1.1 The Dethronement of Laws in Science

Nancy Cartwright

From the faceless particles of fundamental physics to marshes, mountains, and rain forests, fleas, walruses, and traffic jams, we used to live in a world governed by eternal, all encompassing laws, laws discovered by the experiments of physics and encoded in its mathematical equations. At one time these laws were laid down by God, who commanded all His creations to act in accord with them, without exception or excuse. After a while God faded from the world that science pictured -- it seemed nature needed only the laws to know how to carry on, relentlessly, predictably. Now the laws are fading too.

This 400-year old image of the governance of nature by eternal, universal laws is today being undermined by exciting new modes of understanding across the sciences, including physics and biology, as well, perhaps less surprisingly, as in the study of society. There is order visible in the world, and invisible. But if we trust to these new ways of understanding, this need not be order by universal law. It can be local, piecemeal, and contextual -- much like the world as we see it around us.

We live our everyday lives in a dappled world quite unlike the world of fundamental particles regimented into kinds, each just like the one beside it, mindlessly marching exactly as has forever been destined. The everyday world is one where the future is open, little is certain and the unexpected intrudes into the best laid plans, where everything is different from everything else, where things change and develop, where different systems built in different ways give rise to different patterns.

Philosophers call the reflective picture of what we experience in our everyday lives, the *manifest image;* 'manifest', as the *Concise Oxford Dictionary* says: ‘shown plainly to the eye or mind’. The philsophical contrast is with the *scientific image*, the image of the world as our best science pictures it. For centuries since the Scientific Revolution, the two have been at odds. But many of the ways we do science today bring the scientific and the manifest images into greater harmony. For much of science understands and models what was previously unintelligible e devises methods to predict what will happen in the future (and with very great precision) and builds new technologies to manipulate the world around us without resort to universal laws.

This breakdown of universal law is not along any one fissure nor provoked by any one great discovery like the quantum of action, nor does it emerge from just one science. Rather, it appears in many distinct, highly detailed studies of scientific practice. Though generally unrelated to one another, these diverse studies have in common a radical split from the standard view. They propose alternatives to universal laws as the central explanatory and predictive mechanisms of employed in the sciences. This questioning of the order of science has come from analyses of successful scientific practice across the disciplines, from fundamental physics through biology to political economy.

The reaction against the received view of scientific laws as universal and exceptionless has been as diverse and widespread as the scientific disciplines from which criticisms have been developed. No one view is the same as any other. But they have in common a picture in which laws are not the immutable and exceptionless governors of a nature completely ordered under them. In the next section of this book you will find detailed studies from the natural and social sciences. Here I provide a brief overview of some of the recent more general work exploring the dethronement of universal laws in the two natural sciences discussed there: biology and physics.

Biology

Biology has long been criticized: It is not a proper science because it does not have proper laws. Now those who study its various practices and the many impressive successes they produce are fighting back. Perhaps the traditional view of what counts as proper science with proper laws has been mistaken all along. Contemporary biology seems to have just what it takes to describe nature successfully and to put its knowledge to use.

One of the most vocal scholars arguing that this is in general true across biology and even far more widely is University Pittsburgh philosopher of biology, Sandra Mitchell:

Take…Mendel's law of segregation. That law says in all sexually reproducing organisms, during gamete formation each member of an allelic pair separates from the other member to form the genetic constitution of an individual gamete. So, there is a 50:50 ratio of alleles in the mass of the gametes. In fact, Mendel's law does not hold universally. We know two unruly facts about this causal structure. First, this rule applied only after the evolution of sexually reproducing organisms, an evolutionary event that, in some sense, need not have occurred. Second, some sexually reproducing organisms don't follow the rule because they experience meiotic drive… Does this mean [that] Mendel's law of segregation is not a "law"? [1]

From hosts of cases in biology like this various authors conclude that rather than good old-fashioned 'proper laws' biology offers instead:

- Laws that emerge historically
- Laws that are contingent.

These two come naturally from Mitchell's first two sources of unruliness. They also conclude that biology offers only

[1] Mitchell, S. 2002, p. 334

- Laws that are not exceptionless.

This is a natural account given Mitchell's second source of unruliness.

Different kinds of cases far from evolutionary biology, in molecular or neurobiology for instance, lead others to propose that biology studies not laws that describe regular behaviour that must occur but rather

- Mechanisms that functioning properly, in the right places, can generate regular behaviour, for instance the interactions of the structures of non-RNA strands with tRNA molecules and ribosomes to underwrite protein synthesis.

