Natural laws and the closure of physics

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One usual question commonly debated by philosophers and physicists alike is

> *Realism:* Are the well-confirmed laws of physics likely to be true?

As an empiricist my answer to this question is YES because I take empirical confirmation to be our best guide to what is likely to be true.[1] This question should be clearly distinguished from a very different question that is the topic of this chapter, a question concerning an issue that often goes under the label 'the causal closure of physics':

> *Closure:* Are there (in God's great Book of Nature) laws of physics that dictate everything that happens in the natural world? Or, more narrowly, everything that happens in the physical world?

This is a question not about whether the laws of physics are true but rather about how far they stretch – what are the limits on their dominion? I maintain that we do not have sufficient empirical evidence for a confident yes answer to this question – and as an empiricist, empirical evidence is what I demand. I shall argue that this follows from an

[1] I do however hold much stronger strictures about how far up the ladder of abstraction empirical warrant can flow; hence I may take less than usual to be empirically well-confirmed.

even stronger claim. We do not have sufficient empirical evidence for a yes answer to the question

> *Self closure:* Are there (in God's great Book of Nature) laws of physics that dictate everything that happens that can be reasonably taken to be in the domain of physics itself?[2]

I shall call this the question about *the self closure of physics* – is physics closed with respect to its own effects?[3]

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 my study. I was especially impressed simultaneously by how crucial quantum considerations are for understanding these devices but also by how little they can do for us by themselves. They need to be combined with huge amounts of classical physics, practical information, knowledge of materials and finally exceedingly careful and clever engineering before accurate predictions can be expected, and none of this is described – or looks as if it is even in principle describable – in the language of quantum physics. It was these studies that led to my hesitations about the self closure of physics. What I have

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[2] So I do not think we are in a position to buy wholeheartedly the central premises of the arguments that generate many of the problems about the role of social properties in the determination of events or even in the role of human action that seem to be at the heart of Charles Townes's worries.

[3] Clearly one really needs to ask much more carefully about specific branches of physics at specific times. But I shall speak far more loosely since I am not giving specific arguments here but rather sketching a line of approach from which detailed arguments may be filled in.

come to conclude is that we have strong empirical evidence for a far weaker claim and that there is not strong empirical evidence for the added assumptions that it takes to go beyond the weaker claim to accept the self closure of physics. Crudely put, the weaker claim is this:

> *Narrow self closure:* Physics works well when it can say where it is to be put to work.

I use the word 'say' here and I intend a kind of pun on it. I mean the thesis under both of two different interpretations:

Say = dictate

- Physics works well when it can dictate where it is to be put to work.

Say = describe

- Physics works well when it can describe the conditions under which it is put to work.

As I remarked, I came to these conclusions by studying how physics is used to make accurate and precise predictions about the behaviours of lasers and superconductors, which I take to constitute some of the best evidence for the truth of the physics claims used in those predictions. But talking about lasers and superconductors here is like carrying coals to New Castle. So I propose an alternative approach. Since I have been at LSE I have been studying the social sciences. Looking at some of the ways social scientists have compared their disciplines with physics can provide a good way to see and to articulate my concerns. In particular I shall look at the following:

Giambattista Vico

Tyrgve Haavelmo

Max Weber

Karl Popper

Otto Neurath

Conventional social science concerns about external validity

John Stuart Mill

Giambattista Vico (1668 – 1744. Great Italian social theorist.)[4]

Vico argued that social science should be the easy one. We build social institutions ourselves so they should be intelligible to us. It is natural science that we should expect to be difficult. I shall argue that in a sense physics follows Vico's suggestion: It becomes less difficult because it treats primarily what we make, or, less contentiously, what we can make plus naturally occurring situations that resemble ones we make in an important way I shall explain.

Tyrgve Haavelmo (1911 – 1999. Norwegian economist who won a Nobel prize for his work in founding econometrics.)[5]

[4] Vico (1730/1743) [1976]
[5] Cf. Morgan (1990).

Haavelmo in conversation about physics versus the social sciences remarked that physics has it easy. No-one asks physics to predict the course of an avalanche. But economists are expected to predict the course of the economy.

Where then does physics work best? My answer is that the detailed precise predictions that can give us confidence in the truth of physics claims come for the most part in highly engineered, highly controlled situations, inside a laboratory or inside the wrappings of a technological device, whether it be a laser or an ordinary flashlight battery. There are of course notable exceptions; the planetary system is probably the most striking. But here we have two pieces of extraordinary good luck (or perhaps good planning on God's part).

