Against Fundamentalism

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Abstract

In this essay, I argue that the idea that there is a most fundamental discipline, or level of reality, is mistaken. My argument is a result of my experiences with the "science wars", a debate that raged between scientists and sociologists in the 1990's over whether science can lay claim to objective truth. These debates shook my faith in *physicalism*, i.e. the idea that everything boils down to physics. I outline a theory of knowledge that I first proposed in my 2015 FQXi essay on which knowledge has the structure of a scale-free network. In this theory, although some disciplines are in a sense "more fundamental" than others, we never get to a "most fundamental" discipline. Instead, we get hubs of knowledge that have equal importance. This structure can explain why many physicists believe that physics is fundamental, while some sociologists believe that sociology is fundamental.

This updated version of the essay includes and appendix with my responses to the discussion of this essay on the FQXi website.

1 What is Fundamental?

As a physicist, it is easy to be impressed with the understanding that fundamental physics has gifted us. Through the ingenuity and hard work of thousands of physicists, we have learned that all matter and energy in the universe is composed of interacting quantum fields, and we can in principle predict their behavior to great accuracy using the standard model of particle physics. On the large scale, Einstein's theory of General Relativity, together with the standard model of cosmology, give us an accurate picture of how the universe began, and how it behaves on large scales. Sure, there are a few phenomena that are outside the scope of current physics, such as what happens in the very early universe or near the singularity of a black hole, but, on the scales relevant to human life, we have a pretty complete understanding of all the relevant constituents of matter and fundamental laws. This picture is complete in the sense that it does not seem to need any concepts from the other sciences, except perhaps mathematics, in order to describe all matter. In principle, we could use fundamental physics to predict with the greatest possible accuracy what will happen in any given situation, including those relevant to chemistry and biology, and even in those sciences that deal with the human mind, such as neuroscience, psychology, and sociology. I say "in principle" because those calculations would involve an impossibly detailed description of the initial conditions of the system being studied, as well as infeasible computational power. It would be essentially impossible to identify and model a biological system directly in terms of its constituent quantum fields. So we can admit that, in practice, biological explanations of how cells operate are much more useful than descriptions in terms of fundamental physics. However, the question "what is fundamental?" concerns what is possible in principle rather than with what is possible in practice.

The view outlined above, that everything boils down to physics, is called *physicalism*. Although it is an attractive view for a physicist—I, personally, was drawn to physics because it seemed to be the only way to truly understand the fundamental nature of reality—I shall be arguing for precisely the opposite view in this essay. My position is deeply influenced by the "Science Wars"; a battle that raged in the 1990's between scientists, philosophers, and sociologists over whether science can lay claim to objective truth. In many ways, I am a casualty of the science wars, since they were at their peak during my undergraduate education. Being young enough not to have developed strong opinions about the meaning of science, I have been influenced by the sociology camp to a greater extent than most scientists. The extreme version of the sociology side of the argument, which I call *sociologism*, claims not only that science is not objectively true, but that sociology is more fundamental than physics. It is quite understandable that a sociologist might find this view as appealing as physicalism is to a physicist, and a bit surprising that we do not have even more "isms" where scholars seek to put their own discipline at the top of the tree.

Although I want to incorporate some of the sociological insights into my argument, of course I view sociologism as just as barmy as physicalism. However, the fact that scholars can seriously argue that a discipline other than physics should be considered fundamental lends some support to my thesis that "fundamental" is a mistaken category. If this is so, then we shall need a theory of knowledge that accounts for the fact that subjects like physics can seem more "fundamental" than others when this is not actually so. I shall attempt to develop such a theory as well.

This essay is a sequel to my 2015 FQXi essay "Mathematics is Physics"[1], in which I proposed a theory of knowledge intended to explain why it is not surprising that advanced mathematics is so useful in physics. The theory of knowledge employed here is the exact same one, but I want to relate it more explicitly to my thoughts on the science wars.

2 Dispatches from the Science Wars

I would like to begin with the story of my first encounter with the science wars. When I was an undergraduate studying physics at Manchester, my brother, who studied philosophy, got me interested in philosophy of science by asking me difficult questions over dinner. Since the well-being of my psyche depended on being able to defend the position that physics is the most fundamental way of understanding the world, I jumped at the chance to take a course entitled "The Nature of Scientific Enquiry" when the opportunity came up. At the very least, I figured, it would help me win dinner table arguments with my brother.

