

Thanks to the committee for organizing such a great Colloquium.

My charge is to discuss “fifty years of philosophy of science.”

We had a good opportunity yesterday to think about the extensionalist tradition in analytic philosophy. One of the great extensionalists, of course, was Quine, who famously said that “philosophy of science is philosophy enough.” This Quinean maxim, along with basic extensionalist principles, implies that we should be able to substitute the phrase “philosophy enough” for the phrase “philosophy of science” inside of any context *salva veritate*. In this way, we transform my assigned topic to “50 years of philosophy enough.” Or maybe, “Fifty years of philosophy – enough!” But the attitude expressed by that slogan is shown to be untenable by the fact that this magnificent Colloquium is still a living and flourishing institution, showing no sign of slowing down – thanks in large part to the efforts of so many people sitting in this room.

This I take to be *reductio ad absurdum* on the Quinean extensionalist tradition – indirectly providing yet more support for the Hegelian alternative we enjoyed hearing about yesterday.

But I suppose I still should try to say something about fifty years of the philosophy of science.

Notably, the organizing committee neglected to specify *which* fifty years they wanted me to talk about.

I was leaning towards covering the years 710 to 760, thinking to make things as easy on myself as possible.

But then I noticed a striking correlation: The number of years’-worth of philosophy of science that I had been asked to discuss was precisely equal to the ordinal number appended to this year’s Colloquium.

And then I remembered the principle of the common cause – which happens to have been the topic of a talk by Elliott Sober at the 21st Chapel Hill Colloquium:

“When you find a statistically improbable correlation between two variables, neither of which is plausibly a cause of the other, postulate a third variable that serves as a common cause of the two correlated variables.” That’s only the rough version of the principle, of course, leaving off the leaving off the whistles and Bell’s inequalities.

Also, I remembered the famous principle of Inference to the Best Explanation, which is widely believed to be a central axiom of scientific methodology.

And the best explanation I could think of for the coincidence between the number of years I had been asked to discuss, and the number of times this Colloquium has convened, was this: Since we are this weekend celebrating the first fifty years of the Chapel Hill Colloquium, the organizing committee wanted to hear a talk about the first fifty years of the philosophy of science.

So I set out to prepare some remarks on the first fifty years of philosophy of science.

The trouble is that I’m not sure if anyone knows which fifty years those are.

It is said that Plato’s *Timaeus* contains a philosophy of science, and I’m not aware of any earlier efforts – but I also understand that the experts are uncertain about the dating of that dialogue.

Amid these discouraging reflections, I recalled that in the year 1967 – quite coincidentally, the year of the first Chapel Hill Colloquium – my old teacher John Earman published an article

entitled “Going Backward in Time,” in which he argued forcefully that we have no good reason to believe that time travel to the past is possible.

One might well wonder why Professor Earman thought it necessary to give forceful arguments for a thesis widely believed to be obviously true – as this one was at the time.

Of course, in the intervening years it has been thoroughly established that Earman’s thesis was in fact false. As we’ve learned from such thinkers as David Lewis, John Carroll, Kadri Vivhelin, and Katrina Elliott, time travel is indeed possible, and, no doubt, it happens all the time.

This raises a difficult problem: Why did Earman argue so forcefully for a thesis that is so very very false?

One excellent explanation is provided by the hypothesis that Earman was well aware of the possibility of time travel, and was highly motivated to keep it a secret. And he certainly would have been thus motivated had he been a time traveler himself, responsible for numerous interventions in the past.

No better explanation for these data being available, we have no choice but to conclude that Earman’s 1967 article was indeed cover-up for his role in some mischievous manipulations of human history.

And this makes it clear at last why the philosophy of science propounded in Plato’s *Timaeus* – namely, “*a likely story!*” – sounds so much like something that Earman himself would have said.

The *Timaeus* doctrine can now be seen for what it is: Namely, an error planted in the philosophical record by Earman himself – along with so many other historical errors in philosophy -- only so that he could achieve fame and glory by correcting these errors in the late twentieth century -- An ingenious method of achieving philosophical renown, if an unscrupulous one.

As unlikely as these claims might seem, it can hardly be denied that they explain the otherwise puzzling data very tidily – and I must urge you all to remember the methodological postulate of IBE: The more compelling my explanation appears, the more credence you are obliged to invest in it.

The point that I have driving towards all along should now be clear: Since key ideas in the history of the philosophy of science going at least as far back as Plato can now be confidently attributed to the time-travelling John Earman, who at least as early as 1967 was working to cover his tracks, we can now see that the 1967 has as good a claim as any year to be the historical starting-point of the discipline of philosophy of science.

Thus, in a manner of speaking, the first fifty years of the philosophy of science turn out to coincide with the first fifty years of the Chapel Hill Colloquium. A very happy accident that I am certain the organizing committee could not have anticipated.

