Introduction

Science is measurement; capacities can be measured; and science cannot be understood without them. These are the three major theses of this book.

The third thesis could be more simply put: capacities are real. But I do not want to become involved in general issues of realism, instrumentalism, or idealism. Rather, I want to focus on the special case of causes and capacities, and why we need them. They are a part of our scientific image of the world, I maintain, and cannot be eliminated from it. I use the term 'scientific image';[1] but one should not take the narrow view that this image is projected from theory alone. Until recently the philosophy of science has focused primarily on articulated knowledge. To learn what Newtonian physics or Maxwell's electrodynamics teaches, we have turned to what these theories *say*. To answer, 'Are there causal powers in Maxwell's electromagnetic world?' we have studied Maxwell's laws to see if they describe causal powers or not. This is in part what I will do. I claim that the laws of electromagnetic repulsion and attraction, like the law of gravity, and a host of other laws as well, are laws about enduring tendencies or capacities. But it is not all that I am going to do. I arrive at the need for capacities not just by looking at the laws, but also by looking at the methods and uses of science. I maintain, as many do today,[2] that the content of science is found not just in its laws but equally in its practices. We learn what the world is like by seeing what methods work to study it and what kinds of forecasts can predict it; what sorts of laser can be built, or whether the economy can be manipulated. I am going to argue that our typical methodologies and our typical

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[1] I take the idea of the scientific image from Wilfrid Sellars, who contrasts the world as constructed by science with the world of everyday objects, which he calls the 'manifest' image. Cf. W. Sellars, *Science Perception and Reality* (London: Routledge & Kegan Paul, 1963). This is also the usage of Bas van Fraassen in *The Scientific Image* (Oxford: Clarendon Press, 1983).

[2] Cf. *Science in Context*, 3 (1988); T. Lenoir (ed.), *Practice, Context, and the Dialogue between Theory and Experiment*.

applications, both in the natural sciences and in the social sciences, belong to a world governed by capacities, and indeed cannot be made sense of without them.

My position is opposed to the tradition of Hume. I begin with the assumption that the causal language of science makes sense, and that causality is an objective feature of our scientific image of nature. That does not yet separate me from the Humean tradition. Hume too took causal claims to have an analogue in reality. He began with singular causal claims, looking for some special connection between the individual cause and its effect, a connection that would be strong enough to constitute causation. He failed to find anything more than spatio-temporal contiguity, so he moved to the generic level. This marks the first stage in the Hume programme: (1) for Hume, singular causal facts are true in virtue of generic causal facts. But the programme was far bolder: at the generic level causation was to disappear altogether. It was to be replaced by mere regularity. This is the second thesis of the Hume programme: (2) generic causal facts are reducible to regularities. This book challenges both theses. It begins with the claim that, even if the association is law-like, neither regular association nor functional dependence can do the jobs that causality does. Working science needs some independent notion of causal law.

What kind of a concept could this be? I maintain that the Hume programme has things upside down. One should not start with the notion of generic causation at all. Singular causal claims are primary. This is true in two senses. First, they are a necessary ingredient in the methods we use to establish generic causal claims. Even the methods that test causal laws by looking for regularities will not work unless some singular causal information is filled in first. Second, the regularities themselves play a secondary role in establishing a causal law. They are just evidence—and only one kind of evidence at that—that certain kinds of singular causal fact have happened.[3]

It is the singular fact that matters to the causal law because that is what causal laws are about. The generic causal claims of science are

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[3] I share this view that probabilities are evidence for causal claims and not constitutive of them with David Papineau, but for quite different reasons. Papineau argues that causal truths must be universal associations and not mere probabilistic ones, whereas I maintain that no regularity of any sort can guarantee a causal claim. See D. Papineau, 'Probabilities and Causes', *Journal of Philosophy*, 82 (1985), 57–74.

not reports of regularities but rather ascriptions of capacities, capacities to make things happen, case by case. 'Aspirins relieve headaches.' This does not say that aspirins always relieve headaches, or always do so if the rest of the world is arranged in a particularly felicitous way, or that they relieve headaches most of the time, or more often than not. Rather it says that aspirins have the capacity to relieve headaches, a relatively enduring and stable capacity that they carry with them from situation to situation; a capacity which may if circumstances are right reveal itself by producing a regularity, but which is just as surely seen in one good single case. The best sign that aspirins can relieve headaches is that on occasion some of them do.

