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ORDER: GOD'S, MAN'S AND NATURE'S

Queen Physics: What is the Stretch of her Domain?

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Thesis: the plentiful universe

Despite all of the apparent differences, there is one realm, we are told, and physics is its queen. I reject this claim. There is not one realm, as the ambassadors of physics maintain, nor two, as mind-body dualists proclaim, but many: cooperating, quarrelling, negotiating. I reject the universal rule of physics not because I dissent from her dictates but because I respect her strengths. What she can do, she does exceedingly well. But her very strengths suggest she cannot rule everything with the same iron hand, nor can she do it alone. I claim this because I am an empiricist. As an empiricist I take it that our best guide to the structure of nature is how our science works when it works best. We don't use only physics to build a laser. So I see no reason to suppose that Nature does.

Getting the question right

One usual question debated by philosophers and physicists alike is

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

I am an empiricist – as I urge you to be – and 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.

Realism is distinct from a 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. My answer to this question is NO. This follows from a stronger thesis I am going to defend this evening, concerning physics' autocracy. 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?

I do not want to quibble about the boundaries. Let's focus on things that anyone would reasonably take to be in the domain of physics, lasers or high temperature superconductors or the trajectories of the planets. I have no quarrel with the centrality of physics in building a laser, predicting the perihelion of Mercury or eventually understanding high temperature superconductors. But I deny the autocracy of physics. Even in her own domain physics does not reign on her own. She acts as part of a motley assembly.

I am going to review two kinds of arguments for this: *bottom-up* and *top-down*. The *bottom-up* argument looks at physics at its best and extrapolates from there. It relies on studies of physics' successes – the very successes that make us believe that our best physics gets it nearly right. Here we find not physics the autocratic but rather my motley assembly, where the members may reach a joint agreement about what is to happen but each for their own reasons expressed accurately only in their own vocabulary. The *top-down* argument shows why we should expect the range of physics' rule to be restricted. Here I look to social scientists. For centuries social scientists have lamented the weakness of their disciplines and praised the power of physics. But their very accounts of physics' power show that it is likely to be bought at the cost of restricted dominion.

Bottom-up

When it comes to reasoning about the structure on Nature, I urge an empiricist stand. The way to learn about the world is to look at it. And when it comes to the parts of the world studied by physics, our best lens for looking at them is through our most successful accounts in physics. When I look through that lens I see a quite different world than one where physics reigns, supremely and by herself. I see a dappled world with a hodge-podge of different kinds of features interacting in a variety of

different ways. This is true even for the effects that physics herself is supposed to control.

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, I agree, 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 but also by my hero Otto Neurath. Neurath spearheaded the 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 herself. 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 to 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 wherever possible, he never believed this language would stretch far or last long or capture much of what the separate user's mean by its terms. Neurath advocated not a shared language but a 'universal jargon'. This idea has recently been taken up and defended with a vengeance in Science Studies.

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 5 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.

Peter Galison calls this space of interchange the “trading zone”, where two tribes stuck in linguistic isolation end up surviving through trade and ‘commerce’ of vital concepts. But each group in this trade had 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. They spoke Neurath’s universal jargon or what Peter Galison describes as a kind of pidgin, with each group maintaining different understandings of the terms they used in common. According to Galison this is also often the case for theoretical and experimental physicists even with respect to the very same claim, a claim the theoreticians derive and the experimentalists test.

Other scholars in Science Studies see the same thing.

- Sang Wook Yi’s study of thermodynamics rejects the usual story that it reduces to statistical mechanics. He argues that a more useful way of understanding the relationship is as collaboration and competition among alternative methodologies rather than reduction of one theory to another. The theme of cooperation and competition carries over from Yi’s work on condensed matter physics where he shows how it plays a crucial role in generating the right kind of models for systems with many many bodies.

- Marilena DiBucchianico writes about the quarrelling camps in high temperature superconductivity. It seems they do not share a common meaning for the same terms even in this single narrow domain. Like 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.

- Hasok Chang's important study of our long struggle to measure temperature makes clear how essential were contributions from potters, experimentalists specializing from thermal physics to glassblowing, chemists, doctors, physicists from the most abstruse theoreticians to the most down-to-earth instrument builders, famous inventors, entrepreneurs, soldiers and myriad others.

- Harry Collins in his study of gravity-wave experiments concludes that gravity waves are 'boundary objects' – understood and valued differently by the different cultural groups that share them.

- Or look at the study of the original BCS model of superconductivity by Mauricio Suarez, Towfic Shomar and me. This model was the first successful theoretical account, a tour de force of quantum modelling, which still lies at the heart of our understanding of superconductivity. Though it has been tidied up considerably, it remains a hodge-podge of high quantum theory and ad hoc assumptions grounded in classical electromagnetic theory.

There is of course a ready response you can make to this mass of evidence from Science Studies if you believe in the autocratic powers of physics despite her repeated failure to rule by herself in even the best of circumstance: Blame it on us, not her. The world is totally ordered under the rule of physics law, you may insist. It's us 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 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, as empiricists we had better stick as close to the evidence as possible.