I guess this settles pretty well which period in the philosophy of science I’m supposed to be talking about.

But alas, my problems are not over. Rather they seem to have only begun.

For now, as I look at a large temporal part of philosophy, I am charged with carving off from it the part that answers to the name “philosophy of science.”

To carve in this way, I suspect, is *not* to carve at a joint – or anywhere in the vicinity of a joint. I suspect that philosophy *does have* joints – for it appears to be much more like an arthropod than like an echinoderm – it’s just that this doesn’t seem to be one of them.

You might think this must reflect the fact that philosophy of science has a Lockean nominal essence, which means we ought to be able to give it a nice definition. However, I must admit that I find myself at a loss to give any such definition.

Well, there’s one definition I can give: “Philosophy of science is what people do who publish articles in journals like *The British Journal for the Philosophy of Science*, and *The European Journal for the Philosophy of Science*, -- and of course, the American journal for the philosophy of science, which is just called *Philosophy of Science*.”

A more illuminating definition would be desirable. But I have no idea how to give one – which is a little bit embarrassing, since for almost 20 years now, I have been claiming philosophy of science as my AOS.

Ironically, and unfortunately for me, what is perhaps the most conspicuous overall trend within the philosophy of science since 1967 is that it has only gotten harder to define what philosophy of science is supposed to be, exactly.

This is mainly for three reasons.

The first reason is that since 1967 the subdiscipline of philosophy of science has been busily spawning subsubdisciplines of itself, and subsubsubdisciplines thereof, at an alarming rate. These days, it isn’t enough to be a philosopher of physics – it isn’t even enough to be a philosopher of quantum physics – you’ve got to figure out whether you’re a philosopher of algebraic quantum field theory, or a philosopher of quantum computation, before you can be sure which conferences you’re supposed to go to.

The second reason is that the things that were traditionally supposed to be distinctive of the philosophy of science more and more seem to characterize philosophy as a whole.

The very first issue of the journal *Philosophy of Science*, published in 1934, opens with an editorial by William Marias Malisoff, which begins as follows:

“Philosophy of science is the organized expression of a growing intent among philosophers and scientists to clarify, perhaps unify, the programs, methods, and results of the disciplines of philosophy and of science. The examination of fundamental concepts and presuppositions in the light of the positive results of science, systematic doubt of the positive results, and a thorough-going analysis and critique of logic and of language, are typical projects for this joint effort.”

In this 21st century, of course, a very large portion of the work done by professional philosophers fits Malisoff’s characterization: Collaboration with scientists is now common among philosophers of mind, philosophers of language, and ethicists; virtually every subdiscipline of philosophy – with the possible exception of the history of philosophy – today makes a lot of noise both about the importance of respecting the positive results of the sciences, and about the

appropriateness of directing philosophical critique at the alleged positive results of the sciences. Given Malisoff's definition of philosophy of science, we're all philosophers of science now.

A third reason why it has become more difficult to define philosophy of science is that science itself has grown so prodigiously. If it was hard to solve the demarcation problem before, it is infinitely harder now that so many new things are included under the aegis of "science."

Recently, a non-academic friend asked me, "So, to be a philosopher of science, do you have to keep up with all the new developments in science?" My first impulse was of course to laugh hysterically, my second was to break down and weep. The vector sum of those two responses turns out to be somewhat embarrassing. The answer to my friend's question of course is that that would be impossible: Not even a scientist can keep up with all the new developments in science.

And this suggests that the name "philosophy of science" might well be elliptical, for something like, "philosophy of the tiny fraction of science that the philosophers who regularly publish in a certain half-dozen or so journals happen to know a little bit about."

That's not really fair, though. The point I've been trying to make is not that there's anything wrong with contemporary philosophy of science, but rather that there's no obvious good way of characterizing what is distinctive about philosophy of science as such, for reasons that have been exacerbated over the past fifty years.

And that brings me to my main point, which is aimed mostly at the younger graduate students: There's a lot of pressure in graduate school to pick an AOS; this can be a frightening thing to do, because it seems so laden with consequences: Your choice of an AOS seems to define your identity as a philosopher.

I think it might be helpful in grappling with this problem to remember that the available AOSes are not, like, natural kinds – they don't carve philosophy at the joints – and they aren't artificial kinds that answer to some stipulative definition, either. Or at least, this is true of some of the available AOSes – maybe not all of them. "Philosophy of science" is a catch-all term for a whole bunch of different kinds of philosophical projects, and there doesn't appear to be much rationality to the way its boundaries are drawn. Maybe this wasn't true fifty years ago, but it is now. What will define your identity as a philosopher is the particular project you take on, not the AOS that comes along with it. So don't worry about whether you want to be a philosopher of science, or a metaphysician, or a philosopher of language, or an epistemologist – worry instead about the stuff you want to write, and then let the AOS chips fall where they may.