First, there is the inverse square law. It is reasonable to suppose that every tiny bit of matter must obey the law of gravitational attraction, whatever that law is. As we know the inverse square law has a wonderful feature. Given the (rough) spherical symmetry of the planets, the attraction between their centres of mass will obey the inverse square law if the attraction between all of their parts does. This ensures the kind of regularity we record in Kepler's laws. There might otherwise have been no systematic or lawlike behaviour among these huge massive objects. Second, the planetary system has few perturbations. Little affects the motions of the planets and the sun other than their mutual gravitational attraction. They have, as 17^{th} and 18^{th} century Deists urged, a natural structure, like a clock, and are naturally shielded without need for the kind of thick casing that our flashlight batteries have. This last will matter importantly to the second of my two readings of ‘say’: Physics works in situations that physics can fully describe. For the planets there are no major perturbations that we do not know how to describe in the concepts we have available in physics.

Karl Popper (1902 – 1994. Great methodologist of the social sciences and advocate of the open society) *and Otto Neurath* (1882 – 1945. Founding member of the Vienna Circle and head of the Commission for Full Social Planning during the very short-lived Bavarian socialist government after World War I)[6]

Popper was in favour of piecemeal social planning. He argued in opposition to Neurath, who was impressed by the power of new statistical techniques and the vast amount of information that was gathered by the *Verein fuer Sozialpolitik* and other such groups. Neurath thought that it would be possible to predict the course of the economic avalanche; that with proper planning and coordination the roller coaster of expansion, inflation, depression and unemployment that plagued European economies could be controlled. Popper was extremely sceptical. He advocated picking the problems to solve for which we have the tools for solution. His strategy is the one that Haavelmo and Max Weber (as we shall see) attribute to physics. Admittedly we do a vast amount of detailed difficult work, but in the end we build a laser because we see how – we see how to build a device that will work precisely and accurately for a certain end; we do not approach an arbitrary end and succeed in building a device to serve it.

[6] Cf. Cartwright (1995) with Cat, Fleck and Uebel.

Max Weber (1864 – 1920. One of the founders of modern sociology.)[7]

In efforts to become an exact science physics has a great advantage over any of the social sciences, Weber argued. Physics can adjust its concepts, refining, discarding, adopting new ones, till it finds concepts that have exact relations from which precise predictions can be made. That's a tall order, of course, and it might never have been possible. Social science is even more difficult however, for its concepts can admit little adjustment. Social science is mandated to provide generalizations about the concepts we are interested in and there is no guarantee that these kinds of concepts fit into any exact laws.

Weber's ideas point to both of my different readings of the narrow closure claim. The first is the point I have illustrated with Haavelmo and Popper. The striking successes of physics are for the most part in situations that are made to suit what physics knows it can do, as in a laser or a lab. The second reminds us of the tight constraints on the concepts of physics. 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 that can themselves be described with proper physics concepts. This is the source of the worry about exactly what form of self closure is supported by the evidence. The stronger conclusion certainly does not follow from the admitted fact that there are a vast number of situations where physics-style concepts – concepts proper to physics, satisfying all the demands we make on such concepts– can describe both the causes and the effects and fit into tight laws linking them. We cannot infer from this that there are proper physics-style concepts that can describe for any

[7] Weber (1978).

situation all the causes for even the effects the concepts can describe. This worry about the stronger conclusion is reinforced by considering another social science discussion.

External validity

Social scientists are very attuned to the distinction between internal and external validity. An experimental result is internally valid when the design of the experiment can ensure that the result really does hold in the experimental setting. But that kind of a conclusion is generally of little use. The result has external validity when it can be presumed true of target situations outside the experimental setting. A usual way of claiming external validity is to see the experimental result as an instance of an inductive generalization. It is not just the gyroscopes in Francis Everitt's Gravity probe experiment[8] that are caused to precess by coupling with the spacetime curvature. The inductive generalization we presume is that *any* gyroscope that is not subject to other sources of precession will precess by the predicted amount. The inductive generalization carries the conclusion from Everitt's gyrosopes to all others that satisfy the antecedent conditions.

This illustrates the first point I have been making. Everitt's experiment is beautifully controlled. He tried to fix it so that all other causes of precession are missing; hence all the other causes are, *ipso facto,* describable in the language of physics! Moreover if he had not succeeded and other causes occurred, then any that he couldn't describe would

[8] Cf. Lammerzahl, CC, CWF Everitt, FW Hehl. (eds.) (2001). See http://einstein.stanford.edu/ [Accessed 24/4/07] for updates on the experiment.

make prediction impossible. It should be no surprise then that all the good confirmations of the laws of physics occur in very special situations where we can describe all the causes with proper physics concepts.

