
Good physicists like Stephen Hawking and Lawrence Krauss have recently generated a good deal of controversy by expressing disdain about what philosophers do. And the reverse also happens, if less common and more politely phrased. In my first year of graduate school I walked out of a class taught by Nelson Goodman when he contemptuously opined that, “It is well known that scientists have no idea what it is they actually do.” And yet, I would ask any scientist unconvinced of the steepness of the challenges philosophers face to spend an hour trying to explain why science works. Not whether it works—I’ll let you assume that science is indeed a route to knowledge about nature—but what it is we do that gains us that knowledge.

We all know the outline of how science is supposed to work. A theory should make definite and unique (to it) predictions that can be tested by experiments and observations. When a prediction of a theory is falsified, we abandon it; when a prediction is confirmed, we give more credence to the theory that produced it.

The trouble starts when we realize that in reality things are rarely so simple. There are often loopholes and caveats that argue for keeping a theory whose prediction has been falsified—and sometimes in the light of history that turns out to have been the right thing to do. The same experimental result can confirm two theories that contradict each other. Indeed, there are important historical examples where the same experimental result was taken to affirm a theory and its opposite, depending on the larger theoretical context. It can even happen that experiments are wrong or wrongly interpreted. Perhaps Nelson Goodman did have a point after all. At the very least we have to understand scientific knowledge as tentative and always subject to revision.

Still, history does suggest that in the medium term it is often the case that a single theory rises above all its rivals as clearly offering a far better explanation and basis for prediction than any known alternatives.

Perhaps the best wisdom is the simple fact that in the future we will know more. Or, as the novelist Philip Roth put it in a recent interview, quoting the boxer Joe Louis, “I did the best I could with what I had.”

But what about a case where a theory claims to solve a fundamental problem, but makes no predictions by which it could be tested by a doable experiment? Let’s make it worse: what if the community of experts who study the problem that the theory addresses is split into several sub-communities, one of which enthusiastically embraces the theory as somewhere between probable and obviously correct, while the others are either skeptical or embrace a rival theory with equal enthusiasm? Let me emphasize that there is no disagreement about either the results gotten by theorists or the lack of predictions, let alone tests.
An example of such a case is *Loop quantum gravity* (LQG), a highly developed approach to quantum gravity that has been developed for more than 25 years by a growing community of theorists who now number more than 200.

You might think that these are cases which savvy philosophers would stay away from, or at best counsel that scientists live with an unresolved conflict among rival research programs until the standoff is resolved by the usual sorts of evidence that scientists take as decisive. Indeed, the philosopher Paul Feyerabend observed that such conflicts can be enormously productive and that the disagreements are often resolved by the invention of a new and clearly better theory.

But philosophers love a good challenge, and Richard Dawid has taken it up, offering criteria for how scientists can decide which theory to embrace even in such circumstances. In his provocative book he proposes three criteria can suffice to provide sufficient theoretical evidence to justify the acceptance of a theory as dominant, if not true, even in the absence of any empirical evidence.

To show the surprising strength of Dawid’s criteria, I will illustrate them by the example of loop quantum gravity.

1) **The no-alternatives argument (NAA).** Proponents of LQG argue that there is no alternative to it as a successful solution to the problem of giving a mathematically consistent and ultraviolet finite quantization of general relativity, in 3+1 spacetime dimensions, without extraneous assumptions. They point to its correctly yielding general relativity as its semiclassical limit, while uniquely yielding the correct entropy of non-extremal black holes, which no other approach to quantum gravity achieves. They also emphasize that it satisfies the principle of background independence—one of the principles of general relativity—that rivals such as perturbative quantum gravity and string theory fail to do. There is even a formal uniqueness theorem, which shows that the Hilbert space the theory is built on is a necessary consequence of some very natural assumptions.

2) **The argument of unexpected explanatory coherence (UEA).** Dawid explains that this occurs when “the introduction of a new theoretical principle surprisingly provides a more coherent theoretical picture after the principle’s theoretical implications have been more fully understood.” Certainly, once the basic formulation of loop quantum gravity had been given in 1986 by Ashtekar’s new Hamiltonian variables, “one observes a long sequence of unexpectedly deeper explanations of seemingly unconnected facts or theoretical concepts.” These began with the formulation of the action principle, leading to the unexpected connection to the Plebanski formulation of general relativity. (Apologies here for the unavoidable technical references.) This soon led to an even more surprising and deep connection with topological field theory, a profound application of quantum field theory to define and compute topological invariants. These intimate but unexpected connections between general relativity and BF and Chern-Simons theory led to a revolutionary new
understanding of the dynamics of Einstein’s theory as a minimally constrained topological field theory. This had wide implications from the definition of horizon entropy to the basic formulation of the path integral. Once these new insights had been expressed in the formulation of spin foam models, more surprises were in store such as the emergence of Regge calculus (and hence general relativity) in the infrared limit and the discovery that spin foam models greatly simplify when expressed in the language of twistor theory. As Dawid stresses, when such surprises continually occur, each one deepening our understanding of the theory, it is hard to avoid the impression that we are on the right track.

