Realist and Anti-Realist Approaches in Philosophy of Science: Perspective and Representational Pluralism in Scientific Discovery

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Table of Contents

Abstract

This work traces a thread from what might be called a standard account of scientific realism and anti-realism, through Bas van Fraassen's influential alternative anti-realist accounts of his *constructive empiricism* and later *empiricist structuralism*, expressed in his writings that have stimulated vigorous and extended reactions over many years. Via an examination of *structural realism*, the thread has lead me away from the focus on microphysics, so prevalent in much of the writing in this debate, to a consideration of the problem of complexity in the special sciences, a response from the point of view of biology in particular, where I assert that the complexity of this discipline is incompatible with the idea that biological representation can be usefully mathematized, up to isomorphic description, one of the central tenets of van Fraassen's structuralist thesis. I argue that understanding scientific models only in terms of mathematical structures is too restrictive and is inappropriate for understanding the diverse phenomenal models prevalent in biology. I discuss alternative, less constrained, more pluralistic ways of matching representation to the world, and separately consider the difficulties of dealing with the 'disorder of nature' including the problem of definition of natural kinds, and the associated implications for realism, ending with the question 'realism about what?' I conclude with a tentative advocacy for a moderate, perspectival, epistemic realism, similar to Giere's constructive realism or a species of entity realism, consonant with Paul Churchland's suggestion that our best grasp on the real resides in the representations provided by our best scientific theories.

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All philosophy is founded on two things; an inquisitive mind, and defective sight;…the difficulty consists in our wanting to become acquainted with more than we can see.
Bernard de Fontenelle (1715, p. 8)

Chapter 1

Scientific Realism *versus* Anti-Realism, an Introduction

Preamble

It seems common sense to believe that things we encounter in the world are real and exist independently of observers; this is the notion of so-called 'common-sense realism'. Broadly speaking, scientific realists assert that the objects of scientific discovery and knowledge, the entities that scientists attempt to describe and represent, including those which are ordinarily unobservable by unaided special senses, exist independently of the minds and thoughts of scientists (as observers and investigators of phenomena), in a world where scientific truths are potentially discoverable, and science is a progressive and objective enterprise. However, instrumentalists and other species of anti-realist philosophers have long disputed what has appeared to be compellingly obvious to most, asserting that the doctrine of realism is irrational because, as they see it, there is an unbridgeable disconnection between whatever constitutes the world, and knowledge of that world, given that the knowing mind is dependent on the subjective evidence of the senses. To abbreviate a very long argument, this leads to the anti-realist conclusion that the perceived world is only knowable as a mental construct, as dependent on our conceptualization of the world. (Chalmers 1990; Devitt 2010, pp. 225-226; Fine 2005, p. 950; Godfrey-Smith 2003, p. 173)

Debates about scientific realism concern the very nature and epistemological status of scientific knowledge. The project of science is to describe the world and to explain how and why things are as they are. Usually this involves theorising about various unobservable entities, laws and mechanisms that underlie or cause the things we can see or otherwise sense. Scientists aspire to understand and represent the nature of things at the deepest level. Scientific realists aim at the very truth about the theoretical entities, laws and causes of observed phenomena. The scientific realism/anti-realism argument is centred on the aims or products of science and goes to the question of how best to interpret and represent the theories, concepts or apparent facts of science—how to make sense of what scientists do and say (Ladyman 2002, pp. 5-8; Okasha 2002, p. 60). Standardly, scientific realists assert positive epistemic commitment to established or mature scientific theory and advocate belief in the world as described by scientists, including its observable and unobservable phenomena; they claim that the truth, or approximate truth of a proposition is a necessary condition for scientific explanation. They argue that theories accurately describe or model the world and that they should be interpreted realistically, whereas anti-realists say that all scientists can aspire to is a system of theories sufficient to account for the observable facts of the world and to facilitate predictive success in our interactions with it; the goal is no more than empirical adequacy, that is, that all the observable phenomena are as the theory describes or predicts—theories are only instruments for this purpose, they are no more than pragmatic, convenient and useful fictions, rather than truths.

A major pivot point in this context is the distinction between observable *versus* unobservable entities. Generally, realists argue that reliable claims can be made about both observables and unobservables, but anti-realists say that true knowledge of unobservables is impossible. In a more formal sense, the reference to knowledge prefigures several strands to this matter: one is metaphysical and ontological, asserting either the independent existence, or not, of certain entities (particularly unobservables), the others being epistemological and explanatory, concerning the justification of belief—how we can know what entities exist and the truth of the theories and laws associated with them? (Chakravartty 2013, §1; Fine 2005, p. 950)

An important contrast in these matters is that between the primarily instrumentalist commitments entailed by *empiricism* with its emphasis on the role of sense experience, in science in particular, and scientific realism that commits the realist to considerations of what might be actual. This difference is dramatically highlighted by the historical example of Galileo's conflict with his church over the question of the significance of his postulate of heliocentricity as a model of the solar system. Galileo's inquisitor, Cardinal Bellarmine did not object to the instrumental use of Galileo's hypothesis that the earth orbits the sun as a device for making astronomical predictions, for saving the phenomena. However, he objected in the strongest possible terms, accompanied by associated implied mortal threats and sanctions, that Galileo should not assert that heliocentrism was true.

In the context of science, most empiricism is to realism as agnosticism is to theism, but there is a more extreme atheist empiricism, *fictionalism*, which posits that truth is irrelevant as a theoretical virtue. Another option, that of *instrumentalism,* claims that scientific theories have no truth values at all and are no more than pragmatic tools for facilitating predictions. Anti-realists deny the possibility of objective ontological commitment. (Ross, Ladyman & Collier 2010; Sober 2014)

There are important tensions between science and metaphysics. This seems curious given that both science and metaphysics are concerned with the question of what there is; both seem to have the same aim, that is to describe the nature of things and the entities of which they are comprised. Mumford asks: 'Is either of them logically or epistemologically prior to the other?' (2014, p. 38). Certainly, the two disciplines differ dramatically in their natures: science is grounded in experience, empirical discovery and inductive *a posteriori* reasoning, whereas metaphysics is abstract, non-experiential and based on *a priori* deduction. There is profound disagreement about the significance of metaphysics and its relation to science, with a spectrum of views ranging from the assertion that metaphysics is meaningless nonsense at one end, to the view that there can be no empirical or scientific knowledge without prior metaphysical understanding, at the other. The matter is especially problematic as metaphysicians disagree among themselves. (Mumford 2014) The main emphasis in this thesis will be on epistemological issues.

In addressing these various questions, the philosopher of science is confronted by the dilemma of having to accommodate, on the one hand, the complexities inherent in the often bewildering, detailed, highly specialised, discipline-specific and arcane practices of actual scientists, and on the other, the need to be true to the more general epistemic accounts of knowledge and justification. Informed by the 19th C writings of William Whewell, the former is identified by Bird as the *particularist* tendency of scientists which he sees in potential opposition to the *generalist* tendency of epistemologists, the particularist tendency referring to the *particular* processes of reasoning and details of episodes in the history of science, which he sees as science-specific (Bird 2011, p. 15).

The ideas developed below are partly motivated and coloured by my own career-long experience of, and expertise in, human biology and diagnostic pathology, mainly achieved, over many years, through the microscopic analysis of normal and abnormal human tissues. The initial parts of this thesis are presented as a critique of standard philosophy of science, while the later sections explore the possibility of the application of these arguments to biology. It is the applicability of the various philosophical attitudes and approaches to scientific realism and anti-realism, in the context of biology, that is my ultimate purpose.

1.1 An overview of scientific realism

Scientific realism embodies epistemic commitment to belief in the truth or approximate truth of the claims of scientific investigation. Theoretical claims enable literal knowledge of a theory-independent world—an underlying physical reality, and 'a positive epistemic attitude' towards the content of established best theories and models of the world described by science, including belief in both the observable and unobservable components of that world. On this view, direct human sensory experience is not privileged, and reliable observability is extendable to things (like genes, gravity, atoms or bosons) that are detectable, for example, via secondary effects through instrumental observation or measurement. A common description of realism includes semantic commitment to the truth, or at least to the approximate truth of theories, typically understood through correspondence theory: that is, truth of the statements of a theory consists in correspondence with the facts, the actual state of affairs in the world. The achievements of science and the progress of knowledge for the realist are taken as evidence for the apparent truth of scientists' best theoretical statements, that is, to accurate reference to things in the world, including unobservables. Success is judged by explanatory and predictive efficacy. Another way of putting this is scientific theories refer to real features in the world—that is, to things in the universe (entities, objects, structures, forces and so on) that comprise or cause observable phenomena.

Scientific realists reject positions arising out of idealist alternatives that deny a world external to and independent of the mind. Metaphysically, realists are committed to a mind-independent and objective ontology of things that exist in the world, discoverable and describable by scientists. Realists are committed semantically to literal interpretations of their discoveries; their claims about entities, theoretical statements, laws, processes, properties and relations are construed as having truth values, even if such values are approximate or provisional, whereas strict anti-realists will hold that claims about unobservables have no literal meaning at all. The consequent realist epistemological commitment is to the notion that the claims of science constitute knowledge of the world including both observables and unobservables . Scientific realists assert that it is irrational not to follow the same patterns of inference with respect to arguments about realism as for the conduct of science itself. It is irrational not to assert the truth of the theories that scientists come to accept. That is, to have good or sufficient reason for holding a theory is to have good and sufficient reason for accepting that entities postulated by the theory actually exist. (Chakravartty 2013; Sankey 2016; Smart 1963; van Fraassen 1980, p. 19)

Realist commitment is more nuanced than the foregoing might suggest and there is no single standard form of scientific realism. Nor is the argument precise; it is common to read accounts of realism couched in terms of ‘approximate truth’ and with reference to scientists’ ‘best’ or ‘mature’ theories and models. Such qualifications reflect the inevitable fallibility of science and its dependence on empirical research and inductive inference. The history of science is littered with discarded and falsified theories and broken paradigms. Realists are fallibilists and recognize that all knowledge is partial and incomplete; their commitments, therefore, are to theories that are mature, rigorously and repeatedly tested, developed over time and, preferably, free of *ad hoc* manoeuvres designed to save theory from potentially disconfirming results. Theories that survive and that then lead to successful novel predictions are regarded as converging on the real, are closer to being true, even if one accepts that in the fullness of infinite time a falsifying counter-example is almost inevitable.

Arguments for realism are several; as part of this introduction, I will briefly consider the 'miracle' argument, and the implications of our ability to manipulate and control technology and the environment, plus consideration of experimental methods that enable corroboration, successful explanation, and novel prediction. I will consider arguments against realism separately.

The miracle argument (more accurately the 'no-miracles' argument; also known as the ‘Ultimate Argument’) is an argument from the success of science based on abduction—inference to the best explanation. The starting premise here is that science is remarkably successful in that there are many theories that make accurate and confirmed novel predictions, retrodictions, and explanations of phenomena, and that enable complex manipulations and control over our lives and environment. The no-miracles argument asserts that if a theory is successful (if its observational predictions come out true) then, if the theory says that X, the observed world tends to be as if X is true. Or, prosaically, why do multiple physical phenomena behave as if there are atoms? The realist answer is: because there *are* atoms; any alternative explanation of scientific success would involve miraculous coincidence if successful theories were not approximately true. (Chakravartty 2013 1.3, 2.1, 2.2; Devitt 2010, pp. 227-228; Musgrave 1985, pp. 209-211; Sankey 2016)

Another motivation for realism comes from consideration of the ways scientists conduct their experiments and thereby come to consilient conclusions about the quality of the evidence before them. A single novel result might be interesting but is unlikely to be conclusive about the nature of things. So scientists typically seek to increase confidence in the reality of unobservables by finding corroborative evidence, and by manipulating related phenomena in various ways. Corroboration is achieved by repeating experiments, not the same experiment, by using a variety of techniques to demonstrate, in different ways, a particular property or entity. Hacking uses the example of employing different kinds of microscopes (such as light-, electron- and fluorescence-microscopes) to demonstrate, in different ways, a particular cellular phenomenon (Hacking 1983, pp. 186-209). The argument from corroboration, (essentially an experimental no-miracles argument), depends on the detection of an entity or property by two or more means, each using different, theoretically independent methods of detection employing distinct technologies and detection mechanisms.

Hacking also stresses the significance of experimental manipulation as evidence for the cogency of realism. His prime example involved a search for free quarks by detecting fractional electric charges on super-cooled niobium balls where the charge on the balls was varied by 'spraying' positrons and electrons variously onto the niobium. Discovering the details of this experiment lead Hacking to describe his realist epiphany which he records as: 'So far as I'm concerned, if you can spray them [positrons and electrons] then they are real' (1983, pp. 22-24).

1.2 Anti-realism

Varieties of anti-realism arise out of forms of denial of the realist metaphysical, semantic and epistemological commitments referred to previously. Modern scientific anti-realism has developed from variants of empiricism focussing on direct experience as the principal source of knowledge, and the associated repudiation of the possibility of knowledge of postulated unobservable entities. Anti-realist philosophers typically say that there is no justification for belief in a scientific theory beyond its *observable* implications. This is not to deny that empirical investigation is central to the scientific method—the focus is on the commitments that ensue. Anti-realist empiricism leads to forms of instrumentalism, based on the idea that theories are no more than instruments, heuristics, or convenient fictions that facilitate prediction, reporting and explanation of phenomena. It is not appropriate to think of theories as having truth values, any more than it is for, say, a library catalogue (Chalmers 1999, p. 232). Terms describing unobservables do not refer, are metaphorical and have no literal semantics. Early advocates of such a position were the logical positivists of the early 20th century. Instrumentalism has affinities with fictionalism, a doctrine that asserts that as a matter of convenience and to facilitate enquiry, scientific theories can be construed *as if* they are true, even though there is no prospect of determining whether such theories are true—which, for fictionalists is beside the point. Fictionalists will accept that certain concepts or entities might be indispensable to a scientific explanation, but they will deny any consequent commitment to the existence of those entities. (Blackburn 1996, p. 389; Chakravartty 2013, 3.2, 3.3, 4.1; Horwich 1991; van Fraassen 1980, pp. 34-35).

The more recent reinvention of empiricism in the context of science, via the 'constructive empiricism' of van Fraassen, has been highly influential. I will deal with his work in much more detail in Chapter 2. Briefly, van Fraassen asserts that science can aim for no more than empirical adequacy, where theories are judged pragmatically as sound on the basis of predictive power and the other usual markers of scientific success, without the associated epistemic and metaphysical commitments of the realist. However, diverging from traditional anti-realism, he adopts a realist semantics, interpreting theories as potentially true or false, but, crucially, he recommends belief in our best theories only in their application to observable phenomena, demanding agnosticism when it comes to the truth, reference and ontology of claims about the unobservable world.

His anti-realism resides in this scepticism and is focussed on the justification of belief, an epistemic process that is seen as separate from, and independent of, meaning and metaphysics. Constructive empiricism does not deny that theories have content, or that its postulates are capable of being true or false. The position is pragmatic and sceptical. The data supporting a theory are taken to do no more than suggest that it is likely that the theory will continue to work and, therefore, it is reasonable to use the theory and test its predictions, without making any claims about an underlying reality. (Horwich 1991; van Fraassen 1980).

The long history of the disconfirmation and revision of scientific theories and laws, and the associated epistemological problems arising out of methods relying on inductive inference, have led anti-realists to assert an ontological and semantic 'pessimistic meta-induction' (PMI) at a foundational level (Laudan 1981). They claim that the no-miracles argument is contestable and that science is fallible, unstable and unreliable. It is easy to find historical examples of successful theories that once had predictive efficacy, but where elements of the theory have been found subsequently to be false; the crystalline spheres of Ptolemaic astronomy, the phlogiston theory of combustion, and the postulation of a material ether as part of electromagnetic theory are obvious instances (Chalmers 1999, p. 226; Couvalis 1997, pp. 180-181). Such examples can be taken to show that rather than confirming the inference from empirical success to theoretical truth, success does not warrant presumption of reference. The most that one could conclude is that, in general, science is on the right track. Whether this is the track to truth or just to an empirically adequate representation of reality, however flawed, is unknowable. van Fraassen accounts for the 'no miracles' intuition in suggesting that the success of science is not a miracle because in any theoretical change, both the retained empirical success from the old theory, and any new empirical success are required 'as credentials for acceptance'. In so doing he eliminates any need for explanations of success on the basis of a retained ontology. (2006b, pp. 298-299)

Inference to the best explanation is no more than inference to the best available explanation, and this does not guarantee reliable reference. The measures of explanatory efficacy or success of theories are of uncertain value. They include the generally accepted super-empirical virtues of predictive success, simplicity, consistency and coherence (with respect to existing theories and background knowledge), and scope and unity (in terms of the domain of the phenomena explained). But it is not at all certain whether such virtues can be precisely defined, measured, or applied in practice to the ranking of competing theories. Why, for example, is simplicity a reliable indicator of truth?

Most modern scientific anti-realists do not deny the existence of an independent reality, but are simply agnostic about the implications of scientific discovery, particularly that which deals with unobservables (Couvalis 1997, pp. 179-180; Fine 2005, pp. 950-951; Okasha 2002, pp. 64-65; van Fraassen 1980). Instrumentalists say that there are further concerns because of the uncertainties introduced into scientific methods that use models involving approximations or idealizations, such as treating planets as point objects for the purpose of studying Newtonian cosmology. There is also a need to accommodate realism to the historical examples of radical or revolutionary theoretical and ontological change as famously narrated by Kuhn (Kuhn & Hacking 2012)—the Copernican and Einsteinian revolutions are examples.

Some argue that the reasoning behind the PMI and the no-miracles argument is spurious—it ignores the 'base rate fallacy', as follows: The premise that historically many false theories were empirically successful does not warrant the conclusion that success is not a reliable test for truth, because the false-positive/false-negative rates, that is, the rates at which theories which are false but, none the less, empirically successful, is unknown.

For example, it seems historically likely that false theories vastly outnumber true theories. In that case even if only a small proportion of false theories is empirically successful, then despite a large proportion of true theories being successful, the successful false theories will greatly outnumber the true—the latter will be lost in the noise. A similar related case has been made for the 'turnover fallacy' where small numbers of stable successful and true theories will be lost in a historical record of high rates of replacement (turnover) in a large population of successful but false theories. Saatsi presents arguments against the reasoning behind both fallacies, in support of the PMI. (Lange 2002; Lewis 2001; Saatsi 2005).

A major plank of the anti-realist position is the argument from underdetermination of theory by data. This hangs on the indirect relationship between observational data and theoretical claims. Anti-realists stress that theories about unobservables always depend upon data which are indirect, observational in character and complicated by a maze of auxiliary assumptions including such things as background theories, complex instrumental technology and measurement imprecision. For example, the molecular/kinetic theory of gases, which refers to unobservable molecules, can be tested only indirectly through observations of instrumental measurements of temperature and pressure changes in samples of gases. Anti-realists argue that such data can, in principle, be explained by multiple, different, empirically equivalent theories that may be conflicting or even mutually incompatible—the choice of which theory to advance is 'underdetermined' by the data. That is, theories so derived have empirically equivalent or rival alternatives, rivals that agree on the observable data sets, but which are consistent with alternative inferences about the postulated theoretical unobservables. The anti-realist conclusion is one of either atheism or agnosticism towards claims about unobservable entities (Okasha 2002, pp. 71-73).

Anti-realists have attempted to explain the success of science. For example, van Fraassen suggested by Darwinian analogy that successful theories survive a highly competitive process of repeated challenge and re-examination by multiple and independent scientists; this is itself the marker of success and has nothing to do with the reality, the truth or falsity, of the underlying theory. For example, Newtonian cosmology grounded some 200 years of predictive success before Einstein showed that the underlying theories were false (Chalmers 1999, pp. 235-236; Couvalis 1997, pp. 172-180; Devitt 2010, p. 228; van Fraassen 1980, pp. 39-40).

1.3 Some realist responses

No rational scientist denies the fallibility of the scientific method, or the fact of change, or even radical revolutions in science. Realists do not hold that current science is not making mistakes but rather realists are committed to mature, tested and established theories and their related unobservables. Idealizations are not intended literally but are to facilitate representation, modelling and calculation, and the development of mature theory in the investigative journey from discovery to justification. Idealised models are ubiquitous—they trade off reliability and accuracy of prediction in favour of computational tractability. Realists concede that the truth of a given postulate may be approximate, and promote the idea of theories converging on the truth over time. Falsifications, revisions and even revolutionary changes usually do not cause the abandonment of entire enterprises; replacement theories converge on truth partly by incorporating or conserving successful parts of their predecessors, and by the progressive elimination of error. (Blackburn 2006, p. 176; Chakravartty 2013, 1.3, 3.4; Chalmers 1999, p. 235; Dennett 1991) Continual refinements and developments in scientific methodologies mean that the processes of science are subject to continuous improvement and increased efficacy in the search for successful theories and the more confident characterization of the unobservable world (Devitt 2010, pp. 232-233). And, surely, if the pessimistic induction from the history of failed theories is to undermine realists' belief in truth, then it must also have similar effect on anti-realists' acceptance of—belief in—empirical adequacy (Blackburn 2006, pp. 188-189).

A possible alternative position is that the historical picture of theory change is misleading, that the consequent inductive pessimism is misplaced. For example, Mizrahi (2013), and Fahrbach (2011) have argued that the formulation of the pessimistic meta-induction is fallacious, that it is based on biased and unrepresentative sampling of old rejected theories and that it should be abandoned as an anti-realist argument. Mizrahi shows that when re-examined using random samples of theories and laws from standard modern reference collections, the majority are found to be persistent over contemporary time frames. Fahrbach presents evidence from the exponential growth of science, that most of the scientific investigation that has ever been carried out has occurred in the last 50-80 years. He argues that it can be concluded that modern, successful theories have been proven stable over that period and that the pessimistic meta-induction is invalid. Nonetheless, the problem of induction is untouched by such short-term observations.

Realists respond to the underdetermination argument by saying that even if there are multiple possible explanations for a set of data, it is typically not the case that they are all as good as each other—they are not materially equivalent: typically, they differ in their super-empirical virtues. There are criteria for rational choice between competing theories, including, importantly, explanatory force or efficacy. Furthermore, although, in principle, underdetermination is arguably common, in practice significant, or materially interesting underdetermination is historically rare; more usually, scientists have difficulty finding even a single theory that adequately matches their data, particularly when conjoined with complex auxiliary hypotheses and background conditions.

Realists accuse anti-realists of applying this argument selectively because of the latter's singular focus on unobservables. If applied consistently, the underdetermination argument would rule out knowledge of a great deal of the observable world. For example, most living organisms on earth are never observed by humans although they are potentially observable, and an almost infinite number of observable but unobserved phenomena and entities have occurred in the historical past since the cosmic singularity and will occur in the distant future, so that the fraction of actually observed observables at any given time must approximate zero. The realist conclusion is that the anti-realist position, based as it is on the distinction between what can be known versus the unknowable, between observables and unobservables, is arbitrary, and that the same inferential logic applies to both. (Couvalis 1997, pp. 188-191; Douven 2014; Okasha 2002, pp. 72-76)

Part of the problem here is that the observable/unobservable distinction is itself disputed and vague. In particular, (apart from the problem of bent sticks in water and the like) the relation between observation and detection is not always clear-cut. We detect the presence of a high-altitude jet aircraft by observing a vapour trail; electrons are detected in particle detectors such as cloud chambers by observing tracks of droplets formed in saturated alcohol or water vapour. Biological cellular structures are observed using microscopes, after staining and other manipulations, (always starting with actual tissue), then using dyes, histochemical markers and other surrogates to facilitate (for example) the observation and interpretation of the pathological changes in that tissue, which indicate disease and thence diagnosis and prognosis.

If something can only be seen or detected using complex scientific instruments, is it observable or unobservable? Some would assert unobservable because of the dependence on interpolated theory. But what if we see an image of a cell or an organism, say, through a microscope, or a very distant, otherwise undetectable object in space via a radio telescope?

There is no clear way to answer this question; ‘observable’ is a vague predicate and borderline cases will occur in a continuum from observable to unobservable, perhaps susceptible to multi-valued or fuzzy logics (Okasha 2002, pp. 66-70). The existence of borderline cases undermines the anti-realist position that unobservables have a distinct ontological status and it seems reasonable to argue that the evidence we have for the nature and existence of unobservables is not different in any significant way from that which we have for observables; that is, science provides objective and justifiable knowledge of unobservables (Couvalis 1997, p. 193).

I revisit underdetermination, and the observable/unobservable distinction in Chapter 2.

1.4 Variations and preliminary conclusions

Which doctrine is more cogent? A standard starting position is that of the intuitively plausible 'common-sense' realism, but it is obvious that scientific investigation continually produces surprising results and reveals new phenomena that conflict with ordinary or ‘common’ sense; indeed, for scientists, this is one of the drivers of further discovery. Comparison between realism and anti-realism is not an easy matter, not least because the arguments are made using two opposed theories of meaning. Realists centre their argument in terms of truth conditions, while some anti-realists rely on reference to 'assertability conditions'—those conditions which would justify particular assertions (Craig 2005, p. 887).

Even though I am attracted to the aspirational realist idea that scientific theories are gradually converging on truth, the weaker realist appeal to the notion of approximate truth invites scepticism. On the other hand, I suggest that belief merely in empirical adequacy of our best theories leaves anti-realists unable to explain the phenomena they describe. (Ladyman 2014)

One of the major points of contention centres on the observable/unobservable distinction. Alternative realist approaches have been sought in the form of suggested variations to generic realism by picking out components of theories that seem most plausibly to warrant epistemic commitment to unobservables. For example, *explanationists* pick out the parts of theories that are indispensable for the explanation of phenomena and for the empirical success of associated theory. *Entity realists*, such as Hacking, focus on causal effectiveness of putative entities as validated by experimental manipulation and intervention in associated phenomena (as discussed above) at the expense of the role of theories, which remain as intellectual tools; and *structural realists* take the view that we can only be sure about the formal parts of the frameworks or structures of theories (such as relations between structural elements) that are preserved through scientific change, rather than speculating about unobservable entities and processes.