My central point in introducing this example had to do with external validity and inductive generalization. In general the breadth of an inductive generalization supported by an experimental or observational outcome – the range of cases it can encompass – depends on the level at which we describe the outcome. If we describe the trajectory of Mars in terms of its position across time, it can serve as an instance of Kepler's laws. But if we describe it more abstractly, say in terms of the accelerations it experiences and the forces imposed on it, it can be seen as an instance of Newton's laws. If we can correctly describe the outcome of our observations in this more abstract way then, via the greater breadth of the inductive generalization the outcome speaks for, we secure a far greater breadth of external validity for it. Thus the result can speak in favour not just of elliptical orbits for planets circulating the sun but also for, say, parabolic orbits for cannonballs. This is a common feature of inductive generalizations. In general the maxim is true that

> We can buy greater breadth in the inductive generalization that an outcome supports, and hence in the external validity of the outcome, by climbing up the ladder of abstraction in describing that result.

But there is a well-known problem. What goes up must come down. Generalizations at a high level of abstraction are of little use. The New Testament urges that we should love our neighbours as ourselves. But what specific actions constitute loving our Iraqi neighbours in our current muddled situation? Physics has the same kind of problem. In exact science just as in everyday life abstract terms need to be translated back into more concrete terms at the point of application if they are to be of practical use. One of Weber's points is that this is very difficult with social science concepts. Consider a modern example from what must be the most exact of our social sciences, economics. *Utility* is a key concept; it plays a central role in almost all current theoretical models. What does it mean more concretely? In the context of a given model there is often no problem in figuring that out. In game theory models the pay-offs are laid out. The maxim, 'Rational agents act so as to maximize their expected utility' turns into 'Rational agents act so as to maximize their payoffs'. In other models the interpretation also comes almost for free. There is nothing in the model for agents to care about except, for example, profit, wages and leisure, or power, prestige and portion of the legislative body. The trouble comes when we want to move outside these theoretical models. Then what utility amounts to is up for grabs – too much up for grabs to allow us to make precise predictions even though the theory itself might be expressed in precise mathematical equations.

Physics, we know, is in a far stronger position. There are *rules* for how to apply its concepts, strict rules, even though the concepts are abstract. Physics concepts are abstract in two different ways and the application of both kinds of concepts is heavily policed.

The first kind of abstraction occurs when one concept piggy-backs on more concrete ones.

Consider for instance the abstract concept of *the quantum Hamiltonian*. It is never just true that a system in a given situation evolves under a particular Hamiltonian. Whatever Hamiltonian applies, it applies because something more concrete is true of the situation. For instance, the first of the three components of the Hamiltonian in the original BCS model of superconductivity[9] is the so-called 'Bloch Hamiltonian'. It is appropriate for situations that can be more concretely described as a certain kind of periodic lattice, a Bravais lattice. To be moving in a Bravais lattice is what it is for an electron to be subject to the Bloch Hamiltonian and the Hamiltonian is not legitimately applied to an electron without the commitment that it is located in a Bravais lattice. This kind of constraint is always in play with Hamiltonians. There are rules linking them with more concrete descriptions, ultimately very simple descriptions like *central potential*, *Coulomb interaction*, *scattering*, or *harmonic oscillator*. It is not proper physics just to write down a Hamiltonian that will produce correct predictions. There are rules for how to do it and they must be obeyed.

Other concepts in physics are not abstract in this very particular way that they piggy-back on more concrete descriptions but just in the sense that they are highly technical and have mathematical definitions that link them with other parts of the theory. *Acceleration* is an example, or *charge*. These do not apply in virtue of some more concrete characteristics obtaining. Nevertheless there are extremely strong constraints on their application. These

[9] Bardeen *et al.* (1957). For further discussion of this example see Cartwright (1999).

quantities are subject to precise measurement and it is expected that the results from different procedures will converge; they must behave as they are predicted to under the huge network of interlocking laws accepted in physics; etc.