Top down

For my top-down arguments I shall look at
Gian Battisto Vico
Tyrgve Haavelmo
Max Weber
Karl Popper
Otto Neurath
Conventional social science concerns about external validity
John Stuart Mill

The lesson that I extract from these social thinkers about the autocracy and dominion of physics exploits a pun:

Physics works best when she can say where she is to work.

The pun is with the word ‘say’. I mean this thesis under both of two different interpretations:

Say = dictate

- Physics works best when she can dictate where she is to work.

Say = describe

- Physics works best when she can describe the conditions under which she is put to work.

Giam Battista Vico (1668 – 1744*.* Great Italian social theorist.)

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.)

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 and most on her own? When she predicts what happens in highly engineered, highly controlled situations, inside a laboratory or 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 notice that the planetary system, unlike an avalanche, is seldom subject to unruly intrusions.

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.)

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 sceptical. He advocated focussing on the problems 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 build a device to serve it.

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

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 an understanding about the

concepts we are interested in and there is no guarantee that these concepts fit into any exact laws.

Weber's ideas point to both of my different readings of my theisi about where physics works best. 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 suit what physics 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 reasonably supposed to be in her own domain.

Perhaps we can admit that

- *There are* situations where physics-style concepts – concepts proper to physics, satisfying all the demands we make on them – can describe not only the targeted effects but all the causes as well.

Even if this is true, we cannot infer from it the stronger conclusion that

- *For all situations* where physics has concepts to describe the targeted effects, there are proper physics-style concepts that can describe all the causes of those effects.

Let alone the conclusion in favour of universal dominion:

- *For all situation*s there are proper physics-style concepts that can describe all the causes and all the effects.

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 ensures that the result holds in the experimental setting. But that kind of 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 the Stanford Gravity-probe experiment 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 the gyrosopes in the experiment to all others that satisfy the antecedent conditions.

This illustrates the first point I have been making. The Gravity-Probe experiment is beautifully controlled. The experimenters 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 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.

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. 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

> We can buy greater breadth in the inductive generalization that an outcome supports, and hence in the external validity of the conclusion, 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 in practice; they need to be translated back into more concrete terms at the point of application. One of Weber's points is

that this is 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 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 about the real situations outside the models, even though the theory itself might be expressed in precise mathematical equations.

Physics is in a far stronger position. There are *rules* for how to apply its concepts, strict rules, even though the concepts are abstract. It is not proper physics just to write down a force function that will produce correct predictions. There are rules for how to do it and the rules must be obeyed.

So the abstract 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, 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 the dominion and autocracy of physics. We have strict constraints on concepts in physics and so in thinking about its range of application we must bear keep clearly in view the underlying principle Neurath appeals to:

> 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.

Now to the issue of autocracy. Autocracy requires that for any outcome in the well-confirmed laws of physics in any situation, all the factors relevant to determination of that outcome can be subsumed under proper concepts. But proper 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 physics is autocratic, we had better have very good empirical evidence – and our bottom-up arguments suggest that no such evidence is available.

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

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, or situations where consciousness matters and those where it doesn't, or (as with Aristotelian physics) between heavenly masses and earthly masses. Instead the distinction 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 behave, but how they *tend* to behave. The tendencies result in orderly and predictable behaviours only in the right environments. For instance, women, he argued, 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 outcome. Messy input yields messy output.

I see Queen Physics operating in the same way. We can think of the laws that govern 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, and lasers, batteries, and bicycles. 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. But most situations do not seem well-structured. Physics does not have concepts available to describe all the causes operating in them. And a good many of the causes do not look to have the right features to allow them to be described in the precise kind of language that makes physics so powerful. If they aren't then physics cannot dictate all the effects in its domain.

In Sum

I want to dislodge a certain 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 from an empiricist point of view is the next step, the assumption that everything that happens to these fundamental entitles 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 physics, and especially not of any one single consistent theory in physics. 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 and other researchers in Science Studies see physics working when it works best, that is, when it provides accurate and precise predictions. In most cases it works in situations engineered – whether by us or God – 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,[1] whatsoever they might be, not by bringing them

[1] 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.

under the concepts of the theory. As an empiricist I insist that these cases must be taken as our evidence base for claims about the rule of the laws of physics, for these are the cases that provide evidence for the truth of these laws.

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 questions about the domino and autocracy of physics, 'might' is not enough. We should have strong empirical evidence. I have argued that we do not have that evidence. And without empirical evidence it should play no role in science.

Conclusion

Let me turn to for a few closing sentences to broader questions at the centre of attention in both philosophy and science right now, questions about the relations between mind and nature. I have argued that physics is not enough, even in her own domain. She does not act on her 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 our most successful models, is a dappled world. But if the world of physics is dappled, as most of our successful models of it are, 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 other physical features? Does it leave room for these features of the mind to cause physical features? That is a question to be settled by future empirical research. But for good empiricists at least, the assumption of the autocracy and universal dominion of physics should not play any role in settling it.