Mechanisms are a central topic of our chapter on biology in the Section 2 of this book.

Mitchell also points to authors who claim that biology studies

- Ceteris paribus laws, laws that hold only in special circumstances.

She herself advocates something more practical and this is a piece of advice that she proposes to carry outside of biology across the disciplines, from economics to physics:

We need to rethink the idea of a scientific law pragmatically or functionally, that is, in terms of what scientific laws let us do rather than in terms of some ideal of a law by which to judge the inadequacies of the more common (and very useful) truths [of the kind biology teaches].[2]

Mitchell argues that we should do away with the old dichotomy 'law vs. non-law' or what is universal, exceptionless, immutable versus all the rest, to be replaced by a sliding scale, and along a variety of different dimensions:

[All] general truths we discover about the world vary with respect to their degree of contingency on the conditions upon which the relationships described depend. Indeed, it is true that most of the fundamental laws of physics are more generally applicable, i.e. are more stable over changes in context, in space and time, than are the causal relations we discover to hold in the biological world. They are closer to the ideal causal connections that we choose to call 'laws'. Yet, few of even these can escape the need for the ceteris paribus clause to render them logically true.[3]

---

[2] Mitchell, S. 2002, p. 333
[3] Mitchell, S. 2002, p. 331

Looking at how the successes of science are produced across the disciplines, it is truths of varying forms with varying degrees of universality and exceptionlessness, describing various degrees and kinds of order that let us do what we need to do to produce those successes.

Emergence and vertical reduction

Mitchell's final remarks bring us to physics, which is eventually where the conversation goes when order is challenged: 'Surely the world of physics is totally ordered and if physics is ordered, so is all the rest.' Notice that there are two claims here. The first is that the world of physics is totally ordered. The second is that physics fixes all the rest. This second doctrine usually goes under the title 'reductionism' but we call it 'vertical reductionism'. (You will see why in a moment.)

The opposite of vertical reduction is generally labelled 'emergence': the idea that the more 'basic' levels of reality do not determine or fix what happens at 'higher' levels; that new phenomena, new characteristics, even new laws of nature emerge at larger dimensions, more mass, higher velocities or increased complexity. There has been a great deal of work on emergence recently and there are many non-technical accounts available of the central ideas and issues so I will comment only briefly on it before turning to our central topic for physics and to our topic of focus there, what might be thought of as *horizontal reduction:* the idea that the cover of natural law, at any one level or crossing all levels, may not be complete; that order may remain to be made in nature by us, not just discovered.

In philosophy of science, vertical reductionism probably reached its zenith in the early 1960s, in the wake of Ernst Nagel's *The Structure of Science*, which attempted a rigorous reconstruction of classical mechanics. But those who attempted to carry on Nagel's project outside classical mechanics soon found that classical mechanics was, at best, a special case where reductive strategies came close to working. And indeed they did not ever truly meet the standards of demonstration even there.

Here is a partial list of problems:[4]

- Often important features of the macrosystem were left unexplained by the supposed reduction base. For example, the supposed reduction of the ideal gas laws to the collisions of particles in statistical mechanics is often held up as a paradigmatic example of successful reduction. But even here, a crucial thermodynamic property – the direction of entropy – is left unexplained. Statistical mechanics is temporally symmetrical, thermodynamics is not.

[4] Thanks due to Martin Carrier for this list.

- In some sciences, the classification of objects uses functional principles – say, reproduction and nutrition in biology, or organs in anatomy. It may well be that a micro explanation of each individual organism can explain why a certain process works as reproduction or nutrition, or a particular organ functions as a heart. But human hearts are very different anatomically from earthworm hearts, and there is an even greater difference between the mechanisms of nutrition and reproduction of animals, plants, protista and fungi. And so what unites human and earthworm hearts into a single category, or digestion and photosynthesis into types of nutrition, is not explained by looking at the subsystems.

- Some scientific kinds, like biological species, have an irreducibly historic dimension. If there are beings just like us structurally on some distant world but not members of a common lineage, they are, from a biological standpoint – or at least one important way of understanding species within biology – different species.

- Macrosystems may also have properties that are not predictable from lower levels and may play an important top-down organizing role, as for instance in living systems, where there are important jobs for high-level regulative processes that exert top-down control.

