 and die two showing 3. However, in QM these combinations are considered to be one and the same. The dice - now quantum dice, signifying quantum particles - cannot be individuated in the Bose-Einstein statistics used in QM; the individuality of quantum particles is undefined (see Ladyman 2016 and Esfeld 2013). We now, therefore, have 21 different, equiprobable outcomes - 6 doubles and 15 pairs - when the dice are thrown, instead of 36. Classically a double is half as likely as a pair, but in QM they are equiprobable. This is counter-intuitive, but verified experimentally. It seems that particles really do lose their individuality in the quantum realm, if we take the formalism of QM literally.

van Fraassen (1991) and Fine (1987) understand Bell's theorem to show that no causal account of the correlations between entangled physical systems is possible. These correlations are brute: not explicable in terms of something else. Ladyman replies assertively that "the acceptance of such brute correlations, properly understood, entails a metaphysical commitment to the objective modal structure of the world" (2016: 187). This is because the correlations are probabilistic, hence modal and not merely occurrent. The conceptual structure of QM makes use of probabilities irreducibly, rather than merely epistemically, concludes Ladyman. Fine (1986) considers the most profound aspect of Bells theorem to be that:

> it has made us recognise a general problem in connecting the fact of suitably independent causal histories for stochastic processes with the requirement of stochastic independence of their outcomes (fn. 23)

Appendix C. What is *Ψ*?

According to Newton's laws of motion, if the position and the velocity of a particle are known at some time, then it is theoretically possible to predict exactly where the particle will be at some later time. Particles, however, sometimes behave like waves, as in the (in)famous double-slit experiment. "When a particle behaves as though it has a wave character, the wave representing it is called a matter wave" (Kumar 2009: 380), but waves are not localized at a single place the way particles are. Waves are disturbances carrying energy through a medium, and can be described by a wave equation that maps their motion. The wave function "represents the wave itself and describes its shape at a given time" (214). The wave function for a water wave specifies the size of the disturbance (the amplitude) of the water at any point at some time.

The conventional understanding in physics is that the *quantum mechanical Ψ* (alternatively wave function, quantum state or state vector) is a mathematical expression containing information about some isolated quantum system. It is a function associated with the wave properties of a system or particle, given that particles are treated as wave-like in QM. The standard view has the wave function representing everything that can be known about the state of a physical system. Ψ is a complex-valued probability amplitude, and the probabilities for possible measurement results on the system can be derived from it. For example, using the wave function of a hydrogen atom, one can calculate the probability of finding its electron at a certain point around the nucleus (ibid: 385). Given Ψ one can:

> derive probability distributions for all the physical quantities pertaining to the system, usually called its observables, such as its position, momentum, angular momentum, energy, etc. The operational meaning of these probability distributions is that they correspond to the distribution of the values obtained for these quantities in a long series of repetitions of. . . measurement (Hilgevoord and Uffink 2016: n.p.).

If, for example, we have a single radioactive atom with a half-life of one hour, it is impossible to predict when it will decay. We know that it *will* decay, but can only predict the probability relevant to a sample of multiple such radioactive atoms. This limitation cannot be helped; it is an unavoidable result of the statistical/probabilistic nature of the quantum rules. According to Ψ, after one hour the atom will be in a superimposed state of both decayed and not decayed. Once a measurement is made on the atom, Ψ ‘collapses’ into one definite state. Ψ only concerns the outcomes of measurements (via the Born rule, discussed on pages 68 and 69); it is silent on what is *true* or *real* in the absence of observation/measurement. This operative framework, however, presents a quandary. If a particle is represented as a matter wave, what is the medium through which it ripples? In other words, what does the wave function represent? This is *the* big question about QM still passionately debated among physicists and philosophers of physics.

Schrödinger proposed doing away with particle representation; instead an electron should be thought of as a 'continuous density distribution'. He theorized that the wave function assigns 'fuzzy' values to the quantities of the theory (e.g. position and energy). Einstein objected that this fuzziness should ripple from the micro up to the macro world, but this is not what we see happening. The orthodoxy now is to think of particles as 'invariants under transformation':

> Every object that we perceive appears in innumerable aspects. The concept of the object is the invariant of all these aspects. From this point of view, the present universally used system of concepts in which particles and waves appear simultaneously, can be completely justified (Schrödinger 1954: 266).

We treat the wave function as assigning a probability distribution over exact values, and "we use the assigned probability distribution to tell us how likely we are to find the various exact values of the quantity when we measure it" (Fine 1988: 4). This is done by squaring the absolute value of the wave function. This value expresses a probability amplitude for the outcome of measurements. However, says Born, while the "motion of particles follows probability rules. . . probability itself propagates according to the law of causality" (Born quoted in Pais 2000: 39). For Heisenberg (1962), and for a minority of current scientists/philosophers, the probabilities are objective, rather than subjective. This interpretation holds that "the probabilities are genuine chancy outcomes, and that when a measurement is made there is an irreversible transition from potentiality to actuality" (Ladyman and Ross 2007: 165).

Moving on, Ψ cannot be observed, and its value cannot be directly measured. Being a complex number, it has no physical 'meaning'. It exists in:

> the mysterious, ghost-like realm of the possible. It deals with abstract possibilities, like all the angles by which an electron could be scattered following a collision with an atom (Kumar 2009: 219).

QM "can generate only the relative probabilities of obtaining certain results from the measurement of an *observable*" (ibid: 381). According to Fine's pragmatism, QM "treats the wave function instrumentally. It assigns probabilities that we can use as reliable guides to certain actions" (1988: 4). Echoing the Copenhagen interpretation, Fine holds that between measurements (or observations) the electron has no existence outside of the abstract possibilities of Ψ. When a measurement is made, Ψ 'collapses' as one of the possible states of the electron becomes the actual state, and the probability of all other possible states becomes zero *instantaneously*.

Every quantum system may be in a superposition of different states because a combination of wave functions is also a wave function. The 'universal wave function' is the quantum state of the whole of existence. This meta-*Ψ,* was introduced by Everett (1973), whose many-worlds interpretation of QM is preferred by Ladyman. One can regard the many wave functions of the many systems in the physical world "as the fundamental entities, and one can even consider the [wave] function of the entire universe. . . all of physics is presumed to follow from this function alone" (Everett 1973: 8-9). The universal wave function obeys a deterministic wave equation, and:

> our notions of the unique outcome of a measurement and the collapse of the wave function must be understood as thoroughly relativized to a branch of the universal wave function. Just as, contrary to appearances all times are real, so all possible outcomes of a measurement are real. What we refer to as the actual is the branch in which we happen to find ourselves. (Ladyman and Ross 2007: 175).

Each measurement outcome corresponds to a physical world, and these worlds are all equally real. As time goes on, "there is a proliferation of these worlds, as situations arise that give rise to a further multiplicity of outcomes" (Myvold 2018).