

XIII

FUNDAMENTALISM vs THE PATCHWORK OF LAWS

NANCY CARTWRIGHT

I

For realism. A number of years ago I wrote *How the Laws of Physics Lie*. That book was generally perceived to be an attack on realism. Nowadays I think that I was deluded about the enemy: it is not *realism* but *fundamentalism* that we need to combat.

My advocacy of realism—local realism about a variety of different kinds of knowledge in a variety of different domains across a range of highly differentiated situations—is Kantian in structure. Kant frequently used what should be a puzzling argument form to establish quite abstruse philosophical positions (\emptyset): We have X—perceptual knowledge, freedom of the will, whatever. But without \emptyset (the transcendental unity of apperception, or the kingdom of ends) X would be impossible, or inconceivable. Hence \emptyset . The objectivity of local knowledge is my \emptyset ; X is the possibility of planning, prediction, manipulation, control, and policy setting. Unless our claims about the expected consequences of our actions are reliable, our plans are for nought. Hence knowledge is possible.

What might be found puzzling about the Kantian argument form are the X's from which it starts. These are generally facts that appear in the clean and orderly world of pure reason as refugees with neither proper papers nor proper introductions, of suspect worth and suspicious origin. The facts that I take to ground objectivity are similarly alien in the clear, well-lighted streets of reason, where properties have exact boundaries, rules are unambiguous, and behaviour is precisely ordained. I know that I can get an oak-tree from an acorn, but not from a pine-cone; that nurturing will make my child more secure; that feeding the hungry and housing the homeless will make for less misery; and that giving more smear tests will lessen the incidence of vaginal cancer. Getting closer to physics, which is ultimately

Reprinted from *Proceedings of the Aristotelian Society*, 93/2 (1994): 279–92, by permission of the Editor of the Aristotelian Society. © 1994.

our topic here, I also know that I can drop a pound coin from the upstairs window into the hands of my daughter below, but probably not a paper tissue; that I can head north by following my compass needle (so long as I am on foot and not in my car), that . . .

I know these facts, even though they are vague and imprecise, and I have no reason to assume that that can be improved on. Nor, in many cases, am I sure of the strength or frequency of the link between cause and effect, nor of the range of its reliability. And I certainly do not know in any of the cases which plans or policies would constitute an optimal strategy. But I want to insist that these items are items of knowledge. They are, of course, like all genuine items of knowledge (as opposed to fictional items like sense-data or the synthetic a priori) defeasible and open to revision in the light of further evidence and argument. If I do not know these things, what do I know, and how can I come to know anything?

Besides this odd assortment of inexact facts, we also have a great deal of very precise and exact knowledge, chiefly supplied by the natural sciences. I am not thinking here of abstract laws, which as an empiricist I take to be of considerable remove from the world to which they are supposed to apply, but rather of the precise behaviour of specific kinds of concrete systems, knowledge of, say, what happens when neutral K-mesons decay, which allows us to establish c-p violation, or of the behaviour of SQUIDS (superconducting quantum interference devices) in a shielded fluctuating magnetic field, which allows us to detect the victims of strokes. This knowledge is generally regimented within a highly articulated, highly abstract theoretical scheme.

One cannot do positive science without the use of induction, and where those concrete phenomena can be legitimately derived from an abstract scheme, they serve as a kind of inductive base for that scheme. *How the Laws of Physics Lie* challenged the soundness of these derivations and hence of the empirical support for the abstract laws. I still maintain that these derivations are generally shaky, but that is not the point I want to make here. So let us for the sake of argument assume the contrary: the derivations are deductively correct, and they use only true premisses. Then, granting the validity of the appropriate inductions,¹ we have reason to be realists about the laws in question. But that does not give us reason to be fundamentalists. To grant that a law is true—even a law of ‘basic’ physics or a law about the so-called fundamental particles—is far from admitting that it is universal, that it holds everywhere and governs in all domains.

¹ These will depend on the circumstances and on our general understanding of the similarities and structures or kinds and essences that obtain in those circumstances.

