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1. What is the relationship between philosophy and physics? What should the relationship be?

To someone who does not work in philosophy of physics, it can be hard to distinguish what a theoretical physicist does from what a philosopher of physics does. The differences lie in two areas: their goals and their methods.

The highest goal of theoretical physicists is to find the next theory. That profoundly colors the way they approach foundational ideas. Any idea that aids in finding the next theory is deemed useful. Sometimes the most suggestive ideas are so because they are on the edge of plausibility. However if they show promise of opening new pathways, physicists are quite willing to suspend critical scrutiny. There is no point abandoning a goose about to lay a golden egg because you suspect it may be a turkey! Gold is gold. As a result they may put up with what seems like patent nonsense to a philosopher.

For philosophers of physics, the goal is different. The basic questions remain those asked by philosophers for milennia: What is the nature of space? What is the nature of time? What is the nature of matter? How are things in the world connected? And so on. They seek answers from our best understanding of space, time and matter–modern physics. There is no room for tolerance of fringe thinking for that would compromise the project. They ask: What is our understanding now on the basis of our best science?

[1] My thanks to Zvi Biener and Balazs Gyenis for comments.

Philosophy of physics also differs from physics in its method. Philosopher of physics bring the sensibilities of philosophy to physics. To those outside physics, philosophy is synonymous with gazing in wonder at intractable mysteries. To the professional philosopher, the project is just the reverse. It is to take things that are conceptually puzzling and, through rigorous analysis, render them simple and transparent so that the original sense of mystery evaporates. Their method looks to the traditional demands of philosophy that theses must be clearly enunciated and defended by clear and cogent argumentation; and that these demands cannot be compromised. Physicists also value rigor of thought and are used to demanding it throughout their work. However they often relax that same rigor of thought when it comes to the deeper, foundational questions that cannot be settled by some experimental investigation or the proof of a theorem. Precisely because these instruments of decision are unavailable, philosophers of physics redouble the demands for rigor, for now the only real barrier to sloppy thinking is one's own self-discipline.

Einstein once remarked on the ability of our thought and conceptual systems to order experience as a "fact... which leaves us in awe, but which we shall never understand" and "The fact that it [world of sense experience] is comprehensible is a miracle."[2] That is an analysis of last resort for a philosopher, much as a physicist would resist as long as possible the conclusion that some striking physical phenomenon is just plain inexplicable. Take the wonder often expressed over the apparently unreasonable effectiveness of mathematics in physics. Should we not say that it is miraculous that the fundamental truths of a physical theory can be captured in the simplest of mathematical formulae? A philosopher would prefer to suggest, as I and others have,[3] that the perfection of the fit of mathematics to the physical world might be explained more by the creative power of mathematicians, who rework their mathematics retrospectively so that physical laws appear simple in the newer formalisms.

Another example that has recently intrigued me is the notion of being "physical" that is often called up by physicists to rule

[2] Albert Einstein, "Physics and Reality," pp. 290-323 in Ideas and Opinions. New York, Bonanza, n.d., on p. 292

[3] John D. Norton, "'Nature in the Realization of the Simplest Conceivable Mathematical Ideas': Einstein and the Canon of Mathematical Simplicity," Studies in the History and Philosophy of Modern Physics, 31 (2000), pp.135-170; on pp. 166-68.

out certain formal possibilities. For physicists, it is a notion with great power that can demand instant and instinctive assent from other physicists. For the philosopher, precisely because of its great power to prohibit without evident grounding, it stands as an authority in urgent need of analysis. As far as I can tell, it is a heterogeneous notion whose real content varies dramatically from context to context. It may merely record a gauge freedom, a case of empirical falsity or a failure to describe a system fully. Each has a different basis and justification. It proves not to be a unified oracular power that transcends ordinary means.[4]

2. How did philosophers contribute or fail to contribute to the development of physics in the 20th century?

It is impossible to give a direct assessment. All physicists work in a larger intellectual environment that has absorbed and continues to absorbs ideas developed by philosophers, just as philosophers in turn draw on new work by physicists. Much of what now counts as truisms for physicists about the relation of physical theory to experience and the "scientific method" were first introduced by philosophers. Einstein remarked in his 1916 obituary of the physicist-philosopher Ernst Mach that "...those who consider themselves to be adversaries of Mach scarcely know how much of Mach's outlook they have, so to speak, absorbed with their mother's milk."