- The categories and processes typical of a macrosystem may also need to be understood in terms of how that system is embedded in an even larger system, for example when we are describing a feedback system or engaged in ecological biology.

- And perhaps most fundamentally, there are multiple ways of decomposing a macrosystem into parts. Consider the case of an organism – say the various ways one needs to understand the workings of a human body in order to have a grasp of modern medicine. These include of course a way of decomposing the organism into more basic anatomical components. But there are also biological "systems" that do not break down across anatomical lines, but are distributed all over the organism, like nutrition and the immune system. Gene expression also takes places everywhere in the organism. And while the endocrine system has a few anatomical organs that produce hormones, the activity of these likewise takes place throughout the organism. Moreover, medicine is very much in the business of classifying the functioning of an organism as normal and abnormal in various ways, at least some of which require appeal to the organism's relationship to its environment and in some cases to the ancestral environment for which a system is adapted.

Physics and horizontal reduction

Let us turn now to horizontal reduction: Does physics offer laws that dictate a complete order among the very kinds of events that physics studies? Before looking at reasons in recent studies for thinking not, it is good to make sure the question is clear. One usual question debated by philosophers and physicists alike is

Realism: Are the well-confirmed laws of physics likely to be 'approximately' true?

This book on the whole takes a realist stance: Their remarkable success at precise prediction and at new technologies give us warrant to suppose that our modern sciences are getting at the truth. This is controversial in some quarters. We propose to sidestep this controversy because the horizontal reduction we focus on is about a very different question, about the dominion of physics:

Dominion: Does physics dictate everything that happens in the physical world?

This question is not about whether the laws of physics are true but about how much territory they govern. A number of different kinds of considerations about the way physics functions when it functions best at predicting and intervening into the world around us argue that the answer is no.

Ceteris Paribus laws. The first suggests that, just like the laws of biology as Mitchell sees them, the laws of physics are *ceteris paribus* laws: laws that come with the caveat that they hold so long as 'other things are equal' or, closer to what is really intended, 'in particular circumstances'.

Consider philosopher of physics Marc Lange:

To state the law of thermal expansion [which says that the change in length of an expanding metal bar is directly proportional to the change in temperature]… one would need to specify not only that no-one is hammering the bar on one end, but also that the bar is not encased on four of its six sides in a rigid material that will not yield as the bar is heated, and so on. [5]

There is naturally a canonical reply by defenders of the universal rule of physics. The list indicated by 'and so on'

…is indefinite only if expressed in a language that purportedly avoids terminology from physics, the condition is easily stated: The "law" of thermal expansion is rigorously true if there are no external boundary stresses on the bar throughout the process.[6]

---

[5] Quoted in Earmon, J., Roberts, J.T, & Smith,S. 2002, p. 284
[6] Earmon, J., Roberts, J.T, & Smith,S. 2002, p.284

This brings into focus the technical terms of physics, like “stress”. How far do these stretch? Here the very virtues of physics get it into trouble. The terminology of physics is tightly controlled. This is what distinguishes it from disciplines that hardly count as science at all, that use terminology like ‘globalisation’ or ‘unconscious desire’, terms that have no such rigid criteria for their application. There are rules in physics for how to use language, how, for instance, to ascribe a quantum field or a classical force. Rules like $F = GMm/\mathbf{r}^2$ <u>when</u> two masses a distance **r** apart interact. And in most situations there are a number of factors affecting the outcome that we do not know how to describe using these regimented descriptions. Yes, the law of thermal expansion may be exactly true if the restriction ‘and there are no external boundary stresses’ is added. But will it continue to be rigorous? Is ‘no external boundary stresses’ a proper scientific description with precise rules for its application, or is it rather, as defenders of ceteris paribus laws argue, a loose catch-all term that marks that the law is not universal and that there is no universal using proper physics concepts that can stand in its stead?

Consider the Stanford Gravity Probe Experiment, on which I was for a while a participant observer. After over two decades in development the Gravity Probe put four gyroscopes into space to test the prediction of the general theory of relativity that the gyroscopes would precess due to coupling with space-time curvature. The Stanford Gravity Probe prediction about its gyroscopes was about as condition free as any claim in physics about the real world could be. That’s because the experimenters spent a vast amount of time – over twenty years – and exploited a vast amount of knowledge from across physics and engineering. The experimenters tried to fix it so that all other causes of precession are missing; hence all the other causes would be, *ipso facto,* **describable in the language of physics**! Moreover if they had not succeeded and other causes occurred, then **any that they couldn’t describe would make precise prediction impossible**. If you can’t describe it, you can’t put it into your equations.

