Living with the Abstract: Realism and Models*

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Abstract A natural way to think of models is as abstract entities. If theories employ models to represent the world, theories traffic in abstract entities much more widely than is often assumed. This kind of thought seems to create a problem for a scientific realist approach to theories. Scientific realists claim theories should be understood literally. Do they then imply (and are they committed to) the reality of abstract entities? Or are theories simply—and incurably—false (if there are no abstract entities)? Or has the very idea of literal understanding to be abandoned? Is then fictionalism towards scientific theories inevitable? This paper argues that scientific realism can happily co-exist with models *qua* abstracta.

1. Introduction

Idealisation and abstraction are indispensable for the construction of theoretical representations of worldly systems, but their products—the models—are not worldly systems. What exactly models are might still be a matter of dispute, but a central thought is that models (or at least *some* models) are abstract entities. If theories are taken to be collections of models (or, less controversially, if theories employ models to represent the world), theories traffic in abstract entities much more widely than is often assumed. This creates problems for a realist approach to theories. Scientific realists claim theories should be understood literally. Do they then imply (and are they committed to) the reality of abstract entities? Or are theories simply—and incurably—

* A draft of this paper was presented at the conference *Models & Simulations II*, at the University of Tilburg, Holland in October 2007. Many thanks to a number of participants for comments, but especially to Roman Frigg, Chris Pincock and Mauricio Suarez. Many thanks are also due to Martin Jones for penetrating comments on an earlier draft and to an anonymous reader for *Synthese*.

false (if there are no abstract entities)? Or has the very idea of literal understanding to be abandoned? Is then fictionalism towards scientific theories inevitable?

This paper argues that scientific realism can happily co-exist with models *qua* abstracta. Section 2 lays out the problem that a literal reading of theories faces in the form of a dilemma: either all theories are (born) false or the literal reading has to be abandoned. Section 3 turns to the nature of representation in science and motivates a distinction between the process of representation (based as it is on idealisation and abstraction) and its products—which may well be an abstract object standing in representational relations to concrete ones. The key point is that focusing on the *process* of the construction of the theoretical description associated with a model favours seeing the model itself as a fictitious entity. But if, instead, we focus our attention on the *product* of the theoretical description—on the model itslef—we can see that it is an abstract entity. Section 4 examines two interestingly related ways in which commitment to models as abstract objects might be avoided: Quine's and Duhem's. It is shown that a) their shared hostility to models is (at least partly view) to focusing on the process of model-construction; b) there is a Quinean argument for the defence of models *qua* abstract entities; and c) all this enables realists to adhere to literalism but claim that theories are committed to concrete as well as abstract entities. Section 5 outlines how realists can live with the abstract.

A note on philosophical terminology and ontology. The claim that models are abstract entities is meant to imply that a) they are not concrete; and b) they are not causally efficacious. In this sense, models are like mathematical abstract entities. But there is a notable difference. I take models to be cases of what Michael Dummett (1991, 300) has aptly called 'abstract physical objects'. As such, they are characterised by non-logical (and in particular, physical) concepts relating to some domain of reality. Besides, their very existence is both contingent (i.e., they do not exist necessarily) and also dependent upon the existence and behaviour of concrete objects: they are such that if all concrete physical objects were to be wiped out, they would be wiped out too. The Equator, the centre of mass of the solar system, inertial frames, a frictionless inclined plane, the Linear Harmonic Oscillator are pertinent examples. But physical quantities too are cases of abstract physical entities. (Massive bodies, for instance, are concrete causal objects, but *mass* is an abstract quantity, shareable by many particulars.) The pertinent point is that abstract objects are real and this requires a conception of reality that is not exhausted by whatever is concrete and causally efficacious. At least this is what I will argue for in this paper. Hence, it is my intention to contrast this view with the claim that models are fictions (or fictitious entities).

There is no agreement as to how exactly ‘fiction’ should be understood. Characters in novels or myths are fictitious; sometimes, we say that an untrue story is a fiction. For the purposes of the issues raised in this paper, fictionalism is the view regarding a set of putative entities that they are not real but they are (useful) fictions. To say, then, that models are fictions is to say that since nothing worldly satisfies the description associated with a model (e.g., the description of a linear harmonic oscillator), this description refers to nothing real. In a useful paper, Roman Frigg (forthcoming) takes models to be ‘hypothetical’ or ‘imagined’ entities or physical systems, treats them as non-existing objects, and claims that models should be understood along the lines of the characters that occur in fiction—capitalising on a pretence theory of fiction.[1] Frigg’s (and others’) view presupposes that the real is equated with the concrete and the causal. This is the kind of view I want to resist. There is a view in the philosophy of fiction (explored by Stacie Friend 2007) according to which fictitious objects (like the characters in a novel) exist and are abstract entities ‘akin to novels and plots’. This ‘realist’ stance is contrasted to the anti-realist view that fictional characters are non-existing and that when we engage in discourse about them, we engage in a *pretence* that there are relevant entities or individuals. This certainly needs exploration, but my considered view is that the sense in which models are abstract entities is not quite the same sense in which Harry Potter is an abstract entity in that scientific theories are not like novels or plots and in that the existence conditions of models (*qua* abstract entities) are more demanding than the existence conditions of fictional objects (on the foregoing realist construal of them), which are brought into existence simply because an author creates them by writing a novel.