Sometimes the influence can be delineated. Two examples are worth mentioning. In discussing his discovery of special relativity years later, Einstein reported on the importance of earlier philosophical studies: "The type of critical reasoning required for the discovery of this central point [of the illicit character of the absoluteness of simultaneity] was decisively furthered, in my case, especially by the reading of David HumeÕs and Ernst MachÕs philosophical writings." I have urged that the reading of Hume and Mach did not specifically provide ideas on time in this context, but a new view of the nature of concepts.[5] A second well

[4] See John D. Norton, "The Dome: An Unexpectedly Simple Failure of Determinism," Prepared for the Symposium "The Vagaries of Determinism and Indeterminism,"

PSA 2006: Philosophy of Science Association Biennial Conference, Vancouver,

November 2006. http://philsci-archive.pitt.edu/archive/00002943/

[5] John D. Norton, "How Hume and Mach Helped Einstein Find Special

known example is the role that philosophers of physics have played in reviving the scrutiny of and proposing solutions to the measurement problem of quantum mechanics. That is work that has been advanced as much by philosophers of physics as by physicists.

Finally I will mention work by John Earman and me, following work by John Stachel, on Einstein's "hole argument." It supplies the clearest statement of how spacetime is treated in general relativity and presents a challenge to any account of quantum gravity that relies on a fixed spacetime background.

3. What aspect of current work in physics can benefit the most from collaboration with philosophy?

This is a question that takes some hubris. For, as a philosopher of physics, I am one step removed from the latest researches in physics and so less likely to know where the advances can be made and what ideas may be useful to those making them. However with that said, I will suggest two areas in which I believe a change of course is called for.

While we have made significant advances in the measurement problem of quantum mechanics, the principal advance consists largely in the sense that the terrain of logical possibility has been thoroughly explored. For, after decades of work by some of the smartest minds, we have yet to achieve a consensus on what the right solution might be. That seems good reason to me to doubt that any solution is the correct one. Recall that the essential problem is to reconcile the linearity of the Schrödinger equation at microscopic scales with the known non-linearity at macroscopic scales. The obvious default is just to assume that the linearity breaks down through some as yet undiscovered physics that becomes active on these larger scales. The alternative that drives the measurement problem literature is the idea that the non-linearity can be evaded somehow by "interpreting" the theory differently. More bluntly, that means that the problem can be made to go away merely by thinking differently about the same equations. Indeed some of the re-thinkings proposed are so extreme as to count as their own reductio ad absurdum. Should we really believe that an unobserved linearity on a macro-scale is so sacred

Relativity," in M. Dickson and M. Domski, eds., Synthesis and the Growth of Knowledge: Essays at the Intersection of History, Philosophy, Science, and Mathematics. Open Court, forthcoming.

that it must be saved by imagining that the world of our experience is one of many of a vast multitude of equally real worlds, in which all possible outcomes of measurement are realized? Experience gives us only one world. When we are so desperate as to take such excesses seriously, the time has surely come to revert to the default idea that a non-linearity of who knows what form will intervene on macroscopic scales.

The notion of information has become pervasive in some circles of modern physics. Some of the work attached to it is quite enthralling. Here especially I think of work in quantum computing. It exploits the superposition of quantum theory in a most intriguing way, although it does not illuminate its foundations. Unfortunately much of the information talk elsewhere seems to be confused and, whenever I hear foundational work in which the notion of information figures centrally, I am alerted that extra critical scrutiny will be needed. In my view, the longest lasting excess lies in the literature that proclaims that information theoretic analysis provides a novel exorcism for Maxwell's demon. In joint work with John Earman, I have argued that these exorcisms rely on demonstrations that are circular or groundless[6] and, elsewhere, that work on Landauer's Principle depends on a misapplication of statistical physics.[7] There is a reluctance in the physics community to take these warnings seriously since information theoretic notions seem so fertile. Yet, in my view, decades of theorizing have shown that they are fertile only in producing impressive castles that float in mid-air without sound foundations.