So the concepts in physics unlike most in social science are strictly constrained in how they apply to the world. This is the fact that Popper praised so highly in his well-known demand that proper science be strictly *falsifiable.* It is what gives physics its great powers of precise prediction. But there is a cost and it is a cost that was pointed out by Popper's adversary in the debate over social planning, Otto Neurath. Most of the world, as Neurath saw it, does not lend itself to description by strict scientific concepts of the kind Popper praised. That means that strict science, where concepts are tightly constrained by a web of mathematical laws and by highly precise criteria for application, may not be universally possible but at best constrained to pockets of reality. Neurath's worries bear immediately on issues of closure in physics. We have very strict constraints on concepts in physics and so in thinking about its range of application we must bear clearly in view the underlying principle Neurath appeals to[10]:

> The more highly constrained a category of concepts is in its rules of application, the more narrow will be the possibilities for applying concepts from that category.

I think it should be clear how these worries bear on closure. Self closure requires that for any outcome in the well-confirmed laws of physics in any situation, all the factors relevant to its determination can be subsumed under proper concepts. But proper

[10] Neurath (1983).

concepts in physics are highly constrained and thus have severely narrowed possibilities for their application. In the face of this ‘a priori’ worry, if we want to claim that self closure is true, we had better have very good empirical evidence. Neurath certainly doubted self-closure. My point is that whether and to what extent Neurath was right is an empirical question. And it is a question that we do not have anything like sufficient evidence to answer, one way or the other; in fact it seems likely that it will never be settled.

There are naturally arguments to be had on the subject. Perhaps the history of successes in expanding physics to treat new kinds of phenomena can support closure. But there is equally a history of failures. To establish any reliable results one way or the other by this method seems hopeless. Surveying and weighing the history of science, the reasons for successes and failures and the areas in which they occurred, is well beyond any methodology we have, or can ever hope to have, in history. Alternatively there is Pythagoreanism, the view that Nature is at base mathematical and thus necessarily describable with mathematical representations. But this view, though venerable, is a metaphysical doctrine, not a well-established result of empirical enquiry, and thus not one that we should use to arrive at consequences about the closure of physics.

My overall point indeed is just this. The claims of closure for physics, whether self closure or even grander claims that physics can account for every feature of the empirical world that can rightly be called physical, or that physics can account for every feature of the empirical world full stop – these are all claims that call for empirical evidence. And we simply do not have the right kinds of evidence to have confidence in any answer. Any answers now, one way or the other, are sheer metaphysics and as an empiricist I am resolute that we must not let answers to these questions play any role in our scientific considerations.[11] As with David Hume I recommend, if it is metaphysics, consign it to the flames.

There is an additional caution. Consider various other metaphysical doctrines that play a role in science: the claim of universal determinism; or of the determinism of the macro-world; or of individualism in the social sciences – the doctrine that all social phenomena are reducible to the actions and characteristics of individual people. It has been urged repeatedly that these are metaphysical doctrines and should play no role in science. In an effort to salvage their role advocates sometimes urge that, although we can not hold them as true, we should adopt them as methodological guides: Look for deterministic theories; look for individualist theories.[12] But why should we do this? Ultimately we want to look for true theories, or at least effective theories and why should we hunt in one special category only when we have no assurance that it is the right category? Metaphysics should be avoided equally in our claims and in our methods.

[11] That naturally does not mean that we cannot support research programmes that presuppose some answer one way or the other; it means rather that the programme must be judged on its immediate scientific credentials and not get any extra dollops of support because it presupposes our favourite metaphysics.

[12] Sometimes the dictum has a more plausible form: Wherever possible formulate a theory deterministically (or individualistically, or …). Whether or not this is good advice depends on what one intends to do with it. We often do this in econometrics, where relations are made to look deterministic by adding in 'unknown' 'error' terms. Sometimes this can be harmless but it is not when we are misled about the probabilistic features of the phenomena by thinking of them as generated by these unobservable hidden variables. (For one such problem, see my discussion of the causal Markov condition in Cartwright (Forthcoming)).

John Stuart Mill (1806 – 1873. Influential British economist, philosopher, administrator.)[13]

But, you may ask, do you really think that fundamental particles behave differently inside the laboratory and outside? That's daft. I agree, that's daft, and it is not what I argue. The distinction I draw is not between inside the laboratory and outside, nor between the large and the small, nor between situations where consciousness matters and those where it doesn't, nor (as with Aristotelian physics) between heavenly masses and earthly masses. Instead it is between environments that are properly structured so that the laws of physics can act without interferences not subsumed under proper physics concepts and those where the environments are more messy.

John Stuart Mill thought that the laws of physics and of political economy were *tendency laws*. They describe not how things do behave, but how they *tend* to behave. The tendencies result in the canonical behaviours only in the right environments. For instance, women, he believed, have the natural capacity for independent and creative thought. But we will develop independence and creativity only if provided with the proper education, the proper stimulation and the proper opportunities to practice our developing skills. What happens otherwise? We do not know and perhaps there is nothing systematic to know. Without the right environment the natural capacities for independence and creative thought may have no systematic or predictable outcomes. Messy input yields messy output.