Such selectivity seems arbitrary and sanctions *post hoc* rationalization. It takes a piecemeal approach to the different strands that constitute realism, comprising a matrix of ontological, epistemological, causal and explanatory elements. (Chakravartty 2013, 1.3, 2.3; Hacking 1983, pp. 27-29)

The ideas contested here and the suggested approaches to these matters are wide-ranging and diverse. Whether resolution or agreement on the fundamentals will ever be possible is questionable. This problem, of 'potentially irresolvable dialectical complexity' as Chakravartty has put it (2013, 4.5) has led to some alternatives. For example, Fine argues the neither standard realism nor anti-realism is tenable. He advocates a middle way based on what he calls the 'natural ontological attitude' which attempts to find a neutral point centred on a core of positions or attitudes of acceptance of the best available theories common to both realists and anti-realists, without the associated metaphysical and epistemological claims—the baggage that derails the arguments. Other alternative themes and attempts to set aside realist/anti-realist approaches are pragmatic efforts to replace the usual disputes with alternatives such as using positive utility as a marker of truth, or for quietists, eliminativists and sceptics, assertion that the dispute concerns a pseudo-problem. (Blackburn 1996, pp. 319-320; Fine 2005, p. 950)

Finally, for mature, established and successful disciplines, the realist and the anti-realist would probably readily agree that the major theories are clearly empirically adequate, that the science works, the evidence is pragmatically reliable. *The* point of difference is one of acceptance or belief in 'how far evidence reaches' (Blackburn 2006, p. 186). The anti-realist accepts the sufficiency of empirical adequacy while the realist believes in the real. Blackburn thinks that such a difference is spurious (pp. 185-196). I suggest that theory acceptance simply to the point of 'saving the phenomena' is itself a matter of belief—the distinctions are not compelling.

Bunge refers to 'the advancement of knowledge—which, as Socrates is said to have discovered long ago, proceeds chiefly through the clash of (internally consistent!) ideas and systems of ideas' (1979, p. 286). In quotidian science, apparently successful theories are repeatedly tested and challenged in a dialectical process where multiple scientists explore empirically equivalent or similar competing theories. This results in the discarding of some theories and the acceptance of others over time. Revisions and disconfirmations give increasing credence to surviving mature theories which typically are those with the demonstrated power to predict novel facts. The realist goes beyond the observation that this sort of methodology is merely instrumentally reliable, to claim its successful theories as approximately true (Couvalis 1997, pp. 190-191; Devitt 2010, p. 229).

Whether realism is any more coherent and rational than anti-realism, and whether it provides a more complete account of science will be explored below. I will begin with a standard account of the issues, observed mainly through the lens of van Fraassen's alternative anti-realist *constructive empiricism*, before examining structural realism, including van Fraassen's *empiricist structuralism* and the role of models and representation in science. I will then attempt to apply some of the principles learned to a consideration of the problem (as I see it) of dealing with biological complexity and the diverse phenomenal models prevalent in biological science, arguing for the need to recognise the validity of representational pluralism, before tentatively advocating a moderate, perspectival, epistemic realism.

Chapter 2

Anti-Realism: van Fraassen and His Critics

2.1 van Fraassen's 'arguments concerning scientific realism'

One approach to understanding the diversity of competing interpretations of realism and anti-realism is by analysis of Bas van Fraassen's anti-realist position as propounded in his influential book, *The Scientific Image* (1980). This work, and others, has prompted a continuing debate among philosophers of science, now over decades, arguing the merits or otherwise of various metaphysical and epistemological positions in response to his *constructive empiricism*. The issues are by no means settled, and there is no agreed orthodoxy, with arguments ranging from the conservative to the radical, reflecting very different views of the actual achievements of science (Churchland & Hooker 1985).

The arguments are old and stem from a perceived need by early proponents of modern empirical scientific enquiry to account for apparent regularities in natural phenomena. While realists have sought explanations through searches for causes or causal properties, some anti-realists have denied the reality of such properties and thereby rejected the demands for explanation claiming no evidence for causal or other connections between phenomena and putative unobservable entities or processes producing the observed regularities.

van Fraassen deconstructs modern philosophy of science into two main trajectories or aims: one concerned with the nature, structure and content of theories and their acceptance, and the other with relations between theories and the world. On van Fraassen's account, scientists construct theories to account for observable objects and processes (the phenomena) by inferring other phenomena not directly accessible to observation; that is, by reference to sets of unobservable entities and processes. The nature of the relations between theories (those that transcend observational data) and the world, and rejection of the need to commit to an ontology of unobservables behind the observable world are at the heart of van Fraassen's epistemic scepticism.

One possible relation between theory and world is that of being true, that is of giving a true account of the facts. A standard realist position is that science aspires to develop true theories that describe various unobservable processes that then explain the observable world; to paraphrase van Fraassen, merely possible states of affairs are used to explain the actual. Alternatively, he proposes that the standard empiricist approach to the natural world be modulated by no more than a requirement that the postulates of science need not be true except in what they say about what is observable, actual and, where possible, empirically testable, with any hypothetical secondary underlying architecture being regarded as merely a means to that end. (1980, pp. 1-5)

2.1.1 van Fraassen's account of realism

van Fraassen begins his detailed dissection of realism with an examination of what might comprise a standard realist manifesto. A simple statement might assert that science aims to reveal or describe a true picture of the world and, the entities discovered by science exist and are real. This naïve position identifies two important characteristics of realism: (i) that theory is an account of reality, of what exists in the world and, (ii) science is a work or journey of discovery, not invention. A more nuanced position, however, will recognize that the history of science is a story of error, falsification, modification and correction and this leads to the recognition that the theories of mature science are likely to be no more than approximations of truth. A new question then emerges: what is it to hold or accept a theory, given the evidence available? van Fraassen suggests a minimum statement of scientific realism, introducing a qualification to the naïve assertion, that science can only *aim* at literal truth:

> Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true. (1980, p. 8)

He explains that, simply expressed, this means that science aims to tell a true story, with possible subsidiary aims such as the satisfaction of various non-empirical or super-empirical virtues. Parenthetically he notes that this is to be distinguished from the 'aim' of individual scientists who might separately aim for quite different things depending on their own personal motives, and markers or criteria for success in the particular enterprise, such as fame and glory.

He further explains that he has used the term 'literally', to exclude as realist, positions such as positivism and instrumentalism that accept that science can be true if 'properly understood' but otherwise, literally false or meaningless. He means that the language of the realist account of science is continuous with natural language, is to be construed literally and that the statements of science are capable of being true or false. This is in contrast with a positivist interpretation of science where theoretical terms gain their meaning only on the basis of their connection with their observable consequences. A positivist could hold that two contradictory theories (for example, matter comprises atoms *versus* matter comprises a continuous medium—van Fraassen's example) say the same thing, provided that their observable consequences are the same.

Hence, he holds that there are two categories of anti-realism: one claims that science aspires to be true, 'properly (but not literally) construed'. The other 'holds that the language of science should be literally construed, but its theories need not be true to be good.' (1980, p. 10) For van Fraassen, this is an important distinction; his anti-realism is of the second form and is an expression of agnosticism about truth-claims in science. Note that the literal construal of scientific statements does not necessarily entail a realist interpretation; empirically equivalent theories can be ontologically incompatible: for example, a theist and an atheist might agree about the meaning of a statement that angels exist without agreeing that angels are real. (Ladyman 2002, p. 219; van Fraassen 1980, pp. 1-11)

2.1.2 Constructive empiricism

So, van Fraassen recommends acceptance of our best theories to the extent that they explain observable phenomena, his scepticism is in relation to unobservables and the theory statements of science which, he holds, can only be measured in terms of empirical efficacy; their truth values cannot be determined:

> Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate. …a theory is empirically adequate exactly if what it says about the observable things and events in the world, is true—exactly if it 'saves the phenomena'. (1980, p. 12)

'Saving the phenomena' implies that a given theory is empirically adequate if what it says about *observable* things and events is true. 'A little more precisely: such a theory has at least one model that all the actual phenomena fit inside'. van Fraassen devotes an entire chapter to this notion, which goes to his account of the structural concepts of scientific theory. His view is that physical theories do describe more than what is observable, but that 'what matters is empirical adequacy, and not the truth or falsity of how they go beyond the observable phenomena'. Empirical adequacy 'relates the theory to the *actual* phenomena (and not to anything which *would* happen if the world *were* different, assertions…which have…no basis in fact but reflect only the background theories with which we operate'. (1980, p. 64)

So, what is it to accept a scientific theory? This is van Fraassen's own question which he answers by suggesting two further questions pointing to the separate epistemic and pragmatic dimensions to the argument. The epistemic question is how much belief is involved in theory acceptance? The pragmatic question asks what else is involved besides belief?

van Fraassen's starting points are as follows: that acceptance of a scientific theory is only to the extent that it saves the phenomena—it correctly accounts for what can be observed. But then, the pragmatic qualification, he goes on to say that theory acceptance demands more than mere belief because theories are never fully developed and complete in every detail. The consequence is that choices need to be made where the data are conflicting or when there are competing empirically adequate theories in the frame; that is, when theory choice is underdetermined by the data. Such choices (when they truly exist) are made pragmatically and on the basis of context and consistency within the relevant research programme or paradigm, and secondarily, by applying the test of the non-evidential super-empirical virtues. Acceptance involves commitment to the theory, its models and conceptual resources which will be applied to explanation and to the accounting for new data and phenomena. As van Fraassen argues, 'commitment is of course not true or false: the confidence exhibited is that it will be *vindicated*.' (1980, p. 13). Neither the evidence of the empirical data nor pragmatic virtues compel a commitment to the truth of a given theory. While realists and anti-realists might not disagree with each other about the pragmatic applications of theory acceptance, realists accord explanations objective validity whereas anti-realists do not. (1980, pp. 9-13)

van Fraassen's constructive empiricism arises out of his strongly empiricist anti-realism (but note that he categorically rejects the ametaphysical empiricism of the positivists). His characterization of empiricism is based on the epistemic thesis that 'experience is the sole legitimate source of information about the world' (1985, p. 286). Constructive empiricism demands restriction of belief in the reality of entities, processes and phenomena to the observable domain and, as such, might be regarded as similar to traditional instrumentalism. However, van Fraassen rejects this reading because his doctrine rejects all but literal construals of the language of science, thus, according to his lights, ruling out both positivism and instrumentalism. (1980, p. 10)).

He advocates a 'constructive [empiricist] alternative to scientific realism' on the basis that, on his view, scientific endeavour is a matter of the construction of theories, models, methods and experiments, rather than discovery—the 'construction of models that must be adequate to the phenomena, and not discovery of truth concerning the unobservable'. (1980, pp. 1-5, 71)

van Fraassen discusses at some length the argument for realism based on the theory-dependence of experimental design. He acknowledges the importance of theory to the investigating scientist and the role of theory construction in the testing and analysis of regularities in the world and the predictions arising out of the resulting theories. Foreshadowing his later structuralist doctrine, which I detail in Chapter 3, he suggests that in traditional philosophy of science,

> everything is subordinate to the aim of knowing the structure of the world. The central activity is therefore the construction of theories that describe this structure. Experiments are then designed to test these theories, to see if they should be admitted to the office of truth-bearers, contributing to our world-picture.

Invoking Kuhn's 'normal science' (Kuhn & Hacking 2012) he asserts that the aim of scientists is rather 'to discover facts about the world' and, in particular, to explore the associated regularities discoverable in the observable world. This sort of discovery depends on experimentation and testing rather than on reason and reflection.

> But those regularities are exceedingly subtle and complex, so experimental design is exceedingly difficult. Hence the need for construction of theories, and for appeal to previously constructed theories to guide the experimental enquiry.

van Fraassen goes on to say that for theory construction, experimentation has two purposes: to test the theory for empirical adequacy, and to 'fill in the blanks', to guide the building, the construction of the theory to some sort of completion. Further, theory has a two-fold role in experimental design: to guide the formulations of the associated questions to be answered, and to guide the design of the experiments to answer those questions.

This is a well put realist account, but van Fraassen's purpose is to limit the aim to empirical adequacy:

> In all this we can cogently maintain that the aim is to obtain the empirical information conveyed by the assertion that a theory is or is not empirically adequate. (1980, pp. 73-74)

2.1.3 van Fraassen on "The Theory/Observation 'Dichotomy'" and the observable/unobservable distinction

Early positivist interpretations of science advocated the use of an 'observation language' centred on basic protocol statements and free of theoretical terms, the aim being to separate theory-free observational statements from theoretical postulates. This has been long rejected as unachievable, a position reiterated by van Fraassen: 'All our language is thoroughly theory-infected.' The language of science is guided by the theoretical concepts derived from earlier work; information is meaningless without prior knowledge. However, van Fraassen suggests that a distinction should be drawn between the use of theoretical terms and concepts as part of theory construction, and the classification of unobservable events and entities—this goes to the ontology of unobservables. van Fraassen's concern here is that rejection of the theory/observation dichotomy, and acceptance of the fact of the theory-ladenness of language, should not be construed as making a case for realism in science. (1980, pp. 13-14)

van Fraassen's account of the observable-unobservable distinction begins by challenging the work of Maxwell (1962) who advances a strongly realist view of science and theoretical entities, arguing that the distinction between theory and observation cannot be made because it is contingent and vague. It is important to understand van Fraassen's position here, for what follows. For him the term 'observable' refers to a class of putative entities that may or may not exist. Non-existent objects (his example is a flying horse) are observable in principle—that's how we know that there aren't any; numbers are not observable. Unaided perceptions of objects in the world are observations, but 'a calculation of the mass of a particle from the deflection of its trajectory in a known force field, *is not an observation of that mass*' (1980, p. 15, emphasis added).

It is also worth noting that he does not consider something observable as 'simply a fact disclosed by theory' or theory-dependent: 'I deny this; I regard what is observable as a theory-independent question. It is a function of facts about us *qua* organisms in the world'. (1980, pp. 57-58)

According to van Fraassen, Maxwell argues against the possibility of making such distinctions, and against the significance of such distinctions. Using the example of the in-principle continuum of cases lying between direct observation and inference, starting with unaided visualisation and proceeding via observation through window-glass, through spectacles, through binoculars and also through low-power and high-power microscopes, Maxwell contends that there is no non-arbitrary line that can be drawn between observation and associated theory (Maxwell 1962, p. 7; van Fraassen 1980, pp. 15-16). van Fraassen's response to this is to deny that the continuum includes entities beyond direct human perception. So, seeing an object with the unaided eye is an unequivocal case of observation; even seeing an object in space through a telescope counts as observation on the grounds that one day an astronaut might be able to see it unaided, but detecting something like a charged particle indirectly in a cloud chamber, is not a case of observation of the particle on the ground that this is impossible. Such a particle, unobservable in principle, must remain a theoretical entity. For van Fraassen, this argument goes to the differences between what is potentially and actually observable, and what is 'unobservable in principle' (Maxwell's terminology) where the associated theory entails permanent inaccessibility to normal unassisted human sense organs. He further insists that it does not matter that the boundary is vague, as long as there are clear-cut cases at the margins.

Maxwell's further assertion is that even if it were possible to make such ontological distinctions, this would be unimportant. Maxwell argues that there is no existential relevance to a division of entities in the world into observable *versus* unobservable or theoretical; the demarcation between observation and theory is merely accidental and arbitrary, it varies with instrumentation, is a function of our physiology and without ontological significance. van Fraassen agrees with the latter statement, that the term *observable* has logically nothing to do with existence if the question being asked is simply "whether 'observable' and 'exists' imply each other—for they do not" (1980, p. 18). However, he suggests that the observable/unobservable distinction does have significance regarding the separate question of the claims of scientific realism judged against van Fraassen's two markers, the aims of science, and the associated epistemic commitment—the degree of belief involved in the acceptance of a theory.

To reiterate the point, does acceptance of a theory permit the belief that it is true (or approximately true), or some alternative? For van Fraassen, the question of what is and is not observable is entirely relevant because 'to accept a theory is (for us) to believe that it is empirically adequate—that what the theory says *about what is observable* (by us) is true' (1980, p. 18). But the permission of belief does not require or compel that belief. Note that observable-by-us allows for modifications of observability because of changes in the range of accessible evidence as accepted by the existent ‘epistemic community’. In other words, as we have seen, van Fraassen is advocating rational commitment to acceptance of theories that are empirically adequate, where the associated models ‘fit the observable phenomena’. His point is that ‘even if observability has nothing to do with existence…it may still have much to do with the proper epistemic attitude to science’. (Maxwell 1962, pp. 3-27; van Fraassen 1980, pp. 13-19)

2.1.4 van Fraassen on inference to the best explanation

Inference to the best explanation (IBE), abduction, is a rule of inference for dealing with a group of phenomena and associated multiple competing hypotheses, each of which is empirically adequate, each accounting for the phenomena. The rule requires inferring the truth of the hypothesis that best explains the phenomena in question, that gives the best explanation of all the available evidence. van Fraassen cites the example of the phenomenon of ‘mousely presence’, the sounds of scratching and animal movement behind a wall coupled with the discovery of missing cheese; the observable phenomena are as if there is a mouse; the rule of best explanation allows the inference that there really is a mouse. Well and good, but van Fraassen will not allow extension of this method to cases of unobservable entities. He cites two objections. First he asks ‘what is meant by saying that we all *follow* a certain rule of inference?’ Does this mean that practitioners of IBE follow a formal set of rules that can be applied in the same way that the rules of classical logic are used? van Fraassen sees this as too literal and restrictive. Alternatively, acting in accordance with a set of rules might be construed more broadly such that it is enough to proceed by ensuring that any conclusion that might be reached from a given set of premises satisfies some sort of rule-based criteria for the best explanation. This is too loose for van Fraassen; potentially any conclusion may be so inferred from any premise. This approach would seem to permit believing all conclusions so allowed, and, similarly, rejection of discordant conclusions.

Hence, van Fraassen’s first objection: that the posit that we use such rule-based methodology is merely a psychological hypothesis concerning the limits of ‘what we are willing and unwilling to do’. It is also an empirical hypothesis and thereby testable in terms of the available data, and subject to rival hypotheses. So, he counters with an alternative hypothesis, that we are always willing to believe or accept that the theory that best explains the evidence available is empirically adequate (accounts for the observable phenomena) but this does not compel the belief that the theory is true, particularly for unobservables.

van Fraassen acknowledges that his objections could be construed as an argument not so much about IBE but more about whether scientists *do* follow some sort of rules of inference when seeking explanations, or whether they *ought* to do so. However, the point he is trying to make is that the rule of inference to the best explanation is found wanting in cases requiring definitive choices between potentially rival theories; his alternative anti-realist position is that explanatory power is only *one* criterion of theory choice. (1980, pp. 19-21, 71)

His second objection goes to the different explanatory endpoints sought by realists and anti-realists when choosing between competing hypotheses. Even accepting rule-based IBE as cogent, all IBE allows or dictates is a choice according to the evidence available. An anti-realist will choose between hypotheses satisfying the requirement for empirical adequacy, but the realist, according to van Fraassen, needs some further extra premise or premises for the argument because the end-point is a realist account. In van Fraassen's words:

> So the realist will need his special extra premiss that every universal regularity in nature needs an explanation…It should at least be clear that there is no open-and-shut argument from common sense to the unobservable. Merely following the ordinary patterns of inference in science does not obviously and automatically make realists of us all.

Here, van Fraassen is questioning what might be wanted as evidence for the truth of a theory, adding: 'a realist will have to make a leap of faith. The decision to leap is subject to rational scrutiny, but not *dictated* by reason and evidence. (1980, pp. 19-23; 36-37)

Taking this argument further, and using what he acknowledges as an 'over-simplified picture of science' van Fraassen addresses what he perceives to be disconnections between levels of understanding and description in science. He refers to levels of *fact*, of *empirical law* and of *theory*. Singular observable facts are typically explained by inductive generalizations and regularities, that is, by empirical laws, laws that have no observational counterparts. So, numbers of singular observations of black crows lead to the generalization 'all crows are black'. Such inductive generalizations are further explained by 'highly' theoretical statements or hypotheses that usually include reference to varieties of unobservable entities. He asserts that theories do not explain empirical laws, they don't even entail them—'they only show why observable things obey these so-called laws to the extent they do'. He means that apparent laws are actually heavily qualified and goes on to suggest that 'perhaps we have no such empirical laws at all'. That water boils at 100°C occurs only at standard temperature and air pressure, and so on. At the level of observable entities, apparent laws are merely putative and always subject to (often unstated) *ceteris paribus* qualifications (see Chapter 4). Hence, there is nothing to explain about their truth; they are useful approximations but do not provide sufficiently explanatory robustness to support scientific realism.

Although he suggests that this might just be a methodological quibble, van Fraassen's point here is that the same uncertainty extends to postulated underlying unobservables: 'but a theory which says that the micro-structure of things is subject to some exact, universal regularities, must imply the same [qualifications] for those things themselves'. Explanation is not possible without asserting the existence of variables outside the observable world. van Fraassen questions whether for science there really is such an explanatory imperative when (he asserts) there is no consequent gain in empirical prediction. He concludes with the essentially pragmatic suggestion that perhaps there might be a different anti-realist rationale for using 'a micro-structure picture' in the development of theory. He proposes

> that the true demand on science is not for explanation *as such*, but for imaginative pictures which have a hope of suggesting new statements of observable regularities and of correcting old ones. (1980, pp. 32-34)

van Fraassen concludes his overview of scientific realism by suggesting that one motive for scientific investigation is that '[s]cience, apparently, is required to explain its own success'. There are regularities in the world, science tries to account for them. Scientific predictions are regularly successful, a regularity that itself is in need of explanations. Explanation is fundamental to the project of science. He suggests that explanatory power and success underlie what he refers to as the 'Ultimate Argument' for scientific realism, that it is the only philosophical position that does not make the success of science a miracle, but he goes on to take issue with a traditional explanation for such success, that the success of science is a matter of 'the "adequacy" of a theory to its objects [via] a kind of mirroring of the structure of things by the structure of ideas'. van Fraassen's alternative is that science is competitive and adaptive:

> science is a biological phenomenon, an activity by one kind of organism which facilitates its interaction with the environment. And this makes me think that a very different kind of scientific explanation is required.

Hence his Darwinian claim that the success of science and its theories is not miraculous:

> It is not even surprising to the scientific (Darwinist) mind. For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which *in fact* latched on to actual regularities in nature. (van Fraassen 1980, pp. 39-40)

Towards the conclusion of *The Scientific Image*, van Fraassen reflects upon the complexity and the depth of commitment demanded by theory formulation in science: 'Since all men are mortal, commitment to a theory involves high stakes'. Theories are never complete. Even if two theories are empirically equivalent the associated research programmes are usually very different. Validation of research

> may depend more on the theory's conceptual resources and facts about our present circumstances than on the theory's empirical adequacy or even truth. That is why the commitment involved in the acceptance of a theory runs so deep, and why we need not postulate belief in its truth to account for that….To be an empiricist is to withhold belief in anything that goes beyond the actual, observable phenomena, and to recognize no objective modality in nature.

He recapitulates that his account of science demands the search for truth to be only about the empirical world, about the actual and observable. But, he acknowledges that science is a rich and complex cultural phenomenon demanding accompanying background auxiliary explanatory theories, conceptual commitment, modal language and much else.

> But it must involve throughout, a resolute rejection of the demand for an explanation of the regularities in the observable course of nature, by means of truths concerning a reality beyond what is actual and observable, as a demand which plays no role in the scientific enterprise. (1980, pp. 202-203)

2.2 Responses to van Fraassen

The story sketched above deals with some of the preliminaries in this discussion, but it is incomplete by substantial measure. The scope of van Fraassen's vision is wide ranging, more detailed and nuanced than this account can ever hint at. Some of his attitudes develop and are modified in his more recent writing. There are important further considerations omitted above that I will touch on or, occasionally, explore in more depth in what follows where I consider some of the many and protean responses to his provocations.

In arguing for constructive empiricism van Fraassen says that the acceptance of the best theories in science does not justify or require belief in the unobservable entities postulated by those theories. He asserts that the success of modern science can be understood without invoking the existence of such entities. It is notable that van Fraassen's empirical adequacy is based on the strong requirement that the belief involved in accepting a scientific theory is not just that it 'saves the phenomena'—that is, it correctly accounts for what can be observed—but that it saves *all* of the actual phenomena, it accounts for all of the observed and unobserved (observable in principle) relevant phenomena, including those in the past and future in addition to the present. This is a very demanding ampliative claim that goes, at least in some measure, to the nature of the observable/unobservable distinction.

The realist and the constructive empiricist disagree about the aims and purpose of science. The realist makes an existential ontological claim, saying that the scientific enterprise aims at the truth of its assertions about unobservable processes and entities that then explain observable phenomena, and explanation is central to realism; the constructive empiricist says that the aim is to tell the truth about what can be observed. Any postulated unobservable substructure is no more than an instrumental fiction that facilitates this purpose; there is no need for an explanation of all the observable regularities. van Fraassen rejects the focus on explanatory power as a pre-eminent super-empirical virtue, saying that explanation is not a 'rock bottom' virtue, not the main criterion for theory success, whereas consistency with the phenomena is (van Fraassen 1980, p. 94). (Ladyman 2002, pp. 185-186)

From the many responses to van Fraassen, in this section I will concentrate on three main themes: first, dealing further with the question of the distinction between observables and unobservables, then with the problem of underdetermination of theories by evidential data, followed by a consideration of the role of super-empirical values and other epistemic virtues in theory evaluation. (Ladyman 2000, pp. 837-856; 2002, pp. 186-193)

2.2.1 Observable versus unobservable

As detailed above in the discussion of Maxwell's earlier position, a standard response to van Fraassen's focus on the distinction between things that can be observed versus unobservable theoretical entities is that the two domains or categories form parts of a continuum and the demarcation is contingent upon human physiology and technology. Therefore, constructive empiricism grants ontological significance based on an arbitrary distinction. van Fraassen does not deny the existence of unobservables; his claim is that existence (an ontological claim) is not entailed by observability. van Fraassen's argument is essentially an epistemic one.