It should be no surprise then that all the good confirmations of the laws of physics occur in the special situations where we can describe all the causes with proper physics concepts. That is the real content of the ‘ceteris paribus’ clause: This law holds so long as all the factors that affect the behaviours under study can be described by proper physics concepts that have the kind of strict rules for their application that good, rigorous science demands; it holds only in specially structured environments where we can describe all the causes at work – indeed these are the only environments where we can produce such predictions. Whether there is systematicity outside environments structured like this is speculation. So too is the assumption that all environments are secretly structured in the right way, even if we have not yet discovered it.

Laboratories are structured in the right way, and lasers, batteries and bicycles. So too are a very great many naturally occurring situations. The planetary system is so structured and seems to have little disturbance that cannot be subsumed under proper physics concepts. But most situations do not seem to be structured in the right way. In physics there is no rule that takes you from “the bar is being hammered” to “the bar is subject to certain stresses”. The rules for assigning terms like “stress” require not loose terms of everyday use to apply but a far more technical, regimented vocabulary. Stresses and strains are characterized as forces and force functions, as I already noted, have strict rules for application, rules like $F = GMm/\mathbf{r}^2$ <u>when</u> two masses **r** apart interact, $f = \in_0 q_1 q_2/\mathbf{r}^2$

when two charges interact, and so forth. And it is not clear that every time a bar is hammered one of these more technical descriptions obtains. Physics is above all an exact science. Its concepts must be precise, measurable and fit in exact mathematical laws. This means that they may well not be able to describe everything that affects even outcomes reasonably supposed to be in physics' own domain.

Those who argue that the laws of physics are for the most part ceteris paribus, holding in special circumstances rather than universally, are generally great admirers of physics. There is no implication in this suggestion that these laws are not getting at the truth. You can be a realist about ceteris paribus laws, and for the same reasons that are often alleged in support of the approximate truth of universal laws: The power the laws of physics – now seen as ceteris paribus laws – exhibit to produce precise predictions and to help us build fabulous new technologies to intervene in the world and to change it.

In defense of the universality of the laws one can of course *say* that the bar must be subject to a stress because of the way its behaviour is affected. But that reduces physics to the status of psychoanalysis. We can *say* that I have certain unconscious desires because of the odd way my behaviour is affected. A physics that allows us to say things on that kind of basis is not the physics that yields the precise predictions and exact control of nature that gives us reason to think its laws are true. Better to suppose the laws are ceteris paribus than to deprive them of their power to predict precisely and of the huge empirical support this power provides.

Incommensurability. A second kind of study arguing against horizontal reduction looks at the interactions among different kinds of physicists, for example instrument physicists, experimentalists and theorists. Much of this work comes from Harvard historian of physics, Peter Galison, beginning with his studies of neutral currents and of the hunt for the Higgs particle. This particle plays a central role in the Standard model of particle mechanics and in the Grand Unified Theory. It was first predicted 50 years ago was finally detected in Large Hadron Collider in Cern in 2012. Galison's original studies were on the relation of theory to experiment in the 1970's but he has continued work on the issues ever since. Galison points out that much of the time experimentalists and theoreticians use many of the same words but mean something very different by them. That's because the meaning of the technical terms in physics is given not by single definitions in language antecedently understood but rather by the whole network of assumptions and inferences with other technical terms that can be made using them. Experimentalists and theoreticians make very different assumptions and inferences, caring little about the bulk of what the other says about neutral currents or the Higgs particle and often not understanding it. This is like the well-known incommensurability thesis of Thomas Kuhn: that the meanings of the terms used in physics depend intimately on the whole web of assumptions in the theory that use those terms and relate them to others. So, although two different theories may use the same sounding term and seem to make opposing claims using it, they cannot really contradict one another because the term does not mean the same thing in the two different theories. Galison's thick descriptions of the kinds of different assumptions that the experimentalists and theoreticians make about neutral currents or the Higgs particle flesh out this thesis for a real case in contemporary physics.