My claims, then, are doubly anti-Humean.[4] I take singular causes to be primary, and I endorse capacities. Nonetheless, I am in sympathy with Hume's staunchly empiricist outlook. I want to argue for the centrality of singular causes and capacities in an empiricist world. That is why this book begins with the sentence 'Science is measurement.' This motto is meant to mark the kind of empiricism I presuppose: a strong practical empiricism, which for better or for worse wants to make a difference to how science carries on. It is a kind of operationalism, but without the theory of meaning. Empiricists have traditionally been concerned with two different questions: (*a*) where do we get our concepts and ideas? and (*b*) how should claims to empirical knowledge be judged? The empiricist answer to the first question is: 'Ideas come immediately from experience.' It is the second question that matters for my project and not the first. Indeed, I shall throughout ignore questions about meanings and the source of ideas. In a sense I will be returning to an early form of British empiricism uncontaminated by the Cartesian doctrine of ideas, an empiricism where causal connections not only made sense but where they were in principle observable. Joseph Glanvill, in his estimable apology for the mechanical philosophy which won him his fellowship of the Royal Society in 1664, tells us that Adam could see these connections distinctly:

> Thus the accuracy of his knowledge of natural effects, might probably arise from his sensible perceptions of their causes. . . . His sight could inform him whether the Loadstone doth attract by Atomical Effluviums; . . . The Mysterious influence of the Moon, and its causality on the seas motion, was

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[4] Though see below, ch. 5.

> no question in his Philosophy, no more than a Clocks motion is in ours, where our senses may inform us of its cause.[5]

Like Glanvill and his more scientific colleagues—Robert Boyle or Robert Hooke or Henry Power—I shall not worry about what causal connections are, but ask rather, 'How do we learn about them once our sight has been clouded by the fall from grace?' My concern is not with meaning but with method, and that is why I give prominence to the second empiricist question, where the point is not to ground the concepts of science in pure observation or in direct experience. It is rather to ensure that claims to scientific knowledge are judged against the phenomena themselves. Questions about nature should be settled by nature—not by faith, nor metaphysics, nor mathematics, and not by convention nor convenience either. From Francis Bacon and Joseph Glanvill to Karl Popper and the Vienna Circle empiricists have wanted to police the methods of enquiry to ensure that science will be true to nature. That is the tradition in which I follow.

I look to scientists as well as philosophers for ideas and inspiration. William Thomson (Lord Kelvin) was a physicist who maintained just such an empiricism as I assume here. Kelvin's concepts of work and potential crystallized the shift in structure of late nineteenth-century mechanics, and his views on entropy and waste shaped the newly emerging thermodynamics. He wanted to take over electromagnetic theory too, but he lost in a long battle with Maxwell, a battle fought in part over Maxwell's infamous invention of the unmeasurable and unvisualizable displacement current. Kelvin also laid the Atlantic cable, and that for him was doing science, as much as fashioning theories or measuring in the laboratory. Indeed, the two activities were inseparable for him. The recent biography of Kelvin by Crosbie Smith and Norton Wise describes how practice and theory should join in Kelvin's philosophy. According to Smith and Wise, Kelvin's

> deep involvement in the Atlantic cable sheds considerable light on his lifelong rejection of Maxwellian electromagnetic theory. The laying and working of the cable required the complete unity of theory and practice that he had always preached . . . [Kelvin's] natural philosophy . . . opposed the metaphysical to the practical. In proscribing mathematical analogies that extended beyond direct experimental observation, it eliminated the flux-

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[5] J. Glanvill, *The Vanity of Dogmatizing* (London, 1661), 6–7.

> force duality from his flow analogies to electricity and magnetism. Maxwell's theory, by contrast, introduced a physical entity which no one had ever observed, the displacement current. But most specifically, [Kelvin] opposed 'metaphysical' to 'measurable', and it is this aspect that the telegraph especially reinforced. The only aspects of Maxwell's theory that [Kelvin] would ever applaud were those related to measurements.[6]

In this passage we see that Kelvin followed the empiricist convention: what he did not like was called 'metaphysics' and consigned to the flames. Yet the real enemy for Kelvin was not so much metaphysics, as philosophers think of it, but instead a kind of abstract and non-representational mathematics. There is a famous saying, 'Maxwell's theory is Maxwell's equations.' That saying is meant to excuse the fact that Maxwell's theory gives no coherent physical picture. A theory need not do that, indeed perhaps should not. What is needed is a powerful mathematical representation that will work to save the phenomena and to produce very precise predictions. Kelvin called this view about mathematics 'Nihilism'. He wanted the hypotheses of physics to describe what reality was like, and he wanted every one of them to be as accurate and as sure as possible.