Alternatively, Ladyman responds (2000, pp. 840-841), it is legitimate to attribute epistemological significance to the observable/unobservable distinction. Referring to the continuum, he says regardless of the cases that can be unambiguously classified into observable and unobservable, it is not certain that the possibility of making such a distinction can support scepticism about the theoretical and unobservable: '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' (2002, p. 188). In drawing attention to things '*in principle* impossible to observe' (1962, p. 9), Maxwell is, in fact, arguing that *nothing* is unobservable in principle, because this could only mean that the entities entailed by a given theory could not be observed under any circumstance. Ladyman agrees that this latter proposition is never the case, that different circumstances might involve different sensory powers, even if the example of Paul Churchland's thought experiment (1985) involving aliens with electron microscope eyes is unconvincing. The point is that the demarcation between observable and unobservable can potentially change with time, evolution and technology, and variant epistemic communities; observability varies with detectability. And, as Hooker observes, 'accepted observationally based facts do not belong to an eternal, theory-free category but are theory-laden and subject to theoretical criticism' (1985, pp. 156-157).

van Fraassen is a direct realist when dealing with perceptions, observations, about macroscopically visible objects: 'we can and do see the truth about many things: ourselves, others, trees and animals, clouds and rivers – in the immediacy of experience' (1989, p. 178), although in his account, detailed previously, of the 'apparent signs of mousely presence' in his discussion of inference to the best explanation, as Musgrave notes he seems to demonstrate a prejudice in favour of visual perception. Why is it unreasonable to conclude that if it sounds, behaves and smells like a mouse, it really is a mouse? (1985, pp. 205-206)

Musgrave notes that van Fraassen does not permit observation using instruments, that is, he distinguishes between unmediated true observation and mediated detection. What van Fraassen is claiming here is that whereas there is no inference involved in direct perception, instrumentally mediated detection relies on fallible inference as to what is thereby detected.

Wilson suggests that *all* human detections require an extensive set of supporting conditions to facilitate associated observations, such as 'the presence of an electromagnetic field in low excitation' to enable vision, and that the distinction between mediated and unmediated detections is without merit. (1985, pp. 235-236)

van Fraassen allows the status of 'observable' to indirectly accessible objects like the moons of Jupiter because if astronauts could travel close enough to them, they would be directly observable. His scepticism, as we have seen, is for entities unobservable in principle, such as atoms and sub-atomic particles. Churchland, Ladyman and others respond by asking why is it acceptable to determine what is observable on the basis of imagining changing spatio-temporal location, but not by imagining changing the 'size' (I suppose this means the magnification and resolving power) or the configuration of our sensory apparatus?

There may be some circularity here. In the case of dinosaurs, or Jupiter's moons, the belief that they are observable requires us to believe that in all respects, we are similar to beings, say archaeologists or astronauts, located such that they could observe dinosaurs or moons directly; the only difference is that they would be closer, temporally and spatially, to the objects of interest. But don't we already have to have formed the belief that the moons of Jupiter exist to know or to understand this proposition? Not exactly, says Ladyman, (in support of van Fraassen), because belief just in the empirical adequacy of a theory that Jupiter has moons is sufficient to entail that we would be able to see them if they were present, and if we could get close enough to them.

I interpolate here that the admission of the counterfactual possibility of observability involves admitting modal facts that are theory-independent; this seems at odds with van Fraassen's repudiation of objective modality that I discuss below and in Chapter 3.

The situation with atoms and electrons is different. That a theory of electrons is empirically adequate does not say anything about what would be the case if observers were differently constituted. That is, we need not believe that being differently constituted, with different sensory powers, would allow observation of sub-atomic particles, unless we already had formed a belief in their existence. Ladyman summarises:

> Of course, the realist has contrary intuitions, and realists do not see why our physical constitution, as a contingent feature of our evolution, has any philosophical significance whatsoever. One response to this is simply to restate the opposite intuition: what else but *our* (biologically determined) observational capacities would one consider relevant to *our* epistemology? (2002, p. 190)

Musgrave asks, rhetorically, whether the evidence for the existence of electrons is better or worse than the evidence for the existence of the mouse in van Fraassen's wainscoting? Musgrave opines that 'it is a curious sort of empiricism which sets aside the weight of *available* evidence on the ground that a casual observer might one day see his mouse…while the scientist can never see (but can only detect) his electrons'. He notes that not even van Fraassen can avoid thinking and talking in realist terms. van Fraassen writes of *detecting* an electron in a cloud chamber, and he describes how Millikan *measured* the charge of the electron (1980, pp. 75-77). Seeming to invoke entity realism, Musgrave then asks '[c]an one say truly that one has measured some feature of an object without also believing that the object really exists?' Musgrave details what he thinks is van Fraassen's reply to this 'very obvious question' which is, in brief paraphrase, that even a scientist 'totally immersed in the scientific world-picture', someone who thinks that something corresponding to electrons exists in the world and who is totally committed to electron theory and the objectivity of the science, is not committed to the truth of theory: 'it is possible even after total immersion in the world of science…to limit one's epistemic commitment while remaining a functioning member of the scientific community'.

Musgrave is prompted to accuse van Fraassen of 'sleight-of-hand' and of endorsing 'philosophical schizophrenia'. The sleight-of-hand converts a belief in the reality of electrons (as evidenced in belief in the objectivity of electrons and in the belief that the term corresponds to something in nature), into belief in and commitment to 'the theory' of electrons. But, Musgrave demurs, there have been several competing theories about the electron and no scientist believes that they are all true. It is in the nature of credible scientists that they would not believe even the most up-to-date theory of the electron to be definitive, pending further investigation. For scientists, theory is always provisional. For Musgrave, 'this is quite consistent with a pretty firm belief in the reality of electrons' and there is no need to accept van Fraassen's insistence that the ontological implications of electron theory can simply be bracketed away.

The uncharitable reference to philosophical schizophrenia and to 'split-minded' scientists stems from van Fraassen's 'immersion' metaphor that is interpreted by Musgrave as suggesting that scientists should be expected to believe in their objects of interest (herein electrons) while 'immersed' in their work, but 'should become agnostic about everything they cannot observe once they leave their laboratories. (Musgrave 1985, pp. 206-207; Rosen 1994; van Fraassen 1980, pp. 80-83)

Musgrave's commentary seems to misinterpret van Fraassen's use of the language of science in the terms described above. Reference to detecting and measuring electrons remains consistent with his denial of the observability of electrons and with denying the inference from what was observed instrumentally to the existence of electrons.

2.2.2 Churchland on observation, empirical adequacy and ontological commitment

Churchland, who describes himself as a 'realist, of unorthodox persuasion', puts the difference between scientific realism and constructive empiricism this way:

> I assert that global excellence of theory is the ultimate measure of truth and ontology at all levels of cognition, even at the observational level. Van Fraassen asserts that descriptive excellence at the observational level is the only genuine measure of any theory's truth and that one's acceptance of a theory should create no ontological commitments whatever beyond the observational level. (1985, p. 35)

Churchland argues against van Fraassen's assertion that descriptive excellence at the observational level—empirical adequacy— is the only real measure of a theory's truth, saying that this is only one of several epistemic virtues, of equal importance in determining such adequacy. Similarly, he argues against van Fraassen's denial of the possibility of any ontological commitment beyond the unaided observational level: Churchland suggests that theory is 'wholly blind to the idiosyncratic distinction between what is and what is not humanly observable, and so should our own ontological commitments' (1985, p. 35). He is critical of van Fraassen's '*selective* skepticism' that favours observable over unobservable ontologies (see below), and also of van Fraassen's view that the super-empirical virtues are no more than pragmatic and cannot be used in any estimation of theory truth.

Before elaborating on these opinions, in a discussion on observation and ontological commitment, Churchland (1985) pursues an interesting digression to emphasize certain points of agreement with van Fraassen, and to discuss some significant background issues. Churchland says that he shares the convictions that theories should be literally interpreted and that they have truth values. Further, he agrees that the observable/unobservable distinction is entirely distinct from the non-theoretical/theoretical distinction, and that all observation sentences are theory-laden. Speaking more broadly he challenges some common realist assumptions that in mature science, beliefs must be approximately true, and that its terms must refer to real things: 'I very much doubt that the reason of *homo sapiens*, even at its best and even if allowed infinite time, would eventually encompass all and/or only true statements' (1985, p. 36).

This scepticism is grounded in the problem of induction; Churchland sees this as intractable, even for modern theories that appear to be better founded than their predecessors. Even well-founded theories, on the basis of historical induction appear to be doomed to falsification (the pessimistic meta-induction); this appears to be an inevitable and universal journey.

Evolutionary and anthropological cognitive considerations also warrant scepticism. Human reason is 'a hierarchy of heuristics' reflecting evolutionary and sociological adaptations for discovering, understanding, storing and exploring information from many sources. It would be another miracle if these complex processes were free from defect, error and cognitive limitations. It would be even more miraculous if theory acceptance managed somehow to evade this defective infrastructure.

Despite these considerable obstacles, Churchland remains a scientific realist because, as he sees it, the objections fail to discriminate between what he refers to as 'the integrity of observables and the integrity of unobservables' (1985, p. 36). He notes that if anything is compromised by the above considerations it is the integrity of theories in a more general sense and, indeed, of cognition itself. He puts this as follows:

> Since our observational concepts are just as theory-laden as any others, and since the integrity of those concepts is just as contingent on the integrity of the theories that embed them, our observational ontology is rendered *exactly as dubious* as our non-observational ontology. (1985, p. 36)

Human history contains numerous examples of errors in ontological commitment in both non-observational and observational domains. In addition to the theoretical non-observable entities that are well known to philosophers, such as phlogiston and the luminiferous ether, examples of observational error abound. Churchland's examples are witches, and 'the starry sphere', the latter clearly observable and observed, and misinterpreted, by most of humanity on a daily basis, to this day.

Churchland elaborates on the different reasons why processes and entities may be unobserved by us. For example, relative to unaided human sensory capabilities, things may be unobserved because they are not in appropriate spatial or temporal positions—they may be too remote in space or time. Or, they may not have appropriate spatial or temporal dimensions—they may be too small, too large, too brief, or too protracted. Their energy output may be inappropriate—too feeble or too powerful to permit detection or discrimination, or the wavelength of emissions may be beyond the limits of human detection. Objects will be undetectable if they lack an appropriate mass, or if they do not interact with our sense organs in any way: the neutrino flux is an example, despite the fact that its energy density exceeds that of light. Churchland's posit is that there are multiple ways in which an entity or process can fall beyond the compass of human observation, and that this has no relevant ontological or epistemological import whatsoever. (1985, pp. 38-39)

Churchland recognizes van Fraassen's particular focus on things that are unobservable in principle—things that can never be observed by humans at all, but he denies the significance that van Fraassen accords this distinction. The logical and epistemological problems of ampliative inference and underdetermined hypotheses are the same for all categories of observables and unobservables. On this view, there is no epistemic difference between an individual's beliefs concerning observables and unobservables. For Churchland, the epistemic status of observables is the same as that of unobservables; each is as dubious as the other. It is simply their causal history (in terms of interactions in sensory pathways) that is different. There is no privileged ontology.

> I…fail to see how van Fraassen can justify tolerating an ampliative inference when it bridges a gap of spatial distance, while refusing to tolerate an ampliative inference when it bridges a gap of, for example, spatial size. Hume's problem [of induction] and van Fraassen's problem collapse into one. (Churchland 1985, p. 40)

Churchland posits that we are misled by a casual use of 'observes' as what he calls a 'success verb'. We tend to conclude that an object clearly visible and before us is real; we tend to accept that the metaphysical status of the entrenched and familiar is obvious, but the reality is that the ontology presupposed even by direct observational judgements is often speculative, revisable and subject to falsification.

What this amounts to is that these sceptical considerations are indifferent to the observable/unobservable distinction and, therefore, they provide no grounds for a commitment to observable ontologies while at the same time disallowing commitment to unobservable ontologies. Churchland acknowledges that empirical success of a theory is one reason for thinking it might be true, while conceding that the sceptical problems outlined above should 'severely temper' the notion of inference from success to truth. His point is that the scepticism of van Fraassen only with regard to unobservable ontologies is dubious because of its selectivity, and entirely unwarranted. Churchland's own scientific realism is, as he says, 'highly circumspect, but the circumspection is uniform for unobservables and observables alike. (1985, pp. 36-37)

However, it should be understood that van Fraassen does accept an in-principle difference between the contents of perception of observable things, which involves those very things as part of the content *versus* the contents of perceptions of unobservable things which do not involve those actual things as part of the content (they are inferred from instrumental data).

Churchland acknowledges that van Fraassen's arguments for scepticism, for denying factual belief and ontological commitment outside of the observable domain, go beyond the 'selective scepticism' noted above. As an aside, Churchland remarks that van Fraassen does not include in his arguments against realism consideration of historical induction or 'evolutionary humility' (two of Churchland's posits for scepticism—but not for anti-realism). Churchland sees van Fraassen's mission being to deflate the standard realist argument that the aims of science demand that there is no alternative but to admit unobservables into its literal ontology. And, as we have seen, van Fraassen also mounts a forceful argument that the super-empirical virtues, even including explanatory power, are no more than pragmatic, and contribute nothing to the measure of theory truth. For him, on Churchland's view, it is only empirical adequacy that can fulfil this function, empirical adequacy being construed by Churchland as 'isomorphism between some observable features of the world and some 'empirical substructure' of one of the theory's models': 'Roughly, a theory is empirically adequate if and only if everything it says about *observable* things is true. Empirical adequacy is thus a necessary condition on a theory's truth' (Churchland 1985, pp. 37, 43). He further asserts that van Fraassen's position depends on the claim that for any theory depending on *unobservables* in its ontology, its truth

> is always radically underdetermined by its empirical adequacy, since a great many logically incompatible theories can all be empirically equivalent. Accordingly, the inference from empirical adequacy to truth now appears presumptuous in the extreme, especially since it has just been disconnected from additional selective criteria such as simplicity and explanatory power, criteria which might have reduced the arbitrariness of the particular inference drawn. (Churchland 1985, pp. 37-38).

Note, that for van Fraassen, this is not the case for theories dependent only on observables, where truth and empirical adequacy coincide; this is the basis for Churchland's accusation of selective scepticism. He counters van Fraassen by challenging the integrity of the very notion of 'empirical adequacy' and of its cognate 'empirically equivalent', the latter being central to the idea of underdetermination of theory by the data where the data are either insufficient for the determination of theory choice, or where more than one theory, law or explanation is consistent with the available evidence (see also *§2.2.4*).

A possible response to Churchland here is that in actual scientific practice an overriding aim of many scientists is accuracy in novel predictions. If this is achieved on the basis of empirical adequacy alone, that is taken as sufficient. Predictive power typically trumps other virtues and usually forces a choice if there are two truly empirically equivalent theories (I think this is rare in practice). I concede, however, that the aims of philosophers of science are not necessarily concordant with those of the practitioners of science.

In defending his position, van Fraassen adds that as to what is observable, this too is subject to limits, the most obvious of which is that we can only experience what has actually happened to us so far. Any observable structures are confined to the absolute past light cone of space-time events corresponding to a particular observer. Such structures are finite and very limited in a cosmological context. It is obvious that theoretical claims based on observations 'tell stories that go way beyond the limits of experience', even more so if modality is included. For van Fraassen, such considerations point to the distinction between truth and empirical adequacy.

There are further limits relevant to the human epistemic community, because of the limitations of our sensory apparatus. Those limitations are a matter of empirical discovery. van Fraassen claims that all his critics agree on '…the vagueness of observability and the irrelevance of exactly where the line is drawn. An electron is so unimaginably different from a little piece of stone…that minor adjustments would make no difference to the issues' (1985, p. 254). Expressing mild surprise van Fraassen notes: 'Yet these special limitations provide the focus of so much criticism'.

Is this the nub of the entire argument here? Is it just a matter of a misunderstanding of the difference between electrons and rocks? And what 'minor adjustments' does he have in mind? As I have attempted to describe above, the arguments around the observable/unobservable distinction are several and searching; it is clear that few of his critics would countenance such ready dismissal of the significance of the fuzziness of the observable/unobservable boundary, even if there are apparently clear-cut cases at the extremes.

2.2.3 van Fraassen, the microscope, and 'public hallucinations'

In a confusing discussion on detection, observation, and the nature of images, including those produced by microscopes, mirages and reflections, van Fraassen (2010a) refers to rainbows, his prototype of what he describes as 'public hallucinations' (2010a, pp. 101-109). Considering the rainbow, he says that observers soon realise that there is no 'material shining arch standing above the earth', and that despite the fact that we refer to rainbows as things, these are not things at all. On the other hand, he says, when we refer to rainbows 'we are not hallucinating'. This is clearly true; as he says, rainbows can be photographed, they can be reflected by water. However, he continues 'rainbow observations are like hallucinations, in that they are not real things. But they are unlike hallucinations because they are public'. And here is his emphatic, startling conclusion: '*Nature creates public hallucinations*'.

This seems contradictory. Part of the evidence he cites for the unreality of rainbows is that no two people see the same rainbow—although, in principle, they can if they can stand in precisely the same location. The matter becomes more confused when van Fraassen cites 'a difference between two differences', the difference between a reflected image of a tree in a pond, and a rainbow, both public hallucination on his view, but the reflected image being an image, "a 'picture' of something real", while the rainbow is not.

I think the problem here is that van Fraassen is confusing the reality of the images with the reality underlying these phenomena. There is no doubt that these images are real—they can be photographed, reflected and seen by multiple individuals. I suggest a different interpretation of the tree reflection *versus* rainbow phenomena: the fact that both trees and rainbows can be reflected is evidence that rainbows are real too. They are real phenomena, the reflection being a real image of a real thing, the rainbow being the perceptible part of larger and more complex real optical phenomenon, the part which can only be observed at any one time and place as the rainbow arch, because of the physics involved. It is certainly not the case that these phenomena are hallucinations. At best, van Fraassen's interpretation is based on a mistaken use of that term (this seems unlikely), or it might be allowed that the entire phenomenon is much more than the perceptible portion; at worst, the idea is incoherent and this misinterpretation is driven by his severely constrained epistemological view of the nature of instrumental detection.

Of more significance for this writer, van Fraassen also mistakenly includes microscope images in his catalogue of public hallucinations. These images, too, are real and can be seen by multiple observers, photographed in various ways, projected and digitised. They are the result of passing focussed light (or electron beams in the case of electron microscopy) through very thin slices of cellular tissue or non-organic material, treated in various ways to make the constituent parts visible and interpretable. Technically, the objective lens forms a focussed real image of the specimen within the microscope; this image is then further magnified by the eyepiece lens. Whether one sees microscope images as representations, or the microscope as something closer to a window on the actual world, the resulting real images are as close as I can imagine to non-mathematical isomorphic representations of real things. van Fraassen says that 'the microscope *need not* be thought of as a window, but is *most certainly* an engine creating new optical phenomena' (2010b, pp. 108-109). I agree with latter, but perhaps not the former. Microscopes are windows on the 'unobservable', on the micro-world, at least to the limits of the instruments' resolving power, in the sense that they provide, through a series of highly controlled and well understood artefacts, observable information about the tissues under examination; (note that imaging at nanoscale is now commonplace, and the atomic-force microscope can manipulate and form images of individual atoms). I assert this as evidence further supporting the thesis that the observable/unobservable distinction is contingent (on the detection technology), and that van Fraassen is susceptible to the accusation of selective scepticism.

Nonetheless, there are limits to the resolving power of microscopes and there are objects with determinable degrees of smallness that cannot be imaged. We should ask whether 'the image delivered by the microscope is a better image, or a deeper or truer image, rather than simply another image?' (Wilson 1997, p. 255). This is separate to the question of the observable/unobservable distinction. Wilson suggests that

> to admit that what we see in inspecting the [microscopic] details of a cell membrane is an appearance produced by some deeper reality that is not simultaneously visible is not to deny that what we observe there might explain a process of a coarser grain, such as the healing of a wound. (p. 255)

This seems right, but the more interesting focus is on the 'deeper reality'. I contend, in the same vein, that at least in terms of modern microscopy, it might be reasonable to accept that just as we see, unaided, more and more detail of small things when looking more closely, instrumental image-making reveals more and more of the real, at least in the coarser grain, to the resolution-limits of the equipment. Wilson gets this right when she suggests that the 'epistemology of immediate apprehension give[s] way to one of negotiated meaning' (1997, p. 218), that is, the early doubts and scepticism about the reliability of the interpretations of microscope images have been replaced by an interpretive confidence and reproducibility underpinned by deep, multi-disciplinary, theoretical understanding, through technological refinement and the understanding and practice of modern microscopy, developed over the many decades since the publication of Rudolf Virchow's seminal *Cellular Pathology* in 1858.

I return to microscopical observation in an examination of responses to van Fraassen's empiricist structuralism, and his account of theoretical models and representation in chapters 3&4.

2.2.4 Underdetermination

On Ladyman's view, underdetermination of theory by evidential data is the only positive argument for preferring constructive empiricism over realism. But, he suggests, underdetermination equally applies to the acceptance of which theories are empirically adequate, or which theories are true or approximately true. That is, constructive empiricism and scientific realism are equally vulnerable to epistemic scepticism. Hence his claim that advocacy of constructive empiricism is an expression of arbitrary and selective scepticism. (2000, pp. 837-856; 2002, pp. 186-193)

But note that van Fraassen's position is that claims about unobservables are always deniable whereas claims about observables are not: inductive inference from present to future observations is fallible but is empirically testable, unlike abductive inference to unobservables which is not. But surely this also means that we can never know for certain that a theory is empirically adequate?

Churchland contributes further to an examination of underdetermination in a discussion of the nature of theory content. Theories are typically complex, multi-facetted and context-relative. Newly proposed solutions to a particular problem will typically be found unacceptable by scientists unless theoretically plausible according to existing established theories; or as Boyd has put it, citing Kuhn, 'the ontology of the received "paradigm" is crucial in determining the range of acceptable problem solutions (and thus the range of projectable patterns in data)', that is, those patterns the theory predicts (Boyd 1985, p. 7).

Addressing van Fraassen's central claim that in a theory depending on unobservables, its empirical adequacy underdetermines its truth, Churchland questions the 'doubtful integrity' of the notion of empirical adequacy, as follows.

Different scientific communities use incommensurable languages, jargon, technologies, and detecting or transducing instruments. If we attempt to describe a theory's content and meaning in terms of its observation sentences, then we enter a labyrinthine matrix of entailment, probabilities, conjoined background information, artefacts, controls and sampling variables, and supporting theories, laws and data, usually partly incomplete or uncertain. This will generate a potentially infinite variety of possible interpretations, explanations and speculation that might lead to different outcomes or interpretations in different contexts, with varying degrees of certainty and clarity of definition. Such complexities and uncertainties might suggest that true underdetermination is, in practical terms, rare. But, as Churchland suggests, van Fraassen recognizes and avoids this difficulty by arguing rather for a semantic or model-theoretic explication of theory content as the basis for his claim of underdetermination as a fatal problem for realist accounts of science. Churchland finds this unconvincing, accusing van Fraassen of failing to deal adequately with the difficulty of explaining how the equivalence of more than one theory might be determined when "the so-called 'empirical equivalence' of two incompatible theories remains relative to *which* background theories are added to the evaluative context, especially background theories that in some way revise our conception of what humans can observe" (Boyd 1985; Churchland 1985, p. 38).

Churchland intentionally sidesteps the complexities of this issue to introduce 'a much simpler objection'. He points out that the empirical adequacy of *any* theory 'is itself something that is radically underdetermined by *any* evidence conceivably available to us'.

Strict empirical adequacy demands that a theory saves *all* of the phenomena, as previously discussed, including all of the observable phenomena past, future and 'in the most distant corners of the cosmos', but the available data are clearly finite and limited. This is Hume's problem of induction writ large; it is the basis for Churchland's argument for *radical* underdetermination. The point is that even observation-level theories must be underdetermined by the data. There is no difference between theories about observables or unobservables. To put this another way, whereas van Fraassen's view is that justifying inference to entities that are, in principle, unobservable is a problem different from and additional to Hume's, Churchland's view is that there is no such difference. The point is reinforced by Ladyman who argues that van Fraassen 'owes us an account of how we can have any inductive knowledge at all in the face of underdetermination'. Ladyman asks why should we believe a theory to be empirically adequate, rather than believing merely that 'it is empirically adequate until next week, or when we are looking but not otherwise?' van Fraassen cannot escape the radical underdetermination problem, a difficulty further exacerbated by his dismissive attitude to explanatory power which he cannot invoke to solve this problem. (Churchland 1985, pp. 38-39; Ladyman 2002, p. 193)

A possible ambiguity in van Fraassen's development of constructive empiricism is explored by Worrall (1984) who identifies a weaker and a stronger version of the thesis. The weaker position is that theory acceptance involves *at least* belief in its empirical adequacy but remains agnostic on whether further belief that the theory is true is also involved; the stronger position is that theory acceptance demands *exactly* belief that the theory is empirically adequate and nothing more. The latter is inconsistent with realism, the former is not. Worrall points out that van Fraassen 'explicitly allows that coherent sense can be made of observation-transcendent truth' (but is agnostic on the question of associated ontological significance). However, as discussed, his account of theory acceptance requires the 'highly unjustifiable belief' that the theory saves all past, present and future, potentially and actually observed phenomena, without exception. This appears to commit him to the stronger thesis. For Worrall if this is justifiable (he thinks it is not), then why shouldn't a scientist be prevented from 'a little extra belief' that a given theory points at approximate truth? The standard anti-realist response is to invoke the problem of the pessimistic meta-induction.

