This is a topic I touched on in previous editorials, where I pointed to the difficulty we philosophers oft experience in explaining what we do. Since then, I've tried improving the way I communicate to non-philosophers, and especially non-academics.

Perhaps the next challenge is to definitively get off the armchair and to find effective ways to sneak into the scientists' living rooms and to talk to the public. I am perfectly aware that this sounds naïf and in fact, as you shall see, John has a much less disenchanted view on the issue. But I don't want to anticipate too much, nor to hold you up for too long, and I leave the floor to John.

Federica Russo
Philosophy, Kent

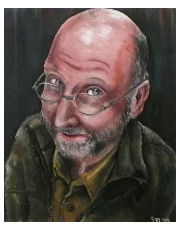
§2

Features

Interview with John Norton

John Norton is Director of the Center of Philosophy of Science and professor at the Department of History and Philosophy of Science at the University of Pittsburgh.

Federica Russo: Thanks for agreeing to answer a few questions, John. First of all, could you tell our Reasoners something about your intellectual history and especially how you ended up running the Center so successfully?



John Norton: My first degree was in chemical engineering. Then I moved into history and philosophy of science. My early work was in history of general relativity. Most notably, I gave the first analysis of Einstein's "Zurich Notebook" in which we can reconstruct the steps Einstein took to his greatest discovery, general relativity, and at a level of detail we could scarcely dare to hope for. If you want to look over Einstein's shoulder as he makes his greatest discovery, I've put some choice pages with commentary on my website here.

From there I moved into many further topics in history and philosophy of physics and general science. For me the interest in Einstein's discovery had always been essentially epistemological. Einstein's theories are astonishing. How could Einstein find them and know that they are right? That epistemic orientation has never left me, so I've increasingly been working on inductive inference. One powerful motivation has been to find an approach to inductive inference that is both philosophically sound and applicable to the intricacies of the real case studies in science that have long fascinated me. The failure of the existing literature to provide such an account led me to the "material theory of induction." It urges that there are no universal schema for inductive inference. What warrants an induction are facts. What likely would interest readers of the Reasoner most is this corollary: a Bayesian or probabilistic account of inductive inference, while it may often work very well, cannot succeed universally. I've elaborated on this in papers on my website. They include counter-examples that I believe are unassailable.

For five years, I chaired the Department of History and Philosophy of Science at the University Pittsburgh. Since my tenure did not run the department into the ground, my reward was to be pressed into more administration in the Center for Philosophy of Science. Unexpectedly, directing the Center turned out to be the most fun I have ever had, academically. I am surrounded by the good cheer of visiting philosophers of science. They come expecting to meet lots of interesting people, to hear lots of talks, to engage in discussion of their work and that of others and then to write great papers.

The success of the Center is entirely due to this extraordinary confluence of people with good will and intellectual vitality. My role has simply been to look after everyone as best I can when they are here and keep the doors open to everyone. Indeed that has been the thing at which I've worked hardest. The Center is a resource available to everyone in philosophy of science and I encourage everyone to come and visit. To help you picture what it is like, the Center's website documents what we do here at a quite personal level. See the "donuts" pages, for example.

FR: Physics has been the queen of the sciences for a long time. As a leading scholar in the philosophy of relativity and of spacetime, do you think reasoning in physics has something peculiar that makes it different from the other sciences? Or that the other sciences should learn from? Or . . . ?

JN: I don't think that there is anything special about philosophy of physics. What we do in philosophy of physics is what everyone does in philosophy: we try to reason clearly and soberly about philosophical puzzles fully able to explode our heads. However philosophy of physics has proven to be especially fertile since it provides us with a seemingly endless parade of precisely defined, but profoundly intractable problems. That they are precisely definable assures that there is plenty for philosophers to work with; that they are intractable assures that the work will continue indefinitely.

The prominence of philosophy of physics in twentieth century philosophy of science depends in part on an historical accident. The advent of relativity theory in the early part of the century and the resulting challenge to old philosophical wisdoms riveted a generation of philosophers just as they were creating the new field of philosophy of science. That meant that the particular theories of physics and methods amenable to them were located at the foundation of the new field. It happened to be relativity theory specifically in the 1910s and early 1920s that served this role.

