dislocations to move in just the way predicted by the prevailing theory of dislocations, and took this to confirm both that theory and the fact that his images represented dislocations. In these cases, the happy match between theory and observation constituted significant support for the theory. On the other hand, the experimental situations were sufficiently messy and ill-understood to permit plenty of explanations for failure other than implicating the theory of dislocations under test. I suggest the kind of situation I have described here can be expected to occur commonly in experimental science.

Deborah Mayo's characterisation of severity is able to accommodate these examples.[1] She will ask whether the confirmations would have been likely to occur if the theory were false. Both in the case of my Copernican example and the dislocations example, the answer is that they would be very unlikely to occur. Consequently, in each case the relevant theories received significant support from the observed coincidences between theoretical prediction and observation. Mayo's conception of severity is in line with scientific practice.

Further reading

Hacking (1983) was a pioneering work in the new experimentalism. Other works in that category are Franklin (1986) and Franklin (1990), Galison (1987) and Galison (1997) and Gooding (1990). A summary of the position is given by Ackermann (1989). The most sophisticated philosophical defence of the position is Mayo (1996).

CHAPTER 14

Why should the world obey laws?

Introduction

In the foregoing chapters we have been concerned with *epistemological* questions, that is, questions concerning how scientific knowledge is vindicated by appeal to evidence, and the nature of that evidence. In this and the next chapter we turn to *ontological* questions, questions about the kinds of things there are in the world. What kinds of entities are assumed or shown to exist in the world by modern science? Part of an answer to that question has been taken for granted in this book up until now. It has been taken for granted that there are such things as laws which govern the behaviour of the world and which it is the business of science to discover. This chapter is concerned with what kinds of entities these laws are.

The idea that the world is governed by laws that it is the business of science to discover is commonplace. However, the question of what this idea amounts to is far from being unproblematic. A fundamental problem was highlighted by Robert Boyle in the seventeenth century. The notion of a law originates in the social sphere where it makes straightforward sense. Society's laws are obeyed or not obeyed by individuals who can comprehend the laws and the consequences of violating them. But once laws are understood in this natural way, how can it be said that material systems in nature obey laws? For they can hardly be said to be in a position to comprehend the laws they are meant to obey, and, in any case, a fundamental law as it applies in science is supposed to be exceptionless, so there is no correlate to an individual's violating a social law and taking the consequences. What is it that makes matter conform to laws? This is a reasonable and straightforward question, it would appear, and yet it is not one that is easily answered. I take it that Boyle's answer, namely that God makes matter behave in accordance with the laws He has ordained, leaves a lot to be desired from a modern point of view. Let us see if we can do better.

Laws as regularities

One common response to the question "What makes matter behave in accordance with laws?" is to deny its legitimacy. The line of reasoning involved here was forcefully expressed by the philosopher David Hume, and has been influential ever since. From the Humean standpoint it is a mistake to assume that lawlike behaviour is caused by anything. Indeed, the whole idea of causation in nature is brought into question. The reasoning goes like this. When, for example, two billiard balls collide, we can observe their motions immediately before and immediately after collision, and we may be able to discern a regular way in which the speeds after impact are connected to the speeds before impact, but what we never see is something in addition to this which can be identified with the causal effect of the one ball on the other. From this point of view causation is nothing other than regular connection, and laws take the form "Events of type A are invariably accompanied or followed by events of type B". For instance, Galileo's law of fall would take the form "Whenever a heavy object is released near the earth's surface it falls to the ground with a uniform acceleration". This is the so-called regularity view of laws. Nothing makes matter behave in accordance with laws because laws are nothing other than de facto regularities between events.

A standard, and telling, set of objections to the regularity view of laws involves the claim that it does not distinguish between accidental and lawlike regularities. Popper gives the example "no moa lives beyond fifty years' as an example. It may well be the case that no moa, a species now extinct, ever lived beyond fifty years, but some might well have done so had the environmental conditions been more favourable, and for this reason we are inclined to discount the generalisation as a law of nature. But it qualifies as a law on the ground that it is an exceptionless regularity. It may well be the case that whenever the factory hooter sounds at the end of the working day in Manchester the workers down tools in London, but even if there are no exceptions to this generalisation, it hardly qualifies as a law of nature. Examples of this kind abound, and they suggest that there is something more to a law of nature than mere regularity. Another difficulty with the regularity view is that it fails to identify the direction of causal dependency. There is a regular connection between instances of smoking and lung cancer, but this is because smoking causes lung cancer, not the reverse. That is why we can hope to decrease the occurrence of cancer by eliminating smoking, but cannot hope to combat smoking by finding a cure for cancer. A regularity exhibited by events is not a sufficient condition for the regularity to constitute a law for there is more to lawlike behaviour than mere regularity.