Worrall goes on to say that although there is historical evidence of high-level theoretical discontinuity in science, there is a persisting strong intuition that scientific development has been 'essentially continuous at the empirical level'. This should allow van Fraassen to make a strong case for constructive empiricism, but, on Worrall's view he cannot because his account of empirical adequacy is imprecise and weak; his 'empirical adequacy is itself inadequate'. Worrall's claim is that if van Fraassen is advocating a 'genuine' rival to realism then he needs to make a more persuasive argument for the greater plausibility of his strong position: 'if he is advocating merely a *weakening* of realism, then he should tell us why, rather than merely assume that, weakening a philosophical position makes it better'. (Worrall 1984, pp. 66-70; 74)

2.2.5 Boyd's defence of realism: underdetermination and the importance of theoretical induction

Boyd elaborates on the above themes, saying that just as scientists make theory-dependent judgements about which patterns in observable phenomena are projectable, so similar judgements are made about patterns in the properties and behaviour of theoretical entities. Scientific experimentation facilitates rational choices between various theoretical proposals which might pass the preliminary tests for probable or approximate truth assessed by plausible inductive inference from theoretical knowledge. Boyd notes that 'the very methodological principles which govern scientific induction about observables are, in practice, parasitic upon "inductive" inferences about unobservables' (1985, pp. 13-15). Indeed, Boyd takes this further and argues strongly for scientific realism on the basis of the theory-dependence of the scientific method and its experimental methodology:

> So theory-dependent are the most basic principles for the assessment of experimental evidence that it must be concluded that these are principles for applying the knowledge which is reflected in currently accepted theories as a guide to the proper methods for the evidential assessment of new theoretical proposals; any other conclusion makes the instrumental success of the scientific method a miracle. (1985, pp. 13-14)

By scientific realism, Boyd means the doctrine that evidence favouring the acceptance of a scientific law or theory is evidence for the approximate truth of the law or theory as 'an account of the causal relations obtaining between the entities quantified over in the law or theory in question'. That is, experimental evidence for a theory describing causal relations between theoretical—unobservable—entities is evidence for the correctness of the observational consequences of that theory, and is evidence that the causal relations in question explain and produce the predicted regularities in the behaviour of the associated observable phenomena. As Boyd also describes, this feature of realism has been attacked by empiricists as subject to the problem of underdetermination because of its dependence on non-observational assertions which, they insist, are always susceptible to alternative, empirically equivalent theories consistent with the same observational consequences as the original theory, but which entail incompatible causal explanations at the theoretical level. Such theories will be equally confirmed or disconfirmed by any possible experimental evidence.

In his 1973 paper, Boyd mounts a refutation of the resulting empiricist argument that it is impossible that we can find experimental evidence supporting any particular account of the causal relations between unobservable entities. The paper is detailed, but the crux of the matter is that no established theories have non-trivial consequences, and all theories depend on auxiliary hypotheses—established laws and generalizations, existing theories, assumptions and background conditions—the complexity of which means that no two theories will ever be equivalent. Underdetermination is only problematic if it is presumed that individual theories and their observational consequences can be considered in isolation and without reference to the complete subset of all associated, auxiliary and collateral hypotheses and their observational consequences. (1973, pp. 1-5)

He further argues that the theory-dependent features of scientific practice and methods are 'absolutely central' to scientific justification and confirmation of theories. He refutes the 'standard empiricist response' that the theory-dependent features of scientific practice—those features that 'depend on the theoretical structure of received theories'—are merely heuristic. For Boyd, theory-dependence is central and fundamental to the understanding and methodologies of science. Inductive inferences at the theoretical level play a crucial epistemic role in arguments for realism.

He says that realism 'provides an epistemologically coherent rebuttal to the empiricist principle that empirically equivalent theories are equally supported or refuted by any body of observations'. He says that the evidence for a theory is not just a matter determined by the accuracy of its empirical predictions. There is the further test of the theory's plausibility in the light of established or received associated theories *versus* the plausibility of potential rival theories. Background theories provide a basis for judgements of plausibility because they themselves have been previously tested by experiment. Theoretical plausibility gives inductive warrant for the belief that the theory is approximately true.

On Boyd's view, the empiricist conception of experimental evidence (in the context of underdetermination) 'fails to include an account of methodologically crucial inductive inferences at the theoretical level'. He concludes then, when these are taken into consideration 'the doctrine of evidential indistinguishability of empirically equivalent theories is evidently false'. (1985, pp. 15-16)

He stresses the crucial role of collateral and previously accepted theories in guiding experimental design and the testing of subsequent propositions. Notice that van Fraassen does not deny this; he makes it very clear in *The Scientific Image* that he allows background theoretical belief a very important role in confirmation of theory by evidence, including Bayesian, probabilistic evidence. Also allowed is individual variation in how background theories influence confidence, in guiding credence and acceptance. That is, observers faced with a body of evidence can disagree rationally and come to different conclusions about theory choice. However, this doesn't guide taking a realist stance either way. But, whereas van Fraassen accepts the cogency of collateral theories only as indicators of empirical adequacy, Boyd argues for realism.

Reasoning further against van Fraassen's focus on underdetermination, Boyd (1973) insists that what the realist seeks is 'an explanation of the contribution of theoretical induction to the identification of the appropriate experimental tests for proposed theories'. The problem is not just how collateral theories suggest alternatives to proposed theories, or how suitable experiments can be devised to test one against the other. 'The problem is why the alternatives suggested in this way have a privileged epistemic status, why it is against them and not against other logically possible alternatives that a proposed theory must be tested if it is to receive significant evidential support'. The potential possible alternatives, generated inductively, are infinite. Good scientific practice proceeds via informed choices, by reasoned sampling (the problem of sampling was one of the targets of Boyd's 1973 paper) as suggested by induction from an accepted body of theories. 'What the empiricist apparently cannot do is to explain why it is *this* solution to the problem of sampling which is instrumentally reliable'. (1973; 1985, p. 21)

Perhaps a response to Boyd's criticism here is suggested by his reference to informed choices. In practical terms, the theories of interest are those enjoying significant peer support at any one time, those that are in fact being proposed and defended currently. This does not mean that they are epistemically privileged, except contingently.

Boyd concludes that van Fraassen fails to provide an anti-realist answer to his 'basic question':

> Suppose you always "guess" where theories are most likely to go wrong experimentally by asking where they are most likely to be false as accounts of causal relations, given the assumption that currently accepted laws represent probable causal knowledge. And suppose your guessing procedure works—that theories really are most likely to go wrong—to yield false experimental predictions—just where a realist would expect them to. And suppose that these guesses are so good that they are central to the success of experimental method. What explanation beside scientific realism is possible? (1973, p. 12)

Boyd's contention is that scientific realism is the only reasonable explanation for the reliability of those features of scientific methodology that are important in experimental design and in the assessment of scientific evidence: 'these are the features of scientific methodology relevant to the assessment of the "degree of confirmation" of a proposed theory, given a body of observational evidence'. By 'reliability' he means that 'if scientific realism is true, then the methodological practices of science provide a reliable guide to approximate truth about theoretical matters and, no doubt, only scientific realism could provide a satisfactory explanation for this fact'. He acknowledges that it would be question-begging to suggest that this alone is a good enough reason to accept realism—only realists believe such a position. His arguments against anti-realists, including van Fraassen, are that they are selective in their scepticism such that they 'define the reliability of the methods of science in such a way that no questions are begged against the position of the typical antirealist' (1985, p. 4).

Boyd (1985, pp. 30-32; 1991) also challenges the empiricist rejection of the legitimacy of inductive inference to the best explanation, in particular when the conclusions of those inferences are about unobservables. He asserts, citing Kuhn, that the methods of science and the justifications that scientists give for their inductive generalizations about *observables* 'are profoundly theory-dependent'. The choice of such generalizations and the assessment of the associated evidence, for and against, depend upon theoretical entities and inferences which, in turn, depend on abductive inferences that empiricists reject. It follows that the

> inductive justification for theory-dependent inductions about observables cannot be invoked by the empiricist, because the generalization whose justifiability we are discussing is a *premise* for that inductive justification. Therefore, the *consistent*

empiricist cannot even justifiably conclude that the methods of science have been instrumentally reliable in the past, much less that they will be reliable in the future. I conclude that the consistent empiricist can justify neither the methods and empirical findings of science nor the methods and findings of empiricist philosophy of science. (1985, pp. 31-32)

2.2.6 Ladyman and Ross, and Ellis on underdetermination

Ladyman and Ross (2010) bring some clarity to the underdetermination problem by distinguishing between two generic forms, weak and strong underdetermination. Their arguments are largely consonant with Boyd's. Weak underdetermination is the kind faced by scientists on a daily basis whereby two rival theories, say T and T#, can be described that are consistent with all evidence available to date; they agree with respect to all of the relevant phenomena so far observed. They can be said to be weakly empirically equivalent. A consequence is that if all of the available evidence for T is consistent with the alternative, T#, then there is no reason to believe T to be true but not T#.

Scientists deal with such a problem by trying to find some phenomenon about which the theories will give different predictions, subject to experimental investigation designed to elucidate the differences. As Ladyman and Ross point out, the weak underdetermination argument is no more than a version of the problem of induction. If scientific anti-realism is to use underdetermination to bolster its epistemic claims for difference then the argument needs to amount to more than just one depending on fallibilism about induction, particularly in the context of the observable/unobservable distinction.

Strong underdetermination arguments for scientific theories assert that for every theory, potentially there exist an infinite number of strongly empirically equivalent but incompatible rival theories, taking into account not just what has been so far observed, but all possible future observations. Further, such arguments typically say that if two theories are strongly empirically equivalent, then they are evidentially equivalent. It follows that no evidence can ever support one particular theory more than any strongly equivalent alternatives. Therefore theory-choice is radically underdetermined and scientific realism cannot be supported.

Ladyman and Ross suggest ways of arguing that strong empirical equivalence is 'incoherent, or at least ill-defined' as follows:

> (a) The idea of empirical equivalence requires it to be possible to circumscribe clearly the observable consequences of a theory. However, there is no non-arbitrary distinction between the observable and unobservable.
> (b) The observable/unobservable distinction changes over time and so what the empirical consequences of a theory are is relative to a particular point in time.
> (c) Theories only have empirical consequences relative to auxiliary assumptions and background conditions. So the idea of the empirical consequences of the theory itself is incoherent. (2010, pp. 80-81)

Further, these authors suggest that there is no reason to think that there will always be strongly equivalent rivals to any given theory, either because strong empirical equivalence is rare, or because such alternative theories are not genuine theories. They also argue that even if two theories are strongly empirically equivalent, they are unlikely to be evidentially equivalent. Two theories might predict the same phenomena but it is likely that they would have differing degrees of evidential support, and if super-empirical virtues are taken into account then there is a further basis for choice between theories.

Anti-realists might agree that such non-empirical features break underdetermination, but argue, as does van Fraassen, that these are no more than heuristic––they facilitate choice but give no warrant for a claim that the theory is true. Note, too, that van Fraassen would disagree with the claim above that the observable/unobservable distinction is arbitrary. The fact that it is vague does not mean that it has no utility: for example, compare the distinction between tall and short.

Ladyman and Ross suggest that van Fraassen does not appeal to strong underdetermination in his argument for constructive empiricism, contrary to the claims of some of his critics, but 'he uses cases of strong empirical equivalence to show that theories have extra structure [van Fraassen calls them empirical substructures] over and above that which describes observable events, in defence of the claim that belief in empirical adequacy is logically weaker than belief in truth simpliciter' (Ladyman & Ross 2010, pp. 81-82; van Fraassen 1980, pp. 41-69).

These authors' conclusion is that the underdetermination arguments, so expressed, do not unequivocally support either realism or anti-realism. But, they suggest, if there are genuine cases of strong empirical equivalence, that would pose 'a particular problem for the scientific realist'. They cite Jones (1991) who identified the existence of alternative formulations of theories in physics that have evolved over time but that coexist in science and seem to be cases of strong empirical equivalence. This prompted Jones to ask the significant question 'Realism about what?' I return to Jones' question in Chapter 4.

The standard realist option is to break cases of underdetermination by reference to super-empirical virtues. The underdetermination problem cannot be ignored by realists but since it equally 'threatens any positive form of anti-realism such as constructive empiricism, it does not give us compelling grounds to abandon standard scientific realism'. (Ladyman & Ross 2010, pp. 79-83)

Underdetermination also provides an argument in support of structural realism, a subject I discuss in Chapter 3.

Ellis (1985) adds an interesting twist to these considerations in reiterating the important point that theories do not occur in isolation and that we can never know what new or unexpected relevant theoretical developments might occur. He refers to the '*open-ness of the field of evidence*' or the set of all possible empirical discoveries relevant to the truth or falsity of a theory. That is we cannot ever say in advance what evidence might become available which would distinguish between theories that presently appear to be empirically equivalent. It follows that it is never possible to claim that no evidence could ever distinguish between incompatible but empirically equivalent theories. Hence, a scientific realist 'can argue that the underdetermination thesis *cannot be demonstrated*. It cannot be shown, except by fiat, that there is *any* genuine case of empirical underdetermination' (1985, pp. 64-65). (Boyd 1991; Ladyman & Ross 2010)

2.2.7 Epistemic virtues and theory evaluation: Churchland on beliefworthiness and the super-empirical virtues

Churchland also deals with the question of whether the theoretical (super-empirical) virtues are epistemic virtues that might be used in estimations of theory truth or strength, as is traditionally asserted, or whether they are no more than pragmatic virtues, as van Fraassen claims in arguing for the primacy of the test of empirical adequacy.

On van Fraassen's account (1980, pp. 87-96), there are many features beyond empirical adequacy for which a theory is 'praised' or otherwise epistemically assessed including mathematical elegance, simplicity, scope, consistency with the facts, completeness, explanatory power and a capacity for the unification of accounts of previously disparate phenomena. He sees these as 'specifically human concerns, a function of our interests and pleasures which make some theories more valuable or appealing to us than others' (1980, p. 87). Such virtues provide guidance in the choice and use of a theory 'whether or not we think it true'. In other words, these values are no more than pragmatic aids to theory choice, usefulness or acceptance and 'cannot rationally guide our epistemic attitudes and decisions'(1980, p. 87). They are values personal to individual scientists and are coloured by needs, context and social and cultural considerations, and by commitments to relevant research programmes. For van Fraassen, these theoretical virtues are not indicators of truth, they shine no light on the relation between a theory and the world.

Churchland disagrees and advocates the traditional view because he rejects the idea that there is some way in which 'the empirical facts' can be construed, conceived or represented independently of confounding speculative assumptions. He says that when researchers are confronted with theoretical alternatives, they are forced to 'choose between competing modes of conceiving what the empirical facts before us *are*', in which case it is illusory to think that such choice can be made by comparing the degree to which any alternatives conform to some 'common touchstone', that is, the so-called 'empirical facts'.

For Churchland, such choices are better made on super-empirical grounds. At issue, 'saving the appearances', that is, all appearances including observables, is a matter of setting them into the context of a 'larger unity', and that "it is a decision between competing 'larger unities' that determines what we count as 'the true appearances' in the first place". For him, there is no independent way to address such questions, and if, as van Fraassen asserts, these choices can only be made on pragmatic grounds, "then it would seem to follow that any decision concerning what the observable world contains must be essentially 'pragmatic' also!" Churchland sees this as opening the way to 'inflationary metaphysics'. In making these criticisms, he is aiming at one of the central tenets of van Fraassen's argument. Churchland goes on to say

> What all of this illustrates, I think, is the poverty of van Fraassen's crucial distinction between factors that are 'empirical, and therefore truth-relevant', and factors that are 'superempirical and therefore *not* truth-relevant'. (1985, pp. 41-42)

Churchland regards the super-empirical virtues as values that form important and central cognitive criteria enabling the recognition of information, 'for distinguishing information from noise'. This just is the case when dealing with unobservables. On his view, the theoretical virtues are more fundamental than empirical adequacy because they constitute a mechanism for constructing, evaluating and, ultimately, disconfirming and rejecting entire conceptual frameworks for the representation of empirical facts:

> One's observational taxonomy is not 'read off' the world directly; rather, one comes to it piecemeal and by stages, and one settles on that taxonomy which finds the greatest coherence and simplicity in the world and the most and the simplest lawful connections. (1985, p. 42)

Churchland's regard for traditional theoretical 'virtues' is supported by Worrall (1984) who suggests that van Fraassen has ignored the role of a particular criterion of scientific success which he sees as carrying 'the most pro-realist persuasive power', that of *novel* predictive success. Simple predictive power is less persuasive. Scientific theories may have many correct observational consequences; these might simply be built into the theories. The most striking and significant outcome of a theory developed around one set of phenomena is when the theory is found to predict, in an uncontrived way, something completely different and unexpected. The novelty of an unpredicted outcome is a counter to van Fraassen's evolutionary explanation of scientific success; it did not exist in the initial environment. Worrall does not claim that such predictive success entails realism, but suggests that van Fraassen has not convincingly dealt with this important traditional pro-realist component of theory evaluation. Leplin (1997) makes a similar pro-realist argument, defining predictive novelty on the basis of uniqueness and independence—the prediction should not be derivable from any other theory, and the provenance of the theory should be independent of the prediction.

Churchland's project is to advocate what he calls a 'more rational realism'. He finds value in van Fraassen's aim to reconceive the cognitive relation of theory to the world in the context of science, through construction rather than discovery. That is, the advancement of science proceeding via the construction of models accounting for the phenomena and not by the discovery of the truth of theories and unobservables. As Churchland describes, van Fraassen rejects the traditional view that the unit of cognition, the building block of human knowledge, is the sentence—the proposition expressed linguistically—and that the 'cognitive virtue' of observational and theoretical propositions is truth. van Fraassen replaces the set of sentences, the linguistic formulation of theory, with the idea of theory as a set of models, the virtue of which is judged by empirical adequacy and not truth.

While both rejecting van Fraassen's model-theoretic reconception and its selective scepticism, and applauding his attempt to move beyond the traditional, Churchland accuses him of not going far enough because of his constrained focus on "some arbitrarily or idiosyncratically segregated domain of 'unobservables'". If truth as the measure of the aims and products of cognitive activity is to be reconsidered, then Churchland suggests the need for a reconsideration of its pragmatic applicability in the much broader context of all cognitively able organisms, not only conscious humans. In a non-human context, truth and belief would appear to be meaningless notions, and even if this is not so, these would be irrelevant as measures of cognitive virtue when cognition mainly serves survival behaviour, environmental adaptation and reproductive success. Churchland goes as far as to suggest that 'it is highly unlikely that the sentential kinematics embraced by folk psychology and orthodox epistemology represents or captures the basic parameters of cognition and learning even in humans'. His advocacy is for a reconception of the 'dynamics' of cognition in terms of something other than the sentential model and the truth of the propositions of science.

If this could be achieved, a normative consequence would be the elimination of truth as a criterion or marker of the success of science, not because of a reduction of epistemic standards, as Churchland accuses van Fraassen of promoting, but because we, the interested epistemic community, would pursue 'some epistemic goal even *more* worthy than truth'. Churchland is unable to identify what this might involve, but expresses a confidence in the likelihood of future conceptual progress in this direction. What he is certain of is that the idea of truth is suspect on metaphysical grounds because of the unattainability of 'The Complete and Final True Theory' as expressed in the infinite set of all true sentences. Two consequences of this are that there is no best theory, and that truth is unattainable.

> 'If we [are]…unable to speak of *the* set of all true sentences, what sense could we make of truth sentence by sentence?' (Churchland 1985, p. 46)

In the broader context of realism, and the narrower focus of scientific realism, Churchland suggests that a 'constructive' reconception of the underlying cognitive activity or substructure is necessary, in which truth plays no more than a 'highly derivative' role. As he puts it:

> The formulation of such a conception, adequate to all of our epistemic criteria, is the outstanding task of epistemology. I do not think we will find that conception in van Fraassen's model-theoretic version of 'positivistic instrumentalism', nor do I think we will find it quickly. But the empirical brain begs unravelling, and we have plenty of time. (1985, p. 46)

Finally, in this critique of constructive empiricism, Churchland reasserts his commitment to scientific realism, despite his radical scepticism concerning the relevance and applicability of the notion of truth. He does so because regardless of the epistemological and metaphysical status of truth, theories about unobservables have 'just *as much* a claim to truth…as theories about observables', and because he believes

> that there exists a world, independent of our cognition, with which we interact and of which we construct representations: for varying purposes, with varying penetration, and with varying success….Our best and most penetrating grasp of the real is still held to reside in the representations provided by our best theories. Global excellence of theory remains the fundamental measure of rational ontology. And that has always been the central claim of scientific realism. (1985, pp. 46-47)

2.3 Giere's 'modest' alternative: 'constructive realism' and the importance of modality

Acknowledging his 'enormous' debt to van Fraassen, Giere (1985; 1999a, pp. 174-199) declares the task of philosophy of science to be the construction of a 'general *theory of science*' from the point of view that 'realism is right and empiricism wrong'; for him it is a difference that matters. The task, a 'battle', as he sees it, is to develop a 'theoretical background for diverse studies in the history, philosophy, psychology and sociology of science'. The thesis of his 1985 paper is that '[i]n liberating empiricism from its positivist shackles…van Fraassen has unintentionally also set free the realism he abhors. … It is his account of what theories are that liberates both empiricism and realism'. Giere presents a somewhat detailed and interesting response pointing towards a 'modest, constructive realism' as an alternative. I attempt a summary below. (1985, pp. 75-78; 80)

Part of Giere's responses to van Fraassen focusses on the model-theoretic approach of the latter (1980, pp. 41-44; 64-65) that he sees as central to scientific endeavour replacing the logical empiricists' preoccupation with the linguistic structure of scientific theories. Giere agrees with this emphasis. In Giere's words: 'On [van Fraassen's] view, interesting foundational problems in the various sciences are generally not such that they can be removed merely by reformulation in the proper linguistic framework. They reside in the structure of the models employed'. While it is true that philosophy of science must be more than a part of the philosophy of language, *interpretive* language is obviously a critical part of a scientist's armamentarium. Formal structures, for example, as expressed in mathematical models and equations, cannot stand alone; additional semantic categories such as meaning and reference are also important, however difficult and contested such categories may be in application.

Theoretical models are devices used by many scientific disciplines as representations of theoretical entities. They may be generalized models or families of more specific models, the latter obtained and defined by specification of values for various parameters and initial conditions. Giere's example is the harmonic oscillator, where, for example, the oscillator can be modelled mathematically and graphically in explicitly defined ways that represent possible inputs, outputs and states, situations in a state- or phase-space—a logical space. So, the mass in the modelled oscillator, suspended on a spring, exhibits perfect sinusoidal motion. This is a matter of definition. An immediate question that arises is what is the relationship between the model, an abstract ideal, and its instances in the world, real oscillating systems such as springs or pendulums or vibrating molecules? On Giere's view, this is where 'we confront the difference between constructive empiricism and his alternative, constructive realism'. (1985, p. 78)

Giere takes van Fraassen to task for characterising realists as claiming 'to have a model which is a faithful replica, in all detail, of our world' (van Fraassen 1980, pp. 68-69). The context was to compare claims that a theory is true *versus* merely empirically adequate. Clearly, theoretical models of oscillators do not isomorphically replicate bouncing weights under experimental conditions. The behaviour of actual springs is modified by a series of forces and other influences reflecting their relations with the world, including, for example, internal friction and the effects of being on a rotating planet subject to centrifugal and Coriolis effects. Observed motions will differ measurably from those predicted in the model. One response might be to build a more detailed model such that a claim could be made that there will be *some* Newtonian model that exactly captures and predicts the observed behaviour. But since Einstein we have known that this claim is false; even pre-Einstein this claim could not have been justified because all evidence is finite and imprecise; there are limits to the precision and accuracy of detection. It is a matter of principle that such experiments have no chance of detecting the falsity of resultant theoretical claims. No real system is ever completely captured by models. Hence most realists allow for *approximation*, in terms of truth, and model systems. van Fraassen has suggested that 'approximation' can be accounted for in terms of a class of models, one member of which fits, is exactly true (1980, p. 9). Giere rejects this explanation: no Newtonian models exactly conform to any real systems, but they do provide good approximations for real world systems; van Fraassen's analysis seems to deny this. Giere proposes a general form for theoretical hypotheses:

> The designated real system is *similar* to the proposed model in specified *respects* and to specified *degrees*. (1985, p. 80)

Such a characterization of the general form allows variations corresponding to grades of realism and empiricism. So, an extreme realist hypothesis would demand that real systems resemble a model in all respects. This seems untenable, hence Giere's constructive realism enunciated as follows: 'I…recommend a more modest, constructive realism that claims only a similarity for *many* (or perhaps most) aspects of the model' (1985, p. 80). The most obvious difference between this notion and van Fraassen's constructive empiricism is that the empiricist doctrine is much more restrictive, requiring the limitation of claims of approximate similarity to observable features of the world, through the designation of subsections of models as 'empirical substructures' as noted above. Claims of empirical adequacy then depend on picking out a particular empirical substructure that approximately matches or represents the observed facts or phenomena. Giere is somewhat dismissive of van Fraassen's account of such entities, saying he is 'uncharacteristically unclear' and that 'his examples [Newton's claims about absolute space] and theoretical announcements do not always cohere'. Giere's target is van Fraassen's suggested approach that the correct method for determining observability is through the empirical study of human perception such that determining what is observable depends not on scientific theory but on psychology and physiology. Giere contends that apart from the consequent possible vicious circle threatening 'logical catastrophe' ('we must use psychology and physiology to tell us what are the observable substructures of our models of psychological systems'), it is unclear as to how this might work: 'it is very difficult to see how the study of human perceptual capabilities could tell us that velocity is observable while mass is not'. (Giere 1985, pp. 81-82; van Fraassen 1980, pp. 44-47; 56-59; 64)

Giere suggests that the key to defining observability is not human perception unaided, but scientific *detectability*. What is detectable is contingent upon two things, the structure of the applicable models with associated 'interpretations and identifications', and the design and building of appropriate experimental apparatus. The latter depends on other models and, presumably, their associated theoretical substructures. Of course, the outputs of the apparatus must be observable in some way; this is simply a matter of engineering. Presumably evoking Protagorean relativism, Giere says of van Fraassen that

> [h]is empiricism…is a vestige of the classical empiricist philosophy that sought to ground all knowledge in *experience*—and thus make man the *measure*, rather than merely the *measurer* of all things.