When the time came, I went to see my director of studies, the late Dr. Anthony Phillips (still the best physics teacher I have ever known) to tell him that I wanted to enrol in the course. His first reaction was, "Wouldn't you rather take a course in fluid mechanics?". After I rejected that option his next response was, "OK, but don't believe a word they tell you." At the time, I thought this rather uncollegial, but I did not realize that the course was run by the sociology department, and was being taught by a proponent of the "strong program" in the sociology of scientific knowledge (SSK), a major school on the sociology side of the science wars. I did not know that we were supposed to be at war, but, in light of that, Dr. Phillips comments make a lot more sense.

The first half of the course proceeded along the lines of a generic philosophy of science course. We studied Bacon [2], logical positivism [3], Popper [4], Kuhn [5], and Lakatos [6]. However, unlike a standard philosophy course, Kuhn was given a ringing endorsement, and then we went off to study SSK.

The strong program of SSK is most closely associated with David Bloor and his collaborators at the University of Edinburgh [7]. It is intended as a response to earlier approaches to the sociology of science, which are deemed "weak". In "weak" studies, sociological factors are only deemed important in understanding why "failed" or "false" theories are sometimes accepted. For example, one might look at how Stalin's totalitarian rule allowed Lysenco's ideas of environmentally acquired inheritance to become the dominant theory of genetics in the Soviet Union in the 1930's and 40's. In modern times, one might look at why the anti-vaccine movement or the idea that human activity is not causing climate change are being increasingly accepted in large segments of the US population.

In contrast to this, the strong program states that sociological factors are equally important in understanding how successful scientific theories, which are usually deemed "true", gain acceptance. If a theory is accepted science, it is very easy to fall back on the argument that the reason it became accepted is simply that it is "true". Proponents of the strong program reject this asymmetry of explanation, and want to study the sociological reasons why science progresses the way it does period, without regard to whether a or not a theory is "true". In order to do this they adopt, as a methodological principle, a ban on using the "truth" or "correctness" of a theory an an explanation for its acceptance.

Although this ban is supposed to be merely methodological—a corrective for decades of studies which ignored sociological factors other than in cases of "error"—studies in the strong program tend to show strong sociological influences in every case they look at. Unless you are being deliberately contrarian, it is very hard not to infer that, if you can actually find sociological reasons why theories are accepted in every case, then scientific theories must be social constructs, with no claim to be the ultimate arbiters of objective truth. Although defenders of the strong program like to emphasize that the ban is meant to be methodological, and they are simply "hands-off" on the question of ultimate truth, it is pretty bizarre to adhere to a methodology and, at the same time, not contemplate the most obvious reason why that methodology might work well. This leads to cultural relativism about scientific truth and, despite protests to the contrary, the language of cultural relativism does seep through the rhetoric of the strong program. Nonetheless, I define "sociologism" as the position that scientific theories are merely social constructs, in contrast to the strong program itself, which insists on only adopting this as a methodology. Sociologism implies that sociology is the most fundamental science, since it means that understanding the content of any scientific theory is equivalent to understanding the social factors that led to its acceptance.

To see how easily SSK devolves into sociologism, I want to relate an experience from the Nature of Scientific Enquiry course. In one of our assignments, we were asked the question, "If sociological factors always play a role in determining which scientific theories are accepted, does science still tell us anything about the real world?" In the seminar discussion of this, the graduate TA proposed the answer, "Yes, because sociology is a science, so the study of sociological factors is still a study of the real world." This is sociologism writ large. Not only do proponents of sociologism want to take physicists down a peg or two, but they also want to view their own subject as more fundamental than the sciences they are studying. Everything hangs off sociology, as it were.

It is easy to ridicule sociologism. After all, advocates of this view still get on airplanes to fly to conferences. If you really believe that science is just a social construct, then you have no good reason for believing the airplane will not simply fall out of the sky. I, for one, would not take the fact that flying airplanes is a tradition of my culture as a convincing argument to get on board. So, proponents of this view seem to act like they believe at least some aspects of science are objectively true, while simultaneously propounding the opposite.

In the throes of intellectual enquiry, it is common to adopt overly extreme views, which later have to be walked back. This happens all the time on the speculative end of theoretical physics, e.g. the claim that the universe is literally a quantum computer [8], or that all entangled systems are literally wormholes [9], or that the universe is made of mathematics [10]. So let's not hoist all of sociology on the petard of their most extreme proponents, and instead look at the evidence on which their claims are based.