Apart from difficulties with the idea that regularities are a sufficient condition for a law, straightforward considerations about laws as they figure in science strongly suggest that regularity is not a necessary condition either. If the view that laws describe exceptionless regular connections between events is taken seriously, then none of the claims typically taken to be scientific laws would qualify. Galileo's law of fall, mentioned above, is a case in point. Autumn leaves rarely fall to the ground with a uniform acceleration. On an unqualified regularity view this would make the law false. In a similar fashion Archimedes' principle, which claims in part that objects denser than water sink, is refuted by floating needles. If laws are taken to be exceptionless regularities, then it is very difficult to find a serious candidate for a law for want of the appropriate regularities. More to the point, most if not all of the generalities taken to be laws within science fail to qualify.

From the point of view of scientific practice, and commonsense for that matter, there is a ready response to these observations. After all, it is well understood why Autumn leaves do not fall to the ground in a regular fashion. They are influenced by draughts and air-resistance which act as a disturbing influence, just as the sinking of a needle can be inhibited by surface tension. It is because physical processes are hindered by disturbing influences that physical laws characterising those processes need to be tested in contrived experimental circumstances in which the hindrances are eliminated or controlled. The regularities of relevance to science, and which are indications of lawlike behaviour, are typically the hard-won results of detailed experimentation. Think, for example, of the lengths to which Henry Cavendish had to go to get attracting spheres to exhibit the inverse square law of attraction and how J. J. Thomson eventually succeeded, where Hertz had failed, to exhibit the regular deflection of moving electrons in an electric field.

An obvious response that the defender of the regularity view of laws can give to these observations is to restate that view in a conditional form. Laws can be formulated in the form "events of type A are regularly followed, or accompanied, by events of type B provided disturbing factors are not present". So Galileo's law of fall becomes "heavy objects fall to the ground with a uniform acceleration provided they do not encounter a variable resistance or are not deflected by winds or other disturbing factors". The phrase "other disturbing factors" is indicative of a general problem concerning how a precise statement of the conditions to be satisfied for a law to apply can be formulated. But I will leave that difficulty aside, because I suggest there is a much more fundamental one facing the regularity view here. If we accept the characterisation of laws as regularities stated in conditional form, then we must accept that laws only apply when those conditions are satisfied. Since the satisfaction of the appropriate conditions will normally only obtain in special experimental set-ups, we are forced to conclude that scientific laws generally apply only within experimental situations and not outside of them. Galileo's law of fall will be considered to apply only when heavy objects are dropped in situations where air resistance and the like have been removed. So Autumn leaves are not subject to Galileo's law of fall, according to this revised version of the regularity view. Does this not clash with our intuition? Do we not wish to say that an Autumn leaf is governed by the law of fall, but is also governed by the laws governing air-resistance and aerodynamics as well, so that the resulting fall is the complicated result of the various laws acting in conjunction? Because the regularity view, in its conditional form, restricts the applicability of laws to those experimental situations where the appropriate conditions are met, it is incapable of saying anything about what happens outside of those conditions. On this view, science is incapable of saying why Autumn leaves usually end up on the ground!

The difficulty here echoes a problem which arises if the new experimentalism is taken as exhausting what can be said of scientific knowledge. For, as we saw in the previous chapter, although it may well be the case that the new experimentalism can capture a strong sense in which the progress of science can be understood as a steady accumulation of experimental knowledge, to leave it at that leaves us with no account of how knowledge arrived at inside experimental situations can be transported outside of those situations and used elsewhere. How are we to explain the engineer's use of physics, the use of radioactive dating in historical geology or the application of Newton's theory to the motion of comets? If scientific laws are assumed to apply outside, as well as inside, of experimental situations then laws cannot be identified with the regularities that are achievable in experimental situations. The regularity view of laws will not do.