Giere goes on to say that such an empiricism may 'deliver us from metaphysics' as van Fraassen declared (1980, p. 69), but 'it delivers us into the hands of an anthropocentrism that is antithetical to the whole modern scientific tradition'. (Giere 1985, pp. 81-82)

A further part of Giere's counter to van Fraassen centres on the question of modality, as I sketch below.

2.3.1 Giere, van Fraassen, and Ladyman & Ross on modality

In *The Scientific Image*, van Fraassen writes on modality in the context of probability, 'the new modality of science' which he describes as 'a possibility-with-degrees, [that] has taken centre stage in physical science'. He suggests that scientific realists deal with modalities 'by reifying certain corresponding "entities" '. (1980, pp. 158-159) I do not intend to discuss probability, but he makes a number of important points about modality.

Many theoretical models are probabilistic and each such model is constructed such that it consists of elements representing alternative possible sequences of event outcomes with certain probability. Only one of these can correspond to any one actual experimental outcome. It is impossible to construct a model of a theory such that every part corresponds to something actual. Or, to put it another way, in modelling the behaviour of a system, such as a harmonic oscillator, modelling, say, the possible trajectories of a bouncing mass on a spring, many points in those trajectories will have no correspondence to the oscillator's actual history, or, even to the actual history of *any* such entity. It follows that models of scientific theories contain substructures that do not correspond to anything observable or to anything actual and there is no logical relation between observability and things in the real world. As van Fraassen reminds us in a response to Giere, we can only detect what is actual, and if we wish to examine the truth of a modal statement, then the best we can do is to investigate related non-modal ones. (1985, p. 291)

Empirical adequacy only claims that 'all that is *both* actual *and* observable finds a place in some model of the theory'. In contrast, the truth of a theory depends on 'an exact correspondence between reality and one of its models' and if a model has substructures corresponding to possible alternative sequences of events, there can only be complete correspondence between the model and things in the real world if those alternatives are also real. van Fraassen makes three assertions:

> *First:* probability is a modality. *Second:* science introduces irreducibly probabilistic theories. *And third:* there is no modality in the scientific descriptions of the world.

'Probability is a modality' is explained as 'it is a kind of graded possibility'. He then goes on to explain that to make sense of such a claim is to accept that modality appears in science through the language used in theory acceptance, that is, through modal language that becomes superimposed upon the models, phenomena and the relationships between them that constitute much of the output of experimental science.

> And the problem of doing justice to modality will have been solved to an empiricist's satisfaction if we can explicate the use and structure of that [modal] language without concluding that anyone who does use it is committed to some sort of metaphysical beliefs such as that alternative possible worlds are real. … To be an empiricist is to withhold belief in anything that goes beyond the actual, observable phenomena, and to recognize no objective modality in nature. (1980, pp. 196-198; 202-203)

That is, while for van Fraassen the modal structures of theoretical representations are important for our understanding of reality, he explicitly denies that modal structures exist as parts of the physical world (Giere 1985, p. 84; 1999a, pp. 174-199). van Fraassen's intention is to sanction 'modality without metaphysics', otherwise that would commit him to allowing that scientific theories tell us about more than the actual observable phenomena. He explains that in being guided by accepted scientific theories, we freely use modal expressions in our language, such as 'it is impossible to observe a muon directly'. But, more richly, modal expressions also reflect the fact that in models of theories are embodied structures corresponding to an infinite number of possible alternative courses or sequences of events, not all of which (if any) will be actually instantiated—constructive empiricism does not require belief that all aspects of theoretical models have corresponding counterparts in reality: 'The locus of possibility is the model, not a reality behind the phenomena'. (1980, pp. 201-203)

In the context of the distinction between observational and theoretical aspects of science, Giere (1985, pp. 82-85) takes up van Fraassen's special consideration of the status of physical modalities. For Giere this is 'the crucial dividing line between empiricism and realism'. The focus is on van Fraassen's posit that while real systems may mirror the *theoretical* structure of our models, we cannot justifiably assert such correspondence and, in Giere's words: '[r]egarding physical *modality*…he is not merely agnostic but atheistic', that is, possibilities and necessities are figments of our models but are not candidates for reality.

On Giere's view, 'even if we substitute detectability for observability, there remains a vast difference between empiricism and a realism that extends to physical modality'. Within a modal realist framework, Giere presents 'a number of possible claims, of increasing strength' describing relationships between an idealized model and a real system, again using the example of the harmonic oscillator to define six different positions, as follows:

1. (Extreme empiricism) The model agrees with the positions and velocities of the real mass which have been observed up to the present time.
2. (Extended empiricism) The model agrees with all of the positions and velocities that ever have been or will be observed.
3. (Actual empiricism) The model agrees with all the actual positions and velocities of the real system, whether they are observed or not.
4. (Modal empiricism) The model agrees with all possible positions and velocities of the real system.
5. (Actual realism) the model agrees with the actual history of all (or most) system variables.
6. (Modal realism) The model agrees with all possible histories of all (or most) system variables. (1985, p. 83)

On this analysis, constructive empiricism becomes a version of actual empiricism, while Giere identifies his constructive realism with either actual realism or modal realism, the latter being his preferred fit. He adds that, either way, he sees his constructive realism as a variety of structural realism (see Chapter 3).

For Giere, '[o]ne's attitude towards modalities has a profound effect on one's whole theory of science'. He explains that actualists (empiricists and realists) must hold that the aim of science is to describe the actual state of affairs in terms of the history of the world. An actual history is just the one possibility realized. In contrast, for modalists the aim is to describe the world in terms of the structure of physical possibility or necessity. On Giere's view '[t]his difference in aim is connected with profound differences in how one understands diverse scientific activities such as causal attribution, explanation and experimental design'.

He again cites, as an example, the 'bouncing spring' as a causal system in which the theoretical model describes various functional relationships, such as the relation between the oscillation frequency and the mass of the suspended weight. He says that if such a functional relationship only obtains between *actual* values of the quantities observed experimentally, then it would be difficult to see what more there is to causality than merely this functional relationship. Empiricists usually ground causal claims in universal generalisations such as, all bouncing springs exhibit such and such relationships. But, he suggests, '[o]n examination, such generalizations turn out to be either false or vacuous'. In contrast, Giere explains, for the modal realist, 'the *causal* structure of the model, and thus, to some degree of approximation, of the real system, is identical with the *modal* structure'. Accordingly, in any real system the functional relationships between the actual values of the associated variables are causal 'not because they hold among the *actual* values in *all* such real systems but because they hold among all the *possible* values in *this* particular system'.

It is not to be expected that questions about explanations and causal accounts are uniquely determined by scientific hypotheses. However it does not follow that such questions cannot generate unique answers in restricted systems for which complete models have been developed, that is where there are fully specified sets of parameters and initial conditions. This allows for us to vary the initial conditions—then the hypothesis embodied in the model gives a new and unique answer. For Giere, from a standpoint of modal realism, 'to *understand* a system is to know how it works. And this means knowing how it would behave under conditions other than those which in fact obtain. It is knowing the causal structure'. Giere suggests that this is close to van Fraassen's view except that the latter would replace 'knowing' with 'having an empirically adequate model with the given modal structure'.

> But this reduces scientific understanding derived from a theoretical model to understanding conferred by a good historical novel, one which remains faithful to the known historical facts. They both provide a good story, which, however, we

have no reason to believe is true. I am not sure what motivates van Fraassen to advocate such a degree of epistemic caution, but I am fairly confident that it is not justified. (Giere 1985, pp. 84-85)

I note that van Fraassen (above) stressed that 'doing justice' to modality is possible without commitment to objective modality in nature. He assumes that a proposition like *p is possible* is true iff there is a possible world in which *p*. His 'possible worlds' are to be taken as features of our models, not things in nature. This is important in the understanding of his position: modality is an artefact of scientific representations. So, if Giere denies this, he could be interpreted as committing realists to a reality of possible worlds, of causally isolated alternative universes, a position that some would find incredible. Note too, that it is possible for van Fraassen to accept Giere's position that a model's causal structure is identical with its modal structure because, on van Fraassen's view, consistent with Hume's idea, causation is similarly an artefact of representation; the idea of necessary connection does not come from experience but from theorising about experience. This, is not what Giere meant, however, in this context. His intention, as indicated above, is to describe real systems and objective modality.

Ladyman and Ross (2010) share with Giere broadly similar views on the importance of modality. As they put it: 'Scientists always look for theories of the observable, not the observed; in other words, theories always involve modalities. This fact is utterly mysterious on van Fraassen's empiricist view'. Scientists almost never develop theories that refer only to what actually happens, their theories are always modalised to allow for a variety of different background assumptions and initial conditions, to allow for counterfactual states of affairs. Empirical adequacy is not achieved by a list of actual and observed or even observable phenomena. These authors also agree that Giere's 'modal empiricism' is a form of structural realism because, according to modal empiricism, the *structure* of scientific theories 'represents the modal structure of reality'. A commitment to objective modality is compatible with the no-miracles argument because if science aims to describe objective modal relations among phenomena in addition to what actually happens, then the success of a theory-laden scientific methodology that builds on existing background theory and theory conjunction is not miraculous. Without modality, there is no connection between phenomena and unobservables other than constant conjunction, 'and that doesn't explain anything'. Further, according to Ladyman and Ross, the success and evolution of science despite historical theory-change can be accounted for because ‘all the well-confirmed modal relations expressed by old theories are approximately recovered in their successors’. (2010, pp. 107-111; 122-123)

2.4 Summary: from constructive empiricism to structuralism

In later writing, van Fraassen moves away from constructive empiricism, as espoused in *The Scientific Image*, towards a structuralist empiricism centred on model-theoretic representation. In foreshadowing this change in stance and addressing the series of critics assembled by Churchland & Hooker in 1985, he addresses ‘epistemic policy’ and how it might require the setting of boundaries around what conditions we could accept in determining how far we might be prepared to go beyond the evidence for our beliefs, suggesting that this could not be achieved in ways independent of our opinions about those boundaries. He expresses ‘disdain’ at claims for theory truth because these require beliefs extra to those required for claims of empirical adequacy, claiming that those extra beliefs establish no further confirmation of the theory: ‘as far as the enterprise of science is concerned, belief in the truth of its theories is supererogatory’, that is, otiose. In other words, ‘evidence for the truth of a theory [is] only via evidential support for its empirical adequacy’. He accuses those who hold that the extra step to truth leads to ‘a richer, fuller picture of the world’ as indulging in ‘empty strutting and posturing’. This attitude goes to his rejection of the value of the super-empirical virtues in theory evaluation, a position that many of his critics repudiate.

van Fraassen opines that scientific realists seem ‘baffled by the idea that our opinion about the limits of perception should play a role in arriving at our epistemic attitudes towards science’. Perhaps this is because of rejection of the empiricist enterprise, but, he speculates, that it is partly because of

> a different appreciation of just how unimaginably different is the world we may faintly discern in the models science gives us from the world that we experientially live in (the scientific image from the manifest image, the intentional correlate of the scientific orientation from the phenomenological life-world). (1985, pp. 254-258)

This is well put, but does this point to matters different and separate from his advocacy for the idea that ‘acceptance in science does not require belief in truth’? Might it be more fruitful to explore the relation between these models, what they represent and their associated intentionality. What are these 'intentional correlates' and what structures or entities in the world are they really about?

In a consideration of the nature of the scientific method, in a section dealing with the relational theory of space-time, van Fraassen explains how in exploring the relational structure of events in the world, he came to realise that 'without exceedingly strong empirical postulates, this structure would not coincide with the spatiotemporal order'. The reason, which had been understood by Leibnitz, is that the actual is only a fragment of the possible. And so, in the specific example, not every possible geodesic is the path of an actual particle or ray of light. 'At this point we can call metaphysics to the rescue' including either the existence of space-time 'as an arena in which events take place', or we can give possibility ('connectabilty, as opposed to actual connections'; possible geodesics, as opposed to actual geodesics) the status of fact. The problem then is that '[t]he former course gives up on the relational theory; the latter saves it only by giving up the empiricism which for me was its main motivation'. van Fraassen's solution is to regard space-time as an 'ideal entity', to conceptualise it as a mathematical model that guides all our thinking in spatiotemporal terms. The relation of the model to reality is that the structure of any actual connections, whatever that may be, must be 'embeddable' within the idealised model. The model

> represent[s] the complete general form that all phenomena allowed by the theory fit in a fragmentary way. Phenomenal reality need not be fragmentary in itself, but its chaotic nature vis-à-vis human understanding forces us to treat it, conceive of it, as fragmentary. Once we see that this is what we are doing in science, we can do it in good conscience without requiring a metaphysical justification. We do not need to postulate that there are elements of reality corresponding to all elements of the model. (1985, pp. 275-276)

The scene is set for a consideration of structural realism in the following chapter.

Chapter 3

Towards Structuralism

For the purposes of this chapter, it is my intention to accept for now the major historical lessons for philosophy of science and to abandon further examination of the more traditional realism/anti-realism arguments. I intend to explore the possibility of what might be referred to as a more moderate realism, by concentrating here on structuralism, in particular on structural realism and its anti-realist counterpart, structural empiricism or empiricist structuralism.

Scientific structuralism is a term used to describe a family of approaches to the epistemological and ontological problems of standard accounts of realism and anti-realism, with the emphasis on the *structural* features of scientific theories. Put simply, the structural features of a theory are contrasted with its ontology; structure is understood as the relations between the objects or elements of the representations of theory—the broadly Kantian posit is that our knowledge is limited to the structural features of the world while the physical objects themselves are unknowable. For example, where a standard realist might be committed to an ontology of electrons as real objects, a structural realist is committed to the reality of the relations between the electromagnetic phenomena represented by Maxwell's equations of electrodynamics. The idea is that what science gets right about the world are its structural features, not its putative ontology. (Bokulich & Bokulich 2011)

3.1 Structuralism

Although earlier philosophers had alluded to structuralism, Worrall (1989) is credited with introducing the idea of structural realism into contemporary philosophy of science as a response to the impasse that results from taking seriously the most important arguments for and against scientific realism: the 'no-miracles' argument for the proposition on the one hand, and, on the other, the underdetermination argument plus the argument from scientific revolutions—the pessimistic meta-induction—that points to the history of failure of reference for unobservables, and abandonment of scientific theories across time. Worrall's contention is that although science seems to have failed in terms of defining the furniture, the ontologies, of the world, successful scientific theories do give correct descriptions of the underlying structure of the modeled world, and there is a continuity of theory structures and relations or 'the structure of the relations between things', even when theories undergo radical revision.

The structural realist claims that 'theories represent the relations among, or the structure of, the phenomena' and that 'well-confirmed relations among phenomena must be retained by future theories' (Ladyman 2014; Ladyman & Ross 2010, p. 157). The best known examples of structural continuity are those expressed in terms of continuity of mathematical structure of successive theories, most notably in physics. A realism that is committed metaphysically only to the structure of theories and not to an ontology of entities might survive theory change. A more minimal, non-realist or instrumentalist form of structuralism focussing only on empirical structure has been referred to as minimal or instrumental structuralism, or structural empiricism. (Ladyman 2014) Scientific structuralism is a further label for what has become a doctrine emphasising the structural features of a scientific theory, where the goal of science is understood to be the study of 'relations and connections' rather than its ontology (Bokulich & Bokulich 2011, pp. xi-vxii; Thomson-Jones 2011).

Before proceeding, I note, as an aside, that in the various descriptions and accounts of structuralism in the context of philosophy of science, the emphasis frequently shifts such that it is not always clear to what the author is referring when using terms such as structures or 'theoretical structures' or 'form or structure', or relations, or the relations between structures or the 'structure of the relations between things', or even 'relational structures' and 'relational facts' (Ladyman 2014).

The key concept is that of 'the structure of the relations between things', that is, structure as the set of relations. This was made clear as early as 1928 in a commentary by Newman on Bertrand Russell's *Analysis of Matter* (1927) in which Newman emphasises the importance of structure, or 'systems of relations', in scientific inference. Newman explains that in considering structure and relations in the context of science, it is not necessary to define the single word 'structure' but rather, the meaning of the statement that 'two systems of relations have the same structure'. Briefly, two systems or sets of objects of the same number can be said to have the same relational structure if there is a one-to-one correlation (isomorphism) between the structured sets according to their relations. For example, a set A might be a group of people with R, a binary relation of being acquainted. A map of A can be generated using dots to represent each person and lines joining those pairs of dots representing acquainted persons. The map becomes a second system, B, with the same structure as A on the basis of the 'generating relation', that of being joined by a line. The important feature is that it is not necessary for the objects comprising A and B, nor the relations between them to be qualitatively similar. As Newman says '[i]n fact to discuss the structure of a system A it is only necessary to know the *incidence* of R; its intrinsic qualities are irrelevant'. The only important statements about a structure are those that describe the associated relations, and it is meaningless to refer to the structure of a mere collection of things having no specified relations. Furthermore, Newman shows that any collection of objects can be organised such that they have a given structure provided there is the right number of them; this is the basis of Newman's 'objection' referred to below. (1928, pp. 137-148)

Russell himself is very clear about what he means by 'structure': 'The notion is not applicable to classes, but only to relations or systems of relations' (1927, p. 249). And van Fraassen puts it, succinctly: a structure is 'a set of elements with certain relations between those elements…'. This quotation runs on with the addition '…described precisely in the terms we use for geometry'; the latter refers to the possibility of the representation of phenomena by mathematical structures, a subject to which I will return. (2010a, p. 137)

Worrall's structural realism thesis is an attempt to reconcile the empirical success of science with the history of repeated falsification of well-established theories of mature science over time; to give the argument from scientific revolutions its due while at the same time to find a way that allows the adoption of 'some sort of realist attitude towards presently accepted theories in physics and elsewhere'. He gives credit to Poincaré for enunciating a structural realist position in his *Science and Hypothesis* as early as 1905.

In mounting his essentially epistemic structural realist case, Worrall cites various historical examples of radical theory change including, for example, the history of the fundamental shifts in the science of optics and the theories about the constitution of light. Following earlier work by Descartes, Huygens, Newton, Hooke and others, the 18th C notion that light was a shower of tiny material corpuscles or particles was rejected in the 19th C in favour of Fresnel's theory of light as wave-like motion carried by a pervasive material ether. This was then replaced by Maxwell's electromagnetic theory, and then in the 20th C, by photon theory—the idea that photons are discrete quantum entities exhibiting particle-wave duality. Despite such radical, revolutionary change at the theoretical and metaphysical level there was a steady, cumulative, increasingly systematized body of information at the empirical level—successful empirical content is carried over into the new theory. So, where the particle theory could account for simple reflection, the classical wave theory explains the additional phenomena of refraction, diffraction, interference and polarization effects; electromagnetic theory conceptually links electrical and magnetic fields, and photon theory accounts for the photoelectric effect, lasers and more besides.

A similar picture can be painted in the case of the transition from Newtonian to Einsteinian cosmology. Einstein's theories render Newton's false, their ontologies are different, yet at an instrumental level, Einstein's theories appear to be 'a sort of "extension with modifications" of Newton's…'. For low velocity situations, predictions of the two theories will be strictly different but will be observationally indistinguishable. The errors involved will be more or less negligible.

Worrall's point is that the development of science is manifest empirically as cumulative and progressive, despite sharp, sometimes radical, non-cumulative change at the top theoretical levels; his project is to address the apparent paradox that science works and is highly fruitful in its application, while being based on theories that, according to the pessimistic inductivist historians of science, always, or nearly always turn out to be false.

Accepting historical theory change as a problem for realists, Worrall suggests two very different alternative possibilities. One is to accept the historical lesson and its implications for realism, that theories are best construed as making no claims beyond their empirical consequences, or that if they are construed in realist terms, this cannot be considered to be part of any rational account of science. We are left with some sort of pragmatic or 'constructive' anti-realism; the 'no-miracles' argument is sacrificed. This leaves the pragmatist in the position of being unable or unwilling to account for the undeniable success and fruitfulness of the associated theoretical science that he or she alleges to be insubstantial and as having no more than a 'codificatory' function.

An alternative, for someone who accepts a picture of science as empirically cumulative but theoretically non-cumulative, on Worrall's view, is to posit a species of conjectural realism whereby the observation-transcendent parts of scientific theories are more than just taxonomic, 'they are *attempted* descriptions of the reality hidden behind the phenomena', (note that this is similar to constructive empiricism). However, the conjectural realist accepts that while our best theories capture more empirical results, save more phenomena, than their predecessors, they are probably not even approximately true. It is difficult to see this as very different from standard anti-realism, as Worrall acknowledges, but I suggest that for a practising scientist, to have the ambition of aiming at revealing the underlying reality might be more satisfying than accepting the bleaker pragmatist view of the extraordinarily rich world that transcends mere human sensory perception and observation.

3.2 Structural realism

In attempting to find 'the best of both worlds' Worrall recognises the 'valid intuitions' underlying realist claims, and suggests that rather than accepting alternative explanations of the success of science in terms of partial preservation of the mechanisms of discarded theories, or that we can be realists about the surviving working parts of older theories, or that older theories are limiting cases of newer ones, he suggests that realist intuitions are better captured by *structural* realism. (1989)

When scientific realists say that their best theories are approximately true, their epistemic claim, on Worrall's view, goes beyond the simple empirical level to the claim that they are approximately true 'at the level of "deep structure"' behind the phenomena. The miraculous predictive and empirical success referred to above is not trivial, it is sometimes stunning. Worrall's examples include the discovery of Neptune, and the fact that quantum electrodynamics predicts the value of the magnetic moment of the electron to better than one part in a billion! (2014)

Structural realism respects the realist's intuitions by allowing that the evidence of empirical cumulativity and novel predictive success is evidence that mature theories have 'latched on to reality' albeit in some approximate way, not in terms of the content—the entities or objects in contention—but at the level of form or structure and relations. That is, that scientific theories describe only the form or structure of the unobservable world, and not its nature.

But, latching onto reality is problematic. Worrall turns to the classical ether: Fresnel's wave theory presumed that light comprised periodic disturbances originating in a source and propagated in a pervasive, attenuated, elastic, material medium. The theory had real predictive success—witness the prediction of the famous bright circular diffraction spot of light in the centre of the shadow of an opaque disc illuminated by light emerging from a single aperture. But, it is difficult to argue for the retention of the ether or any component of it in subsequent replacement theories even as an approximation. A particle does not approximate an electromagnetic field. A curving geodesic in spacetime does not approximate an action-at-a-distance gravitational force, and light as a periodic disturbance in an elastic medium obeying the ordinary laws of mechanics is very different from a similar disturbance in a disembodied electromagnetic field. Yet, despite the fact that

Fresnel mis-identified the nature of light, there is a sense of continuity between Fresnel and Maxwell and this is explicable because Fresnel can be seen to have attributed to light the right form or *structure*. (Worrall 1989, pp. 107-117)

What this means is that regardless of the precise nature of the phenomenon, the structure predicted, that of a periodic disturbance, an oscillation at right angles to the light's trajectory, constituting a wave, has the same form in both Fresnel's and Maxwell's models. Fresnel was wrong about what it is that oscillates. But in both cases the wave structure obeys formally similar laws and is described by similar mathematical equations. So, if we restrict the analysis of the two models to the level of mathematical representations and not to the level of the phenomena, there is continuity between the two theories. What is carried over in the transition from Fresnel's to Maxwell's theory are not entities but mathematical structures. (Massimi 2011)

To reiterate, while the description of what vibrates in the two theories is entirely at odds (material particles in an elastic ether on one hand, *versus* disembodied electric and magnetic field strengths on the other), the structure of the vibrations in both cases is the same and is subject to the same formal analysis. Fresnel's predictions were successful because his theory accurately identified particular relations between elements intrinsic to and constitutive of the optical phenomena, and not because his proposed theoretical mechanisms were approximations or limiting cases of the equivalent components of Maxwell's newer theory.

It is salutary to be reminded that Poincaré recognised the validity of the notion of continuity of structure clearly in 1905, and he anticipated the pessimistic meta-induction:

> The laity are struck to see how ephemeral scientific theories are. After some years of prosperity, they see them successively abandoned; they see ruins accumulate upon ruins…they conclude that these [theories] are absolutely idle. This is what they call the *bankruptcy of science*.

But, Poincaré goes on to counsel against such 'superficial' scepticism, referring to the aim and role of scientific theories and suggesting (wrongly, according to Worrall) that Fresnel's aim was not to find out whether there actually was an ether formed of atoms that could 'really move in this or that sense', Fresnel's 'object was to foresee optical phenomena'. Poincaré continues:

> Fresnel's theory always permits of this, today as well as before Maxwell. The differential equations are always true…these equations express relations, and if the equations remain true it is because these relations preserve their reality. They teach us, now as then, that there is such and such a relation between some thing and some other thing; only this something formerly we called *motion*; we now call it *electric current*. But these appellations were only images substituted for the real objects which nature will eternally hide from us. The true relations between these real objects are the only reality we can attain to, and the only condition is that the same relations exist between these objects as between the images by which we are forced to replace them. If these relations are known to us, what matter if we deem it convenient to replace one image by another. (1905, Chapter 10)

Of course, this is not the last word on optics and electromagnetism, but the example convincingly illustrates the thesis that theory change in science can include cumulative growth at the structural level despite radical theoretical ontological discontinuity.