II

Against fundamentalism. Return to my rough division of law-like items of knowledge into two categories: (1) those that are legitimately regimented into theoretical schemes, these generally, though not always, being facts about behaviour in highly structured, manufactured environments like a spark chamber; (2) those that are not. There is a tendency to think that all facts must belong to one grand scheme, and, moreover, that this is a scheme in which the facts in the first category have a special and privileged status. They are exemplary of the way nature is supposed to work. The others must be made to conform to them. This is the kind of fundamentalist doctrine that I think we must resist. Biologists are clearly already doing so on behalf of their own special items of knowledge. Reductionism has long been out of fashion in biology, and now emergentism is again a real possibility. But the long-debated relations between biology and physics are not good paradigms for the kind of anti-fundamentalism I urge. Biologists used to talk about how new laws emerge with the appearance of 'life'; nowadays they talk, not about life, but about levels of complexity and organization. Still, in both cases the relation in question is that between larger, richly endowed, complex systems, on the one hand, and fundamental laws of physics, on the other: it is the possibility of 'downwards' reduction that is at stake.

I want to go beyond this. Not only do I want to challenge the possibility of downwards reduction, but also the possibility of 'cross-wise reduction'. Do the laws of physics that are true of systems (literally true, we may imagine for the sake of argument) in the highly contrived environments of a laboratory or inside the housing of a modern technological device, do these laws carry across to systems, even systems of very much the same kind, in different and less regulated settings? Can our refugee facts always, with sufficient effort and attention, be remoulded into proper members of the physics community, behaving tidily in accord with the fundamental code? Or must—and should—they be admitted into the body of knowledge on their own merit?

In moving from the physics experiment to the facts of more everyday experience, we are not only changing from controlled to uncontrolled environments, but often from micro to macro as well. In order to keep separate the issues which arise from these two different shifts, I am going to choose for illustration a case from classical mechanics, and will try to keep the scale constant. Classical electricity and magnetism would serve as well. Moreover, in order to make my claims as clear as possible, I shall consider the simplest and most well-known example, that of Newton's third law and its application to falling bodies: $F = ma$. Most of us, brought

up within the fundamentalist canon, read this with a universal quantifier in front: for any body in any situation, the acceleration it undergoes will be equal to the force exerted on it in that situation divided by its inertial mass. I want instead to read it, as indeed I believe we should read *all* nomologicals, as a *ceteris paribus* law: for any body in any situation, *if nothing interferes*, its acceleration will equal the force exerted on it divided by its mass. But what can interfere with a force in the production of motion other than another force? Surely there is no problem: the acceleration will always be equal to the *total* force divided by the mass. That is just what I want to question.

Think again about how we construct a theoretical treatment of a real situation. Before we can apply the abstract concepts of basic theory—assign a quantum field, a tensor, a Hamiltonian, or in the case of our discussion, write down a force function—we must first produce a model of the situation in terms the theory can handle. From that point the theory itself provides ‘language-entry rules’ for introducing the terms of its own abstract vocabulary, and thereby for bringing its laws into play. *How the Laws of Physics Lie* illustrated this for the case of the Hamiltonian—which is roughly the quantum analogue of the classical force function. Part of learning quantum mechanics is learning how to write the Hamiltonian for canonical models—for example, for systems in free motion, for a square well potential, for a linear harmonic oscillator, and so forth. Ronald Giere (1988) has made the same point for classical mechanics.

The basic strategy for treating a real situation is to piece together a model from these fixed components; and then to determine the prescribed composite Hamiltonian from the Hamiltonians for the parts. Questions of realism arise when the model is compared with the situation it is supposed to represent. *How the Laws of Physics Lie* argued that even in the best cases, the fit between the two is not very good. I concentrated there on the best cases, because I was trying to answer the question ‘Do the explanatory successes of modern theories argue for their truth?’ Here I want to focus on the multitude of ‘bad’ cases, where the models, if available at all, provide a very poor image of the situation. These are not cases that disconfirm the theory. You can’t show that the predictions of a theory for a given situation are false until you have managed to describe the situation in the language of the theory. When the models are too bad a fit, the theory is not disconfirmed; it is just inapplicable.²

Now consider a falling object. Not Galileo’s from the leaning tower, nor the pound coin I earlier described dropping from the upstairs window,

² Here I follow Alan Musgrave (1981: 381): ‘We do not falsify a theory containing a domain assumption by showing that this assumption is not true of some situations . . . ; we merely show that that assumption is not applicable to that situation in the first place.’

but rather something more vulnerable to non-gravitational influence. Otto Neurath has a nice example. My doctrine about the case is much like his.