2. Trouble for Realism

Historically, the realist view that scientific theories should be understood *literally* has meant to oppose reductive empiricist and instrumentalist conceptions of scientific theories (see my 1999, chapters 1 and 2). The core of the idea of a literal understanding of theories is that theories should neither be re-interpreted in terms of a

[1] For a closely related approach to Frigg’s see Peter Godfrey-Smith (2006). For a nice criticism of it, see Martin Jones (forthcoming).

supposed privileged (observational) vocabulary nor be taken to be uninterpreted (or partially interpreted) calculi. Instead, theories should be taken at face-value. A literal interpretation of, say, the theory of electrons, takes it to be about *electrons* and their properties. It is then ontologically inflationary in that *if* the literally interpreted theory is true, the kinds of entities that are required for this truth are real.[2]

Suppose, however, there are reasons to think that *some* of the entities implied by a literal reading of a theory are not real. If literally read, the theory cannot be true—even if some other of the entities implied by a literal reading are indeed real. Alternatively, the theory may not be understood literally. For many philosophers, a literal reading of scientific theories implies commitment to a host of entities with a (to say the least) questionable ontic status: numbers, geometrical points, theoretical ideals, models and suchlike. Hence, realists face a dilemma—let's call it, the *central dilemma*:

C-D

Either theories should be understood literally, but then they are false. Or, they should not be understood literally, but then realism itself is false (at least insofar as it implies that theories should be understood literally).

Trouble lurks for realism, whichever horn is chosen. Taken at face value, theories make *mixed* claims, that is, claims such that, if true, they imply the existence of both OK-entities (such as electrons and their ilk) and supposedly non OK-entities (such as numbers or models or theoretical ideals). More specifically, some mixed statements implied by scientific theories (of the form 'P is (or has the property) M', where 'P' is meant to refer to a concrete physical object and 'M' to a model) require for their truth mixed facts: abstract entities are related to concrete entities to render mixed statements true. Going for the first horn of the dilemma allows the realist to stick to uniform semantics, but she has to claim that all mixed statements are *false*. Accordingly, scientific theories are *born* false: the world is not such that it can make a

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[2] I have found this claim of literal understanding to be prone to misunderstanding. It is meant to be a claim about semantics, not epistemology. The question I am interested in is this: what is the world like if a theory is literally understood, that is, if we take it at face value (refrain from re-interpreting it etc.)? A literally understood theory need not be a true theory—simply because the world might not co-operate. In this paper, my focus is not on epistemology, though the overall Quinean perspective I adopt can, in well-known ways, answer epistemological questions about how to ascertain the truth of theories such that, if they are literally understood, they make mixed claims about concrete *and* abstract entities.

literally understood scientific theory true, simply because it lacks some of the stock of entities required for its truth. The realist optimism that theories succeed in uncovering what the world is like is seriously challenged.[3] Going for the second horn of the dilemma amounts to a rejection of a face-value reading of theories. Realism has to go for a non-uniform semantics of scientific theories, and a story should be told as to where exactly, and why, the line between the literally understood and the non-literally understood part of a theory is drawn.

A quick answer on behalf of scientific realism would be an attempt to *qualify* her defence of the literal interpretation of scientific theories. She might admit that in many cases, there will be *local* independent reasons to take some posited entities as fictitious. Take, for instance, the Carnot engine. The model of a (fully reversible) Carnot engine is such that it cannot represent exactly and accurately any worldly engine. This was known to Sadi Carnot himself—as well as to anybody else—and this knowledge might be enough to justify taking the Carnot engine as a fiction. It does not follow from this that one cannot take literally *other* parts of the theory (thermodynamics) in which the description of the Carnot engine is embedded. Nor does it follow that one cannot take literally the theoretical description of the (fictitious) Carnot engine. If we did not do this, we could not explain why this theoretical fiction is so *useful*: it is useful because worldly engines are *inexact* counterparts of the Carnot engine and their behaviour resembles and approximates the behaviour of a Carnot engine. *Inexact* counterparts of the Carnot engine are less efficient than it (that is, from the efficiency a Carnot engine would have, were it concrete), but their efficiency is independent of the nature of the working substance and dependent on the temperature limits through which they operate, just as the (description of the) Carnot engine predicts.

