BAS C. VAN FRAASSEN

TO SAVE THE PHENOMENA* 


Realism

"To have good reason to accept a theory is to have good reason to believe that the entities it postulates are real," as Wilfrid Sellars has expressed it. Accordingly, an anti-realist is a position according to which the aims of science can well be served without giving such a literally true story, and acceptance of a theory may properly involve something less (or other) than belief that it is true.

The idea of a literally true account has two aspects: the language is to be literally construed; and, so construed, the account is true. This divides the anti-realists into two sorts. The first sort holds that science is or aims to be true, properly (but not literally) construed. The second holds that the language of science should be literally construed, but its truth to the reality pheno-

I

mena to be saved from the reality postu-

lated. He distinguished the "absolute magnitudes" that appear in his axioms from their "sensible measures" which are deter-
rmined experimentally. He discussed care-

fully the ways in which, and extent to which, "the true motions of particular bodies [may be determined] from the apparent," via the assertion that "the apparent motions . . . are the differences of true motions." 3

The "apparent motions" form relational structures defined by measuring relative distances, time intervals, and angles of separation. For brevity, let us call these relational structures appearances. In the mathematical model provided by Newton's theory, bodies are located in Absolute Space, in which they have real or absolute motions. But within these models we can define structures that are meant to be exact reflections of those appearances and are, as Newton says, iden-
tifiable as differences between true motions. These structures, defined in terms of the relevant relations between absolute locations and absolute times, which are the appropriate parts of Newton's models, I shall call imitative motions. 4

When Newton claims empirical adequacy for his theory he is claiming that his theory has some model such that all actual appearances are isomorphic with imitative motions in that model.

Newton's theory goes a great deal further than this. It is part of his theory that there is such a thing as Absolute Space, that absolute motion is motion relative to Absolute Space, that absolute acceleration causes certain stresses and strains and thereby deformations in the appearances, and so on. He offered, in addition, the hypothesis (his term) that the center of gravity of the system is at rest in Absolute Space. But, as he himself noted, the appearances would be different if that center were in any other state of constant absolute motion.

Let us call Newton's theory (mechanics and gravitation) TN, and TN(o) the theory TN plus the postulate that the center of gravity of the solar system has constant absolute velocity. By Newton's own account, he claims that the theory is adequate for TN(o); and also claims that, if TN(0) is empirically ade-
quate, then so are all the theories TN(v).

Recalling what it was to claim empirical adequacy, we see that all the theories TN(v) are empirically equivalent exactly if all the motions in a model of TN(v) are isomorphic to imitative motions in a model TN(v + w), for all constant velocities v and w. For now, let us agree that

these theories are empirically equivalent, referring objections to a later section.

III

What exactly is the "empirical import" of TN(0)? Let us focus on a fictitious and anachronistic philosopher Leibniz*, whose only quarrel with Newton's theory is that he does not believe in the existence of Absolute Space. As a corollary, of course, he can attach no "physical significance" to statements about absolute motion. Leibniz* believes, like Newton, that TN(0) is empirically adequate; but not that it is true. For the sake of brevity, let us say that Leib-

niz* believes that the theory he can believe it; when confusion threatens we may

expands that idiom to say that he accepts the theory as empirically adequate, but does not believe it to be true. What does Leibniz* believe, then?

Leibniz* believes that TN(0) is empirically adequate, and hence, equivalently, that all the theories TN(v) are empirically adequate. Yet we cannot identify the theory that Leibniz* holds about the world—call it TNE—

with the common part of all the theories

TN(v). For each of the theories TN(v) has such consequences as that the earth has some velocity v. We must then establish that v exists. In each model of each theory TN(v) there is to be found something other than motions, and there is the rub.

To believe a theory is to believe that one of its models correctly represents the world. A theory may have isomorphic models; that redundancy is easily removed. If it has been removed, then to believe the theory is to believe that exactly one of its models correctly represents the world. Therefore, if we believe of a family of theories that all are empirically adequate, but each goes beyond the phenomena, then we are still free to believe that each is false, and hence their common part is false. For that common part is phrasable as: one of the models of one of those theories correctly represents the world.