In some cases a physicist is a worse prophet than a [behaviourist psychologist], as when he is supposed to specify where in St. Stephen's Square a thousand dollar bill swept away by the wind will land, whereas a [behaviourist] can specify the result of a conditioning experiment rather accurately. (1933: 13)

Mechanics provides no model for this situation. We have only a partial model, which describes the 1,000-dollar bill as an unsupported object in the vicinity of the earth, and thereby introduces the force exerted on it due to gravity. Is that the total force? The fundamentalist will say no: there is in principle (in God's completed theory?) a model in mechanics for the action of the wind, albeit probably a very complicated one that we may never succeed in constructing. This belief is essential for the fundamentalist. If there is no model for the 1,000-dollar bill in mechanics, then what happens to the note is not determined by its laws. Some falling objects, indeed a very great number, will be outside the domain of mechanics, or only partially affected by it. But what justifies this fundamentalist belief? The successes of mechanics in situations that it can model accurately do not support it, no matter how precise or surprising they are. They show only that the theory is true in its domain, not that its domain is universal. The alternative to fundamentalism that I want to propose supposes just that: mechanics is true, literally true we may grant, for all those motions whose causes can be adequately represented by the familiar models that get assigned force functions in mechanics. For these motions, mechanics is a powerful and precise tool for prediction. But for other motions, it is a tool of limited serviceability.

Let us set our problem of the 1,000-dollar bill in St Stephen's Square to an expert in fluid dynamics. The expert should immediately complain that the problem is ill defined. What exactly is the bill like: is it folded or flat? straight down the middle, or . . . ? is it crisp or crumpled? how long versus wide? and so forth and so forth and so forth. I do not doubt that when answers can be supplied, fluid dynamics can provide a practicable model. But I do doubt that for every real case, or even for the majority, fluid dynamics has enough of the 'right questions' to ask to allow it to model the full set of causes, or even the dominant ones. I am equally sceptical that the models that work will do so by legitimately bringing Newton's laws (or Lagrange's for that matter) into play.³ How, then, do airplanes stay afloat?

³ And the problem is certainly not that a quantum or relativistic or microscopic treatment is needed instead.

Two observations are important. First, we do not need to maintain that no laws obtain where mechanics runs out. Fluid dynamics may have loose overlaps and intertwining with mechanics. But it is in no way a subdiscipline of basic physics; it is a discipline on its own. Its laws can direct the 1,000-dollar bill as well as can those of Newton or Lagrange. Second, the 1,000-dollar bill comes as it comes, and we have to hunt a model for it. Just the reverse is true of the plane. We build it to fit the models we know work. Indeed, that is how we manage to get so much into the domain of the laws we know.

Many will continue to feel that the wind and other exogenous factors must produce a force. The wind after all is composed of millions of little particles which must exert all the usual forces on the bill, both at a distance and via collisions. That view begs the question. When we have a good-fitting molecular model for the wind, and we have in our theory (either by composition from old principles or by the admission of new principles) systematic rules that assign force functions to the models, and the force functions assigned predict exactly the right motions, then we will have good scientific reason to maintain that the wind operates via a force. Otherwise, the assumption is another expression of fundamentalist faith.