As previously noted Fresnel's equations are transferrable to Maxwell's model but this is not always the case; the more common story is that in theory change, the old and new equations are strictly inconsistent. In some examples, the 'correspondence principle' applies—the old equations can be construed as limiting cases of the new—and the structure is retained, but there are more problematic cases where this does not seem to be so; Worrall's example is that there is no sense in which the Newtonian action-at-a-distance gravitational field is a limiting case of, or approximates Einsteinian space-time curvature, or where Newton's proposed mechanisms explaining gravity are 'carried over' into the general theory of relativity. Yet, he says 'Einstein's equations undeniably go over to Newton's in certain limiting special cases. In this sense, there is "approximate continuity" of structure in this case'. The correspondence principle has been cited as evidence for standard scientific realism—but Worrall claims it as evidence supporting structural realism. On the structural realist view, what Newton actually discovered were the relationships between the phenomena of interest, as described or expressed in the associated equations. (1989) Or, as Jones puts it: a theoretical account of a subject in a mature science is typically represented by an explanatory model 'the relationships between whose fundamental explanatory concepts are expressed in terms of an underlying mathematical theory which makes possible quantitatively precise and qualitatively novel predictions'. (1991, p. 190, fn 3)

Schurz notes, in an interesting aside, that the Fresnel/Maxwell example is atypically straightforward and that other cases of theory change are much more problematic, such as the phlogiston to oxygen transition, the replacement of caloric by kinetic theory, and the relations between classical and quantum mechanics. Alternatively, by examining the case of phlogiston and developing a formal 'structural correspondence theorem', he proposes that the idea of the clear separation of structure and content as described by Worrall be modified. He offers the thesis that parts of theory *content* are preserved through theory change—those parts that are explicable by preservation of structural relations. His claim is that in theory change, 'certain theoretical expressions' that account for a superseded theory's empirical success *correspond* to certain theoretical expressions in the new theory and, at the same time, they refer indirectly to the entities denoted by those expressions. (Schurz 2009)

Another example of structural equivalence between theories is from thermodynamics. In the transition from the theories of Carnot to Clausius the ontology changes (Carnot's theory included the concept of caloric), but the second law of thermodynamics is preserved (Ladyman 2014).

Since Worrall's paper, structuralism has developed as a subdiscipline within philosophy of science offering a framework that can accommodate a variety of issues in addition to questions of realism *versus* anti-realism, such as scientific representation—including the role of models and idealisations in physics— and inter-theory, theory-model and theory-data relations. That is, structuralism offers a way of representing theories, models and data by identifying relevant underlying structures and through the capture of associated relations both horizontally between theories and models, and vertically between these and the associated data models. Typically, this is realised within a semantic set-theoretical, or model-theoretical meta-level representational framework. (French 2011)

The question of whether structural realism is a defensible position continues to be debated. Worrall concedes that one of the criticisms is that the doctrine is not strong enough as a credible variant of realism, particularly if the criterion of real-world reference of theoretical terms is insisted on. If we are not all talking about 'the same thing' how can we compare truth-values of our claims? Worrall's response is to acknowledge that structural realism is as fallibilist (that is, 'approximativist') about reference as it is about truth, that standard referential semantics is untenable. What structural realism posits is that the structure of a theory may 'reflect' reality without reference to the objects of that reality. The test of the efficacy of that reflection is the sort of predictive success that motivates the 'no-miracles' argument. For Worrall, structural realism 'is arguably the strongest form of realism compatible with the history of theory-change in science'. (2014, pp. 319-322)

The structural realist position on the standard view leaves open the metaphysical question of the nature of things in the unobservable world—should the nature of things be posited to be unknowable, or simply, eliminated? Worrall prescribes an epistemic constraint on realism coupled with an agnosticism about the content of theories. He posits that structural realism is a modification of standard scientific realism such that we should only believe what theories tell us about the relations between unobservable objects, while suspending judgement about the ontological status of those objects, or, briefly, all that we *know* is structure. Most, but not all defenders of epistemic structural realism assume that there must be individuals (objects with their properties) that are ontologically prior to relational structure.

In discussing different approaches to structural realism, Ladyman (2014) records several authors who have adopted a neo-Kantian thesis. While I do not want to pursue this, it is interesting that he notes, for example, that Poincaré's structuralism has a 'Kantian flavour' in the sense that the unobservables of scientific theories could be construed as similar to noumena, as unknowable things-in-themselves, but in contrast to Kant, Poincaré argued that rather than being inevitably unknowable, the noumenal objects postulated by science could be known indirectly because it is possible to know their relations. Ladyman suggests that Poincaré sought 'the neo-Kantian goal of recovering the objective or intersubjective world…from the subjective world of private sense impressions', Poincaré's objective reality being 'the harmony of mathematical laws'. It could be argued that structural realism is a form of Kantian transcendental idealism, the position being that science can never tell us more than the *structure* of the noumenal world, whereas the intrinsic nature of the objects, entities and properties of it remain epistemically inaccessible to us.

Massimi too (2011) has presented structuralism from a neo-Kantian perspective, arguing that structural realism should not be understood as a form of semantic realism and does not address referential discontinuity across theory change. She argues that the proper function of scientific structuralism is 'to fix the epistemic conditions under which one can make justified assertions about unobservable entities. …what the structural relations expressed in [a] theory's mathematical formalism cash out is truth—not reference'.

Structural realism was taken in a different direction when Ladyman and his colleagues (French & Ladyman 2003, 2011; Ladyman 1998; Ladyman & Ross 2010; Ross, Ladyman & Collier 2010) and others (French & Saatsi 2011, pp. 548-559) proposed construing structural realism alternatively as a metaphysical doctrine, that of ontic structural realism, whereby relational structure is taken to be primitive, that is, structure is all that there is in nature—the doctrine is eliminativist about objects. I am of the view that this is an argument for speculative metaphysics (Dorr 2010). It is beyond the scope of this work and will not be further addressed except for brief reference below in a consideration of the objections to structural realism.

3.3 Objections to structural realism:

3.3.1 Structural realism collapses into standard realism

Psillos (1995) suggests that structural realism collapses into standard realism. He criticises a key tenet of structural realism, the distinction between the form and content of theories that separates our ability to know the *structure* of the world from our ability to know the *nature* of the world. He denies this distinction. He sees science as focused on descriptions of the properties of things, and in mature science, properties are defined by the laws in which they feature. It follows that the nature of individuals consists in their basic properties and the mathematical equations representing the laws these obey. He suggests that the more we discover about the structure of an entity, the more we discover about its nature. For Psillos, structural realism can only be distinguished from standard realism if we accept what for him is the dubious distinction between structure and nature, which for him form parts of a continuum. He says that the alleged dichotomy between structure and nature assumes that the nature of an entity is something more than and separate from its structure.

Psillos concedes that there may be properties beyond those specifiable mathematically; these could include causal properties and others, subject to empirical discovery. But, this does not mean that there is 'excess nature' in the entity that cannot be captured by further investigation and exploration of the set of laws to which the entity is subject. Psillos concludes that nature and structure are continuous and that knowing one entails knowing the other. Structural realism is no different from scientific realism. (1995, pp. 31-32)

The response by Ladyman & Ross is cursory: they assert that standard realism includes the understanding that entities in nature are individuals and that 'the metaphysics of relations makes it clear that standard scientific realism has been saddled with traditional metaphysics' (2010, pp. 156-157). This is a reference to their own speculative doctrine of ontic structural realism about which van Fraassen (2006b), in an earlier commentary, referring to eliminativist ontic, 'radical' structuralism, makes the paradoxical observation that if structure is all there is, if there is no non-structure, then there can be no structure either. He says that radical structuralism must imply that

> *what has looked like the structure* of something with unknown qualitative features *is actually all there is to nature.* But with this the contrast between structure and what is not structure has disappeared. Thus, from the point of view of one who adopts this position, any difference between it and 'ordinary' scientific realism also disappears.

The essence of this criticism (supported by a much more detailed argument than here) is that the difference between abstract mathematical structure and instantiated physical structure cannot be explained only in structural terms. At least part of the problem is the 'old-fashioned' problem of dealing with entities, and families of properties and relations that can be further divided into structural and non-structural ones. So, whatever bears the structure implied by radical ontic structuralism must be denied certain properties, (other than existence). van Fraassen repudiates radical structuralism, saying 'we must take as at best metaphorical any attempt to equate particle talk, of any sort, with descriptions of structure'. And, 'if structure is not just there as mathematical or abstract entity, then it is not true that structure is all there is'. He also makes the observation that the division between structure and content never seems to be discernible in prospect; structure is only recognised as what remains in retrospect, as the part surviving theory change, after a mistake about content. It follows that there are no independent criteria for distinguishing structure and content. Nonetheless, he goes on to argue for a form of anti-realist structuralism as I detail below. (2006b, pp. 290-295)

3.3.2 Structure is lost in theory change

Stanford (2003) argues that it is not necessarily the case that structural commitments survive theory change. He says that it is by no means clear that we can plausibly distinguish 'merely structural' claims from the content of theories or the 'nature' of entities citing the historical failure of past claimants to make such discrimination successfully. He accuses structural realists of indulging in '[a]ppeals to vague intuitions' and suggests that at least some structural claims are mistaken or uncertain, and also subject to failure on historical induction. For example, an early theory about inheritance postulated that somatic phenotypic heterogeneity was the result of differential distribution of the germinal materials within different cells, an erroneous claim styled by Stanford as being one about the 'structure of inheritance and ontogeny'; (the modern understanding is of genotypic identity, with variable expression—a phenomenon that is independent of the structure of the gene). He also argues (citing the mathematization of Galton's 'stirp' theory of inheritance) that 'there is something extremely misleading in saying even that the abstract mathematical relationships posited by past successful theories have described the "structure" of the natural world in ways that are still embraced by current theories'. Proponents of structuralism are repeatedly forced to fall back on the few convincing successful examples of historical mathematical formalism recoverable from later science, such as Avogadro's number and Fresnel's equations. Stanford concludes that the structural defence of scientific realism is just as subject to pessimistic induction over the historical record as any other. (2003, pp. 569-572) And van Fraassen, asks if it is not 'a little embarrassing' to posit that structure is preserved through theory change, and then to be forced to identify that structure by identifying what has been preserved (2006b, p. 303)

Ladyman *et al* accept that Stanford has 'an important point' but they appear to dismiss this as an epistemic problem 'surely not analogous to the one the realist faces with respect to ontological discontinuity'. They insist that it is not the case that successor theories 'lose all or part of the well-confirmed empirical structures of their predecessors [and that]…we know that well-confirmed relations among phenomena must be retained by future theories'.

It is true that some of the structure of theories is lost in change episodes; for example, the structure of absolute space and time in Newtonian physics is not carried over into relativity theory, but much of the empirical content remains. French and Ladyman, consistent with the views of structural empiricists who agree that theories represent the relational structure of the relevant phenomena, contend that in cases such as the one cited (and others), 'the empirical content of a well-confirmed old theory is recovered as a limiting case of the replacement theory' and there are no so-called 'Kuhn-losses' in the sense of more radical or revolutionary loss of the established or well-confirmed empirical structures of predecessor theories. It follows that '[i]f this is all that the structural realist is saying then their position does not go further than structural empiricism'. (French & Ladyman 2011, p. 31; Ladyman 2014, §5; Ladyman & Ross 2010, p. 157)

Bueno (2011) notes that capturing the 'structure of the world', one of the principal assertions of structural realism, is compatible with introducing radically different ontologies, since the same structure can be instantiated in multiple different ways. This is an argument against ontic structural realism, to which he adds the difficulties, on his view, of the elusive nature of structure (especially in its metaphysical form), the difficulty of carrying over structural realism into quantum mechanics and the existence of structural losses in theory change. But he does accept that the key features of scientific structuralism support structural empiricism. His claims are consonant with those of van Fraassen's empiricist structuralism (see below). van Fraassen has also commented on the vagueness of the structural concept, asking what exactly is the difference between matter, form, content and structure, suggesting that 'such distinctions are painfully context-dependent'. He continues: 'Is there really an objective difference in nature, as opposed to merely in our representations of nature?' (2006b, p. 303).

3.3.3 Models and 'shared structures'

Brading, and Brading & Landry (2011; 2006) develop an alternative structuralist thesis in the context of the notion that theories capture the structure of the phenomena of the world. They suggest distinguishing objects, particulars, from 'the *theoretical kinds of objects* of a high level theory [that] are exhibited in the *shared structure* of the models of that theory'. They suggest that what theory 'talks about' is not particular physical objects, but theoretical *kinds*. That is, on a structuralist view, a theory of, say, 'the electron' does not refer to particular electrons, not to *'this'* electron, but rather to electrons as theoretical kinds of objects. The claim is that the way 'theory succeeds in talking about these theoretical kinds is via the shared structure of the models of the theory'; the theoretical kinds of objects are the subjects of the theory 'presented' as generic solutions to particular theoretical 'problems'. The use of 'presented' rather than 'represented' is deliberate, the intention being to distinguish between *theoretical* objects and their physical instantiation or realisation, so facilitating description at theoretical levels without commitment to a particular ontology—'presentation' *versus* 'representation' expresses this difference—representation is a distinct and separate step.

The use of the term 'shared structure' draws on the earlier work of Suppes and others (Da Costa & French 1990; Suppes 1960) and is based on the contention that modification is needed to the standard semantic view of theories that underlies structuralism as presented above, including the need to present theories not just as in a simple relationship with their representations but with the recognition that scientific theorising is based on complex hierarchies of theories and models (including, for example, data and 'mediating' models) that bridge the gap between higher-level theory and its associated lower-level phenomena. The connections at the various levels in this scheme standardly depend on a relation of isomorphism between models at one level and those at the next, isomorphism being a one-to-one mapping that preserves all the relevant structure. However, as argued by Portides, citing the above authors, the theory-experiment relation is better interpreted in terms of partial isomorphism because structural isomorphism 'never obtains in scientific practice' (2014, pp. 436-437).

Brading *et al* suggest that insistence on strict isomorphism is unnecessary and is not universally applicable. The suggested modification is relaxation of the requirement for isomorphic mapping, so allowing that not *all* of the relevant structure need be preserved in mapping between levels in a theory's hierarchy of models, but a weaker notion allowing relevant *shared* structures is sufficient to do the work, with the additional suggestion that other types of morphisms are also permissible. I will return to alternatives to isomorphic representation in Chapter 4.

Allowing the weaker thesis, however, carries with it a risk that because there is human choice involved in the structuring of models, proliferating data models and mediating models may carry with them a proliferation of relational structures, including potentially incompatible structures, such that it may become less certain that higher-level theory accurately represents *the* structure of the world. For example there is some freedom in how a scientist might decide to draw a graph through a set of data points, or in separating a data-point pattern from noise. (Brading 2011, pp. 48-57)

In responding to this idea, I suggest that the ineliminability of the human factor is undeniable and important, and it is crucial that any such proposed epistemological theses in philosophy of science are responsive to actual scientific practice, otherwise they will lack authenticity.

I have attempted here to give an account of the main arguments for and against structural realism as an alternative to standard scientific realism. My conclusion is that as an epistemological doctrine, the thesis has considerable merit although its more general applicability to science is probably limited because of the vagueness of the nature of structure as alluded to by Bueno, and van Fraassen (above), particularly in the special sciences where structural isomorphisms are difficult to define, if they exist at all.

What follows is a return to van Fraassen, and his *empiricist structuralism*, an anti-realist account of structuralism and scientific representation, which foreshadows a consideration of the roles and application of models and representation in science generally; I will extend this to include the special sciences, and biology in particular. A central question in this discussion is how can we link abstract theoretical representations to manifest phenomena?

3.4 van Fraassen's anti-realist empiricist structuralism

van Fraassen (2006b; 2010a, pp. 198, 237-261), argues for a structuralist view of science that, on his view, can only be properly articulated in an empiricist, non-realist setting—he calls his alternative conception *empiricist structuralism*. This is on the basis that there is, indeed, a steady accumulation of knowledge in the sciences and, he claims, this is knowledge of observable phenomena and of the associated structure. However, he objects to such structuralist slogans as 'all that science describes is structure' or 'all that we can know is structure', citing them as 'provocative': 'Doesn't science tell us…what water really is, namely H_2O' and that, for example, when water is cooled it contracts, then expands as it freezes, dissolves sugar, and so on? 'None of this sounds at first blush like a description of the structure of water, but rather of what it is and what it is like'. He concedes that an empiricist might discount the question of what water is, but that is not the point. The issue is the inadequacy of the claims of the structuralist slogan and its failure to distinguish between watery phenomena as experienced in the observable world *versus* the unobservable substructure of water as described by scientific theory, where stories about what things are 'like' are treated quite differently.

Unsurprisingly, his is an epistemic structuralism. The setting for this accounting is his *Scientific Representation: Paradoxes of Perspective,* first published in 2008, where the focus is how we and scientists represent the world and its phenomena via theories, expressed through diverse forms of models and their substructures, a thesis based on the *Bildtheorie*—'picture theory'—of science, a theory that, he says, takes a general form consistent with structural realism. From van Fraassen's empiricist perspective, theories are artefacts, constructions, epistemic aids to understanding and practical application, and 'scientific representations are drawn on so as to apply scientific knowledge in practice'. In presenting a historical context for structuralism, he too turns to Maxwell who drew attention to similarities between the phenomena of the propagation of heat and electricity:

> Maxwell speaks of the envisaged mechanisms as merely analogies [with other forms of material propagation], *partial analogies*, that allow us to get an imaginative grasp on the equations. The equations must on the one hand fit the observed magnetic, electrical, and optical phenomena, and on the other hand allow of some understanding of the theory as a description of a physical process. (van Fraassen 2010a, p. 195)

But van Fraassen cites Maxwell's own caution against thinking that analogy, or similarity, leads to a true description of the reality behind the phenomena:

> We must not conclude from the partial similarity of some of the relations of the phenomena of heat and electricity that there is any real similarity between the causes of these phenomena. The similarity is a similarity between relations, not a similarity between things related. (Maxwell 1881, cited in van Fraassen 2010a, p. 195)

Acknowledging the complexities of modelling the unobservable, van Fraassen also proposes a structural alternative. He says that "imagery may trade on 'higher-order' resemblance: not a sharing of properties, but of relational structure. That the models provided by science may have that sort of less direct relationship to the phenomena becomes a guiding theme for structuralism in the next century". In terms of what might count as criteria for the adequacy of representations, 'scientific models trade for their success on resemblance with respect to structure alone'. (2010a, pp. 195; 198-201).

Empiricist structuralism depends on the representational relation between observable phenomena and theoretical models. Theories represent phenomena via models that share structure with the phenomena, and the phenomena are understood as embeddable in 'beautifully simple' but much larger mathematical models. Embedding means displaying an isomorphism to selected parts of the models. The phenomena are the target of scientific representation; the models are the vehicles through which the representation is realised. On this semantic view of theories, theories describe abstract systems through models that represent real systems, without making any ontological claims about those systems (Horan 1988).

For van Fraassen, theoretical models are mathematizable abstract structures: ‘All abstract structures are mathematical structures, in the contemporary sense of “mathematical”, which is not restricted to the traditional number-oriented forms’. Because mathematical structures are not distinguished beyond relation-preserving isomorphism, ‘to know the structure of a mathematical object is to know all there is to know’. By this he does not mean that knowledge of structure is complete, but rather he implies that there is a limit to what is knowable by science, there is a gap between the scientific and the manifest image.

Scientific theories represent phenomena via mathematical structures and those structures ‘fit into’ larger structures, the theoretical models. Hence, the empiricist structuralist construes the slogan ‘all we know is structure’ as follows:

> Science represents the empirical phenomena as embeddable in certain *abstract structures* (theoretical models).
>
> Those abstract structures are describable only up to structural isomorphism. (2006a; 2010a, pp. 238, 247)

The slogan, then, is qualified. Implicit is that we should not rely *only* on theory to know ‘things’ about nature, about phenomena, and (to return to watery macro-phenomenalism), certainly not for getting around in the observable world. But it is the case, with regard to unobservables, that the meaning of ‘all we know is structure’ is, at best, ‘all we know *through science* is structure’. Empiricist structuralism offers the thesis that all scientific representation is mathematical; it is a view ‘not of what nature is like but of what science is’. Or, to put it another way, ‘what science succeeds in knowing is merely the structure of appearances, which answer only to the conditions of empirical adequacy’ (Bokulich & Bokulich 2011, p. xiii).

But, there is a quandary, a ‘fundamental question’ at the heart of this argument, that van Fraassen recognises:

> *How can an abstract entity, such as a mathematical structure, represent something that is not abstract*, something in nature? (2010a, p. 240)

Or, as van Fraassen himself reconstrues the question, what makes the above assertions ‘true of reality’, what links experimental measurement procedures and empirical data to their abstract mathematical representations, and to the associated phenomena (in part, this goes to the principle, or 'problem', of coordination, or how theoretical terms, expressed or represented by reference to abstract mathematical objects, can be linked to physical objects)? And, in particular, if the representational target is not a mathematical object, 'how can we speak of an embedding or isomorphism or homomorphism or whatever between the target and some mathematical object?' (2010a, pp. 115-139)

van Fraassen addresses this question in *Scientific Representation* via a detailed digression into the history of the development of a structuralist conception of science, and representation, and an account of several paradoxes and puzzles generated as challenges to the central ideas, including Newman's response to Russell, referred to above (Lewis 1984; Putnam 1977; van Fraassen 2010a, pp. 225-235).

van Fraassen's answer to his own question follows Putnam's writing (1977) on the roles of context, use and interpretation in the application of scientific theory, pointing to an important ingredient of successful representation, namely its indexicality or 'self-appropriation by its user' as Ghins puts it (2010, p. 528).

The basis of the model-theoretic approach is that there exists a certain function that maps a model and something in nature one-to-one onto each other. On van Fraassen's view, Putnam's argument proceeds via reasoning leading to the conclusion that 'we can regard the [world] as having a certain structure'…[and that] is an assertion *about us*' (emphasis added). The point is, as Putnam himself realised, that 'the world is not describable independently of our description' (1977, p. 496). van Fraassen concludes that his own quandary can be resolved by focusing on us, the users of theories. As he puts it: Putnam's response

> is Wittgensteinian, in that it focuses on us, on our use of theories and representations, and brings to light the impasses we reach when we abstract obliviously from use to use-independent concepts. …we must emphasize the crucial role of the indexical here. A theory says nothing to us unless we can locate ourselves, in our own language, with respect to its content. (van Fraassen 2010a, pp. 232-235)

Newman suggested to Russell that one possible solution to Newman's problem was to distinguish between important and unimportant relations, a point taken up by van Fraassen in developing his argument for the centrality of the user in this context. Linking the criterion of relevance or importance to one's own experience is a key step, on van

Fraassen's view, 'towards the crucial clue of self-reference', that is, to the role of indexicality. The argument can be summarised as follows:

To use a theory or a model and to base predictions on it we have to locate ourselves with respect to it, just as we do when we use a map to represent a geographical part of the world. This certainly applies if the theory can be represented by an abstract mathematical structure where 'the relevance of the matching consists precisely in the user's relation to that structure'. So, when we use a theory or model to 'find our way around in the world', we refer to this or that phenomenon as the one '*we are presently witnessing*… . We have to locate our situation in the theory's logical space, in a way that is similar to our "We are here" with respect to a map'. To make this point is not to deny that every phenomenon is, in principle, completely representable.

> A New York subway map or Paris Metro map is not incomplete because it comes without a "you are here" tag. It is selective, it neither does nor purports to represent more than certain aspects of the topological structure of the system—but that it does completely. (van Fraassen 2010a, pp. 239-261)

To put the point about indexicality another way, a particular model is related to a phenomenon in a relevant way 'because it was constructed on the basis of results gathered in a certain way, selected by specific criteria of relevance, on certain occasions, in a practical experimental or observational setting, designed for that purpose'. Or, '[r]epresentation is a relation between the abstract structure and the phenomena constituted by the user. Nothing represents anything except in the sense of being used or taken to do that job or play that role for us'. There is nothing in an abstract structure itself that signifies relevance. For example, a graph of an exponential function could represent the growth of a bacterial colony, the accumulation of compound interest in a bank account, or radioactive decay over time. There is nothing intrinsic to the graph or its structural relations that indicates the representational target. Even the use of labels or other signs only pushes this back. On van Fraassen's view, what determines the 'representation relationship' 'must be a relation of *what is in the artefact* [the graph] to factors *neither in the artefact itself nor in what is being represented*. …[t]here is no representation except in the sense that some things are used, made or taken'. This places us and the user, and the role of indexical judgement, 'centre-stage in the analysis of scientific representation'.

The key concepts include use, and interpretation, the latter, in the Peirceian sense that signification—representation—is triadic, it does not depend on a simple dyadic relation between sign and object or between structure and phenomenon, but involves a crucial third component, that of interpretation or, the '*use of something by someone to represent something as thus or so*' (van Fraassen 2010a, p. 258). In the context of science, the meaning of a representation and the link to reality depends on the interpretation that it generates in its users. Accordingly, in the chain theory-model-reality, 'the last link is one that is expressed in indexical judgements…[and] the ability to self-attribute a position with respect to the representation is the *condition of possibility of use* of that representation'. In answering his own question, van Fraassen makes it very clear that he means this in the sense of theory being both empirically adequate to the phenomena and, at the same time to the phenomena as described—as represented. These are the same, and to use a theory, to base predictions on it 'we have to locate ourselves with respect to it. …[T]he relevance of the matching [between model, mathematical structure and phenomenon] consists precisely in the user's relation to that structure'. (Atkins 2013; van Fraassen 2010a, pp. 253-261; 2010b)

The focus on users has clear affinities with the idea of scientific *perspectivism*, as explicated, for example, by Giere (2010) who reminds us that just as art works are typically produced from particular points of view—so scientific observation, instrumental detection and theorising is perspectival in the sense that the products of scientific investigation, including its instrumentation, are artefacts of human creation. Scientists construct their models to 'make claims about specific aspects of the world' and such claims apply only to some aspects of the world (driven, at least in part, by the particular interests of particular scientists). Theories are developed on a case-by-case basis and will never achieve complete realisation. It follows that 'full objectivist realism ("absolute objectivism") remains out of reach, even as an ideal', and it is likely that any ambition to define universal principles, to make general claims about the world, is misplaced:

> the grand principles objectivists cite as universal laws of nature are better understood as defining highly generalized models that characterize a theoretical perspective. Thus, Newton's laws characterize the classical mechanical perspective; ...the principles of natural selection characterize an evolutionary perspective, and so on.