Most studies in the mould of the strong program proceed along the following lines. We first consider the modes of enquiry that are claimed to be the hallmarks of the scientific method, including such things as induction, falsifiability, the role of crucial experiments, skepticism of hypotheses that are not strongly supported by evidence, rational choice between programs of research, etc. Whichever of these (often conflicting) accounts of scientific enquiry you subscribe to, the sociologists find that they are violated in almost every case they look at, and identify sociological factors that played a role in theory choice instead.

There is not space to delve into specific examples here, so I will just mention Collins and Pinch's study of the role of the Michaelson-Morely experiment in the acceptance of Einstein's relativity [11], since that is of relevance to fundamental physics. In the usual story told to students, the Michaelson-Morely experiment is a crucial experiment that led physicists to reject the luminiferous ether, i.e. the idea that light waves must propagate in some medium in the same way that you cannot have water waves without there being some water to do the waving. The ether was replaced by Einstein's theory, which eliminates it. Collins and Pinch show that Michaelson-Morely experiments never produced conclusive evidence against the ether, despite attempts spanning several years under different experimental conditions.

Now, one might argue that the weight of experimental evidence for relativity that has been acquired since then is justification for its acceptance today, but still it was accepted long before any of this was acquired. One might also argue that Einstein's theoretical explanation of the symmetry of Maxwell's equations is the real reason why relativity was accepted, but this was not universally regarded as compelling at the time. Indeed, the controversy over this is the reason why Einstein won the Nobel prize for his explanation of the photoelectric effect rather than for relativity. While it is a stretch to conclude from this that relativity is just a social construct, the process of its acceptance was rather less rational than one might otherwise believe. At the very least, the story we tell about how relativity became accepted, which is part of the pedagogy of relativity, is largely a social construct.

However, the problem with case studies like these is that philosophical theories of science are not supposed to have the same status as mathematical theories. In the latter, if you find one counter-example to a theorem then the theorem is false[1]. Instead, philosophical theories of science propose norms, which we should strive to adhere to if we want to create reliable scientific knowledge. These norms include skepticism of hypotheses that have no evidential support, designing experiments that remove as much bias as possible, etc. Nobody is claiming that these norms are strictly adhered to 100% of the time, and that sociological factors play absolutely no role. Instead, the claim is that by attempting to adhere to these norms, the community as a whole, over long periods of time, will develop knowledge that is more reflective of the objective world than otherwise.

To put it another way, the "scientific method" cannot really be characterized in a precise way that is applicable to all cases. For any methodological principle that you might propose, one can find cases where it is not really applicable. But that does not mean that, upon looking at the particulars of a specific theory, one cannot decide whether the evidence supports it. We may use different methods and standards of evidence in fundamental physics, climate science, and psychology, but these all bear a family resemblance, and an expert in one of those fields can use the available evidence to decide how likely a given claim is to be true. The fact that we cannot give a discipline-invariant definition of *the* scientific method does not seem to have gotten in the way of the progress of any scientific discipline in particular.

Nonetheless, the studies of the strong program do show that social factors have played a larger role in the construction of "true" theories than you might otherwise have thought, so the idea that we should only pay attention to sociology in cases of "error" is suspect. Generally, all scientific discourse takes place within a language, and is conducted by entities that are situated within a society, with all the baggage that that entails, so social values are implicitly used in the construction of science whether we like it or not. Although physics makes heavy use of mathematics, so is arguably less influenced by the particulars of common language than other sciences, few physicists believe that the content of physics is entirely contained in its mathematical equations. We need discourse to understand what our theories mean, how they are connected to observations, and even what questions are sensible to ask of the them. Hence, the idea that physical theories may not be completely objective, and that sociological factors may play a role in their very construction, should at least be an option on the table, regardless of how small or large you think that role is.

One example where sociological factors have had a strong influence on physics is the dominance of the Copenhagen interpretation in the foundations of quantum mechanics. To modern eyes, it looks like the founders of quantum mechanics jumped to conclusions about the nature of (un)reality based on scant evidence. While much evidence that can be construed as supporting this kind of view has been acquired in the meantime, the Copenhagen view was accepted by the majority of physicists for decades without many physicists actually feeling the need acquire this evidence. Although there is more tolerance for diverse views on the interpretation of quantum mechanics today, Copenhagen has had a lasting influence on what physicists think a physical theory should look like, which may be cutting off fruitful research directions.