III

Ceteris paribus laws versus ascriptions of natures. If the laws of mechanics are not universal, but nevertheless true, there are at least two options for them. They could be pure *ceteris paribus* laws: laws that hold only in circumscribed conditions or so long as no factors relevant to the effect besides those specified occur. And that's it. Nothing follows about what happens in different settings or in cases where other causes occur. Presumably this option is too weak for our example of Newtonian mechanics. When a force is exerted on an object, the force will be relevant to the motion of the object even if other causes for its motion not renderable as forces are at work as well; and the exact relevance of the force will be given by the formula $F = ma$: the (total) force will contribute a component to the acceleration determined by this formula. For cases like this, the older language of *natures* is appropriate. It is in the nature of a force to produce an acceleration of the requisite size. That means that, *ceteris paribus*, it will produce that acceleration. But even when other causes are at work, it will 'try' to do so. The idea is familiar in the case of forces: *trying* to produce an acceleration, F/m , consists in actually producing F/m as a vector compo-

ment to the total acceleration. In general, what counts as ‘trying’ will differ from one kind of cause to another. To ascribe a behaviour to the nature of a feature is to claim that that behaviour is exportable beyond the strict confines of the *ceteris paribus* conditions, although usually only as a ‘tendency’ or a ‘trying’. The extent and range of the exportability will vary. Some natures are highly stable; others are very restricted in their range. The point here is that we must not confuse a wide-ranging nature with the universal applicability of the related *ceteris paribus* law. To admit that forces tend to cause the prescribed acceleration (and indeed do so in felicitous conditions) is a long way from admitting that $F = ma$ is universally true.⁴ In the next sections I will describe two different meta-physical pictures in which fundamentalism about the experimentally derived laws of basic physics would be a mistake. The first is wholism; the second, pluralism. It seems to me that wholism is far more likely to give rise only to *ceteris paribus* laws, whereas natures are more congenial to pluralism.

IV

Wholism. We look at little bits of nature, and we look under a very limited range of circumstances. This is especially true of the exact sciences. We can get very precise outcomes, but to do so, we need very tight control over our inputs. Most often we do not control them directly, one by one, but rather we use some general but effective form of shielding. I know one experiment that aims for direct control—the Stanford Gravity Probe. Still, in the end, they will roll the spaceship to average out causes they have not been able to command. Sometimes we take physics outside the laboratory. Then shielding becomes even more important. SQUIDS (superconducting quantum interference devices) can make very fine measurements of magnetic fluctuations, which helps in the detection of stroke victims. But for administering the tests, the hospital must have a Hertz box—a small metal room to block out magnetism from the environment. Or, for a more homely example, we all know that batteries are not likely to work if their protective casing has been pierced.

We tend to think that shielding cannot matter to the laws we use. The same laws apply both inside and outside the shields; the difference is that inside the shield we know how to calculate what the laws will produce, but

⁴ I have written more about the two levels of generalization, laws, and ascriptions of natures, in 1989. See also my 1992.

outside, it is too complicated. Wholists are wary of these claims: if the events we study are locked together, and changes depend on the total structure rather than the arrangement of the pieces, we are likely to be very mistaken by looking at small chunks of special cases.

Consider a scientific example, the revolution in communications technology due to fibre optics. Low-loss optical fibres can carry information at rates of many gigabits per second over spans of tens of kilometres. But the development of fibre bundles which lose only a few decibels per kilometre is not all there is to the story. Pulse-broadening effects intrinsic to the fibres can be truly devastating. If the pulses broaden as they travel down the fibre, they will eventually smear into each other, and destroy the information. That means that the pulses cannot be sent too close together, and the transmission rate may drop to tens or at most hundreds of megabits per second.

We know that is not what happens—the technology has been successful. That's because the right kind of optical fibre in the right circumstance can transmit solitons—solitary waves that keep their shape across vast distances. I'll explain why. The light intensity of the incoming pulse causes a shift in the index of refraction of the optical fibre, producing a slight non-linearity in the index. The non-linearity leads to what is called a 'chirp' in the pulse. Frequencies in the leading half of the pulse are lowered, while those in the trailing half are raised. The effects of the chirp combine with those of dispersion to produce the soliton. Stable pulse shapes are not at all a general phenomenon of low-loss optical fibres. They are instead a consequence of two different, oppositely directed processes. The pulse widening due to the dispersion is cancelled by the pulse narrowing due to the non-linearity in the index of refraction. We can indeed produce perfectly stable pulses. But to do so, we must use fibres of just the right design, and matched precisely with the power and input frequency of the laser that generates the input pulses. By chance, that was not hard to do. When the ideas were first tested in 1980, the glass fibres and lasers readily available were easily suited to each other. Given that very special match, fibre optics was off to an impressive start.