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It may be objected that theories will seem empirically equivalent only so long as we do not consider their possible extensions. The equivalence may generally, or always, disappear when we consider their implications for some further domain of application. The usual example is Brownian motion; but this is imperfect, for it was known that phenomenological and statistical thermodynamics disagreed even on macroscopic phenomena over sufficiently long periods of time. But there is a good, fictional example: the combination of electromagnetism with mechanics, if we ignore the unexpected null results that led to the replacement of classical mechanics.

Maxwell's theory was not developed as part of mechanics, but it did have mechanistic models. This follows from a result of Koenig, as detailed by Poincare in the preface of his *Electricité et Optique* and elsewhere. But the theory had the strange new feature that velocity itself, not just its derivative, appears in the equations. A host of thought experiments was designed to measure absolute velocity, the simplest perhaps that of Poincare:

Consider two electrified bodies; though they seem to us at rest, they are both carried along by the motion of the earth;...therefore, equivalent to two parallel currents of the same sense and these two currents should attract each other. In measuring this attraction, we shall measure the velocity of the earth; not its velocity in relation to the sun or the fixed stars, but its absolute velocity.

The null outcome of all experiments of this sort led to the replacement of classical by relativistic mechanics. But let us imagine that values were found for the absolute velocities; specifically for that of the center of the solar system. Then, surely, one of the theories $TN(v)$ would be confirmed and the other falsified.

This reasoning is spurious. Newton made no point of introducing relativistic mechanics without presupposing more than that basic mechanics in which Maxwell's theories has models. Each motion in a model of $TN(v)$ is isomorphic to one in some model of $E(v + w)$, for all constant velocities $v$ and $w$. Could this assertion of empirical equivalence possibly be contested by these nineteenth-century reflections? The answer is no. The thought experiment, we may imagine, confirmed the theory that added to $TN$ the hypothesis:

**H0:** The center of gravity of the solar system is at absolute rest.

**E0:** Two electrified bodies moving with absolute velocity $v$ attract each other with force $F(v)$.

This theory has a consequence strictly about appearances:

**CON:** Two electrified bodies, moving with velocity $v$ relative to the center of gravity of the solar system attract each other with force $F(v)$.

However, the same consequence can be had by adding to $TN$ the two alternative hypotheses:

**H1:** The center of gravity of the solar system has absolute velocity $w$.

**E1:** Two electrified bodies moving with absolute velocity $v + w$ attract each other with force $F(v)$.

More generally, for each theory $TN(v)$ there is an electromagnetic theory $E(v)$ such that $E(0)$ is Maxwell's and all the combined theories $TN(v)$ plus $E(v)$ are empirically equivalent.

There is no originality in this observation, which Poincare discusses the equivalent immediately after the passage I cited above. Only familiar examples, but rightly stated, are needed, it seems, to show the feasibility of concepts of empirical adequacy.

We still need a general account of empirical adequacy and equivalence. It is here that the syntactic approach has most conspicuously failed. A theory $T$ is perceived as identifiably the set of its theorems in a specified language. This language has a vocabulary, divided into two classes—the observational and theoretical terms. Let the first class be $E$; then the empirical import of theory $T$ was said to be its subtheory $TE$—those theorems expressible in that subvocabulary. $T$ and $E$ were declared empirically equivalent if $TE$ was the same as $T^E$.

Obvious questions were raised and settled. Craig showed that, under suitable conditions, $TIE$ is axiomatizable in the vocabulary $E$. Logicians attached importance to the question of whether $TIE$ and $T$ are equivalent in $E$. And this was apparently enough to make philosophers think them important.

The distinction between observational and theoretical terms was more debatable, and some changed the division into "old" and "newly introduced" terms. But all this is mistaken. Empirical import cannot be isolated in this syntactic fashion. If that could be done, then $TIE$ would say exactly what $T$ says about what is observable, and nothing else. But consider: the quantum theory, Copenhagen version, says that there are things which sometimes have a position in space and sometimes do not. This consequence I have just stated without using theoretical terms. Newton's theory $TN$ implies that there is something (to wit, Abso
lute Space) which neither has a position nor occupies space. As long as unobservable entities differ systematically from observa
table entities with respect to observable characteristics, $TIE$ will say that there are such things if $T$ does.