3) The meta-inductive argument from the success of other theories in the research program (MIA). Loop quantum gravity began with the expression by Ashtekar of general relativity in the language of connections, or gauge fields, which reveals general relativity to be an aspect of Yang-Mills theory. The metric—a distinctive feature of general relativity not shared by other gauge theories—was relegated by Ashtekar to secondary status as an aspect of the electric field, making Yang-Mills gauge fields the primary carrier of all the forces in nature including gravity. The project of quantizing general relativity then is revealed as an aspect of the very successful project of quantizing gauge theories. Indeed the loops in LQG were borrowed from Polyakov, Migdal, and Wilson’s loops and, as in those works, express the fundamental duality of quantum gauge theories and extended objects. Indeed, LQG is the unique (see point 1 above) expression of that basic duality principle consistent with the diffeomorphism invariance and background independence of general relativity. As mentioned in point 2 above, LQG also extends the profound mathematical discoveries of topological quantum field theories in the unique, minimal extension to systems with local degrees of freedom. Thus, LQG is the unique extension of the most successful research program of the 20th century—quantum gauge theory in both physics and mathematics—to gravitation, and hence inherits all the success of that program.

Thus, when expressed in terms of Dawid’s three non-empirical criteria, the success of LQG as the unique correct approach to quantum gravity seems inevitable. And indeed these three criteria do explain why LQG has captured the hearts and minds of many of the most gifted young theoretical physicists for almost three decades now.

There is just one problem with this picture. Dawid uses these three criteria to argue for the unique inevitability of string theory. And indeed, the argument he frames for string theory does capture the reasons so many bright theorists have been captivated by string theory in spite of its complete failure to make any experimental predictions.

But if Dawid’s criteria can be used equally well to support rival research programs, and to justify the attentions of two competing research communities, his argument must be judged to fail. It fails as advice for scientists. And it fails as philosophy of science, because criteria for theory selection cannot be decisive, either descriptively or prescriptively, when they can be used equally well by both sides of a debate that at most one can win, if science is to progress from rivalry to consensus. The fact that his criteria may describe the reasons for consensus among adherents to
one research program means little when the same criteria explain consensus among a competing group of scientists. As Feyerabend argued so eloquently in his book *Against Method*, competition is healthy for science, but that competition must give way to consensus for a field of science to be recognized to have progressed.

The problem Dawid is supposed to be solving is how competition and disagreement give way to consensus, such victories being the steps by which science progresses. At best his criteria explain why disagreements harden into groupthink as consensus between rivals fails to be achieved. That is, his is a theory that explains only why science fails to progress in the absence of convincing empirical evidence.

To be fair, Dawid does not dispute the primacy of empirical evidence, when it is available and decisive. But he does say that his “core message” is that, “The novelty of current theories in fundamental physics is not limited to the conceptual level of those theories themselves. Rather, it extends to the meta-level of theory assessment where a shift in the balance between empirical and theoretical elements can be observed…Beyond that, it may eventually alter the philosophical understanding of the relation between a physical theory and the world.” The problem is then how do we distinguish genuine progress from attempts to use rhetoric to cover up the failure of a research program. Or, as Feynman put it, “String theorists don’t make predictions, they make excuses.”

The sad thing is how unnecessary this whole move is. String theory has inspired enough intriguing ideas and developments in both physics and mathematics to justify its continued study, in spite of its failures. What is at stake is only whether string theory should be prematurely declared a paradigm, to justify the abandonment of competing research programs. Dawid’s mistake is to dismiss the successes of the competitors in an effort to justify their exclusion.

Given more space I could detail Dawid’s several misunderstandings, which lead him to minimize the fact that string theory has robust competitors, thus mistaking consensus among a subgroup of experts for consensus among a whole community of experts. Dawid writes as if all experts in quantum gravity and unification were adherents of string theory, which means he fails to recognize that the consensus that so impresses him is limited to a subcommunity of experts, thus failing to recognize or credit the existence and significance of large communities of experts who follow rival research programs. This shows up in many misstatements he makes about these rival research programs, some of which are two decades out of date. By trying to explain a consensus that is not there in reality, he fails to address the real issue for the methodology of science, which is how disagreements among experts evolve to consensus under the weight of public evidence from experiment.

The point is that while we can all be seduced by a beautiful story, science is not about what might be true. Science is only about what people of good will can be forced—despite initial disagreement—to agree on by rational deduction from public evidence.
In the end, Dawid fails to explain why the arguments that convince string theorists they are on the right track fail to convince loop quantum gravity adherents—and vice versa. And this failure only highlights what we knew already, that the challenge of explaining and predicting the results of experiments is necessary for the progress of science. The claim that the usual empirical criteria for the success of a theory can be replaced by non-empirical criteria, and that these can be used to justify the elevation of a theory beyond the level of speculative hypothesis in the absence of empirical confirmation, is dangerous as it can only result in the fracturing of science into several stubborn groups of adherents to unproven speculative hypotheses who, blinded by groupthink, are each convinced of the irrelevance of the rival’s claims.

So how does science work? Perhaps we should all listen to the rock and roll band, They Might be Giants, who tell us why “science is real:”

“I like the stories
About angels, unicorns and elves
Now I like those stories
As much as anybody else
But when I’m seeking knowledge
Either simple or abstract
The facts are with science
The facts are with science

A scientific theory
Isn’t just a hunch or guess
It’s more like a question
That’s been put through a lot of tests
And when a theory emerges
Consistent with the facts
The proof is with science
The truth is with science”

lyrics by John Flansburgh and John Linnell, from “Here comes science”

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