Solitons are indeed a stable phenomenon. They are a feature of nature, but of nature under very special circumstance. Clearly it would be a mistake to suppose that they were a general characteristic of low-loss optical fibres. The question is, how many of the scientific phenomena we prize are like solitons, local to the environments we encounter, or—more importantly—to the environments we construct? If nature is more wholistic than we are accustomed to think, the fundamentalist's hopes to export the laws of the laboratory to the far reaches of the world will be dashed.

It is clear that I am not very sanguine about the fundamentalist faith. But that is not really out of the kind of wholist intuitions I have been sketching. After all, the story I just told accounts for the powerful successes of the 'false' local theory—the theory that solitons are characteristic of low-loss fibres—by embedding it in a far more general theory about the interaction of light and matter. Metaphysically, the fundamentalist is borne out. It may be the case that the successful theories we have are limited in their authority, but their successes are to be explained by reference to a truly universal authority. I do not see why we need to explain their successes. I am prepared to believe in more general theories when we have direct empirical evidence for them. But not merely because they are the 'best explanation' for something which seems to me to need no explanation to begin with. 'The theory is successful in its domain': the need for explanation is the same whether the domain is small or large or very small or very large. Theories are successful where they are successful, and that's that. If we insist on turning this into a metaphysical doctrine, I suppose it will look like metaphysical pluralism, to which I now turn.

V

The patchwork of laws. Metaphysical nomological pluralism is the doctrine that nature is governed in different domains by different systems of laws not necessarily related to each other in any systematic or uniform way: by a patchwork of laws. Nomological pluralism opposes any kind of fundamentalism. We are here concerned especially with the attempts of physics to gather all phenomena into its own abstract theories. In *How the Laws of Physics Lie* I argued that most situations are brought under a law of physics only by distortion, whereas they can often be described fairly correctly by concepts from more phenomenological laws. The picture suggested was of a lot of different situations in a continuum, from ones that fit not perfectly but not badly to those that fit very badly indeed. I did suggest that at one end fundamental physics might run out entirely ('What is . . . the value of the electric field vector in the region just at the tip of my pencil?'), whereas in transistors it works quite well. But that was not the principal focus. Now I want to draw sharp divides: some features of systems typically studied by physics may get into situations where their behaviour is not governed by the laws of physics at all. But that does not mean that they have no guide for their behaviour or only low-level phenomenological laws. They could fall under a quite different organized set of highly abstract principles.

There are two immediate difficulties that metaphysical pluralism encounters. The first is one we create ourselves, by imagining that it must be joined with views that are vestiges of metaphysical monism. The second is, I believe, a genuine problem that nature must solve.

First. We are inclined to ask: how can there be motions not governed by Newton's laws? The answer: there are causes of motion not included in Newton's theory. Many find this impossible because, although they have forsaken reductionism, they cling to a near-cousin: *supervenience*. Suppose we give a complete 'physics' description of the falling object and its surrounds. Mustn't that fix all the other features of the situation? Why? This is certainly not true at the level of discussion at which we stand now: the wind is cold and gusty; the bill is green and white and crumpled. These properties are independent of the mass of the bill, the mass of the earth, the distance between them.