It is probably this aspect of his empiricism that needs to be stressed in our contemporary philosophical discourse. Each scientific hypothesis should be able to stand on its own as a description of reality. It is not enough that a scientific theory should save the phenomena; its hypotheses must all be tested, and tested severally. This, then, is an empiricism opposed at once to wholism and to the hypothetico-deductive method. The logic of testing for such an empiricism is probably best modelled by Clark Glymour's bootstrap theory of confirmation:[7] the evidence plus the background assumptions deductively imply the hypothesis under test. But the point is entirely non-technical. Scientific hypotheses should be tested, and the tests should be reliable. They should be powerful enough to give an answer one way or another. The answers will only be as sure as the assumptions that ground the test. But it is crucial that the uncertainty be epistemological and not reside in the test itself. That is why I call this empiricism a kind of operationalism, and stress the idea of

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[6] C. Smith and N. Wise, *Energy and Empire: A Biographical Study of William Thomson, Lord Kelvin* (Cambridge: Cambridge University Press, 1988), ch. 13.

[7] C. Glymour, *Theory and Evidence* (Princeton, NJ: Princeton University Press, 1980).

measurement.[8] Measuring instruments have this kind of ability to read nature. If the measuring instrument operates by the principles that we think it does, and if it is working properly, and if our reading of its output is right, then we know what we set out to learn. A measurement that cannot tell us a definite result is no measurement at all.

My emphasis on measurement and on the bootstrap methodology should make clear that this empiricism is no kind of foundationalism. It will take a rich background both of individual facts and of general assumptions about nature before one can ever deduce a hypothesis from the data; the thin texture of pure sense experience will never provide sufficient support. Nevertheless, this measurement-based empiricism is a stringent empiricism, too stringent indeed to be appealing nowadays, especially in modern physics.[9] Consider the demand for renormalizable theories in quantum electrodynamics, or the constraints imposed on the range of cosmological models by the desire to eliminate singularities. In both cases it is mathematical considerations that shape the theory, and not judgements imposed by the phenomena: 'nihilism' in Kelvin's language. Einstein is one powerful opponent to views like Kelvin's. Let him speak for the rest:

> It is really our whole system of guesses which is to be either proved or disproved by experiment. No one of the assumptions can be isolated for separate testing.[10]

It is not the intention of this book to argue that Einstein or the renormalizers are wrong. I do not want to insist that science must be empiricist. Rather, I want to insist that the practical empiricism of

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[8] My early papers and lectures on this operationalist-style empiricism used a different terminology and form from that used here. A test of a hypothesis, I maintained, should be *totally reliable*. Davis Baird had taught me to see the exact analogy between tests on one hand and instruments and measurements on the other. Cf. D. Baird, 'Exploratory Factor Analysis, Instruments and the Logic of Discovery', *British Journal for the Philosophy of Science*, 38 (1987), 319–37.

[9] For more about this sort of empiricism and the kind of impact it can have in physics, see N. Cartwright, 'Philosophical Problems of Quantum Theory: The Response of American Physicists', in L. Krüger, G. Gigerenzer, and M. Morgan (eds.), *The Probabilistic Revolution*, ii (Cambridge, Mass., MIT Press, 1987), 417–37.

[10] A. Einstein and L. Infeld, quoted in A. Fine, *The Shaky Game* (Chicago, Ill.: Chicago University Press, 1987), 88–9.

measurement is the most radical empiricism that makes sense in science. And it is an empiricism that has no quarrel with causes and capacities. Causal laws can be tested and causal capacities can be measured as surely—or as unsurely—as anything else that science deals with. Sometimes we measure capacities in a physics laboratory or, as in the gravity probe experiment I will discuss, deep in space in a cryogenic dewar. These are situations in which we can control precisely for the effects of other perturbing factors so that we can see in its visible effects just what the cause can do. But most of the discussion in this book will bear on questions that matter outside physics, in the social sciences, in medicine, in agriculture, and in manufacturing strategies for quality control; that is, in any area where statistics enter. I ask, 'Can probabilities measure causes?' The answer is 'Yes'—but only in a world where capacities and their operations are already taken for granted.

The opening phrase of this introduction was the motto for the Cowles Commission for Economic Research: science is measurement. The Cowles Commission initiated the methods most commonly used in econometrics in America today, and its ideas, in a very primitive form, play a central role in my argument. I will focus on the structures of econometrics in this book, but not because of either the successes or the failures of econometrics as a science; rather because of the philosophic job it can do. We may see intuitively that correlation has something to do with causality. But intuition is not enough. We need an argument to connect probabilities with causes, and we can find one in econometrics.