What Galison contributes that is really new is his account of how the two groups do communicate, as they do when theoretical claims are tested. The two groups meet in what is analogous to a 'trading zone', where neither's home language is understood by the other. They use between them a trading language – a highly specific linguistic structure 'that fits between the two'. Really a pidgin or perhaps even a Creole. Galison stresses that he intends 'the pidginization and Creolization of scientific language to be treated seriously just the way one would address such issues for language more generally',[7] not as a loose metaphor.

This is a nice solution to the incommensurability problem that rings true to the historical descriptions Galison provides. But pidginization raises real problems for the traditional account of laws. The laws of physics are supposed to be universal and comprehensive, but they are also supposed to be well tested. So which laws are these? It is the theoreticians' laws, fitting together into an integrated theoretical package, that have a claim to be comprehensive; it is the experimental laws with their connections to all the requisite experimentalist assumptions that are tested. And the pidgin laws will generally not have enough back up from the home languages to do either. There is a plurality of laws here and together they serve our pragmatic needs, as Sandra Mitchell urges. Picking, choosing and combining, we can do what we need to do. But there is no set of well-tested laws that looks comprehensive enough to support total order in the domain of physics.

The motley assembly. A third kind of study that casts doubt on the possibility of horizontal reduction raises issues about the autocracy of physics. Much of my own work falls into this category. According to the *Concise Oxford Dictionary*, autocracy = 'a system of government by one person with absolute power'. So

Autocracy: Is physics an autocrat within her own domain?

The answer should not depend on a quibble about the boundaries so let's focus on things that anyone would reasonably take to be in the domain of physics, such as lasers or high temperature superconductors or the trajectories of the planets. Surely no-one would quarrel with the centrality of physics in building a laser, predicting the perihelion of Mercury or eventually understanding low temperature superconductors. But it is just these marvellous successes that bring into doubt the autocracy of physics. Even in her own domain physics does not reign on her own. She acts as part of a motley assembly.

Our treatment of the nucleus is a good illustration. Here we use a number of different models all at the same time. The problem of having many different and incompatible models for the same phenomena is often seen as one of the biggest challenges for unified theories. It certainly raises big problems for horizontal reductionism.

---

[7] Galison, in Krige, J. & Pestre, D., 1997, p.675

First, these models are phenomenological in the sense that they are constructed not top down, by derivation from theoretical laws, but bottom up to account for experimental results. They use theoretical concepts, theoretical knowledge and theoretical insight, but piecemeal; they cannot be seen as what the supposedly universal theoretical laws dictate for systems of this sort.

Second, the theoretical concepts and theoretical insights employed come from different theories and sometimes these are theories that that are supposed to be inconsistent with each other -- such as quantum and classical mechanics.

Third, radically different fundamental assumptions about the nature of the nucleus and its theoretical underpinnings are required to account for different experimental data. Different models account for different things. Sometimes we can see that these different models idealized or simplified accounts geared to specific purposes but all in fact, once the approximations and idealizations are understood, consistent with one consistent underlying account. The problem is that this is not at all always possible. So even here where we are considering only abstract accounts of the nucleus and not real engagement in the world, we find we can't do the jobs we need to with one autocratic theory that rules the entire domain.

When it comes to detailed real-life prediction and intervention, autocracy fails even more dramatically. Let me illustrate from my own researches into the practise of physics. When I was at Stanford University I was in love with quantum physics and – being a committed empiricist – particularly with the startling empirical successes that speak for its credibility, especially lasers and superconductors, which I made a special area of study. I was especially impressed simultaneously by how crucial quantum considerations are for understanding these devices and by how little they can do by themselves. They must be combined with huge amounts of classical physics, practical information, knowledge of materials and exceedingly careful and clever engineering before accurate predictions emerge, and none of this is described – or looks as if it is even in principle describ*able* – in the language of quantum physics. Physics can measure, predict and manipulate the world in precise detail. But the knowledge that produces our extraordinarily precise predictions and our astounding devices – the very knowledge that gives us confidence in the laws of physics – is not all written in the language of physics, let alone in one single language of physics. Its wellspring is what I call 'the scientific Babel'.