Whether this was good or not can be debated. However it could certainly have been worse. It was Einstein and his thought on space, time and relativity that informed the field. Had the founders delayed ten years, it might have been quantum theory and the inchoate thoughts of Niels Bohr that informed us.

There was a time when the ways of philosophy of physics exerted an undue influence on philosophy of science as a whole. Those days are long past. Philosophy of biology and philosophy of cognitive science, to name just two fields, have been thriving to the point of venerable maturity and now speak as loudly in the general arena.

FR: All the past visiting fellows of the Center I met have fantastic memories about their stay. Can you tell us what you consider to be the most important aspect of a visiting fellowship at Pitt?

JN: That you have fantastic memories of the Center is gratifying, but actually tells me a lot about you. There is a real opportunity in the Center to meet people, exchange ideas and find new perspectives. As you know from your visit, it is a place in intellectual ferment. When you visit, what you get back depends entirely on your willingness to participate. The more you put in, the more you get out. Knowing that is perhaps the most important thing.

FR: This probably goes a bit off track, but do you think philosophy of science has or should have any relevance for science itself and for society? In other words, do you think philosophers should get off the armchair? And if so, what to do once we stand in the broader academic livingroom or even in the real world?

JN: People in the broader community are interested in foundational questions in science. Major issues of public policy may depend on them. Prominent examples are the issues of climate change and, in the US, challenges to evolutionary theory. We philosophers of science are professionally best equipped to deal the difficult foundational questions that arise. Alas, we have a poor track record as public intellectuals. Our work does inform the public, but typically only after it has been filtered through the thought and work of popular writers who are well-meaning but often have lesser philosophical skills.

My sense is that our professionalization is the obstacle. We are rewarded for ever tighter, ever more cramped analyses of issues. And that is appropriate, for none of the problem we address is simple. Alas, simplicity is what the public wants. They are fed a diet of five second sound bites. Anything more is deemed indigestible by a media that seeks out purveyors of glib sound bites and avoids the droning professors who might really understand the issues.

Take one example. In popular arenas, the idea that falsifiability is the gold standard of science flourishes simply because it is an easy point to make and an easy point to understand. Those of us who work on the problem know that it isn't that simple. Aside from beloved toy examples, bad science is almost always not unfalsifiable but actually falsified. And procuring falsifiability can be done cheaply by contriving some arcane prediction that is, in principle, falsifiable, but testing it outstrips present practicalities. What makes a theory good science is a close and thorough grounding in evidence. But how can we convey what that really amounts to in a five second sound bite?

The relationship between science and philosophy of science is more complicated. For me the interesting issue is to know where the philosophy of science ends and the science starts. In philosophy of physics, the work we do has become so technical that it is often hard to know. Clear principles that separate the two are elusive. I know of one clear division. A line is passed when a philosopher of physics starts to propose new physical theories. For the critical scrutiny of the foundations of physics demands that philosophers maintain the highest critical standards. To propose new theories, however, one must allow some slippage in rigor lest promising but ill-formed nascent theories are lost.

My rule of thumb is that philosophers of physics seek to understand the foundations of current physical theories, to which we apply all due rigor. It is the the job we are best equipped to do. The business of finding new theories is the physicists'. It is the job for which they are best equipped.

FR: One last question, and then I set you free. I know you studied engeneering before going into philosophy. Does it ever happen to you to miss the more practical and pragmatic way of reasoning of non-philosophers?

JN: What I find energizing about my colleagues in history and philosophy of science is the extraordinary breadth of their interests. Essentially no one has a linear history of undergraduate and graduate work in just one area. We all followed paths that meandered until we found in HPS a garden so rich and beautiful that it was our journey's end. My colleagues are all extraordinarily outward looking. Those who work in induction and confirmation are talking to mathematicians and statisticians. Those who work in philosophy of physics are talking to cosmologists and quantum mechanics. Those who work in philosophy of cognitive science are talking to neuroscientists. Our conversations and talks are full of many sciences, many methods and many ideas.

Perhaps I should add that I was never a good engineer. One of my first jobs was to troubleshoot a "sour water stripper" that, from time to time, would dump foul water down the drain. I started with the plant's design drawings and, after some collaboration with the refinery's lab and several month's work, provided a complete account of the chemistry of sour water stripping. In an appendix, I noted that whoever was dumping caustic soda into the refinery drainage system had to stop—that is what was messing things up. That remark was all that really mattered in the report. It was a conclusion a better engineer would have found in an afternoon!