On the other hand, we do not want to endorse sociologism, in which we cannot explain why airplanes do not fall out of the sky, why children should be vaccinated, and why we should take action on climate change. The success of our fundamental physical theories surely means something for the objective physical world. Therefore, we should not replace the claim that physics is fundamental with the claim that sociology is fundamental instead. What we need is a theory of knowledge that can account for why we should trust that airplanes will not just fall out of the sky, but also allows external factors to influence physics in a controlled way. If it can also explain why smart people can be led to believe that physics is fundamental, and other smart people that sociology is fundamental, then so much the better.

[1] Of course, we always have the option of changing the definitions to make the theorem true, if doing so leads to a more useful theory, and this often happens in mathematics [12].

3 A theory of knowledge

To begin, I want to recall my own answer to the question of whether science tells us anything about the real world, that I gave in my undergraduate assignment. It already contains the seeds of the more sophisticated account I want to develop here.

Clearly, I reasoned, it is impossible that scientific theories have nothing to do with the observed empirical world. If a theory implied that airplanes must necessarily always fall out of the sky, then we would rightly reject such a theory as incorrect. At any given time, there is a large space of possible theories that are not in bald conflict with the available empirical evidence. When new evidence is acquired, the size of that space is reduced. It is still very large, so sociological factors can play a strong role in determining which of the theories in that space is "true", but the chosen theory still tells us something about the objective physical world because we cannot choose choose just any theory we like. There is a constrained surface of theories that are compatible with the evidence, and that constraint is reflective of reality.

Whilst I think this is a reasonable response to the assignment question, it is far from giving an accurate account of the nature of knowledge. This is because the set of theories that are compatible with the evidence is still truly vast, and contains many things that we would not want to call science. For example, the theory that is identical to current physics, but also posits that there are green aliens hiding on the dark side of the moon that are completely undetectable because they do not interact in any way with ordinary matter, is compatible with current evidence, but we would not want to call it scientific. In the philosophical literature, this problem is known as the underdetermination of theory by evidence. This problem does not seem to arise all that much in practice, so there are clearly other constraints that determine what counts as knowledge. Some of these may come from social factors, and some from more objective norms. To resolve this, we have to look at the actual structure of human knowledge.

Note that here I am diverging from what epistemologists (philosophers who study the nature of knowledge) usually mean by a theory of knowledge. An epistemologist would usually define knowledge as something like "justified, true belief" and study the way in which knowledge is discovered as a separate question from whether it is justified. For example, if I have an intuition in the shower that leads to a new theory of physics then I do not need to think about why I came up with that intuition (the context of discovery) to understand whether we should believe the theory (the context of justification). I reject the distinction between the contexts of discovery and justification because I think that key aspects of the process by which we uncover new knowledge determine its relationship to other knowledge and to the empirical world.

To understand the structure of knowledge, consider a network of nodes connected by links (see fig. 1). The nodes are supposed to represent items of knowledge. These can include basic facts of experience, e.g. "that car looks red", more abstract physical facts, e.g. "the charge of the electron is 1.602×10^{-19} C.", or even whole theories, e.g. "Electrodynamics". Clearly, the more abstract nodes can be broken down into smaller constituents, e.g. we can break electrodynamics down into its individual equations and explanations, so we can look at the network at a higher or lower degree of abstraction or coarse-graining. The links represent a connection between items of knowledge. I do not want to be too specific about the nature of this connection. It could mean, "can be derived from", "is a special case of", or even "there is a strong analogy between". Depending on the nature of the allowed connections, we would obtain slightly different networks, but that is fine so long as we allow sufficient types of connections to capture what we want to think of as the structure of knowledge.

There is evidence that the knowledge network, so constructed, would have the structure of a *scale free network* [13]. Without getting into the formal definition of such networks, the distribution of nodes and links in such networks has two important properties. Firstly, there are some nodes, called *hubs*, which have significantly more connections to other nodes than a typical node. Secondly, the shortest path you can take between two nodes by following links is much shorter than you would think, given the total number of nodes. This second phenomenon is called "six degrees of separation" after the idea that any two people on Earth can be connected by friend-of-a-friend relationships in about six steps, despite the fact that there are billions of people on Earth.