Finally, I have been impressed by the tension between the enthusiastic reports of successes in string theory and the cries of alarm from critics that the theory is no theory at all and has no experimental confirmation. Since the complaints are essentially methodological, I think it would be very useful if philosophers of physics engaged in the problem. However it is hard for a philosopher of physics, who must be one step removed from the physics community, to develop a sufficiently deep understanding of the rapidly changing landscape of string theory.

[6] John Earman and John D. Norton, "Exorcist XIV: The Wrath of Maxwell's Demon." Studies in the History and Philosophy of Modern Physics, Part I "From Maxwell to Szilard" 29(1998), pp.435-471; Part II: "From Szilard to Landauer and Beyond," 30(1999), pp.1-40.

[7] John D. Norton, "Eaters of the Lotus: Landauer's Principle and the Return of Maxwell's Demon." Studies in History and Philosophy of Modern Physics, 36 (2005), pp. 375-411.

4. What area in contemporary philosophy of physics is most fertile?

There are many smart people working in all areas and I hold high hopes for them all.

5. In your opinion, which area of physics holds the most exciting promise in the coming decades?

Again, there are many smart people working in all areas. Being a philosopher gives me no special powers of prediction concerning new advances.

6. How were you initially drawn to the field and what are some examples of your work that has influenced the discipline?

My initial interest in philosophy of physics came from a real sense of wonder at the content of modern physical theories. That is a sense I have never lost. I was drawn to graduate work in philosophy of physics rather than in physics since philosophical work let me focus most directly on the foundational issues that fascinated me most.

Over time, my interest in the content of the theories expanded to a fascination with how it was possible, first, for ordinary people to discover amazing results and, second, to have good reasons to believe them. The first fascination led to sustained research into Einstein's discovery of general relativity. That inevitably led to a richer understanding of the second. So I regard those historical investigations as a contribution both to history and also to epistemology. Einstein's discovery of general relativity remains today as one of the signal achievements of modern science; and so the details of how he made his discovery must figure in any epistemology that aspires to do more than tell us what happens when we perceive blue patches.

While readers can find a more complete synopsis of my work on my website (www.pitt.edu/~jdnorton), the most lasting contribution of my work was the analysis of Einstein's Zurich notebook. The notebook contains the scratch pad calculations Einstein made during a decisive phase of his work on general relativity and provides a quite fine-grained reconstruction of the course of his discovery.[8] The analysis of Einstein's pathway to general relativity led to another contribution that I believe has proven useful: a sustained examination of Einstein's ideas on general covariance and the protracted debates over them that followed.[9]

Finally, I believe that the analysis John Earman and I gave of Einstein's hole argument has provided a template for later analysis of gauge freedoms and the criteria used to decide when a formal equivalence betokens physical equivalence. The two conditions we described remains those in use today: verification and determinism.[10]

[8]See John D. Norton, "How Einstein Found His Field Equations: 1912-1915," Historical Studies in the Physical Sciences, 14 (1984), pp. 253-315; reprinted in D. Howard and J. Stachel (eds.), Einstein and the History of General Relativity: Einstein Studies Vol. I, Boston: Birkhauser, pp101-159; and more recently, Michel Janssen, John D. Norton, Juergen Renn, Tilman Sauer, Michel Janssen, John Stachel, Commentary in The Genesis of General Relativity: Documents and Interpretation. Vol. 1. General Relativity in the Making: Einstein's Zurich Notebook. Dordrecht: Kluwer.

[9]John D. Norton, "General Covariance and the Foundations of General Relativity: Eight Decades of Dispute," Reports on Progress in Physics, 56 , pp.791-858.

[10]John Earman and John D. Norton, "What Price Spacetime Substantivalism? The Hole Story," British Journal for the Philosophy of Science, 38 , pp. 515-25.

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