On the presuppositions of composite propositions

Consider the following example: when one says, "Zhang San's son has been admitted to Peking University, and Li Si's son has also been admitted to Peking University." What does the speaker presuppose and how to get the presupposition? According to Ewa Mioduszewska ("A Solution to The Projection Problem for Presupposition of Compound Sentences within Ulrich Blau's Three-valued Logic System") we can employ the definitions of "∧", "∨" and "→" in three-valued logic to describe the corresponding presuppositions of composite propositions, so if we define ∧ as follows: the

ϕ \ ψ	1	#	0
1	1	#	0
#	#	#	0
0	0	0	0

presupposition of $\phi \wedge \psi$ expresses the following conditions: (1) the presupposition of each conjunct is true; or (2) ϕ's presupposition fails and ψ is false; or (3) ψ's presupposition fails and ϕ is false. If none of (1), (2) and (3) hold, $\phi \wedge \psi$ is neither true nor false. Conditions (1)–(3) reflect the following cases respectively: (1′) The values of ϕ and ψ are 1 or 0; (2′) The value of ϕ is # and of ψ is 0; (3′) The value of ϕ is 0 and of ψ is #. Obviously, these are the cases in which $\phi \wedge \psi$ gets precise truth-values. However, this does not fit our intuitions if we take them to be presuppositions. Consider the following example under this interpretation: when one says, "Zhang San's son has been admitted to Peking University, and Li Si's son has also been admitted to Peking University", he presupposes the following: (a) Zhang San has a son and Li Si also has a son too; or (b) Zhang San has no son and Li Si's son has not been admitted to Peking University; or (c) Li Si has no son and Zhang San's son has not been admitted to Peking University. As we know, presuppositions are assumptions that are made in advance, and one of its features is that the speaker takes it for granted. However, in any case the speaker would not take for granted that there are certain conditions that would make the conjunction get truth-values in three-valued logic. The reasons are clear, firstly, it requires that the speaker understand and accept the definition of $\phi \wedge \psi$ in three-valued logic. Even if the speaker accepts the meaning of a conjunction, still it requires that the definition of ∧ apply in every three-valued logic system. Otherwise, it may lead to some absurd result, namely, if you want to know the presupposition of a conjunction, you must first ask the speaker what kind of three-valued logic he has in mind. For those who support Bochvarian three-valued logic, only (1) is left from the above to be a presupposition. (For more details, see: L.T.F. Gamut, 1991: *Logic, Language and Meaning*, University of Chicago Press.)

The presuppositions of composite propositions can be divided into several situations: (a) The presuppositions of component propositions are the same, or (b) one component proposition is the positive or negative of another component proposition's presupposition, or (c) one component proposition implies another, or (d) the presuppositions of component propositions are different.

Among the three main kinds of composite propositions, the presupposition of a conjunction is the easiest to handle. When all conjuncts have the same presupposition, it's also the presupposition of the conjunction. (For example, "Zhang San's son is not only excellent, but also very filial".) When the presuppositions of conjuncts are different, the conjunction of those presuppositions is the presupposition of the whole proposition (such as "Zhang San's son is excellent, and Li Si's son is also excellent"). When the presupposition of one conjunct is simply another conjunct, the presupposition can not become the presupposition of the whole proposition; consider "Zhang San has a son, and Zhang San's son is very excellent". When one conjunct's presupposition implies another conjunct, the presupposition is the presupposition of the whole proposition. (Consider "Zhang San has children, and Zhang San's daughters are very excellent".) But when one conjunct's presupposition is implied by another conjunct, the presupposition is not the presupposition of the whole proposition (for example, "Zhang San has daughters, and his children are all excellent"). Such an interpretation meets the following condition: the whole conjunction has no truth-value if the presupposition fails.

As for a disjunction, when each disjunct has the same presupposition, the presupposition automatically becomes the presupposition of the disjunction (such as "Zhang San's son wounded Li Si's son, or Li Si's son wounded Zhang San's.") When the presuppositions of the disjuncts are different, the disjunction of the dis-