Now, obviously, I do not literally have the knowledge network to hand, but there are real world networks that ought to approximate its structure. We could, for example, look at the structure of the world wide web,

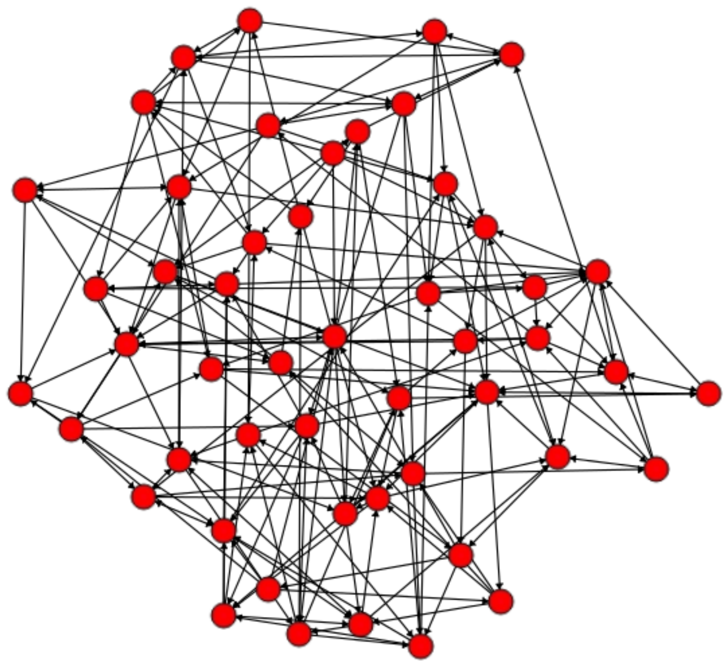

Figure 1: Example of a network of nodes and links.

where web pages are the nodes and hyperlinks are the links, or do the same thing for Wikipedia articles. We could take the nodes to be scientific papers and draw a link when one paper cites another. All of these examples have been found to approximate the structure of a scale-free network [13]. Now, obviously, such networks include things that we would not ordinarily want to call "knowledge", such as the name of Kanye West and Kim Kardashian's latest baby, or authors citing their own papers for no other reason than to increase their citation count. However, whenever a society of intelligent agents form a network of connections organically by a large number of individual actions, they seem to do so in a scale-free way. Since the knowledge network is generated in this way, it seems likely that it would be scale-free too.

In my 2015 FQXi essay, I gave a mechanism for the generation of knowledge by abstraction from analogies that could plausibly lead to a scale-free knowledge network. This process starts with nodes that represent the blooming, buzzing confusion of raw experience, which will end up being the nodes at the edges of the networks. We then draw analogies between nodes that are similar and, at some point, develop a higher level abstraction to capture the commonalities of those nodes. The links between every analogous node are then replaced with links to the higher level node, which reduces the number of links and complexity of the network. This process continues at higher and higher levels of abstraction, drawing analogies between higher level nodes and then replacing those by further abstractions. For further details, I refer to my 2015 essay.

Here, I want to make a few points about the structure of the network so generated. Firstly, the "real world" imposes itself on the network by the edge nodes that represent raw experience. The commonalities of those nodes impose the set of analogies it is possible to draw, and hence the abstractions it is meaningful to define. In this way, the empirical world imposes itself on even very high level abstractions, such as the fundamental physical theories, so those theories do reflect the structure of the physical world. However, there are also many ways in which societal contingencies affect the structure of the network, e.g. the interests of the participating agents affect the order in which analogies and abstractions are drawn, which can affect the global structure of the network. So we can have a strong role for both the physical world and sociological factors in determining what we regard as the "true" structure of knowledge.

It is important to note that *any* large set of interacting agents attempting to make sense of the world could use this process to generate a scale-free knowledge network. Intelligent aliens or artificial intelligences would work just as well as humans. What is important is that there are independent entities interacting via social connections. The structure of the network is partly reflective of the structure of the world, and partly reflective of the fact that a social network of agents is generating the knowledge. I do not really think that it makes sense to speak of "knowledge" outside this context. For me, knowledge is necessarily a shared understanding.