I suppose, though, I have the supervenience story wrong. It is the microscopic properties of physics that matter; the rest of reality supervenes on them. Why should I believe that? Supervenience is touted as a step forward over reductionism. Crudely, I take it, the advantage is supposed to be that we can substitute a weaker kind of reductionism, 'token-token' reductionism, for the more traditional 'type-type' reductionism which was proving hard to carry out. But the traditional view had arguments in its favour. Science does sketch a variety of fairly systematic connections between micro-structures and macro-properties. Often the sketch is rough; sometimes it is precise; usually its reliability is confined to very special circumstances. Nevertheless, there are striking cases. But these cases support type-type reductionism; they are irrelevant for supervenience. Type-type reductionism has well-known problems: the connections we discover often turn out to look more like causal connections than like reductions; they are limited in their domain; they are rough rather than exact; and often we cannot even find good starting proposals where we had hoped to produce nice reductions. These problems suggest modifying the doctrine in a number of specific ways, or perhaps giving it up altogether. But they certainly do not leave us with token-token reductionism as a fall-back position. After all, on the story I have just told, it was the appearance of some degree of systematic connection that argued in the first place for the claim that micro-structures fixed macro-properties. But it is just this systematicity that is missing in token-token reductionism.

The view that there are macro-properties that do not supervene on micro-features studied by physics is sometimes labelled 'emergentism'. The suggestion is that where there is no supervenience, macro-properties must miraculously come out of nowhere. But why? There is nothing of the

newly landed about these properties. They have been here in the world all along, standing beside the properties of physics. Perhaps we are misled by the feeling that the set of properties studied by physics is complete. Indeed, I think that there is a real sense in which this claim is true, but that sense does not support the charge of emergentism. Consider how the domain of properties studied by physics gets set. Here is one caricature: we begin with an interest in motions—deflections, trajectories, orbits. Then we look for the smallest set of properties that is closed (or, closed enough) under prediction. That is, we expand the set until we get all the factors that are causally relevant to our starting factors, and then everything causally relevant to those, and so forth. To succeed does not show that we have gotten all the properties there are. This is a fact we need to keep in mind quite independently of the chief claim of this paper, that the predictive closure itself only obtains in highly restricted circumstances. The immediate point is that predictive closure among a set of properties does not imply descriptive completeness.

Second. The second problem that metaphysical pluralism faces is that of consistency. We do not want colour patches to appear in regions from which the laws of physics have carried away all matter and energy. Here are two stories I have told in teaching the mechanical philosophy of the seventeenth century. Both are about how to write the Book of Nature to ensure that a consistent universe can be created. In one story God is very interested in physics. He carefully writes out all of the law of physics, and lays down the initial distribution of matter and energy in the universe. He then leaves to St Peter the tedious but intellectually trivial job of calculating all future happenings, including what, if any, macroscopic properties and macroscopic laws will emerge. That is the story of reductionism. Metaphysical pluralism supposes that God is instead very concerned about laws, and so he writes down each and every regularity that his universe will display. In this case St Peter is left with the gargantuan task of arranging the initial properties in the universe in some way that will allow all God's laws to be true together. The advantage to reductionism is that it makes St Peter's job easier. God may nevertheless choose to be a metaphysical pluralist.

VI

Conclusion. I have argued that the laws of our contemporary science are, to the extent that they are true at all, at best true *ceteris paribus*. In the nicest cases we may treat them as claims about natures. But we have no

grounds in our experience for taking our laws—even our most fundamental laws of physics—as universal. Indeed I should say ‘*especially* our most fundamental laws of physics’, if these are meant to be the laws of fundamental particles. For we have virtually no inductive reason for counting these laws as true of fundamental particles outside the laboratory setting—if they exist there at all. Ian Hacking is famous for his remark ‘If you can spray them, they exist’. I have always agreed with that. But I would now be more cautious: ‘*When* you can spray them, they exist.’

The claim that theoretical entities are created by the peculiar conditions and conventions of the laboratory is familiar from the social constructionists. The stable low-loss pulses I described earlier provide an example of how that can happen. Here I want to add a caution, not just about the existence of the theoretical entities outside the laboratory, but about their behaviour.