[13] Mill 1836 [1967] and 1843 [1973]

I see physics operating in the same way. We can think of the laws governing even the fundamental features of fundamental entities as tendency laws. Our successes in precise prediction show that these features behave as the laws dictate in properly structured environments – indeed these are the only environments where we can produce such predictions. Whether there is systematicity outside structured environments 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, so is a laser, a battery and a bicycle. So too are a very great many naturally occurring situations. The planetary system is structured and seems to have little disturbance that cannot be subsumed under proper physics concepts. In the BCS model, superconductors are structured too, in just the right way to allow a quantum treatment of them. Recall the rationale that I mentioned before for the first component of the BCS Hamiltonian: The free electrons float in a special kind of periodic lattice – a 'Bravais' lattice; this environment is properly described by a Bloch Hamiltonian. Notice I say here 'according to the BCS model'. That is because I wish to stay neutral about what happens when superconducting materials appear in messy situations outside laboratories and other engineered environments (like a SQUID). In these situations the mutual tendencies of the electrons and the ions of the lattice may or may not give rise to systematic behaviour. That, I take it, is what is at stake in the distinction between my narrow and epistemically conservative hypothesis versus the bolder and – I have been arguing – far less well supported hypothesis of the self closure of physics.

In sum

I want to dislodge a particular vision of how the world must be if the laws of physics are to be true, a vision of a world where all of physics effects are well-ordered under its laws. We begin, unproblematically, with the idea that there are fundamental particles or fields (or whatever is the best choice from some future ideal physics) and these have certain fundamental features. What is problematic is the next step, the automatic assumption that everything that happens to these fundamental entities must be the result of the interactions of these fundamental features. I offer a picture of a far richer world, one with a vast variety of features, most of which cannot be captured under concepts that could be regimented into systems of relations and measurement procedures that look anything like those of modern mathematical theories in physics and especially not of any one single consistent theory. These features too can affect even the behaviour of fundamental particles.

This picture of a rich untidy world is not a fantasy picture. Here I have offered two different but related lines of defence. First, this picture is drawn from how in general I see physics working when it works best, that is, when it provides accurate and precise predictions. In most cases it works by engineering situations so that all the causes that obtain are ones that can be represented under physics concepts; by excluding other features that could have disturbing effects,[14] whatsoever they might be, not by bringing them under the concepts of the theory. As an empiricist I insist that these cases must be

[14] Or by putting special 'ad hoc' terms into the equations to account for their effect. These terms are ad hoc in that they do not provide a description of those factors in the language of physics that can reliably be repeated for similar cases, as the connection between Bravais lattices and Bloch Hamiltonians can.

taken as our evidence base for claims about the extent of the laws of physics, for these are the cases that provide evidence for their truth. We are not after all interested in how far some speculative laws stretch but rather in the extent of the domain of those very laws which have a claim to truth, and are formulated in just the right way that the evidence available will support them strongly, not overstretching the bounds of that evidence.

The second line depends on the nature of the concepts themselves. Physics is undoubtedly the most exact science. Its concepts are subject to huge constraints. They must be precise, they must be reliably measurable by a variety of different procedures that give convergent results. Crucially, as Weber stressed, they must fit together into a web of highly intricate, highly detailed, entirely precise laws. And there must always be a way – an entirely systematic and principled way – of climbing down the ladder of abstraction.

These characteristics of the concepts are what gives physics its great powers of precise prediction. But it would be no surprise if concepts like these were not available to describe the great bulk of causes at work in Nature, even of all the causes that can affect the fundamental behaviour of physics' fundamental entities. There might be such concepts in the great Book of Nature. I have said nothing that argues that there cannot be. But if we are going to give a credible answer – yes or no – to the question of the self closure of physics, 'might' is not enough. We should have strong empirical evidence and in particular evidence that can jump across the gap from my epistemically conservative hypothesis,

> Physics predicts the effects in its domain where it can; and where it can predict, it predicts well

to the bolder conclusion in favour of self closure,

> Physics can (in principle) predict – and predict well – everywhere in its domain.

We do not have good empirical evidence for the stronger claim and without empirical evidence it must not play a role in science.

Concluding remark

I think it is a miracle that we have what we do have in physics; I await the evidence that we can have it all.

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