This kind of reply is on the right track, but it does not cut too deep. A vast array of posits of physical theories would turn out to be fictions. Physical theories posit *imponderable* fluids, *frictionless* planes, *ideal* gases, *perfectly* spherical objects, mass-*points*, *perfectly isolated* systems, as well as linear harmonic oscillators, Hilbert spaces and the like. The very nature of representation in science makes the use of what appears to be useful fictions ubiquitous. Differently put, the very nature o

[3] The realists may then pin their hopes to nominalistic versions of scientific theories or to some kind of commitment to the nominalistic adequacy of theories. There are a number of problems with this view, which are discussed in my (forthcoming).

representation in science makes mixed statements such as those noted above the rule and not the exception. The realist cannot simply sit back and relax on the basis of a local criterion of being a fictitious entity. It would appear that fictionalism is the (descriptively) adequate account of scientific theories—sprinkled with pockets of literalism. Can the realist fight back?

3. Representation in science: process vs product

Martin Jones (2005) has offered a unified treatment of the processes of idealisation and abstraction. The gist of it is that in abstraction some features of the system under study are neglected/omitted (without the system being misrepresented); in idealisation, there is misrepresentation—the model attributes properties to the system it does not have and/or denies that the system has some properties that it in fact possesses. For instance, in constructing a model of projectile motion, it is assumed that the gravitational force of the earth is the only force exerted on the projectile (an idealisation—since it is *not* the only force); at the same time, the colour of the projectile or its chemical composition are omitted (an abstraction—since the object is *not* misrepresented).

These two processes result in a theoretical description of something—the model. It is tempting to think that precisely because nothing worldly satisfies the description associated with the model, since no worldly entity is exactly like the model, the thing described is a fictitious entity; a fiction. A fiction need not be totally unrelated to reality. A fiction might resemble *more or less* some real entity. Strictly speaking, however, a fiction is something that cannot be (or simply is not) part of reality. Mauricio Suarez (1999, 181) has persuasively argued that it is wrong to think there must be an ultimate (exact, accurate but hideously complicated) representation of a worldly situation (e.g., the motion of a natural pendulum) within the theory and that to get to it is only a matter of performing successive de-idealisations of the theoretical description of the model (e.g., the linear harmonic oscillator). But even if we were to grant that the world is *in principle* fully describable without any idealisation or abstraction, if we are to take actual science seriously, abstraction and idealisation enter constitutively into the ways scientific theories (better: scientists) represent the world. As Paul Teller (2008, 437) has put it: "all fundamental theories in existing physics are idealisations, among other things idealising the nature of the objects and quantities that they study".

Idealisation and abstraction are processes by means of which descriptions of models are constructed. But apart from the process, there is also the *product*—the model itself. What exactly is its ontic status? Take the familiar harmonic oscillator. Suppose, following Ronald Giere (1988), that it is introduced by means of a theoretical definition. A linear harmonic oscillator (LHO) is whatever entity satisfies ----, where the standard *definiens* is: the motion (along *x* axis) satisfies the second-order differential equation $F= m\, d^2x/dt^2= -kx$. In getting to this definition, idealisation and abstraction have played an important role. It has been assumed that the angle of swing is small, that the velocity-dependent frictional force (air resistance) is zero etc. There is simply nothing in the physical world that satisfies this definition. And yet the definition *is* satisfied by some entity, viz., the linear harmonic oscillator. This is exactly what I have called a physical *abstract* entity.

That models are abstract objects is not news. This view is shared by many who work in this area—though it has been articulated by Giere. He takes models to be abstract entities that satisfy a certain theoretical definition (typically, a mathematical equation, or a set of them). They are then linked to the physical world by means of theoretical hypotheses, the generic form of which is: *X* is ---- to *M*, where the dotted line is meant to be replaced by a relation of representation (e.g., resemblance or similarity). Here is a standard example: the earth-and-moon system is like (to certain degrees and in certain respects) a two-particle Newtonian system. Theories then are mixed entities comprising models and theoretical hypotheses—hence, theories quantify over abstract entities.

If we think of models as products, theories traffic in abstract entities. They assume their existence. They describe them and not concrete objects, at least in the first instance. They are true of them, again in the first instance. They represent the world by means of them. They acquire representational content by mixed statements (*aka* theoretical hypotheses) that link abstract entities with concrete physical systems.