The reduced theory $TIE$ is not a description of the observable part of the world of $T$; rather, it is a hobbled and hamstrung version of $T$'s description of everything. Empirical equivalence fares as badly. In sec-

**The idea that theories may have hidden virtues by allowing successful extensions to new kinds of phenomena, is too pretty to be left. Nor is it a very new idea. In the first lecture of his *Cours de philosophie positive*, Comte referred to Fourier's theory of heat as showing the emptiness of the debate between partisans of calorific matter and kinetic theory. The illustrations of empirical equivalence have that regrettable tendency to date; calorifics lost. Federico Enriques sought to pose a question on the exact reason when he wrote: "The hypotheses which are indifferent in the limited sphere of the actual theories acquire significance from the point of view of their possible extension." To dissociate this suggestion, we must ask what exactly is an extension of a theory.

Suppose that experiments really had confirmed the combined theory $TN(0)$ plus $E(0)$. In that case mechanics would have won a victory. The claim that $TN(0)$ was empirically adequate would have borne out by the facts. But such victorious extensions could never count for a theory as against another rival or equivalent.

Therefore, if Enriques's idea is to be correct, there must be another sort of extension, which is really a defeat—but qualified. For a theory $T$ may have an easy or obvious modification which is empirically adequate, while another theory empirically equivalent to $T$ does not. One example may be the superiority of Newton's celestial mechanics over the variant produced by Brian Ellis; Ellis himself seems to be of this opinion. This is a pragmatic superioriety and cannot suggest that theories, empirically equivalent in the sense explained, can nevertheless have different empirical import.
tion 11, TN(0) and TNE must be empirically equivalent, but the above remark about TN shows that TNO(0) is not TNEU. To eliminate these arguments, extensions of theories were considered in attempts to redefine empirical equivalence.10 But these have similar absurd consequences.

The worst consequence of the syntactic approach was surely the way it focused philo-
sophical attention on irrelevant technical questions. The expressions ‘theoretical object’ and ‘observational predicate’ mark category mistakes. Terms may be the-
oretical, but ‘observable’ classifies putative
entities. Hence there cannot be a ‘the-
oretical/observational’ distinction. It is true
surely that elimination of all theory-laden
terms leaves no usable language; also that ‘observable’ is as vague as ‘bald’. But these
facts imply not at all that ‘observable’
marks an unreal distinction. It refers quite
clearly to our limitations, the limits of obser-
vation, which are not incapacitating, but also
not negligible.

VII

The phenomena are saved when they are exhibited as fragments of a larger unity. For
that very reason it would be strange if scienti-
ﬁc theories described the phenomena, the observable parts, in terms from the rest of the
world they describe. And so an attempt to draw the conceptual line between phenomena
and the transphenomenal by means of a distinction of vocabulary, must always have been
simple to be good.

Not all philosophers who discussed unob-
servables, by any means, did so in terms of
vocabulary. But there was a common assump-
tion underlying the different positions that
marked it is philosophical. Hence it must be
drawn, if at all, by philosophical analysis and, if
attacked, by philosophical arguments. This
attitude needs a Grand Reversal. If there are limits to observation, hence, empirical,
and must be described by
empirical science. The classiﬁcation marked
by “observable” must be of entities in the
world of science. And science, in giving con-
tent to the distinction, will reveal how much
we believe when we accept it as empirically
adequate.

A future Uniﬁed Science may detail the
limits of observation exactly; meanwhile, extant theories are as silent on them. We saw
Newton’s delineation; for relativity the-
tory, we have two revealing studies by Clark
Glymour. The ﬁrst shows that local (hence, I
should think, measurable) quantities do not
uniquely determine global features of space-
time.11 The second shows that these
features also are not uniquely determined by
structures each lying wholly within some
absolute region.12 I should think, by
observable structures. It is the theory of
relativity itself, after all, that places an ab-
bolute limit on the information we can gather,
through the limiting function of the speed
of light.