Giere concludes that both science itself and the study of the nature of science, proceed through the development of changing perspectives on the world, and that 'is the best any of us can do'. (2010, pp. 13-15; 59-95) van Fraassen's empiricist structuralism is a considerable departure from his earlier constructive empiricism. Starting from a standard truth- and reference-based semantics, his earlier view was that theoretical terms might or might not be true of things referred to, but the matter was unknowable. That is, regarding unobservables, 'he was a semantic realist but an epistemological agnostic' (Giere 2009, p. 102). In van Fraassen's doctrine of empiricist structuralism he has abandoned standard semantics, replacing it with a usage-based view of scientific representation in which the emphasis is on the pragmatics rather than the syntax or semantics of representation, and on the role of agents, the users, with goals and intentions. Agents also bestow meaning, context, perspective and intentionality––'*about-ness'* in van Fraassen's terms. He says that representation cannot be defined. At best, it is possible to describe some of its major features and 'place it in a context where we know our way around'. (van Fraassen 2010a, pp. 7, 25)

On Giere's view (2009, pp. 107-109), empiricist structuralism 'is closer to scepticism than agnosticism' because structural accounts are less specific than standard realist accounts. This echoes the criticisms of Bueno and Newman. For example, from the point of view of relational structure, we cannot theoretically distinguish vibrations in excited diatomic gas molecules from vibrations in electromagnetic radiation, both being instances of harmonic oscillation 'and that is as far as our theoretical knowledge can go'. For Giere, '[t]his remains a serious (I think fatal) objection for scientific realists who would be structuralists, but is no problem for an empiricist structuralist'.

Giere poses a further question: 'Once we have adopted an agent-based account of representation, why do we ever need to pass from the physical to the purely mathematical? And in particular, why do we need to make this transition just at the point where we might go beyond the realm of the "truly humanly observable"?' Indeed, Giere goes further to suggest that it may never be possible to reduce scientific representations to abstract mathematical objects.

Giere also suggests that the richness of scientists' accumulated experiences, background knowledge, and successful predictive theories in many established disciplines enables them to make reasoned speculative leaps without necessarily interpolating abstract representation. He gives the example of the observation in the 1990s of a previously undiscovered plume of gamma rays of approximately 0.51 MeV near the Milky Way galaxy centre. Based on the previously established and accepted knowledge that 0.51 MeV is the energy produced by the annihilation of an electron-positron pair, astronomers have concluded that a plume of positrons is being emitted from the centre of the galaxy. Giere suggests that our experiences are 'rich enough' to permit us to make such attributions directly in accounting for newly identified phenomenon such as this one.

3.5 Summary

In this chapter, I have presented a mainly epistemic structuralist view of scientific realism and anti-realism, largely discounting the value of metaphysical theses in this context as either irrelevant or too problematic. The structural realism story has merit but seems to have limited applicability beyond small numbers of well worked-over examples. Even in the classical case of optical theory, generalizability does not obtain; it presents, at best, a minimalist view that light has some features that are successfully modeled as wave phenomena via equations carried over between Fresnel's and Maxwell's theories, but, although it might be the case that similar equations remain as limiting cases in quantum optics, the structural features of the latter are very different from those of Maxwell.

I have also presented arguments that structural realism may not be materially different from standard scientific realism, and that it may not be the case that structural commitments survive theory change or the pessimistic meta-induction; if so the main motivations for structuralism are diminished.

van Fraassen's anti-realist, structuralist, model-theoretic thesis is compellingly argued; his emphasis on the central role of indexicality and the user or interpreter in the analysis of scientific representation is consonant with much scientific practice. However, I am unconvinced that this can fully account for the plurality of the modes of representation prevalent in science, as foreshadowed by Giere's perspectivist commentary above.

In the final chapter I will explore representation and the use of models in the special sciences, with a focus on biology and the difficulties of formalising the representation of biological complexity.

Chapter 4

Beyond physics: Modelling biology. Realism about What?

The emphasis in much of the literature on realist and anti-realist scientific structuralism is on the abstract mathematizable structures embodied in theoretical representations. But is it the case that all theoretical models are mathematizable, as van Fraassen suggests? The paradigm applies readily to the theories of fundamental physics but is much less clearly applicable to the more complex, diverse and interconnected structures of the special sciences, particularly biology. The possibility of representing biological complexity is central to this chapter.

In the previous chapter, I narrowed the focus of this work onto structural realism before returning to van Fraassen and considering his alternative empiricist structuralism and his account of scientific representation. My aim in this final chapter is to explore the possibility, or not, of the application of some of the principles so revealed to a rational account of scientific theorising in a field other than physics; my personal interest is in biology and medicine.

Several themes were explored in the forgoing chapters, but I intend here to continue to follow the empiricist structuralism trail and the resulting implications for theorising, representation and modelling in biology. I have already suggested that there may be properties in the world, subject to empirical discovery, that are not mathematically specifiable; that is, it may not be possible to represent all scientific knowledge in formal, symbolic terms, as some have argued in the world of microphysics. I will contend that van Fraassen's demands for theoretical mathematical formalism are not applicable to biology.

In making this claim, I agree with much of van Fraassen's rich account of representation in science and his placing of the user, and the role of indexical judgement, centre-stage in this process. This is critically true in the case of biological sciences where the human factor is central and ineliminable—in some biological disciplines, the practice of the discipline depends almost entirely on subjective human judgement in ways that cannot be mediated via measuring instruments or expressed symbolically and mathematically. I start this chapter with a personal digression illustrating just such a situation, and the complexity of biological investigation.

4.1 A digression on the role of subjective human judgment in biological investigation

I am a diagnostic medical histopathologist (from *histos* (Gk) web or tissue; *pathology*, the study of disease). I am an expert microscopist and I analyse diseased human tissues, the aim being, in a given case, to arrive at a pathological diagnosis that will inform subsequent therapeutic decision making by treating physicians. I begin with an account of this process:

The microscopical study of diseased human tissues (histopathology) starts with a piece of the tissue obtained via biopsy or surgical excision of a diseased part or of a whole organ. Thence, chemical stabilisation or fixation of the tissue is followed by macroscopical inspection and description, then dissection and sampling of diseased and healthy areas. For light microscopical examination this is followed by laboratory processing of those sampled tissues including embedding in a supporting medium (typically wax) before sectioning (slicing) very thinly (typically around 4 microns) and the placing of the tissue sections onto glass slides to allow further manipulation. This entails further processing before staining the tissue using dyes and other compounds that selectively and differentially colour or label different components of the tissues and cells that constitute the organ of interest. The labelling may include histochemical and immunohistochemical techniques that enable sometimes highly specific identification of cell and tissue types, and even molecular and gene-sequence-specific structures in and on cells. Similar principles apply, with different processing technologies, for electron microscopy and other special modalities.

In examining the processed sections the interpreting histopathologist, a medically trained specialist pathologist, using a modern compound- (or electron- or fluorescence-) microscope, assesses the structure and appearances of the magnified tissues across hundreds to thousands of individual, unique microscopical fields per case, mentally comparing the changes apparent against his or her internalised memory and experience of normal and abnormal appearances, making subjective judgements of the degree of conformity with normal structures *versus* the degree of deviation from normal, based on subtle changes in micro-anatomy and in the staining or labelling characteristics of individual sub-cellular organelles, cells and tissues present. At every stage in this process, variables and artefacts are introduced and the assessment requires a mental sorting of the significant changes from the background normal or irrelevant variations. In complex and difficult cases, the process is sometimes iterative and uncertain, involving consultation and discussion with other pathologists and experts.

The final part of this endeavour involves the responsible pathologist coming to a decision about the significance of the abnormalities discovered, and then generating a report to the referring clinician, which includes a diagnosis. The diagnosis is a shorthand description and representation, a concise synthesis of all of the information available, including clinical, morphological, chemical, immunohistochemical and genetic findings. This permits disease classification and prognosis, and is the basis for treatment of the patient. Reporting back to clinicians may involve other forms of communication including meeting with them, sometimes in multi-disciplinary teams, and demonstrating the pathology using various forms of representation including verbal and written description, photographs, video presentations of the microscopy, and other displays. Apart from simple measurement of some parameters, the representational methods used are entirely qualitative, non-symbolic and non-mathematical. The conclusions may reflect varying degrees of uncertainty, classification ambiguity, and descriptive vagueness—diagnoses may be expressed in terms of probabilities rather than certainties.

Underlying and supporting the practical application of the medical discipline of histopathology that my description portrays, beginning in its modern form with, say, the publication of Rudolf Virchow's *Cellular Pathology* of 1858, is more than 160 years of accumulated knowledge based on a myriad complex hierarchies of inter-related theories, models, and mediating models of cell, tissue and organ structure, function, physiology and pathology, developed through generations of clinical and experimental observation and research involving multiple disciplines and laboratory modalities. These include normal and abnormal macroscopical and microscopical anatomy, cell theory, optics, chemistry, immunology and genetics, to name a few, and, over the last 20-30 years, the revolution that is molecular pathology, whereby the older emphasis on morphological abnormality is being supplemented and enhanced, and to some extent replaced, by relatively highly specific molecular labelling techniques permitting ever more accurate disease classification, prognosis and therapeutic guidance.

The success of this complex diagnostic process is a reflection of the centrality of the interpreting pathologist in the analysis of the microscopical changes and variations seen in each case. This example paradigmatically illustrates the role of first-person judgement in the interpretation of theory and the analysis of the associated representations that van Fraassen so eloquently characterises. And it is consonant with Giere's agent-based account of representation and scientific theorising, dependent on the richness of scientists' accumulated experiences, as discussed in the last chapter. My diagnostic pathologist literally is the last link in the chain joining a complex matrix of background theory, diagnostic assessment, modelling and representation to the reality of the pathology and the diseased patient.

The pathologist's story is intended to give a snapshot of one small fragment of the complex world of biological science and to provide a context for what follows.

My intention is to continue to explore the relationship between abstract models and the phenomenal world on the grounds that ambiguity and uncertainty in this connection is likely to disable the aspirations of realist philosophers. In particular, I will develop the beginnings of an argument that the standard ways of applying the principles I have explored in earlier chapters detailing the representation of theories will be inadequate to the special needs of biology which, I will argue, demands representational pluralism. I will do this by proposing three further problems for biological science. I begin with a consideration of the problem of biological complexity, and then move on to discuss the difficulties of ordering a world of natural kinds where there is major uncertainty about its divisions, given the apparent arbitrariness of scientific classification. My final aim is to consider the more general problem stemming from the resulting epistemological and ontological ambiguities and, via a consideration of the alleged disunity of science asserted by some commentators, I conclude by addressing an associated question: what is it that we can be realists about?

4.2 Biological science—Problem 1: Beyond formal mathematization: complexity and the nature of biological representation

Structural realism might be right for physics where mathematics is a core part of the representational language of the discipline, but formal mathematization is less easily applied to the special sciences, such as the biological sciences where subjective and qualitative assessment of phenomena is prevalent.

It is certainly the case that in many fields of biology, applied mathematical techniques have become important tools in, for example, statistics and probability, population analysis and epidemiology, morphometry, molecular genetics, and modelling of complex systems including molecular modelling. In a very general sense I suppose van Fraassen's assertion that all representations are mathematizable is true in principle—I do not deny that inexactly specifiable things can be mathematically modelled—but, all scientific representation requires the cognitive engagement of the investigating scientists, the users and developers of the biological theories in question where complete mathematical modelling is beyond tractability and utility; it's simply not practically useful. Ghins supports van Fraassen's viewpoint, while noting that some scientific disciplines do not use mathematical representation. Giving the example of a living cell represented by a diagram, Ghins says: 'But the accuracy in predictions – which is an aim pursued by most scientific disciplines – can hardly be achieved without resorting to mathematics. … At any rate, any entity is certainly mathematically tractable in some respect' (Ghins 2010, p. 524).

In my view, this is an unhelpful generalisation and takes no account of the genotypic, phenotypic, anatomical, biochemical and physiological complexity in biology and other special sciences. Biology is a collection of overlapping sub-disciplines; the number of involved variables is overwhelming. Biological systems comprise multitudinous components constructed from or comprising proteins, sugars, lipids, metabolites, salts, water and ions, to mention a few, organised into intricate, internally interacting, self-organising, biochemical, subcellular, cellular, tissue, organ and systems networks. These networks incorporate a hierarchy of levels, structures and processes that together form a complex, changeable whole organism interacting with its environment. Biological laboratory investigations are often qualitative, subjective and based on observer opinion that is not amenable to exact description, and involve the assessment of complex, non-linear, stochastic parameters, dealing with multiple variables and cybernetic feedback systems that change continuously over time. Biological systems and theories usually cannot be modeled isomorphically via formal mathematical constructions and are mainly represented non-symbolically.

There is a distinction between formal and informal notions of structure; scientific theories in biology are rarely handled by formal set-theoretic formulation. Biological models are more like mediators between theories and phenomena constructed so as to describe and explain mechanisms and linkages between related constitutive components independently of any mapping relation that may or may not exist; their 'representational capacity' correlates with explanatory power (Portides 2014, pp. 432-433). Portides refers to these as 'phenomenal models', distinguishing them from theory-driven models that may be amenable to more formalised treatments. That is, the representational capacity of such models will not depend on structural isomorphism. (Auyang 1999a, 1999b; Portides 2014; Thomson-Jones 2011)

Anderson pointed out decades ago (1972) that biology cannot be reduced to chemistry, nor can the understanding of 'fundamental laws imply the ability to start from those laws and reconstruct the universe'. Biology is not merely applied chemistry, because biological systems involve increase in 'non-trivial' complexity that carries with it the manifestation or emergence of entirely new properties and phenomena resulting from the organisation of underlying systems—to the 'origin of novelties'—so, organs are dissimilar to their constituent cells; life is an emergent property of cells (Bunge 2014, pp. 1-25). Quantitative changes lead to qualitative differentiation—'more is different'. It's the difference between quarks and jaguars as Gell-Mann (1995) colourfully suggested, contrasting the fundamental elementary particles that are the basis of matter, on the one hand, with the evolved animal standing for the complexity of the world as manifest in complex adaptive systems, on the other. The whole is more than the sum of the parts; the properties and functions of complex organs, like kidneys and hearts, are very different from those of the constituent cells or their organelles or molecular components. Kim (1999) proposes a doctrine of 'emergentism' or 'nonreductive materialism' that describes the emergence of new, complex, higher-level entities and properties from lower-level entities, properties and relations, that carry with them novel causal powers, the emergent properties being neither reducible to, nor completely predictable or explicable on the basis of the original basal constituents or conditions (Ross, Ladyman & Collier 2010). As part of his systemism, Bunge (2014) stresses that emergence should not be admitted as simply properties unexplainable or unpredictable from lower levels; his ontological thesis is that emergence is the occurrence of novel global or systemic properties not possessed by lower level components or precursors.

Biological systems emerge on evolutionary and developmental time-scales; the number of entities involved and their changing morphological and functional properties will never be usefully captured using formal mathematical tools. There are better and more accessible ways for scientists to describe, represent and understand the biosphere, as I describe below.

I recognise the difficulty of accounting for the emergence of novel ontologies, but taking a moderately reductive view of the organisation of simpler structures into multi-system entities, like cells, with new properties, seems readily acceptable, whereas abandoning the biological level of description for a chemical account of biological phenomena would result in the loss of explanatory power particularly in terms of functional and causal descriptions.

In a much later work, Anderson writes of 'complexity science' and the problem of 'dealing with systems which are so large and intricate that they show autonomous behaviour which is not just reducible to the properties of the parts of which they are made' (2011).

Biological models have been described as 'material models' rather than formal models, as non-mathematical representations of complex systems by simpler elements that represent the properties of interest in the target system. At best, idealisations, computer models and simulations can provide some mathematical purchase, facilitating extrapolation from, and augmentation of existing theory where complete analytical mathematical solutions and descriptions are insurmountably complex and computationally unmanageable. Such manoeuvres are always incomplete and imperfect. (Flannery 1997; Kaznessis 2011)

Hertz famously said 'Maxwell's theory is Maxwell's system of equations' (French 2014, p. 308). Ignoring the fact that equations describe idealised, theoretical, abstract models, it is hard to see that say, William Harvey's model of the blood circulatory system, with its diverse interconnected substructures, could be isomorphically mathematized without introducing extra unmanageable complexity. Biological systems are too rich and complex to be capturable in complete formal treatments. Whether the focus is cellular or extracellular—at the level of the subcellular organelle, cell, tissue, organ, whole organism or species—it is almost impossible to examine individual components in isolation. The inter-connectedness is labyrinthine and may involve extended time-scales. Almost any part of a biological system is subject to multidisciplinary, inter-level and and inter-field investigation leading to myriad ways of representation, typically using analogical models, or constructions based on similarity of structure without recourse to mathematical symbolisation, including, for example, linguistic descriptions, diagrams, photographs and other graphical and pictorial modalities that routinely convey all kinds of visuo-spatial, functional and causal structure, capacities, mechanisms and explanations. The enterprise demands substantial, cooperative, inter-field and inter-level integration. Biologists are investigators 'of a proprietary realm of facts that cannot even be expressed in the vocabulary of physics' (Dorr 2010). (Bechtel & Abrahamsen 2005; Craver & Darden 2013)

Fischbein (1987) distinguishes between abstract models such as mathematical relational models, and intuitive models such as diagrams that typically do not directly reflect a given reality or phenomenon but are based on abstract, synoptic interpretations of that reality (while still maintaining partial structural homomorphisms or similarities). Intuitive models aid productive reasoning by acting as a substitute for an original that may be relatively intellectually inaccessible because it is too complex, abstract, uncertain, otherwise unrepresentable or beyond practical manipulation. Such models code the data of the original properties, processes and relationships in 'intuitively acceptable terms', that is, in ways that are 'heuristically efficient', that facilitate human information-processing characteristics. 'The problem is solved in terms of the model and re-interpreted in terms of the original'. Analogical and homological models are prevalent in biological science because they are so effective in aiding interpretation and problem solving by providing 'compact, structured, relatively familiar, internally consistent mental object[s],…viable component[s] of an active try-and-see reasoning process'. (1987, pp. 121-129)

Giere recognises this reality and counsels 'a dose of pluralism in the philosophy of science':

> One would be hard pressed to convince neuroscientists that all they really know about neurotransmitters is their mathematical structure, linked by means of the likewise abstract structure of observable phenomena to measurement outcomes (appearances). Indeed, the same would be true of physicists' claims regarding their knowledge of electron/positron annihilation. (2009, p. 111)

So, I suggest, it is difficult to accept the formal structuralist view of theories that van Fraassen offers: 'Theories represent the phenomena just in case their models, in some sense, "share the same structure" with those phenomena'. It is clear that he means that phenomena are to be understood as 'embeddable in beautifully simple but much larger mathematical models. …that means displaying an isomorphism to selected parts [substructures] of those models'. That is, a phenomenon and a part of the model must have the same structure, 'and this shared structure is obviously not itself a physical, concrete individual'. (2010a, p. 247)

As described by French (2011), citing Cassirer, promoting a structuralist approach to biology, one way in which biology can be understood is as a study of systems in which the relations between the various elements and systems produce a complex whole. Entities in biology are typically studied morphologically, and this is associated with a representational methodology that is quite distinct from the mathematical structures of physics. Biological understanding allows some reductive physico-chemical explanations of some mechanisms and functions but, on French's view, it is clear 'that certain biological phenomena cannot be explained mechanistically; including, for example, the structures of living things as wholes'. It is difficult to identify laws of biology in the traditional sense unless we admit degrees of necessity in the identification of law-like generalisations. French suggests that rather than relying on strict mathematization for identification of commonalities between theories, an appeal might be made to a more general notion of structure from a more informal model-oriented perspective recognising that biological models exhibit much more diversity than their physical counterparts; for example, they include non-quantitative models, mechanical models, computer-generated models, tissue cultures, and model organisms such as yeast, fruit flies, fish, mice, monkeys, and nematodes. What is common between these and mathematical models is 'their own representational function' which varies between disciplines. For example, some model organisms are useful because of anatomical or functional homologies. Analogies facilitate cognitive accessibility in both biology and physics—they enable us to grasp the unfamiliar in terms of the familiar, using non-mathematical language. In contrast, formal, symbolic mathematical treatments are central to the language of physics. And there are examples of 'hybrid' models that combine features of mathematical models and model organisms, including 'synthetic models' using genetically engineered bacteria. Watson & Crick's 'wire and tinplate' scale model of DNA was based on both mathematical and empirical data. (Flannery 1997; French 2011, pp. 164-170; Rowbottom 2009)

French also suggests that rather than a requirement for strict representational model-theory isomorphism, other mapping relations such as homomorphisms or 'partial isomorphism' might be permitted (French 2011, p. 168; 2014; Portides 2014). Brading (2011), and Brading & Landry (2006) too, suggest relaxing the demand for strict isomophism in model-theory hierarchies, in favour of inter-level 'shared structures' where isomorphic mapping between them is a special case, but where weaker relationships are permitted.

Giere (1999a) also offers an alternative, based on his position that 'downplays' the idea that there might be universal laws encoded in true general statements—that is, that science is possible without laws of nature—where he characterises a relevant model in terms of its *similarity* to a system of interest 'in relevant respects and degrees'. Giere suggests that the relationship between an idealised model and some phenomenon in nature is like ‘the relationship between a prototype and things judged sufficiently similar to the prototype to be classified as of that type’. He acknowledges that there are no simple answers to the question of which features might count for judgements of similarity, beyond the claim that the guidelines come from the model. On his view, empirical data lead to various statements, equations, diagrams and so on, features that define the theoretical model. In turn, features of the model, by a relationship of similarity, fit a real-world system in various ways (and *vice versa*). As Giere puts it, ‘the primary representational relationship is not the truth of a statement relative to the facts…but the similarity of a prototype to putative instances’. (1999a, pp. 60-61; 122-123; 1999b)

Ghins also seems to allow the broader notion of ‘*structural similarity*’ such that a ‘necessary’ condition for scientific representation is satisfied by the presence of ‘some relevant’ relation-preserving mapping between the elements of a phenomenon and its representation (2010, pp. 525-526).

My aim in this section has been to explore the possibility of the application of epistemic structural realism in a general sense, and empiricist structuralism more particularly, to the practice of the special sciences, with emphasis on biological sciences. Because biological investigation has to deal with complexity and uncertainty, involving multi-level interconnectedness, and interactions across systems including molecular substructures, through cellular and tissue arrays, to whole organisms and species, extended across time and coupled with the requirement for inter-level and inter-field integration across multiple disciplines describing phenomena using different vocabularies, my claim is that no single standard form of theoretical representation is possible. I suggest that representational pluralism is warranted for both our deep theories, and our models of data and phenomena, beyond van Fraassen’s prescriptive constraints in his detailed and persuasive account of scientific representation.

I now turn to a different problem for scientific realism, again related to complexity, the problem of ontological ambiguity and uncertainty in biological classification. I begin with a diversion that goes to the further consideration of the possibility, or not, of a unified view of science, not at the level of microphysics, but at the level of natural kinds and the living world.

4.3 Biological science–Problem 2: Dupré's promiscuous diversity of natural kinds: A problem for realism?

It seems implicit in much of the literature associated with realism that science cannot progress without a prior metaphysics, and in particular, without powerful ontological assumptions about the world. So, the Aristotelian picture of the natural ordering of the basic substances of the cosmos on concentric spheres, and the Newtonian vision of a universe of massive objects moving in a void were based more on *a priori* assumptions than on empirical inquiry, with similarly rationalised accounts of other metaphysical matters such as the unity or diversity of the natural kinds in the world, and of the nature of causation.

Modern science has, until relatively recently, persisted with notions of a deterministic, law-governed, potentially discoverable and fully intelligible structure that forms or pervades a material universe. However, it is obvious, given the relentlessly revisionist history of scientific discovery and theory change, that these various assumptions must be questioned. The earlier visions of a profoundly ordered universe and the associated possibility of a reductively unified science are untenable. A mechanistic and predictable universe is replaced by one in which chaos confronts orderly prediction, and deterministic hypotheses are replaced by probabilistic ones. Order and unity are replaced by *The Disorder of Things*, as Dupré describes it. (Cartwright 1999; Dupré 1993; French 2011)

Dupré posits two main theses in this book, the subtitle of which is *Metaphysical Foundations of the Disunity of Science*. The first is a 'denial that science constitutes, or ever could come to constitute, a single unified project' or 'some grand synthesis of all natural knowledge'; and the second is 'an assertion of the extreme diversity of the contents of the world' that comprises 'countless kinds of things' each subject to its own characteristic behaviour and interactions. His further posit is that these two ideas are related: the second thesis shows the inevitability of the first.

Dupré's focus is biology—construed as a discipline supported by empirical scientific inquiry—as a paradigm for science. Along with essentialism and determinism, he rejects reductionist accounts of science, that is, the idea that scientific understanding is promoted via unifying subsumption of all science to the microphysical. Further, he rejects the essentialist view that there is an ordered global structure of objects, an ordered hierarchy of natural kinds individuated by their essences or unique intrinsic properties.