Qua abstract entities models are not, *ipso facto*, fictions. A LHO may not be met anywhere in the natural world, it may not engage in any causal interactions etc., but this does not make it a fiction any more than it does the number one, or the set of all people in this room. There must be some *independent* reason for taking models to be unreal (merely fictions). In all probability, this reason will be based on a general philosophical adherence to nominalism (better: to anti-platonism). The fact that idealisation and abstraction are involved in the process of constructing a theoretical description of a model is *not* in itself a reason to think that whatever satisfies this description is a fiction. Though of course, it is reason to think that *were* the entity that satisfies the theoretical description taken to be a concrete physical system, it would not be found among the inventory of concrete objects that populate the world.

The process and the product should be kept distinct. A literal understanding of a scientific theory implies commitments to models *qua* abstract objects. Insofar as these theories (taken literally) are true, there are these abstract entities. Just by being abstract, these objects are not less real than the concrete ones. The argument that there was idealisation and abstraction involved in the construction of the definitions of these abstract entities *does* imply that these abstract entities cannot be faithful representations of real worldly systems. But it does *not* follow from this that these abstract objects are unreal (*mere* fictions). They are not concrete physical objects. But they are not meant to be thus. They are abstract objects that can stand in representation relations to worldly systems.

The point that needs to be stressed is that focusing on the *process* of the construction of the theoretical description of the model tends favour seeing the product (the model) as a fictitious (that is, unreal) entity. Focusing on the *product*—on what kind of entity the model itself is—favours seeing it as an abstract object. It's not of course that the very same entity is both real *and* fictitious. The problem is not with the model but with the nature of representation. A real entity *A* (a triangle; a LHO) can represent more or less adequately (and for certain purposes) another real entity *B* (a triangular road sign; the motion of the bob in the grandfather clock) and yet, the process by which the theoretical description *D* of this real entity *A* was constructed (based as it is on idealisation of, and abstraction from, concrete physical objects *B*) is such that *D* does not amount to an accurate and exact description of *B*. If we focused exclusively on the process of constructing *D*, then we would end up with a fictitious description of *B*. But this does not imply that there is nothing of which *D* is an accurate and exact description—the model *qua* product (and *qua* abstract object) is what *D* is true of.

Recall *C*-*D* above: Either theories should be understood literally, but then they are false. Or, they should not be understood literally, but then realism is false. It seems there is a way out: theories can be understood literally *and* be true. But if they are true, they are true of abstract entities as well as concrete ones. A price for this move is that the truth of theories does not give them straightforward representational content vis-à-vis the physical world. Their representational content is mediated (at least partly) by abstract objects—the models.[4] Another price is that there is commitment to abstract objects—with all the (notorious) problems this move brings in tow. My view is that the price is worth paying. But before I defend this view, let us look at two ways in which commitment to models *qua* abstract objects might be resisted.

4. Models: Quine vs Duhem

I have chosen Quine and Duhem because they both attribute an ineliminable role to abstract entities in scientific representation but draw importantly different conclusions from it. Quine is famous for the indispensability argument concerning mathematical abstract entities. He adopts a literalist account of scientific theories, thereby being committed to a host of abstract entities, but his attitude towards models is different. He takes them to be *ideal* objects, and as such, not real—they are useful fictions. Though Duhem can also be seen as putting forward an argument for the *instrumental* indispensability of mathematics, he refrains from being committed to abstract entities and adopts a sweeping fictionalist stance towards scientific theories. What Duhem calls theoretical facts or schematic objects (basically models) are taken to be useful fictions, but indispensable for representation in science. Playing Quine and Duhem against each other, I hope to pave the way for commitment to models *qua* abstract entities.

4.1 Quinean Ideal Objects

Due to space restrictions, I will refrain from discussing Quine's indispensability argument. Suffice it to say that though Quine (1960) is fully committed to the reality of abstracta, he is not so committed to the reality of models. He treats them as ideal objects (better: as being constructed out of ideal objects such as mass points and frictionless planes) which, as such, are contrary to physical theory. He goes on to say that being ideal objects, the unreality of models does not falsify the theory. If the antecedent of a conditional such as 'if x is a linear harmonic oscillator, then ---' is

[4] I am not going to deal with this issue in this paper. I am happy to accept that theories are not *directly* true of the physical world. This view would create an epistemic problem for realism only if a) there was a problem with the knowledge of abstracta and b) they theory could not be defended as being approximately true of the physical world. I make some brief points about a) in section 5. If there is not enough already in the literature about b), my answer has to wait for a different occasion.

false (because there is no such thing are the linear harmonic oscillator), the conditional is (vacuously) true. But so is a conditional with a contrary consequent. How are we to tell which models are useful or adequate or whatever? How can models represent (imprecisely etc.) a physical system as opposed to another one?