In the foundations of quantum mechan-
ics much more attention has been given to
measurement. Much of the discussion is
about necessary limitations: the role of noise in ampliﬁcation, or the distinction between
macro- and micro-observables.12 Yet we
have no such clarity as Glymour gave us for
relativity theory, concerning the extent to
which macro-structure determines micro-
structure. The debate over scientiﬁc realism
may at least have the virtue of directing
attention to such questions.

Science itself distinguishes the observa-
table that it postulates from the whole it postu-
lates. The distinction, being in part a distinc-
tion of the limits science discloses on human
observation, is anthropocentric. But, since
science places human observers among the
physical systems it means to describe, it also
gives itself the task of describing anthropo-
centric distinctions. It is in this way that even
the scientiﬁc realist must observe a distinc-
tion between the phenomena and the trans-
phenomenal in the scientiﬁc world picture.

VIII

I have laid some philosophical misfortunes
at the door of a mistaken orientation toward
syntax. The alternative is to say that theories
are presented directly by describing their
models. But does this really introduce a new
element? When you give the theorems of T,
you give the set of models of T—namely, all
those structures which satisfy the theorems.
And, if you give the models, you give at least
the set of the theorems of T—namely, all those
sentences which are satisﬁed in all the mod-
els. Does it not follow that we can as advan-
tageously identify T with its theorems as
with its models?

But there is an ellipsis in the argument. It is
best understood that there is a speciﬁc lan-
guage L which is the one language that
belongs to T. And indeed, the theorems of T
in L determine, and are determined by, the set
of structures in L (that is, structures in which
L is interpreted) in which those theorems are satisﬁed. However, the assumption
that there is a language L which plays this role for T places important restric-
tions on what the set of models of T can be like.

A theory provides, among other things, a
speciﬁcation (more or less complete) of the
parts of its models that are to be direct
instances of the structures described in mea-
surement reports. In the case of Newton’s
mechanics, I called those parts motions; in
general, let us call them empirical substruc-
tures. The structures described in measure-
ment reports we may continue to call
appearances. A theory is empirically adequate
exactly if all appearances are isomorphic to
empirical structures in at least one of its
models. Theorists can therefore no stronger
than theory T exactly if, for each model M
of T, there is a model M′ of T′ such that all
empirical structures of M are isomorphic
empirical structures of M′. Theories T
and T′ are empirically equivalent exactly if nei-
ther is empirically stronger than the other.
In that case, as an easy corollary, each is
empirically adequate if and only if the other
is.

In section V, I distinguished two kinds of
extensions, the ﬁrst a sort of victory and the
second a sort of defeat. Let us call the ﬁrst a
equip extension: this simply narrows the class
of models. We may accept a theory empirically
minimal if it is not empirically equivalent to
any of its proper extensions. Glymour has
convincedly argued, in the work cited
above, that General Relativity is not empirically minimal. The reason, in my
present terms, that only local properties of
space-time enter the descriptions of the appearances, but models may differ in
global properties. This is a further non-
trival example of empirical equivalence.

The second sort of extension I shall not
try to deﬁne precisely. The idea is that mod-
els of the theory may differ in structure
rather than that of the empirical substruc-
tures. In that case the theory is not empirically
minimal, but this may put it in the
advantageous position of offering mod-
ing possibilities when radically new
phenomena could arise. An example may yet be offered by hidden-variable theories in
quantum mechanics.13

In terms of the concepts now at our dis-
posal, and the examples given, we can con-
clude that there are indeed nontrivial cases
of empirical equivalence, non-uniqueness,
and extendability, both proper and
improper. Such cases are now seen to be
quite possible even if the formulation of the
theory has not a single case called
observed, in some way. And now it should be
possible to state the issue of scientiﬁc
realism, which concerns our epistemic
titude toward theories rather than their
internal structure.