Hacking’s point is not only that when we can use theoretical entities in just the way we want to produce precise and subtle effects, they must exist; but also that it must be the case that we understand their behaviour very well if we are able to get them to do what we want. That argues, I believe, for the truth of some very concrete, context-constrained claims, the claims we use to describe their behaviour and control them. But in all these cases of precise control, we build our circumstances to fit our models. I repeat: that does not show that it must be possible to tailor our models to fit every circumstance.

Perhaps we feel that there could be no real difference between the one kind of circumstance and the other, and hence no principled reason for stopping our inductions at the walls of our laboratories. But there is a difference: some circumstances resemble the models we have; others do not. And it is just the point of scientific activity to build models that get in, under the cover of the laws in question, all and only those circumstances that the laws govern.⁵ Fundamentalists see matters differently. They want laws; they want true laws; but most of all, they want their favourite laws to be in force everywhere. I urge that we resist fundamentalism. Reality may well be just a patchwork of laws.⁶

REFERENCES

- Cartwright, N. (1983). *How the Laws of Physics Lie*. Oxford: Clarendon Press.
 — (1989). *Nature’s Capacities and their Measurement*. Oxford: Oxford University Press.

⁵ Or, in a more empiricist formulation that I would prefer, ‘that the laws accurately describe’.

⁶ Paper first presented at a meeting of the Aristotelian Society, held in the Senior Common Room, Birkbeck College, London, 9 May 1994.

- (1992). 'Aristotelian Natures and the Modern Experimental Method.' In J. Earman (ed.), *Inference, Explanation and Other Philosophical Frustrations*, 44–71. Berkeley: University of California Press.
- Giere, R. N. (1988). *Explaining Science: A Cognitive Approach*. Chicago: University of Chicago Press.
- Musgrave, A. (1981). 'On Interpreting Friedman.' *Kyklos*, 34/3: 377–87.
- Neurath, O. (1933). 'United Science and Psychology.' In B. F. McGuinness (ed.), *Unified Science*, 1987:1–23. Dordrecht: Reidel.

THE PHILOSOPHY OF SCIENCE

edited by

DAVID PAPINEAU

OXFORD UNIVERSITY PRESS

1996

Oxford University Press, Walton Street, Oxford OX2 6DP

Oxford New York

Athens Auckland Bangkok Bombay
Calcutta Cape Town Dar es Salaam Delhi
Florence Hong Kong Istanbul Karachi
Kuala Lumpur Madras Madrid Melbourne
Mexico City Nairobi Paris Singapore
Taipei Tokyo Toronto

and associated companies in
Berlin Ibadan

Oxford is a trade mark of Oxford University Press

Published in the United States
by Oxford University Press Inc., New York

Introduction and selection © Oxford University Press 1996

All rights reserved. No part of this publication may be reproduced, stored in a retrieval system, or transmitted, in any form or by any means, without the prior permission in writing of Oxford University Press. Within the UK, exceptions are allowed in respect of any fair dealing for the purpose of research or private study, or criticism or review, as permitted under the Copyright, Designs and Patents Act, 1988, or in the case of reprographic reproduction in accordance with the terms of the licences issued by the Copyright Licensing Agency. Enquiries concerning reproduction outside these terms and in other countries should be sent to the Rights Department, Oxford University Press, at the address above

This book is sold subject to the condition that it shall not, by way of trade or otherwise, be lent, re-sold, hired out or otherwise circulated without the publisher's prior consent in any form of binding or cover other than that in which it is published and without a similar condition including this condition being imposed on the subsequent purchaser

British Library Cataloguing in Publication Data
Data available

Library of Congress Cataloging in Publication Data

Papineau, David, 1947–

The philosophy of science / David Papineau.

(Oxford readings in philosophy)

Includes bibliographical references and index.

1. Science—Philosophy. 2. Realism. I. Title. II. Series.
Q175.P3375 1996 501—dc20 95-49209

ISBN 0-19-875164-8

ISBN 0-19-875165-6 (pbk.)

1 3 5 7 9 10 8 6 4 2

Typeset by Best-set Typesetter Ltd., Hong Kong
Printed in Great Britain
on acid-free paper by
Bookcraft (Bath) Ltd
Midsomer Norton, Avon