Quine (1960, 249) answers this point taking ideal objects (and hence models) to be limiting cases of actual and real objects. Commitment to them can be avoided because talk about them can be 'paraphrased' into talk about limiting cases of actual and real objects. He uses the term 'symbolic' (which, as we shall see, has also been used by Duhem) to refer to the doctrine of ideal objects, of which he says

> it is a deliberate myth, useful for the vividness, beauty and substantial correctness with which it portrays certain aspects of nature even while, on a literal reading, it falsifies nature in other respects (1960, 250).

An ideal object (a model) is the fictitious because it cannot be found among actual concrete objects. But why should this matter to the reality of an entity? Quine himself has drawn a distinction analogous the process/product distinction I have discussed. He (1960, 22) says:

> To call a posit a posit is not to patronise it. A posit can be unavoidable except at the cost of other no less artificial expedients. Everything to which we concede existence is a posit from the standpoint of a description of the theory-building process, and simultaneously real from the standpoint of the theory that is being built. Nor let us look on the standpoint of the theory as make-believe; for we can never do better than occupy the standpoint of some theory or other, the best we can muster at the time.

If we stick to this distinction, models (seen from the standpoint of the theory that is being built) are no less real than other posits. The way the description of the model is constructed (and its usefulness in representing *this* rather than *that* system) has to do with idealising, and abstracting away from, a concrete physical system. But the theory quantifies over the *product*, which is an abstract entity. Why then is the theory *not* committed to it? Because, Quine would say, talk about it can be paraphrased in terms of talk about limiting cases. Even if we accepted this possibility of paraphrase, the very fact that descriptions of the models are constructed in a certain way does not imply that the product of this construction is unreal—it is the abstract entity that satisfies the theoretical definition (which was constructed in *this* way rather than *that*). Quine takes it that what he calls 'the myth of ideal objects' is useful in that it brings simplicity to certain computations; he adds that the simplicity of a theory that squares with observations "is the best evidence for the truth we can ask" (1960, 250). What then is the difference between being committed to the truth of scientific theories about electrons and molecules (for which no better claim than simplicity and empirical adequacy can be made, as Quine urges) and being committed to the models they employ? Note that for Quine abstractness is not at stake here. Being committed to the truth of scientific theories implies being committed to abstracta, like numbers and sets. Why then the evidence for scientific theories, while being evidence for the reality of electrons and sets, is not evidence for the reality of models?

Quine's answer is rather vague: the doctrine of ideal objects "works its simplifications in a limited domain of statements at the cost of more seriously complicating a more inclusive domain" (1960, 250). Even if this is so for point-masses and frictionless planes, it is not so for more complex and interesting models, like the harmonic oscillator. Lots of models work wonders of simplification in many domains. And the complication effected by admitting their reality is not of a different kind from the complication effected by admitting sets—in both cases, the complication has to do with their being abstract entities.

In any case, not all models can be understood as limiting cases of concrete objects. This point has been forcefully made by Margaret Morrison (1999)—who examined cases of models in hydrodynamics that treat the physical system under description in a piecemeal way and are far from being a limiting case of it. But it may be enough to drive this point home to consider mathematical models, like Hilbert spaces or groups, where the model itself is (or is based directly on) a mathematical structure. It is not, then, a limiting case of an actual physical system.

4.2 Duhemian schematic objects

Unlike Quine, Duhem was not a realist about scientific theories—what exactly he was is still a matter of dispute. But like Quine, he did think that the *use* of mathematics is indispensable in science. Very early on, he defended the view that "the instrument of mathematics is necessary for the study of physics" and claimed:

> But it would be illogical to criticise the theory for the complexity of the apparatus used to construct it [the mathematics], unless this apparatus can be replaced with another that will be equally solid and easier to handle (1892, 25).

In this passage, one can see—albeit in somewhat compressed form—the idea that it is not enough to criticise the use of mathematics in science. What is required is showing that mathematics can be dispensed with in the construction of a physical theory—and besides that whatever replaces it has the theoretical virtues that a mathematical formulation of a theory has. For Duhem, mathematics is indispensable because the magnitudes to which the theoretical hypotheses of a physical theory apply are constructed (defined) out of mathematical entities and the theoretical hypotheses themselves state mathematically precise relations among magnitudes.