At this point, one might ask why a scale-free network is a good way of organizing knowledge, i.e. why would nature endow us with the capability to organize knowledge in this way? Any given agent can only learn a small part of the knowledge network. The hub nodes encode a lot of information at a high level of abstraction, such that it is possible to get to any other node in a relatively short number of steps. Our fundamental theories of physics, as well as general theories of sociology, are examples of such hub nodes. In our undergraduate studies, we tend to learn a lot about a single hub node, and work outwards from that as we increase our specialization. The existence of hubs ensures that the six degrees of separation property holds, so that it is possible to get from any two specialized disciplines to a common ground of knowledge in a relatively short number of steps. If, for example, we encounter a problem that requires both a physicist and a biologist to solve, they can work back to a hub that both of them understand and use that as their starting point. This enables efficient collaboration between disciplines. In general, scale-free networks are a very efficient way of encoding information.

The scale-free structure also explains why smart physicists can think that physics is fundamental, while similarly smart sociologists can think sociology is fundamental. If you only learn a limited number of nodes hanging off a single hub nodes, then the structure of your knowledge is hierarchical, with everything seeming to hang off the hub. If you are a physicist, with fundamental physics as your hub, you will see physics as fundamental to everything, whereas if you have a sociological hub you will see sociology everywhere. The reality is that there are several hubs, all with equal importance, that abstract different aspects of human experience. Both physicalism and sociologism assume a hierarchical structure of knowledge, with a different discipline at the top. If, in fact, the structure of knowledge is not a hierarchy, then the question of which discipline is the most fundamental simply evaporates. Now, of course, hub nodes are more important than other nodes because they encode a larger portion of human knowledge, so it does make sense to think of them as more fundamental than the other nodes, but there is no sense in which everything boils down to a single most fundamental node.

4 Conclusion

In conclusion, if human knowledge has the structure of a scale-free network, which is as much a feature of the fact that it is generated by a society of interacting agents as it is reflective of the physical world, then there is no sense in talking about a most fundamental area of knowledge. The question, "what is fundamental?" simply evaporates.

Although I have argued that physical knowledge is reflective of physical reality, we still have the question of how objective it is. Does the physics knowledge network necessarily have to look similar to our current theories of physics, or could there be a very dissimilar looking network that is equally efficient, formed on the basis of the same evidence? Even if we think of the process of acquiring knowledge as looking for the most efficient scale-free encoding, there could be local minima in the space of all possible networks, which would be difficult for a process based on locally adding nodes and replacing links to get out of. If two societies can end up with very different networks based on the same process, then this lends weight to the argument that social construction is the dominant influence of scientific theories. However, if the physics networks generated by this process all tend to look the same up to minor differences, then they are more reflective of the world than of society.

I view this as an empirical question. If we ever encounter an advanced alien civilization that has developed in isolation from us, will its physics network look similar to ours or not? I think it is likely that the answer is yes, but that is not something I can prove. Barring contact with aliens, we could answer the same question by placing a network of sufficiently advanced artificial intelligences on a knowledge gathering quest. This is obviously not a question we can answer right now, but maybe one day we will.

A Responses to Online Discussion

Since it was posted on the FQXi wesbite, this essay has generated an interesting online discussion. Unfortunately, I was not able to participate actively in the discussion at the time, so I respond to some of the more interesting comments here. There is not space to address every comment, so interested readers are encouraged to read the full discussion online [14].

Jochen Szangolies argues that the reliable convergence of ideas in physics should be taken as evidence that physics is objective and fundamental, citing the historical example of the convergence of measurements of the charge to mass ratio of the electron. However, an advocate of sociologism could equally argue that sociological factors are responsible for the convergence. There would be sociological pressure to come up with a unique theory. Discussions of which methods of approximation are appropriate, which systematic errors to take into account, which methods of measurement are most accurate, and which methods of data analysis to use, all occur within the scientific community. These are primarily responsible for convergence, and could be affected by sociological factors. Of course, I do not personally believe that sociological factors are primary in this process, but convergence of ideas in physics is not the knockdown argument against sociologism that it might appear to be.

Szangolies also argues that there is some ambiguity over what constitutes a “node” and what constitutes an “edge” in the knowledge network. He cites the example that if “Socrates is a man” and “Socrates is mortal” are nodes, then the derivation of the latter from the former is connected by the edge “All men are mortal”, which could also be construed as an item of knowledge, and hence a node. Note that we could look at this example differently, viewing all three items as nodes, and the rules of categorical syllogism as the connecting edge, but then perhaps these rules should themselves be a knowledge node.