I was clearly influenced in these views not only by what I saw in the building of lasers and the exploitation of SQUIDS (superconducting quantum interference devices) but also by my philosophy hero Otto Neurath. Neurath spearheaded the 1930's unity of science movement of the Vienna Circle. But his idea of unity was not that physics – or anything else for that matter – could produce predictions by itself. He argued for unity *at the point of action*: We must bring the requite sciences together as best we can, each time anew, to achieve the projects we set ourselves, from building a laser or a radar or even – as Neurath believed we had the intellectual resources for – to organizing and controlling the roller coaster of the economy. And though he urged us to talk the same language whenever possible, he never believed this language would stretch far or last long or capture much of what the separate users mean by its terms. Neurath advocated not a shared language but a 'universal jargon'.

Consider the MIT World War II radar project. Designing the radar took the united efforts of mathematicians, physicists, engineers and technicians each themselves expert in one small domain with a language of its own, put together by the urgency of war and often against their will. It took a year for them to be able to communicate well enough to build a usable device – a year and the redesign of the physical environment. The building used to be arranged floor-by-floor according to prestige, with mathematicians at the top. The radar project mixed researchers from the different disciplines at long tables on each floor, tables that reflected in their very geometry the components of the radar to be built. Success was achieved not by constructing a single language nor by translation but by face-to-face contact that allowed enough interchange to make a go of it.

This is the space of interchange that Galison calls the "trading zone", where two tribes stuck in linguistic isolation end up surviving through trade and 'commerce' of vital concepts. But recall: each group in this trade has to maintain its own language and its own understanding within itself, even if out of kilter in various ways. Otherwise it could not produce the detailed well-founded results needed for the project to succeed. The large MIT team that built the radar spoke Neurath's universal jargon or what we have seen Galison describes as a kind of pidgin, with each group maintaining different understandings of the terms they used in common.

Many other studies by different scholars studying different aspects of the practices of physics in different places see much the same thing. Consider just one more. This one looks at the Balkinization of the high temperature superconductivity community.[8] Low temperature superconductivity is taken to be well understood now. Our understanding of it is based on the original 1957 BCS theory, due to John Bardeen, Leon Cooper and John Schreiffer. BCS derived a formula for the temperature at which a superconducting material makes a transition to the superconducting state and from that an approximate upper limit to the transition temperature of around 30 K. In 1986 new types of materials, like cuprates, broke down that wall and showed unusually high transition temperatures (up to a recent superconductor which exhibits an extraordinary transition of 200 K). Ever since then the condensed matter physics community has been struggling with the problem of how superconductivity arises in some materials at far higher temperatures than that of conventional superconductors.

'Cooper pairs' play a central role in the BCS theory. These are pairs of electrons that, contrary to normal behaviour, attract each other rather than repel, due to their interaction with the crystal lattices that structure the superconducting material. The BCS theory does not serve well to model high temperature not only because the high transition temperatures are not predicted but also because within that theory we do not have an account of how the correlated electron behaviour can arise. There are theories, yes. But no one of them is without serious problems. And the theoretical disputes are vicious.

[8] Cite Marlena DiBucchianico

Most scientists agree that the electrons still pair in the new materials, as they do in

BCS theory, but disagree on how. A variety of theories are on offer and the community is rife with dissent and disagreement. To this date, even the most popular and promising candidates are unsatisfactory. Each, however, is hotly defended by its advocates as the single promising theory we should be pursuing, and each is vigorously attacked by the advocates of opponent theories.

How can each group defend its theory as best when none is satisfactory and there are a variety of alternatives? One clash is over what is most important in a good theory: Top down or bottom up. Is it more important that the account mesh with high theory, with what are counted as well established laws and principles, or that it be as descriptively accurate as possible? Nobel-prize-winning condensed matter physicist Philip Anderson is clear where he stands: Get the fit with theory right and the experimental results will come round your way. You can even expect that some results that seemed to oppose your theory do not after all once all the details are understood, or that it turns out there was a flaw in the experiments to begin with. Bernd Matthias, who discovered and created hundreds of new superconducting materials, took just the opposite stand. He maintained that a model that lacks internal consistency and a first-principle derivation has a good chance of leading to the true mechanism for the phenomena as long as it gets the facts right. From there it may be possible to build a more principled account, if we need or want one (though for purely practical purposes we may not). His National Academy of Science biography explains that he worked a great deal from intuition and that his 'enthusiasm for science was fueled by an unabashed joy in discovering something

new, particularly when it did not depend at all on theoretical input.'