But Duhem's indispensability of mathematics is *instrumental*. As he put it, "it is only a means, not an end" for the construction of physical theory. We could put this point by saying that thought mathematics is theoretically indispensable, it is not metaphysically indispensable. Duhem does not take a physical theory to issue in commitments to mathematical entities. Nor, of course, to any theoretical entities. Theories are judged only with respect to their empirical adequacy and the usefulness of theories is that they provide a single and unified mathematical framework in which all relevant phenomena are embedded, classified and rendered interconnected. Theoretical-cum-mathematical entities are, for Duhem, fictions.

When Duhem talks explicitly about *models* he has in mind the mechanical models used mostly by British scientists of the 19th century in connection with the theories of electricity and magnetism—constructions out of rods and pulleys and wheels—that are meant to offer a mechanical reduction of apparently non-mechanical phenomena. As is well-known, he was very dismissive of them and contrasted them unfavourably with the abstract and highly mathematical theories developed in the continental Europe. But it would be a mistake to think that Duhem did not have a place for models (suitably understood) in his theory of science.

The distinction between the practical fact and the theoretical fact is essential for understanding Duhem's views on representation in science. The practical facts (what, on a different occasion he called "facts brutal and singular") may well be the starting point of theoretical representation, but there is no simple and straightforward relation between them and the theoretical facts. To a good degree of approximation, a theoretical fact is a model. More precisely put, a theoretical fact is created or constructed out of a practical fact by processes that involve idealisation and abstraction—a collective name of which might well be ‘*schematism*’. A practical fact is, strictly speaking, indescribable within theory. A practical fact would be the actual motion of the moon around the earth or of the earth around the sun. A theoretical fact would be the two-body system. Practical facts are imprecise and vague. They are useless to the physicist. The latter *replaces* a practical fact with a theoretical one. The theoretical fact is precise and sharp. Here is how Duhem describes a theoretical fact:

> the body studied is geometrically defined; its sides are true lines without thickness, its points true points without dimensions; the different lengths and angles determining its shape are exactly known; to each point of this body there is a corresponding temperature, and this temperature is for each point a number not to be confused with any other number (1906, 133-134).

Theoretical facts are ubiquitous in science. When Duhem talks about the role of experiment, he stresses that the theory is not about the real instrument that a scientist manipulates, but about a ‘schematic model’ of the instrument—couched in terms of the symbolic representation afforded by the theory. The physicist reasons about “the ideal, symbolic” instrument and compares it with the real one. Similarly, the theory is not about real systems (e.g., gases) but about schematic representations of them (what Duhem for instance calls the “schematic gas”—1906, 174).

All this might sound like Quine’s conception of ideal objects. But Duhem does not think these schematic objects (constituted by theoretical facts) are limiting cases of practical ones. Strictly speaking, an infinity of theoretical objects correspond to the practical one—that is, the process of idealisation and abstraction of a practical fact does not determine uniquely a theoretical fact, even in the ideal limit. The relation between practical fact and theoretical fact is one-to-many. Conversely, one and the same theoretical fact may yield an infinity of practical facts—that is, the process of de-idealisation and concretisation does not determine uniquely a practical fact. For all practical purposes, this might not matter much. The experimental apparatus available might not be able to distinguish between different theoretical facts. A whole bundle of theoretical facts might well be approximately exact with respect to a practical fact. Still, theoretical facts are necessary for doing physics and their construction is not merely a matter of letting some parameters reach a certain limiting value. The theoretical fact in which, say, the force is inversely proportional to the second power of the distance between two masses ($F \sim 1/r^2$) is distinct from, and inconsistent with, the theoretical fact in which the force is inversely proportional to the 2.000001 power of the distance between two masses ($F \sim 1/r^{2.000001}$) even though the two facts approximate each other.

The key feature of a theoretical fact is that it is about an abstract entity—a *schematic entity*. Duhem does not quite put it that way, but he comes close to it when, in offering the most general account of a theoretical fact, he says that it is "the symbolic, abstract formula stated by the physicist" (1906, 151). But, unlike Quine after him, Duhem was reluctant to admit the reality of abstract entities. He thought that the laws of the physical theory state connections between "abstractions" (1906, 166), but he also claimed that because of this, laws (better: law-statements, though that's not the way Duhem put it) are neither true nor false—but only approximate (cf. 1906, 168). Given that Duhem's only irrevocable commitment was to the empirical laws saved by the theory, the laws of theory cannot be true of them simply because they are not—and, by construction, they cannot be—faithful representations of them. If, however, to say that the laws of theory are false is to say too much (after all, as Duhem himself stressed, the laws of the theory *do* resemble the empirical laws in some respects and to some degree), the only way out is to say that the laws of theory are approximate: they match empirical laws, if at all, only approximately. Following some hints offered by Duhem himself, I think we should take this claim to mean that the laws of the theory cannot be called "true in the proper sense of the word" (1906, 172)—that is, *empirically true*. Physical theory, for Duhem, is always approximate. The schematism—the process of idealisation and abstraction—is indispensable in physics but the product of the schematism (the theoretical fact; the law of the theory) is representationally inexact.