I have sounded so far as if my principal conflicts were with Hume. In fact that is not true. Unlike Hume, I begin by assuming the current commonplace that science presupposes some notion of necessity: that there is something somewhere in nature that grounds the distinction between a genuine law and a mere accidental generalization. What I deny is that that is enough. Bertrand Russell maintained that science needed only functional laws like the equations of physics and had no place for the notion of cause.[11] I think that science needs not only causes but capacities as well. So I stand more in opposition to Russell than to Hume, or more recently to covering-law theorists like

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[11] B. Russell, 'On the Notion of Cause', *Proceedings of the Aristotelian Society*, 13 (1913), 1-26.

C. G. Hempel[12] and Ernest Nagel,[13] who accept laws but reject capacities. The most I will grant the covering-law view is that we need both.

My ultimate position is more radical. The pure empiricist should be no more happy with laws than with capacities, and laws are a poor stopping-point. It is hard to find them in nature and we are always having to make excuses for them: why they have exceptions—big or little; why they only work for models in the head; why it takes an engineer with a special knowledge of real materials and a not too literal mind to apply physics to reality.[14] The point of this book is to argue that we must admit capacities, and my hope is that once we have them we can do away with laws. Capacities will do more for us at a smaller metaphysical price.

The book is organized to take one, step by step, increasingly far from the covering-law view. I begin not with the concept of capacity but with the more innocuous-seeming notion of a causal law. Chapters 1 and 2 argue that causal laws are irreducible to equations and regular associations. Nevertheless, they fit into an empiricist world: they can be measured. Chapter 3 introduces singular causes; and finally Chapter 4, capacities. John Stuart Mill plays a major role in the discussion of Chapter 4 because Mill too was an advocate of capacities, or in his terminology 'tendencies'; and my views and arguments are essentially the same as Mill's in modern guise. Laws about tendencies are arrived at for Mill by a kind of abstraction. That sets the theme for Chapter 5. Abstraction is the key to the construction of scientific theory; and the converse process of concretization, to its application. Covering laws seem irrelevant to either enterprise. Chapter 6 gives a concrete example of a question of current scientific interest where capacities matter: 'Do the Bell inequalities show that causality is incompatible with quantum mechanics?' The question cannot be answered if we rely on probabilities and associations alone. It takes the concept of capacity and related notions of how capacities operate even to formulate the problem correctly.

I close with a word about terminology, and some disclaimers. My

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[12] C. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, NJ: Prentice-Hall, 1966).

[13] E. Nagel, *The Structure of Science* (New York: Harcourt, Brace & World, 1961).

[14] See arguments in N. Cartwright, *How the Laws of Physics Lie* (Oxford: Clarendon Press, 1983).

capacities might well be called either 'propensities' or 'powers'.[15] I do not use the first term because it is already associated with doctrines about how the concept of probability should be interpreted; and, although I think that capacities are often probabilistic, I do not think that probability gets its meaning from capacities. I do not use the word 'power' because powers are something that individuals have and I want to focus, not on individuals, but on the abstract relation between capacities and properties. I use the non-technical term 'carries' to refer to this relation: 'Aspirins carry the capacity to relieve headaches' or 'Inversions in populations of molecules carry the capacity to lase'. Does this mean that there are not one but two properties, with the capacity sitting on the shoulder of the property which carries it? Surely not. However, I cannot yet give a positive account of what it does mean—though Chapter 5 is a step in that direction. My aims in this book are necessarily restricted, then: I want to show what capacities do and why we need them. It is to be hoped that the subsequent project of saying what capacities are will be easier once we have a good idea of how they function and how we find out about them. The same should be true of singular causal processes as well; though here the problem is somewhat less pressing, since there are several good accounts already available, notably by Ellery Eells,[16] Wesley Salmon,[17] and the author closest to my own views, Wolfgang Spohn.[18]

[15] For propensities in the sense of dispositions see H. Mellor, *The Matter of Chance* (Cambridge: Cambridge University Press, 1971). For more on powers, see R. Harré and E. Madden, *Causal Powers* (Oxford: Blackwell, 1975).

[16] See E. Eells, *Probabilistic Causality*, forthcoming.

[17] See W. Salmon, *Scientific Explanation and the Causal Structure of the World*, Princeton, NJ: (Princeton University Press, 1984).

[18] See W. Spohn, 'Deterministic and Probabilistic Reasons and Causes', *Erkenntnis*, 19 (1983), 371–96; and 'Direct and Indirect Causes', *Topoi*, forthcoming.