What do we learn about laws of nature from this dispute? Anderson himself is an opponent of vertical reduction, arguing for a kind of emergence of new principles at different levels of organization – which allows a special kind of autonomy to his own field of condensed matter physics. Nevertheless, it seems that if we follow Anderson, we should suppose that the 'well established' principles of high theory that rule in condensed matter physics rule with an iron hand. Ensure your model behaves as these principles dictate and the facts your model predicts will be the right ones. But Matthias attributed his great successes at predicting and intervening into the world – not only by finding new superconductors but with ferroelectricity and ferromagnetism as well – to ignoring the full force of these principles, to paying minute attention to the details of the facts and to the use of a broad swathe of knowledge of different kinds, especially a deep understanding of the periodic table of elements. Matthias's successes it seems relied not on autocratic wide-ranging laws but rather on a motley assembly of knowledge and practice.

The battles between the warring camps also illustrate Galison's thesis about incommensurability. Even where experiment comes to centre stage in these disputes, it generally does not play the role we hope for in adjudicating among the alternative accounts, and for just the reasons that Galison has highlighted: The advocates of different theories do not share a common meaning for the same terms even in this single narrow domain. One very notable example is the "kink", which is an observed unexpected spike in the dispersion curve during photoemission studies. Same word, but different groups construct the kink differently from the same body of data. Or take the *phase diagram*, a type of chart that shows conditions at which thermodynamically distinct phases occur. Often each camp builds and presents its own phase diagram, which contains only a selection of observed features, thus creating a vast series of almost incommensurable theorizations.

If one believes in the autocratic powers of physics despite her repeated failure to rule by herself in even the best of circumstance, there is of course a ready response one can give to this mass of evidence from studies of how physics works when it works successfully to produce novel predictions and technologies: Blame it on us, not on the world studied by physics. The world is totally ordered under the rule of physics law, one may insist. It is we weak intellects who have so far produced only an incomplete and inadequate approximation to what Queen Physics is really accomplishing.

My reply to this mirrors the famous Scottish Enlightenment philosopher David Hume in his reply to those who maintain that despite the appearance of disorder and evil all around us, the world is nevertheless really well governed by an all powerful and beneficent God. If we knew the world were so ordered, we could use human failings and other excuses to accommodate the evidence of how the sciences work. But that is not the simplest, most natural conclusion to draw in the face of the evidence. If we must speculate about the structure of Nature, I recommend we stick as close to the evidence as possible.

Whence order?

You will see specific accounts of how order emerges in special domains in physics, biology and in the study of society in Section 2 of this book. Here I take the liberty of sketching a brief picture I have come to see from my own studies in physics and economics of the kinds of order the world exhibits and what, if anything, is responsible for them. These studies suggest that we do not need to postulate just endless lists of ceteris paribus laws – though indeed, these are the only laws that have good evidence for their truth. Often we can trace these laws to a source and generally a source whose parts interact in intelligible ways, like the innards of the Strasbourg clock, to give rise to visible order.

In *The Aim and Structure of Physical Theory* the early 20th century physicist and historian of science Pierre Duhem distinguishes two kinds of minds: ample and deep. The ample mind 'analyzes an enormous number of concrete, diverse, complicated, particular facts, and summarizes what is common and essential to them in a law, that is, a general proposition tying together abstract notions.' The deep mind 'contemplates a whole group of laws; for this group it substitutes a very small number of extremely general judgements referring to some very abstract idea.' He continues, 'In every nation we find some men who have the ample type of mind, but there is one people in whom this ampleness of mind is endemic; namely, the English people.' The English can keep a thousand different concrete principles in mind at once, without getting muddled up. On Duhem's account their science has no need to replace these with a handful of very general very abstract principles – i.e. with the universal laws that physics is often thought to aim for.

I began my career, in the book titled *How the Laws of Physics Lie,* with reference to Duhem's national differences. God (or whatever deep principles underlie the order of nature), I maintained, has the mind of the English. If this is the way the English can do science, why should it not be the way Nature does it herself? The natural world may be law-governed through and through but the laws that govern it look to be long and complex – at least if, as good empiricists should, we insist on laws that physics' successful predictions provide strong evidence for. These laws are many and diverse, hard to keep in mind and difficult to use – unless one has 'the mind of the English'.