This is not to deny that there are objective divisions between distinctive kinds; indeed Dupré goes on to assert that there are many more divisions in nature than are usually reckoned. The difficulty is to identify the kind to which an object belongs. Dupré contends that this is context-dependent, and dependent 'on the goal underlying the intent to classify the object'; there is 'some degree of arbitrariness' in scientific classification—but he explicitly excludes extreme relativism from this thesis and does not admit the proposal that all theories have equal epistemic value.

Classification might be done, for example, on the basis of the function of the object, but this can change, and context can depend on the goals of particular investigations and investigators, and the taxonomic methodology used. It is not just a matter of essential properties unambiguously unique to each kind. For example, he claims that there is no one answer to the question of whether species are kinds, sets, individuals, or what? This uncertainty applies more generally in biological classification and Dupré argues that there is a general problem of biological individuation that relates to the question of how to divide massively integrated and interconnected systems, such as species, into unique discrete components—there is no unique way of cleaving the phylogenetic tree. His suggestion is to advocate a form of pluralist, or 'promiscuous' realism and to recognise that there are 'many equally legitimate ways of dividing the world into kinds'. His posit is that the disunity of science is not just an unfortunate consequence of our limited cognitive or methodological capacities but is a reflection of the 'underlying ontological complexity of the world, the disorder of things'.

A further part of this doctrine, is his advocacy of a parallel epistemological pluralism. He recognises that there are multiple paths to knowledge and suggests that the choice of path should be determined on the basis of the traditional epistemic virtues including empirical accountability, consistency with common sense, elegance and simplicity. Dupré makes it very clear that his is a realist position and that, on his view, a commitment to many overlapping kinds should not 'threaten the reality of any of them'; he contends that 'one's ontological style' is not just dependent on arguments around the particles described by microphysics. He says that the way to put the insights of science into proper perspective 'is to see the kinds of forces they describe as interacting with a real and sometimes recalcitrant world'. (1993, pp. 1-14; 261-264)

French's (2011) response to Dupré's work offers a structuralist 'prophylactic for promiscuous realism'. Having argued that structuralism is applicable to biological systems, French goes on to address issues of identity in biology, specifically via an account of the problem of the 'radical transformation' of the notion of 'gene' over the history of the discipline of genetics.

Briefly, the problem lies in the tension between the gene conceived as a particular DNA nucleotide sequence that encodes a specific protein, *versus* the gene conceived more broadly as an entity having a complex functional role. Modern molecular genetics and data from sequence analysis have identified no single type of 'gene', but have demonstrated split, repeated, nested and overlapping genes, cryptic DNA and multiple promoters generating differently initiated transcriptions. There is also further variation possible via regulatory mechanisms, including non-genetic or epigenetic causal factors, at the level of protein synthesis and function. The extent of such variation confounds the task of defining the gene at both the molecular structural and functional levels.

While this might seem to support Dupré's argument, French turns this around by proposing alternative structural realist approaches including the idea that the gene could be reconceived not as an object as such, but rather as 'a node in an inter-related set of biological structures'. A further alternative might be to style the gene not as a 'master molecule', but 'as a developmental resource for the construction of biological systems'. This idea includes the notion that structures themselves might be 'causally informed' and presages the possibility of a structural understanding of the unit of selection.

In interpreting French's position, it should be remembered that he is a proponent of ontic structural realism, a posit that I have rejected previously as too speculative to be of value in this discussion. I have included his views here because, nonetheless, his account of the problem of the identity of the gene is cogent. My conclusion, ignoring French's ontological assertions, is that his account of the gene does, indeed, support Dupré's position.

An important response to Dupré's defence of a pluralist approach to realism is Wilson's (1996) defence of a more traditional realism and his challenge to Dupré's views about natural kinds. So, for example, Wilson rejects Dupré's assertion that uncertainty about the nature of species supports the reality of promiscuous realism. Wilson suggests Dupré's argument conflates and confuses questions about the status of species as a form of biological generalisation *versus* the status of individual organisms as members of species. The 'species problem' is because there is no single criterion for species membership. Taxonomic criteria are diverse, being morphological, biological (reproductive), and phylogenetic or genealogical. Where Dupré sees pluralism, Wilson argues for more work towards dealing with the resulting complexity and developing an 'integrative approach to the species problem', by recognising the heterogeneity of the species category and finding better ways to integrate the information arising from the different taxonomic methodologies on offer.

Wilson's criticism is that Dupré has not made a case for promiscuous realism, but rather he has given an account of how we can have one way of dividing the biological world into kinds, and another equally legitimate way of dividing it into individuals. Complexity, of itself, is not an argument for pluralism. Wilson suggests that with regard to the species problem, our epistemic situation is comparable with that of 16^{th} C astronomy where much more explanatory empirical and conceptual work lay in the future and endorsement of a pluralistic realism would certainly have been premature. Wilson asserts that the debate over the ontological status of species is irrelevant.

He also asks the question: 'are the biological sciences a discipline?' pointing out that that the different sub-disciplines within biology will use different methods and approaches, depending on the interests of their members. For example, botanists tend towards the use of morphological taxonomic criteria, while reproductive and genealogical criteria are prevalent in zoology. This may lead to differences in classification outcomes simply because of the heterogeneity of the species category and division of labour amongst biologists, that has nothing to do with a pluralism of species concepts. (Dupré 1993; Wilson 1996)

In a response to Wilson, Dupré rejects the criticism, saying that Wilson has not recognised the complexity of the problem and that 'taxonomic pluralism is grounded in fundamental aspects…of evolution'; there is no evidence that the discovery of 'some privileged monistic taxonomy' is probable (Dupré 1996).

On the broader question of the status of natural kinds, one could take an anti-essentialist view and deny the possibility of natural biological kinds at all (as opposed, say, to specific natural chemical kinds, such as elements). For example, some assert that species are no more than generic cluster concepts, clusters of organisms with similarities defined in special ways. (Ellis 2014) But perhaps there is a more fundamental problem at play here. Wilson's 'discipline' question (above) is odd. It is obvious that biological science comprises many, many sub-disciplines—science*s*—ranging, for example, from joint mechanics to molecular genetics.

A more interesting and more challenging question is whether biological explanation is in any way distinctive or, is biology a science at all? (Rosenberg 2014). I am going to do no more here than assert that it is, citing as evidence the current understanding of the quaternary structure of the oxygen-carrying haemoglobin molecule, found in almost all vertebrates. Its extraordinary capacity to bind oxygen avidly in high saturation states but release it in low oxygen environments is explained on the basis of conformational and chemical changes in the molecule, and genetic mutations explain disease states associated with abnormal haemoglobins. Current knowledge reflects careful multi-disciplinary work going back over 150 years, but modern understanding began with painstaking crystallographic analysis in the 1950s with the work of Nobel Laureate Max Perutz (Ferry 2007). The extent and detail of this body of knowledge is one of the triumphs of science. Defining such work as being outside science on the basis that the typical complexity and interconnectedness of biological problems renders them inaccessible to standard analysis by philosophers of science renders that subject relatively uninteresting.

I am neither going to continue to defend such positions, questions or ideas here, nor to solve the species problem. The point of this discussion goes to the question of the possibility, or not, of any plausible realist doctrine in philosophy of science given the undoubted ontological uncertainties described above. It is not at all clear that we can ever know what it is that we can be realists about?

4.4 Biological science–Problem 3: Realism about what? Cartwright's and Jones's accounts of epistemological and ontological ambiguity

In addition to the commentators above, van Fraassen has also referred to a disconnection between the nature of things in the world and the chaotic nature of the human understanding of phenomenal reality, perceived, inevitably, as fragmentary, through the lens of science. Nancy Cartwright (1999) has written about just this fragmentary or patchwork understanding of the phenomenal world in her *The Dappled World*, a description of 'a world rich in different things, with different natures, behaving in different ways'. The reference of the title is to Gerard Manley Hopkins' poem *Pied Beauty* and points to the complexity and wonder of the natural world, and 'All things counter, original, spare, strange'. Cartwright's focus is the way science as we know it is divided into more or less arbitrarily developed disciplines dealing with

> different sets of properties at different levels of abstraction; pockets of great precision; large parcels of qualitative maxims resisting precise formulation; erratic overlaps; here and there, once in a while, corners that line up, but mostly ragged edges; and always the cover of law just loosely attached to the jumbled world of material things. (1999, p. 1)

Cartwright, with Hacking, is an entity realist; they together identify two significant aims for science: representing and intervening (Cartwright 1999, p. 5; Hacking 1983). She says "I take seriously the realists' insistence that where we can use our science to make very precise predictions or to engineer very unnatural outcomes, there must be 'something right' about the claims and practices we employ".

However, 'The disorder of nature is apparent' on Cartwright's view. It is no wonder that aspirations to develop a scientific theory of everything fail. There is no 'universal rule of law'; rather 'laws are plotted and pieced'. The world as revealed by science is untidy and disorganised. Rather than being subject to deep and necessary principles, it is subject to superficial rules hedged about with *ceteris paribus* qualifications. Science 'works in pockets, primarily inside walls…within which the conditions can be arranged *just so*, to fit the well-confirmed and well-established models of the theory'. Physical laws and theories are true only in very restricted domains, although they do much explanatory work.

Consider Newton's paradigmatic second law of motion, represented as F=ma. The exact mechanical relationships between mass and acceleration can only be expressed by introducing the abstract concept of force. The relation of force to the world can only be mediated by more concrete concepts that are very specific in their form, the forms being given by '*interpretative models*' of the theory, such as two point masses separated by a known distance, or a linear harmonic oscillator, or the model of a charge moving in a uniform magnetic field. This ensures that the calculated forces have precise content, but the theory is limited in application to just those situations that can be represented by highly specialised models that attach the theories, or rather, abstractions of the theory, to the world. The consequence is that "we can have concepts with exact deductive relations among them but those concepts will not be ones that readily represent arrangements found in 'full empirical reality'"; that is, our best theories are severely limited in their scope. So, physics, a powerful tool for making predictions and changing the world, actually has limited utility; deductive-nomological accounts are of little or no value in theory formulation and scientific inquiry that is dealing with high degrees of empirical and inductive complexity and uncertainty, hedged about with experimental limitations, approximations, measurement imprecision and multifactorial interactions between involved related entities, each subject to further theoretical underpinning and technical know-how from numbers of different scientific disciplines. The special circumstances that fit models of single theories are hard to find and difficult to construct.

Cartwright defends three theses in *The Dappled World*, that (i) the impressive empirical success of our best theories may argue for the truth of those theories but not for their universality—laws apply only where models fit the world, and that is only in very limited circumstances; (ii) laws, when they apply, hold only *ceteris paribus*; (iii) our most 'wide-ranging' scientific knowledge is not about laws but about the '*natures*' of things that 'tell us what *can* happen, not what will happen'. By 'natures' she is referring to capacities and dispositions; what things '*tend*' to do (1999, pp. 4, 77-78). The step from the possible to the actual is via hypothesis or 'a bet to be hedged' and not because of a necessary regularity.

> The point is that claims to knowledge we can defend by our impressive scientific successes do not argue for a unified world of universal order, but rather for a dappled world of mottled objects.
> …We aim in science to discover the nature of things; we try to find out what capacities they have and in what circumstances and in what way these capacities can be harnessed to produce predictable behaviours. (1999, pp. 1-10; 137-139)

On this view, realists tend towards universal order on the basis that the laws of our best science are true or are approaching truth, and that the laws are few in number, simple and 'all-embracing'. Cartwright argues that while laws, both phenomenological and abstract, are the best candidates for being true, they are 'numerous, diverse, complicated and limited', and that where *ceteris paribus* laws are concerned, they cover no real cases, and associated explanatory hypotheses must be false.

It should be noted that her major focus in *The Dappled World* is physics and 'how the laws of physics lie' (1983), coupled, strangely, with a treatment of contemporary economics (1999, pp. 1-19). I suggest that 'dappledness' becomes even more apparent when we turn the focus to biology.

Motivated, in part, by Cartwright's claims and recognising that the semantic view of theories is prevalent in biological research, Horan (1988) suggests that the use of theoretical models in biology creates a conflict between predictive success and explanatory power that must lead to anti-realism. She argues that 'model building' requires abstracting from—simplifying—the details of a case so as to pick out or isolate the important or essential features of a system forcing choices between predictive success and explanatory power. The tension is between the individual cases comprising a study of some phenomenon, and the need to generate models that are successful predictors and yield useful generalisations. Predictive generalisability is gained at the expense of the detail needed for adequate explanatory efficacy in biological investigation because of the complex diversity of biological phenomena where simplified models are unlikely to capture the relevant causal factors required for generalisable explanations; it is unlikely that similar effects in diverse cases will have identical causes. A cautionary example she cites is attempting to predict and explain human aggression on the basis of behavioural studies in laboratory rats; a rat hypothesis of aggression is unlikely to capture all of the relevant causal factors in humans: 'predictive success of a theoretical model should lead us to suspect the truth, and thus the explanatory power, of its hypotheses' (Horan 1988, p. 270). This is less of a problem in physics: as she says, 'biological things are complex and diverse, while physical things are complex and uniform' allowing physicists to isolate the systems they study; this is almost impossible in biology (1988, p. 273). Horan concludes that the discovery of reliable explanatory generalisations in biology requires more than just theoretical modelling. This must be supplemented with standard inductive methodology, such as methods of comparison (and difference); on this view, anti-realism seems an inevitable outcome.

While accounts of ontological ambiguity and the absence of a unitary framework, if cogent, compromise the entire realist project, including structuralist stances, French (2011) notes that while one could be a 'disunificationist' and accept that achieving a completely unified structuralist representation of the world is improbable to impossible, it is possible to accept that each ragged piece of the façade revealed by science does represent a fragment of the underlying structure. Cartwright contends that dappledness is a reflection of the way the world just is, it is structurally façade-like, rather than that appearance just reflecting cognitive or methodological failure. French counters, suggesting that her conclusion is not inevitable, that the appearance of a dappled world does not mean that the underlying structure of the world is actually that way.

In his paper *Realism About What?*, addressing the ambiguity of scientific theory, Jones (1991) presents an analysis of the discomforts experienced by philosophers and scientists alike when they are forced to consider the practical difficulties that ensue in 'setting out a carefully described set of objects which adequately account for the phenomena with which they are concerned'. Using examples from physics, his intent is both to challenge the traditional realist account of mature scientific endeavour as moving towards an ontologically well defined picture of the world and, at the same time, to challenge anti-realist alternatives.

To recapitulate, classical realists claim that the theories of mature science are approximately true, and more recent theories are closer to the truth than older ones. Older theories are limiting cases of the more recent ones and are encompassed by them. The entities, properties and processes described by scientists literally refer; that is, they correspond to the ontologies of the associated theories.

Acknowledging Kuhn (Kuhn & Hacking 2012) as providing the first historicist argument against the realist image of science as successfully pointing to a fixed ontology, approximate truth and reliable reference, Jones derails the confident standard realist ideal. His method is to trace the evolution of Newtonian physics, mechanics and cosmology. I will omit the detail but his narrative account begins with Newton's three laws, as might be introduced to physics undergraduates, as applied to observable particle behaviour. The particle is introduced as a unit of matter, 'a gritty bit' with negligible size, shape and structure, but with specifiable positions at different times, leading to functional relationships such as velocity and acceleration. The properties of mass, force and gravity are asserted and become part of the account of two-particle interactions, and can be applied to planetary motion. The initial simple model treats the interacting bodies, such as planets and stars as massive objects with point-particle-like centres of gravity. But massive celestial bodies are not like that, and extended bodies and gravitational fields, potentials and gradients are introduced to the story. The mathematical modelling becomes more complex. Further reformulation is then needed to deal with the dynamics of many-particle systems with more complex trajectories that cannot be described just with simple Euclidean coordinates; Lagrangian and Hamiltonian mechanics are inserted. After Einstein, a major reformulation of the laws of planetary motion specifies space as curved in the presence of matter, and gravitational potential fields are absorbed into the structure of space itself; there is no gravitational force. Rather, unaccelerated massive bodies move along geodesics, 'straight lines' in curved space.

Typically, each new development is characterised as a generalisation of the old in response to the need to deal with new classes of problem, not directly or easily accessible using older methods. Standardly, the reformulations are styled as not introducing new physical theories, but rather as more elegant, powerful and advanced formulations better suited to deal with new phenomena, as they are discovered.

Whatever the degree of complexity and elegance, such sketches 'ignore a whole wealth of difficulties' and none provides a comprehensive causal account that, for Jones, could bear the scrutiny demanded as satisfying that sort of explanation. As he says, in dealing with planetary motion, classical physics, cannot provide a 'univocal, canonical account' of the problem. Even if such accounts save the same phenomena, the very different explanatory frameworks result in very different ontological commitments. At the level of the micro-world, quantum mechanics adds further potential for a multiplicity of interpretations. Jones' young physicists 'don't know, in some canonical sense, about what to be a realist'.

'Interpretive multiplicity' refers to the diversity of the world view that might emerge from attempts at applied realism. There are multiple ways that the mathematical formalism of the theories can be interpreted with disparate commitments and causal imputation. And, on Jones's view, there is a further failure of satisfactory connection in explanatory terms between the mathematical structures of the models and real-world laboratory experience and practice. Jones goes on with further examples, in much more detail than is needed here to make the point; the common feature is to highlight the difficulties for the 'ontological prospector' of the application of idealised treatments to complex systems. (1991, pp. 186-194)

Jones ends this paper with a consideration of the implications of the resulting conceptual enigmas to which his account alludes, for the realism/anti-realism debate. He also suggests that anti-realists (including van Fraassen) should take no comfort from his conclusions citing, for example, the problems for the underdetermination argument consequent upon the prodigious complexities revealed by modern science. The field is now so rich that finding theories that show significant and material empirical equivalence for a given set of data is highly improbable. I have considered this issue previously.

In the end he sidesteps his own starting question and, enigmatically and bleakly, seems to agree with van Fraassen that realists and anti-realists should drop or 'bracket' their mutual concern for epistemic and ontic commitments and, admitting bafflement with the complexity, the 'unimaginable otherness' of it all, together 'contribute to the understanding of these conceptual enigmas' (van Fraassen 1985, p. 258).

Conclusion

This work has traced a thread from what might be called a standard account of scientific realism/anti-realism (Chapter 1), through van Fraassen's influential alternative anti-realist accounts of his *constructive empiricism*, and later *empiricist structuralism*, expressed in his writings that have stimulated vigorous and extended reactions over many years (Chapter 2). Via an examination of *structural realism*, and van Fraassen's alternative *empiricist structuralism* (Chapter 3), the thread has led me away from the focus on microphysics, so prevalent in much of the writing in this debate, to a consideration in Chapter 4 of the problem of complexity in the special sciences, a response from the point of view of biology in particular, where I assert that the complexity of this discipline is incompatible with the idea that biological representation is *usefully* mathematizable up to isomorphic description, one of the central tenets of van Fraassen's structuralist thesis. I have argued that understanding scientific models only as mathematical structures is too restrictive and is inappropriate for understanding the diverse phenomenal models prevalent in biology. And I have discussed alternative, less constrained ways of matching representation to the world.

I separately considered the difficulties of dealing with the 'disorder of nature' including the problem of definition of natural kinds, and the associated implications for realism and anti-realism, ending with the question 'realism about what?'

I have developed these ideas in the context of science and philosophy of science, consonant with my own career-long (and now essentially life-long) focus on, and expertise in human biology and pathology. I conclude that there is a great deal of scope for further work in the natural sciences in general, and in biology in particular, that has the potential to elucidate some continuing and lingering philosophical conundrums, such as the status of natural kinds, whether emergentist doctrines can be sustained, and whether complexity is really a problem or just a matter of methodology and understanding.

But what of the more general question of the status of scientific realism?

There is no doubt that scientists and many scientific realists agree that the world described by scientists is the real world and is approximately the way its established theories assert. However, taking the lead from realist writers like Paul Churchland (1985), Giere (1999a) and Godfrey-Smith (2003), and anti-realist van Fraassen, I conclude that it is a mistake to justify scientific realism by apparently successful arguments that depend on assertions of reference to a putative fixed ontology and the truth of our current theories. If we do, then subsequent falsification of those theories renders scientific realism false too—as Popper realised, failure teaches us that a theory is definitely wrong whereas success tells us no more than a theory is possibly right. Godfrey-Smith asks whether we should worry about the possibility that our best theories might turn out to be wrong. Some, such as Devitt (1991), think that as long as we avoid new and speculative theories this doesn't matter, but many others are persuaded by the historical evidence that subsequent falsification is almost inevitable, although there are writers who have re-examined the inductive record in modern times and who have found evidence for a more optimistic meta-induction.

With regard to the persuasive work of van Fraassen, I have found myself resisting all along his denial that any assertion of a realist ontology of unobservables is justifiable, and I have presented arguments against his particular insistence that the observable/unobservable distinction is immutable, including a suggestion, based on my personal professional experience, that modern instrumental imaging of the micro-world gives us ever more access to the real, at least in terms of its coarser grain.

However, given the difficulties involved and the historical record of science, I accept that complete realist certainty is unobtainable and that van Fraassen's epistemic agnosticism is unavoidable. But, I also suggest that an aspiration to aim beyond mere empirical adequacy sufficient only to save the phenomena—van Fraassen's prescription of the limits of possible knowledge in this context—is justified, not least by the extraordinary achievements of biological scientists in modifying, manipulating, interacting with, and even engineering biological entities such as micro-organisms, fish, plants, genes and other molecules. A realist will contend that the success of these highly integrated enterprises seems to take us ever closer to the real entities themselves. Perhaps the exigencies of dealing with complexity have made biologists better builders of the bridges between the various levels of the macro-world and the micro-world, and we might be justified in asserting a moderate realist manifesto.

But, perhaps a better question to ask is what is the right level of confidence to have in contemporary science and how might that be warranted?

Clearly there is scope for a variety of different attitudes or stances to theory acceptance, as I described in Chapter 2. A standard starting point is 'common-sense' realism, but this is not likely to lead to useful conclusions about the enterprise as a whole because scientific investigation is full of surprises that go well beyond anything that might be regarded as common sense, whatever that is. We must allow for the possibility that science could conflict with common sense; it does so, regularly, and good scientists and philosophers profit from the unexpected outcomes. One response might be to suggest that common-sense realism be modified to make it more responsive to science, to naturalise it, although that probably re-defines the common-sense form in terms of more standard scientific realism.

It seems reasonable to assert that the claims of scientists, including claims expressed in theoretical terms are successful at least some of the time, so assessment of the empirical adequacy of theories is easily achievable. Any move towards assertions of realism will be judged variously according to the degrees of optimism or pessimism exhibited by particular commentators in the application of any assertibility conditions that might be acceptable to them and others.

The move towards structural realism and model-theoretic representation is consonant with some, but not all of the demands and practical realities of dealing with the particular complexity that comes with the special sciences. A major aim of scientists in these enterprises is to construct accurate descriptions and representations of the structure of world they encounter and explore. I think that there is considerable epistemic value in structuralist realist approaches, as detailed in Chapter 3, although the failure of its proponents to deal with conceptual puzzles about how to distinguish structure from content and properties seems problematic, and the lack of specificity of structural accounts is a real problem for structural realists as noted by Giere. The proposals of ontic structural realism are speculative, inadequately explained and ultimately, unhelpful.

In summary, my view is that we should not persist in using claims of objectivist reference and truth as a basis for interpreting the practice of science but rather think that the role of science and its practitioners is to produce models or representations that fit the natural world with more or less fidelity, and in different special ways, in the same way that maps fit the physical world more or less closely and with different emphases depending on the interests, uses and perspectives of the users of those maps. The best and most appropriate fit between model and world in a given case will depend on the available current data, continuously modified according to the state of the dialectic and the available consilience of the appropriate discipline peer groups, and the values and interests of its practitioners. The possible models are protean; the majority in the special sciences will not be usefully formally mathematizable. The aim of science on this view is the construction of representations with the best possible correspondence between models and their intentional correlates—what they are about. The trick then turns on how to justify 'best possible correspondence' via plausible assertability conditions.

The result can be a realism that is perspectival and epistemic, rather than metaphysical, like Giere's constructive realism, or a species of entity realism perhaps, with its principal aims of representing and intervening. Assessments of goodness-of-fit between representation and the world will be based on data derived empirically through observation and detection via well-designed experiments and judged according to standard super-empirical values and peer review, that is, via negotiated meaning. When models are so judged we can have confidence in the idea that parts of the structure of the world are at least similar to the sub-structures of the associated models, while recognising, with Giere that '[r]ealism need not require that we be in possession of a perfect model that exactly mirrors the structure of the world in all respects and to a perfect degree of accuracy' (1999a, p. 241). My position is closely consonant with Churchland's, reiterated here:

> there exists a world, independent of our cognition, with which we interact and of which we construct representations: for varying purposes, with varying penetration, and with varying success….Our best and most penetrating grasp of the real is still held to reside in the representations provided by our best theories. Global excellence of theory remains the fundamental measure of rational ontology. And that has always been the central claim of scientific realism. (1985, pp. 46-47)

The epigraph, by Fontenelle (1715, p. 8), at the head of this dissertation is, of course, part of a much larger work (Wilson 1997, pp. 219, 255). Apart from suggesting that our curiosity outruns our eyesight, Fontenelle was identifying a paradox, commenting on philosophers who spend a lifetime disbelieving what they can see, and conjecturing, theorising, on the invisible. It is true that as Wilson puts it, in reference to microscopy, 'there is an antinomy of surface and interior' that persists despite the success of science, but I suggest that the process of scientific investigation incrementally reveals more and more of the real. That we may never achieve complete understanding of the deepest aspects of the real world is a challenge rather than an encouragement for nihilistic anti-realism.

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