I was deliberately vague about what should constitute a node and what should constitute an edge in the essay, precisely because of this sort of ambiguity. The network can be constructed at various levels of coarse-graining, depending on what we want to regard as the units of knowledge, e.g. scientific papers, entire theories, basic facts, etc. However, scale-free networks are self-similar, which means that the coarse-graining of such a network would also be scale-free, so to a large degree it should not matter exactly how we construct it. It is also important to realize that the knowledge network is only a model for the structure of knowledge, that I hope caputes important features of that structure, but cannot be expected to capture all subtleties. In this sense, it is like a model in physics, where carefully chosen approximations are made in order to yield a useful explanatory theory because working directly with the fundamental equations would be too complicated. I am open to the idea that a more general discrete combinatorial structure might better represent the structure of knowledge, e.g. a hypergraph in which more than two nodes can be linked by a hyperedge. The only important thing is that we can define a notion of scale-free for that structure and that a network can be used to approximate it. The network structure of the scientific citation network, the world wide web, and Wikipedia are meant to serve as evidence that knowledge can be approximately represented this way, but I freely admit that there are subtleties in the structure of knowledge that are not fully captured by these models.

Szangolies also points out that my knowledge network is epistemic, and does not deal with the ontic structure of the world, i.e. what is really out there. I acknowledge that this criticism is appropriate from a scientific realist point of view, but I adhere much more closely to a pragmatist theory of truth, in which what is true rougly corresponds to what is “useful”. This means I view my epistemic account of knowledge as more fundamental than any ontic account, and am skeptical about the meaning of the latter. I am committed to a naturalist metaphysics, in the sense that I think we must look at how the things we call knowledge are actually acquired, rather than positing an a priori structure that they must fit into.

John C. Hodges points out that human societies have often adopted similar social structures, and that

Darwinian natural selection may be responsible for this. A scale-free network is an efficient way of encoding knowledge, and I agree that once evolution has produced an intelligent social species, there would be Darwinian pressure to structure society in this way. So I expect alien species to structure their knowledge in a scale-free network, but this still leaves open the question of whether there is more than one local minimum for the structure of a knowledge network representing our universe.

Ken Wharton argues that the structure of a knowledge network can still be used to assert that physics is fundamental, in the sense that, as a hub node, it is more fundamental than non-hub nodes. Indeed, I recognize that the question of "more fundamental" makes sense. What I reject is the notion of "most fundamental" and the idea, common among physicists, that physics has the special status of being more fundamental than anything else.

Cristinel Stoica posits the idea that, since the world is fundamentally quantum mechanical, the knowledge network should be viewed as emergent from a unitarily evolving quantum state of the universe. Since I am not a straightforward realist about our scientific theories, I strongly reject this idea. The structure of the knowledge network determines in part the structure of our scientific theories, so I would say that quantum states are emergent from the network rather than the other way round.

Alyssa Ney points out the similarity between my view of knowledge and that posted by Quine in his essay, "Two Dogmas of Empiricism" [15]. Indeed, Quine is a major influence on my thinking, and I thank Ney for giving me a reason to reread this essay. Quine writes:

> The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. — W. V. Quine [15].

This is quite similar to my view of the importance of realizing that knowledge is constructed by societies and the role of experience at the edges of our knowledge network.

Ney also questions whether physicalism is in conflict with the strong program in the sociology of science. She argues that even if we have sociological explanations for the uptake of physical theories over time, this does not rule out the idea that there is also a more fundamental physical explanation for why they are true.

While this is true of the formal definition of the strong program, in which the use of the truth of a scientific theory as an explanation for its acceptance is rejected as a methodological principle, I believe that most advocates of this program are (at least covertly) social constructivists. Indeed, if you find sociological reasons for the uptake of physical theories everywhere you look then it becomes difficult to believe that any other explanation for their success is needed, and a descent into sociologism is likely, if not inevitable. Even rejecting sociologism, from my point of view, which is more pragmatist rather than realist, I find it difficult to understand what a "physical explanation" would actually mean in this context. Once I have explained why the theory is a useful addition to the knowledge network, in the sense of enabling an efficient encoding of experience in a scale-free way, I do not see what else is left to explain. I acknowledge that this account is not complete according to scientific realism, but debating the relative merits or realism and pragmatism will have to wait for a future essay contest.

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