It seems fair to say that for Duhem the schematic entities are (useful, even indispensable) fictions. Duhem's fictionalism was quite sweeping. Based on the indispensability of schematic objects for representation in science, he went as far as to argue that *all* theoretical posits are fictions. But (at least part of) the reason for this sweeping view was the fact that he focused his attention on the process of schematism and neglected its product. In criticising the British scientists of his time, he noted dismissively: "for them the equation alone has value. The background of the equation has no interest for them". The background—on which Duhem repeatedly focused his attention—was the process of the construction of the equation. The background becomes also relevant when it comes to the reverse process, viz., the concretisation/de-idealisation of the schematic entity so that it offers a more accurate/faithful representation of the worldly system (see 1906, 174-7). Duhem (1906, 175) is adamant that there is always going to be a *representational gap* between the schematic object and reality. The fit between the two might improve, but it will never become a perfect match. This is already a good reason to think that there is something that answers to the description of the schematic object—viz., the schematic object itself *qua* an abstract entity—and that this entity will be distinct from but related to some worldly object.

4.3 Duhem vs Quine

Duhem's sweeping fictionalism about theoretical-cum-mathematical entities and Quine's selective fictionalism about models have, we have noted, a common source—at least a *partial* common source—viz., that insufficient attention was paid to the product (as opposed to the process) of model-construction. In Duhem's case, this led to sweeping fictionalism because it was taken to be the case that *all* entities posited by theories (all schematic objects) are constructed by a process of schematism. In Quine's case, the fictionalism was restricted because the process of schematism was taken to be much more limited.

Quine does admit abstract entities—lots of them, actually—but thinks that models are not of the right sort. Duhem does not admit abstract entities and hence, he does not admit models—*qua* abstract entities—either. Against Quine, we have argued—taking cues from Duhem—that models should not be understood as limiting cases of concrete physical systems.

What then can be said against Duhem? Let us go back to the *instrumental* indispensability of mathematics. Duhem takes it to relieve him of commitments to theoretical-cum-mathematical entities, although he has no doubt that the aim of saving the phenomena within a unified and simple theoretical scheme cannot be served except with the aid of mathematics. In his (1892, 26) Duhem talked about the "utility" of mathematics for physical theory. He went on to say that on some occasions this utility shows itself vigorously in cases in which mathematical models are applied to systems or entities that were not initially meant to apply.

The question is: is instrumental (or theoretical) indispensability sufficiently distinct from (or weaker than) indispensability *simpliciter*? And the answer is: not from a Quinean perspective. From this perspective, there is no theory-free standpoint from which what there is can be viewed. The question of what there is (better: the question of what one is committed to) can only be settled within a theory (suitably regimented so that it's logical form is transparent) and its answer has to do with what needs to be assumed for the advancement of a unified, coherent and simple image of the world. The operative principles here are convenience, simplicity, unity and economy. Ontological questions, for Quine, are answered by the best theory and there is no extra-theoretical court of appeal. The best theory is just the theory that works sufficiently well—in particular the theory that tallies with the evidence and satisfies a number of virtues, most notably simplicity. All this implies that for Quine the utility of a posit and its reality go hand in hand. In defending the reality of abstract objects, Quine (1960, 236) insists that there are *not* two criteria of commitment to reality, one being proximity to sensory experience, the other being "utility for theory". The latter criterion reigns. Commitment to abstract entities (as well as denial of the reality of sense data) has to do with their theoretical utility (or its lack thereof). Here is how the point is put in a passage that offers the gist of Quine's indispensability argument: "The reason for admitting numbers as objects is precisely their efficacy in organizing and expediting the sciences. The reason for admitting classes is much the same …" (1960, 237).

From a Quinean perspective, it simply makes *no sense* to adopt a generic fictionalist stance towards entities whose positing affords a simple, unified and coherent account of the world. There is simply no standpoint outside science from which it can be judged that these entities are unreal. A sweeping fictionalist, like Duhem, is guilty of "philosophical double talk, which would repudiate an ontology while enjoying its benefits" (1960, 242). The point here is not that a Duhemian ought to be persuaded by the Quinean claim that instrumental indispensability is indispensability enough. The point is that from a Quinean perspective, a scientific realist can coherently avoid treating models and other abstract entities as useful fictions.

5. Living with the Abstract

There is, then, a clean way out of *C-D* above, viz., to adhere to literalism but claim that theories are committed to concrete as well as abstract entities. If we follow the Quinean line—amended so that models are taken to be real but abstract entities—the pressure on realism is eased.