Laws as characterisations of powers or dispositions

There is a straightforward way out of the problems with the idea of a law that we have so far discussed. It involves taking seriously what is implicit in much commonsense as well as science, namely that the material world is active. Things happen in the world of their own accord, and they happen because entities in the world possess the capacity or power or disposition or tendency to act or behave in the way that they do. Balls bounce because they are elastic. Warnings on containers that declare the contents to be poisonous or inflammable or explosive tell us what the contents are capable of doing or how they are inclined to act. Specifying the mass and charge of an electron indicates how it will respond to electric and magnetic fields. An important element of what a thing is, is what it is capable of doing or becoming. We need to characterise things in terms of their potential as well as their actual being, as Aristotle correctly observed. Just as the ability to grow into an oak tree is an important part of what it is to be an acorn, so the capacity to attract unlike and repel like charges, and to radiate when accelerating, is an important part of what it is to be an electron. We experiment on systems to find out how they are disposed to behave.

Once we admit such things as dispositions, tendencies, powers and capacities into our characterisation of material systems, then laws of nature can be taken as characterising those dispositions, tendencies, powers or capacities. Galileo's law of fall describes the disposition heavy objects possess to fall to the ground with a uniform acceleration and Newton's law of gravitation describes the power of attraction between massive bodies. Once we interpret laws in this way, we need no longer expect laws to describe sequences of happenings in the world because those happenings will typically be the result of several dispositions, tendencies, powers or capacities acting in conjunction in complex ways. The fact that the tendency of a leaf to fall in accordance with Galileo's law is swamped by the effect of the wind is no reason in itself to doubt that that tendency continued to act on the leaf in accordance with the law. From this point of view, we can readily understand why experiment is necessary to glean information relevant for the identification of a law. The tendencies corresponding to the law under investigation need to be separated from other tendencies, and this separation requires the appropriate practical intervention to bring it about. Given the irregularities of ocean beds and the attraction of the sun and planets as well as the moon, we cannot hope to arrive at a precise account of the tides from Newton's theory plus initial conditions. Nevertheless, gravity is the major cause of the tides and there are appropriate experiments for identifying the law of gravity.

From the point of view I am advocating, causes and laws are intimately linked. Events are caused through the action of particulars that possess the power to act as causes. The gravitational attraction of the moon is the main cause of the tides, charged particles cause the ionisation responsible for the tracks in a cloud chamber and oscillating charges cause the radio waves emitted from a transmitter. Descriptions of the mode of acting of the active powers involved in such cases constitute the laws of nature. The inverse square law of gravitation describes quantitatively the power to attract possessed by massive bodies, and the laws of classical electromagnetic theory describe, among other things, the capacity of charged bodies to attract and radiate. It is the active powers at work in nature that makes laws true when they are true. We thus have a ready answer to Boyle's question. It is the powers and capacities possessed by particulars and operative when particulars interact that compel those particulars to behave in accordance with laws. Lawlike behaviour is brought about by efficient causation. Boyle faced the problem he did with laws, and needed to invoke God, just because he declined to ascribe dispositional properties to matter.

The majority of philosophers seem reluctant to accept an ontology which includes dispositions or powers as primitive. I do not understand their reluctance. Perhaps the reasons are in part historical. Powers were given a bad name by the mystical and obscure way they were employed in the magical tradition in the Renaissance, and they are alleged to have been exploited by the Aristotelians in a cavalier way under the guise of forms. Boyle's rejection of active properties in his mechanical philosophy can be seen as a reaction, and perhaps an overreaction, to the excesses of those traditions, as well as being motivated by theological concerns. However, there need be nothing mysterious or epistemologically suspect about invoking powers, tendencies and the like. Claims concerning them can be subject to stringent empirical tests to as great an extent as any other kind of claim. What is more, however much philosophers may be averse to dispositional properties, scientists systematically invoke them and their work would be incapacitated without them. It is significant to note in this respect that Boyle, in his experimental science as opposed to his mechanical philosophy, freely employed dispositional properties such as acidity and the spring of the air. Elasticity in various forms was an embarrassment to the seventeenth-century mechanical philosophers. Hobbes complained that Boyle's attribution of elasticity to air was equivalent to the admission that air could move itself. Boyle and other seventeenth century scientists continued to employ the concept of elasticity, and never succeeded in explaining it away by reference to non-dispositional properties. Nor has anyone succeeded since. I do not understand what grounds philosophers have for questioning, or feeling the need to explain away, this common, indeed ubiquitous, usage by scientists of dispositional properties.