Our best evidence still supports that God has the mind of the English. But today I add, 'Moreover, it's the mind of an English engineer!' I mentioned that Otto Neurath is a philosophical hero of mine. Here I take a cue here from Neurath's scientific utopian vision of so-called "social engineering", a dream implicit in his *International Encyclopedia of Unified Science* and many of his other various social and scientific projects. Neurath says, "We could as social engineers also think of new types of patterns just as technical engineers discuss machines which do not exist up to the present.'[9]

So, God is an engineer – but not a mechanic, where I use the terms in the 19th century sense. The 19th century distinction is between *engines* and *mechanisms*. 'Engine' implies productive power, whereas a 'mechanism' is a device for executing a repetitive motion. Automata exhibit mechanical behaviour: repetitious and without creative power. Engineers use principles about how things behave in special circumstances to construct devices that can give rise to totally new phenomena. Order, as I see physics and economics explaining it, where it exists, results from clever engineering, by Nature, God, evolution, even unusually by happenstance, or by us.

Engineered order is, however, a delicate thing. It is both local and fragile. *Locality* first. As the arguments I have been rehearsing suggest, little if any of the order we know about exhibits the regularities dictated by laws that apply universally. The generalizations that express visible order are generally only true locally. They are local to the underlying structure that gives rise to them and to which they are bound (which I call a 'nomological machine'). Consider for instance the myriad cause-effect relationships we rely on in day-to-day life. First thing in the morning I press on a lever on my toaster and it delivers my breakfast. Later I may step on the lever on the floorboards of my car and it propels me out of my driveway. Manipulating the same cause does not produce the same effect given different generating structures.

Most visible regular order is also *fragile*. The generating machine breaks easily. Acorns have just the right internal structure to grow into oaks and they regularly do so – but not if they are smashed or over watered or planted in the dessert or snarfed up by pigs. Many are especially vulnerable to our attempts to exploit them. This is just what Chicago School economists like Robert Lucas claim about

[9] 'International Planning for Freedom' (1942) in *Empiricism and Sociology* 1973: 432).'

even well-confirmed principles involving macroeconomic quantities. Attempts to use these principles to intervene in the economy are likely to fail. Why? Because the interventions are likely to destroy those very regularities. How? By changing the underlying structure of behaviours giving rise to them. People see the interventions that the government is making and change their preferences and behaviours, which then no longer give rise to the original regularity.

Or consider John Stuart Mill in *On the Subjugation of Women* and in his disputes with Comte on the same subject. There may be a well-established regular association between being a woman and being inept at leadership, reasoning and imagination. Happily this regularity is not stable under changes in the nomological machine that generates it. Change the social structure so that the upbringing, education and experiences of women are like those of 19th century British middle-class men and the regularities relating sex to leadership, reasoning and imagination will shift dramatically.

To avoid breakage, machines often come with shields, like the casing of a battery and the shell of an acorn. Happily it seems the planetary system carries on undisturbed without a shield but that is unusual. In any case the order generated depends on the proper arrangement and interaction of the basic features of the generating 'nomological machine' and can only be relied on so long as these arrangements remain stable – or have built in back-up arrangements in case of failure. Visible order then is very different from the stable and global order that would be exhibited by systems following universal and enduring laws of nature.

In conclusion

Let me turn for a few closing sentences to a preview of the issues in Section 3 and 4 of this book, about the relations between mind and nature and between action, freedom and the rule of law. I have rehearsed a number of arguments that conclude that the idea of 'law' in the traditional sense of something eternal that governs everything in its domain entirely by itself with no exceptions allowed seems out of place in modern biology, even those domains of biology that provide impressive explanations and solid predictions. What does the job instead is local, piecemeal, contextual, developmental, holistic.

I then reviewed reasons to think that in these respects physics is not all that different. In particular physics is not enough, even in its own domain. Physics does not act on its own but rather in teams, cooperating with bits of knowledge at all levels, from a mix of sources with a hodge-podge of languages and concepts. So, the world of physics, when looked at through the lens of its most successful practices, is a dappled world. But if the world of physics is dappled and outcomes even in physics' own domain are governed by a motley assembly of features from different realms of knowledge, then there is causation from above, from below and from a thousand angles at the side.

Does this leave room for features of mind not determined by physical features? Does it leave room for these features of the mind to cause physical features? Does it leave room for free will? Or for

Divine action in the world, without violation of the natural order? Those are questions we take up in Sections 3 and 4. What the arguments here teach, if they are sound, is that the assumption of the autocracy and universal dominion of physics (or of any other discipline for that matter) should not play any role in settling it since this assumption itself does not sit well with much of contemporary scientific practice.