In defending the view that models are abstract entities, it's important for the realist to insist that all reasons that have to do with admitting entities as real come from theory and that there is no theory-free standpoint from which reality can be viewed and claims about what is real and what is not can be made. There is no theory-independent criterion of reality. I have argued elsewhere (2005) that the only workable criterion of reality is the *explanatory criterion*: something is real if its positing plays an indispensable role in the explanation of well-founded phenomena. In its essentials, this criterion has been advanced by Sellars's (1963). This is a permissive criterion. It does not dictate the status of entities that are explanatorily indispensable; in particular it does not disallow abstract entities from being real. Differently put, it is one thing to say that *x* is real because it meets the explanatory criterion; it is quite another thing to say that *x* is concrete, or abstract or physical or mental and so on. This explanatory criterion should not be confused with a causal criterion. It is not a version of the so-called Eleatic principle, according to which being causally active, that is having causal powers, is a criterion of objecthood.[5] Causal efficacy may well be a mark of reality, but not everything that is real is causally efficacious.

One may wonder: How can models, *qua* abstract entities, represent concrete entities? I do not think there is a big conceptual difficulty here. Two distinct kinds of entity can be similar to one another or stand in some other representational relation. But having said this, the following must be noted. A model is an abstract entity, but the predicates used to describe it may well be physical. As noted already in the Introduction, it is an *abstract physical entity*. A LHO is characterised by physical predicates such as displacement from rest position, mass, force and so on. It is the

[5] This is sometimes called the Eleatic Principle. According to the Eleatic Stranger in Plato's Sophist (247 D-E): "(…) everything which possesses any power of any kind, either to produce a change in anything or to be affected even in the least degree by the slightest cause, though it be only on one occasion, has real existence". Graham Oddie (1982) has criticised it severely.

description in terms of these predicates that makes the model fit to represent concrete physical systems.[6]

One may further wonder: how are models explanatory? First of all, models are indispensable for scientific representation. As we have already noted, the very nature of representation in science, with its reliance on idealisation and abstraction, renders models inevitable. But models are not just representative; they are also explanatory. In fact, as Morrison (1999, 43) has argued, "the representative and the explanatory capacities of models are interconnected". The processes of idealisation and abstraction are such that the description of the model isolates the explanatorily relevant features of the represented system with respect to the behaviour under study. It specifies the basic or more central explanatory mechanism or regularity. Morrison generalises this point by claiming that in representing some system, the model exhibits "certain kinds of structural dependencies" (1999, 63). And she goes on to claim: "The model shows us how particular bits of the system are integrated and fit together in such a way that the system's behaviour can be explained".

Morrison is surely right, but some care is needed here. The system's behaviour is indeed explained, but the explanation is made possible by the fact that there is a matching—in *explanatorily* relevant respects and degrees—between the model and the system. The model does two jobs, as it were. It fixes the *explanatorily* relevant respects and degrees and, on their basis, it offers an explanation of a certain behaviour (e.g., periodic motion). Morrison's 'structural dependencies' are explanatory precisely because they are shared between the model and the system under study *and* they are deemed explanatory by the model. Suarez (1999, 171) then rightly stresses that "models fix the criteria [scientists] use to refine [their] theoretical descriptions of a phenomenon".

In light of the process/product distinction we drew earlier, models are explanatory because a) their descriptions are constructed in a certain way so that contact with the physical world is not lost; and b) *qua* products of this construction process, they can stand in representational relations with worldly systems and fix the explanatorily relevant properties that characterise the behaviour of these systems.

[6] Clearly, non-mathematical things can be said of mathematical objects (e.g., it can said *of* number ten that it is the number of my fingers) and conversely, mathematical things can be said of non-mathematical objects (e.g., it can be said of a road-sign that its shape is circular).

How can models give causally relevant information about the world, if they are not themselves part of the causal order of the world? Most models are such that had there been concrete entities that matched them exactly, these entities would have been part of the causal order of the world. So models can provide causally relevant information about worldly systems because on their basis certain counterfactual conditionals can be settled.

How then are models known? Not causally, of course, but hypothetico-deductively—as parts of our overall theory of the world.

These last claims are only sketches of answers to pressing questions, and they need to be further developed. But the difficulties these questions raise are not insuperable—they recapitulate the well-known difficulties of living with the abstract. The point of this paper has been that commitment to abstracta in science is much more deeply rooted than is often assumed. Try to do without the abstract and you do away with much of the representative capacities of science. Try to do without the abstract and the very object of scientific theories—models—disappears too.

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