The view that laws characterise the dispositions, powers, capacities or tendencies of things has the merit that it acknowledges at the outset what is implicit in all scientific practice, namely that nature is active. It makes it clear what makes systems behave in accordance with laws, and it links laws with causation in a natural way. It also offers a ready solution to the problem, encountered in the previous chapter, concerning the transportability of knowledge acquired in experimental situations beyond those situations. Once the assumption is made that entities in the world are what they are by virtue of the powers and capacities that they possess, and I claim that that assumption is implicit in scientific practice as well as everyday life, then the laws describing those powers and capacities, identified in experimental situations, can be presumed to apply outside of those situations too. Nevertheless, I cannot leave things here with a good conscience, because there are important laws of science that are difficult to fit into this scheme.

Thermodynamic and conservation laws

Let us refer to the view I have outlined and defended in the previous paragraph, which understands laws as characterising causal powers, as the causal view of laws. There are important laws in physics that do not fit well into this scheme. The first and second laws of thermodynamics do not and nor do a range of conservation laws in fundamental particle physics. The first law of thermodynamics asserts that the energy of an isolated system is constant. The second law, which asserts that the entropy of an isolated system cannot decrease, has consequences such as ensuring that heat flows from hot to cold bodies and not the other way round and ruling out the possibility of extracting heat energy from the sea and putting it to useful work, where the only price paid for the work is a decrease in temperature of the sea. A machine that succeeded in doing this would be a perpetual motion machine of the second kind, distinct from a machine that results in a net increase in energy, which is a perpetual motion machine of the first kind. The first law of thermodynamics rules out perpetual motion machines of the first kind and the second law rules out perpetual motion machines of the second kind. These quite general laws have consequences for the behaviour of physical systems, and can be used to predict their behaviour, quite independently of the details of the causal processes at work. That is why it is not possible to construe these laws as causal laws.

Let me give an example that illustrates my point. If ice is subjected to pressures higher than normal atmospheric pressure its melting point is lowered. This is why a wire from which weights are suspended will cut its way through a block of ice. The explanation of this at the molecular level is far from straightforward and a precise, detailed account is probably not available. Since pressure tends to push molecules closer together, one might expect the forces of attraction between them to increase under such circumstances, leading to an increase in the thermal energy necessary to drag them apart and thus to an elevation in melting point. This is precisely what happens in a typical solid near melting point. But ice is not a typical solid. The water molecules in ice are rather loosely packed, more so than they are in the liquid state, which is why ice is less dense than water. (This is just as well, otherwise lakes and rivers would freeze from the bottom up, and would freeze in their entirety in periods of prolonged cold, thus eliminating fish and anything evolved from fish as a viable life form.) If the molecules in ice are forced closer together than normal, the force between them decreases, so less thermal energy is needed to separate them, and the melting point falls. The precise way in which the forces depend on molecular positions is complicated, depending on fine quantum mechanical detail involving exchange as well as Coulomb forces, and is not known with precision.

Given the above complications, it may come as a surprise that James Thomson was able to predict the depression of the freezing point of water with pressure in 1849 thereby anticipating the empirical discovery of the phenomenon. All he needed for his derivation were the laws of thermodynamics plus the empirically known fact that water is denser than ice. Thomson devised, in thought, a cyclic process that involved extracting heat from water at 0°C and converting it into ice at 0°C. It seemed as if this engine provided a means of extracting heat from water and converting all of it into the work done by the expansion involved, thus comprising a perpetual motion machine of the second kind, ruled out by the second law of thermodynamics. Thomson realised that this unacceptable conclusion could be blocked by assuming the freezing point to be lowered by an increase in pressure.

The feature of this case that I wish to highlight is that

Thomson's prediction was made in ignorance of the details of the causal process at the molecular level. A characteristic feature, and a major strength, of thermodynamics is that it applies at the macroscopic level whatever the details of the underlying causal process. It is precisely this feature of the laws of thermodynamics that prevents them being construed as causal laws.