All the results of measurements are not in
them; they are never all in. Therefore we
cannot know what all the appearances are.
We cannot say that a theory is adequate,
that all the appearances will ﬁt (i.e.,
empirical substructures of) its models.
Though we cannot know this with certainty,
we can reasonably believe it. All this is the
same (but not only for that reason) but
truth as well. Yet there are two distinct epistemic attitudes that can be taken: we can
accept a theory (accept it as empirically ade-
te) or believe the theory (believe it to be
true). We can take either of these attitudes to produce a literally true story about
the world, or simply to produce accounts that
are empirically adequate. This is the issue of scientific realism versus nonrealism.

The intransitive distinction between the
observable and the unobservable is an
Nancy Cartwright

THE REALITY OF CAUSES IN A WORLD OF INSTRUMENTAL LAWS

INTRODUCTION

Empiricists are notoriously suspicious of causes. They have not been equally wary of laws. Hume set the tradition when he replaced causal facts with facts about generalizations. Modern empiricists do the same. But nowadays Hume's generalizations are the laws and equations of high-level scientific theories. On current accounts, there may be some question about where the laws of our fundamental theories get their necessity: but it is no question that these laws are the core of modern science. Bertrand Russell is well known for this view:

"The law of gravitation will illustrate what occurs in any exact science... Certain differential equations can be found, which hold at every instant for every particle of the system... But there is nothing that could be properly called 'cause' and nothing that could be properly called 'effect' in such a system."

For Russell, causes 'though useful to daily life and in the infancy of a science,' tend to be displaced by quite different laws as soon as a science is successful.

It is convenient that Russell talks about physical laws. If the laws he praises are its fundamental equations—Hamilton's equations or Schrodinger's, or the equations of general relativity. That is what I want to discuss too. But I hold just the reverse of Russell's view in favor of causes and opposed to laws. I think that, given the way modern theories of mathematical physics work, it makes sense only to believe their causal claims and not their explanatory laws.

1. EXPLAINING BY CAUSES

Following Bromberger, Scriven, and others, we know that there are various things one can be explaining in science. Two are of importance here: in explaining a phenomenon one can cite the causes of that phenomenon; or one can set the phenomenon in a general theoretical framework. The framework of modern physics is mathematical, and good explanations will generally allow us to make quite precise calculations about the phenomena we explain.

Rene Thom remarks the difference between these two kinds of explanations, though he thinks that only the causes really explain: 'Descartes with his vorticis, his hooked atoms, and the like explained everything and calculated nothing; Newton, with the very same mathematics, explained everything and explained nothing."

Unlike Thom, I am happy to call both explanation, so long as we do not illicitly attribute to them the explanatory features that apply only to causal explanation. There is a tradition, since the time of Aristotle, of deliberately conflating the two. But I shall argue that they function quite differently in modern physics. If we accept Descartes's causal story as adequate, we must count his claims about hooked atoms and vortices true. But we do not use Newton's inverse square law as if it were either true or false.

One powerful argument speaks against my claim and for the truth of explanatory laws—the argument from coincidence. Those who take laws seriously tend to subscribe to what Gilbert Harman has called inference to the best explanation. They assume that the fact that a law explains provides evidence that the law is true. The more diverse the phenomena that it explains, the more likely it is to be true. It would be an absurd coincidence if a wide variety of different kinds of phenomena were all explained by a particular law, and yet were not in reality connected from the point of view. Thus the argument from coincidence supports a good many of the inferences we make to best explanations.

The method of inference to the best explanation is subject to an important constraint, however—the requirement of non-redundancy. We cannot infer the truth of an explanation only if there are no alternatives that account in an equally satisfactory way for the phenomena. In physics nowadays, I shall argue, an adequate causal story is supposed to satisfy this requirement. But exactly the opposite is the case with the specific equations and models that make up our theoretical explanations. There is redundancy of theoretical treatment, but not of causal account.

There is, I think, a simple reason for this: causes make their effects happen. We begin with a phenomenon which, relative to our other general beliefs, we think would not occur unless something peculiar brought it about. In physics we often mark this belief by labelling the phenomena as effects—the Seerbi effect, the laser effect. An effect needs something to bring it about, and the peculiar features of the