The difficulties for the causal view do not stop here. The behaviour of a mechanical system can be understood and predicted by specifying the forces on each component of the system and using Newton's laws to trace the development of the system. Within this approach Newton's laws can readily be interpreted as causal laws describing the disposition of objects to exert and respond to specified forces. However, this is not the only way of dealing with mechanical systems. The laws of mechanics can also be written in a form that takes energy, rather than forces, as the starting point. In the Hamiltonian and Lagrangian formulations of mechanics, where this approach is adopted, what is required is expressions for the potential and kinetic energy of a system as a function of whatever coordinates are necessary to fix them. The evolution of a system can then be completely specified by feeding these expressions into the Hamiltonian or Lagrangian equations of motion. This can be done without a detailed knowledge of the causal processes at work.

James Clerk Maxwell (1965, vol. 2, pp. 783–4), who attempted to cast his electromagnetic theory in Lagrangian form, illustrated this point in a characteristically vivid way. We imagine a belfry in which a complicated piece of machinery is driven by bell ropes that drop to the bell ringers room below. We assume the number of ropes to be equal to the number of degrees of freedom of the system. The potential and kinetic energy of the system as a function of the position and velocity of the ropes can be determined by experiments done with the ropes. Once we have these functions we can write down Lagrange's equations for the system. It is then possible, given the positions and velocities of the ropes at any one instant, to derive their positions and velocities at any other instant. We can do this without needing to know the details of the causal story of what is happening in the belfry. Lagrange's equations do not state causal laws.

It might be objected that these observations about the Lagrangian formulation of mechanics do not constitute a serious counter-example to the causal view of laws. It might be pointed out, for example, that, although a Lagrangian treatment of the mechanism in the belfry can work as well as it does by ignoring the detailed causal story of the mechanism in the belfry, there is such a story to be had that can be formulated in Newtonian, and hence causal, terms once appropriate empirical access to the belfry is gained. After all, it might be observed, Lagrange's equations can be derived from Newton's.

This last claim is no longer true (if it ever was). In modern physics Lagrange's equations are interpreted in a more general way than the version of those equations that can be derived from Newton's laws. The energies involved are interpreted in a general way that includes all kinds of energy, not just energy arising from the motion of massive bodies under the influence of forces. For instance, the Lagrangian formulation can accommodate electromagnetic energy, which includes velocity-dependent potential energies and necessitates such things as the electromagnetic momentum of a field, which is a momentum different from that corresponding to a mass times velocity. When pushed to the limit in modern physics, these Lagrangian (or related Hamiltonian) formulations are not such that they can be replaced by the causal accounts that underlie them. For instance, the various conservation principles, such as conservation of charge and parity, intimately connected with symmetries in the Lagrangian function of the energies, are not explicable by reference to some underlying process.

The outcome of all this can be summarised as follows. A wide range of laws within physics can be understood as causal laws. When this is possible, there is a ready answer to Boyle's

question concerning what it is that compels physical systems to behave in accordance with laws. It is the operation of the causal powers and capacities characterised by laws that make systems obey them. However, we have seen that there are fundamental laws in physics that cannot be construed as causal laws. In these cases there is no ready answer to Boyle's question. What makes systems behave in accordance with the law of conservation of energy? I don't know. They just do. I am not entirely comfortable with this situation, but I don't see how it can be avoided.

Further reading

For a different view of laws than the one characterised here, and for a detailed critique of the regularity view, see Armstrong (1983). The way in which experiment points towards the causal view of laws is shown in Bhaskar (1978). Cartwright (1983) casts doubt on the idea that there can be fundamental laws that are true of the world, but modifies her views to defend something more like the causal view in her 1989 text. The clash between how many philosophers characterise laws and the notion of laws employed by scientists is described with interesting examples in Christie (1994). The material of this chapter is largely derived from, and is dealt with in a little more detail in, Chalmers (1999). Another recent discussion of the nature of laws is van Fraassen (1989).

A.F. Chalmers

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"Like all young men I set out to be a genius, but mercifully laughter intervened."

Clea Lawrence Durrell