Interview:

Alireza Fatollahi

Dr. Alireza Fatollahi is an assistant professor at Bilkent University. He received his PhD in philosophy from Princeton University. His main areas of interest are philosophy of science and early modern philosophy. He has published in *Canadian Journal of Philosophy, Episteme, Philosophy of Science, Pacific Philosophical Quarterly, Synthese*, and *European Journal for the Philosophy of Science.*

What do you think about the famous division of philosophy of science: Popper or Kuhn?

I take it you're alluding to two different approaches to philosophy of science, each represented archetypically by one of these figures. Popper emphasizes the normative side: asking questions like, what must an epistemic enterprise look like to count as science? He aims to sketch an ideal picture of science, even if we never fully attain it. Kuhn, by contrast, insists on describing how science actually works and emphasizes its human and social dimensions. I think philosophy of science risks being either quixotic or reduced to pure history if it takes only one of these sides. We don't want a utopian account of science that is unattainable for human beings. But as philosophers (rather than historians) of science, we also don't want simply to recount what has happened. We want to explain the striking fact – or at least the strong intuition – that science is, epistemically, the best humanity has to offer. In other words, a normative account of why science works can succeed only if it is genuinely informed by how science works.

We observe that scientists are fond of Popper for explaining scientific method. Do you see a reason why?

I've always been puzzled by Popper's popularity among non-specialists, though I didn't realize he was especially admired by scientists. Among philosophers of science today, it's hard to find anyone who thinks Popper has much to offer. Yet outside the field he is often treated as central – as if he were to philosophy of science what Aristotle is to philosophy more broadly. This can't be because of the depth of his ideas: they are now widely regarded as simplistic. So I think the main reason probably is that his signature idea – the importance of falsifiability – is both easy to

grasp and easy to endorse. If you don't have time to wrestle with the complexities of a philosophical account of science, it provides a ready-made view that feels satisfying. In fact, when it comes to easily accessible views, the options are rather limited: (i) Popper, (ii) some version of positivism, or (iii) fundamental skepticism about science's rationality. All these are highly implausible (or so it seems to me), but they are the obvious off-the-shelf choices unless one engages more deeply with philosophy of science.

As a philosopher working on philosophy of science, how has getting a BSc degree on physics affected your perspective on your work?

It has helped me but not in the way one might expect. I use statistical tools in my work, but I actually learned them during my philosophy studies, not as a physics major. To be honest, I wasn't a very strong student during my undergraduate years. I was preoccupied with existential questions and had little time or energy left for physics. Still, one experience from studying physics has been deeply influential in my current research. Even though I attended the top physics program in Iran and had professors with excellent training, I was struck by what seemed to me as a lack of critical engagement with the theories they taught us. Of course, not everyone was the same, but most physicists I encountered were primarily concerned with applying the theories rather than questioning their foundations. Questions that appeared to me as pure physics questions were quickly dismissed as "philosophical." Thus, insofar as my experience was concerned, Kuhn's description of scientists as puzzle-solvers felt remarkably accurate. This raised for me a set of enduring questions: What is the proper aim of science? Was I simply naïve to want to understand the meaning behind the equations of quantum mechanics? Are scientific theories just "black boxes" that matter only insofar as they work? Later, when I turned to the history of science, I realized that things had not always been this way. Figures like Leibniz and Newton approached physics in a manner much closer to my own instincts than to those of my professors. That discovery planted the first seed of what has become central to my research. Put very simply, I believe science has always had at least two principal aims that sometimes clash: (i) explaining why nature works the way it does, and (ii) predicting new phenomena. Yet the history of science suggests a gradual but decisive shift: whereas once explanation was, by far, the most important goal, prediction has gradually come to dominate over explanation. This doesn't mean scientists don't care about explanation today. But if theory A offers slightly better predictive success while

theory B provides far deeper explanatory insights, I suspect most scientists would favor A. This is what one might call the tyranny of data. Much of my research today revolves around this general idea.

You completed your PhD on early modern philosophy.
Do you see the philosophers from the early modern era
capable of answering contemporary questions of philosophy?

My dissertation was in contemporary philosophy of science, although I've always had a serious interest in early modern philosophy, and my research is increasingly shifting in that direction. When it comes to answering contemporary questions, I think no conceptual framework is better suited than the contemporary one. One of Kuhn's central insights is that paradigms tend to ask precisely those questions they can answer. In that sense, early modern philosophy is not equipped to address contemporary problems more effectively than contemporary philosophy itself. But that doesn't mean the history of philosophy is irrelevant. On the contrary, engaging with early modern thought – or other historical periods for that matter – offers us a perspective from outside our current conceptual framework. And this perspective has at least two major benefits. First, it deepens our understanding of our present situation. For example, my own research on the goals of science was shaped by studying historical sources. Without that background, I could not have articulated my vague sense of a tension between two intrinsic aims of science: explanation and prediction.

Second, the history of philosophy brings back into view questions that still matter to us but that we've (largely) abandoned because answering them would require a shift in our current conceptual scheme. Let me give you an example. As you know, Spinoza's magnum opus is called Ethics. However, ethics for Spinoza (and his contemporaries) was that part of philosophy that was concerned with the good life. By contrast, contemporary ethics tends to focus on moral obligation. Personally, I think this shift has been an unfortunate development in the field. The older conception strikes me as both more interesting and more fruitful. And ethics is just one case. If you look closely, many of the most stimulating discussions of some of the topics traditionally associated with philosophy (art, romantic love, friendship, authenticity) are now being carried out by historians of philosophy and mostly in dialogue with historical figures. Alexander Nehamas's work is a particularly good example of this. So for me (and for many others who work in history of philosophy),

history is not just about tracing the past. It is about reopening questions that are more interesting than the ones contemporary philosophy currently sets for itself.

One of your research focuses is simplicity. Do you see simplicity as a reliable or sufficient criterion of truth for scientific method?

Before answering your question, let me clarify what I mean by simplicity. Take the family of parabolic functions: $y = ax^2 + bx + c$, where a, b, and c are adjustable parameters. Changing the parameters yields different parabolas – for example, (1, 0, 2) yields $y = x^2 + 2$, while (3, 5, 13) gives $y = 3x^2 + 5x + 13$. Scientific theories often suggest a family of hypotheses but leave certain parameters to be fixed by data. We can call the number of adjustable parameters of a model its *degree of complexity*. For instance, if a theory says that x and y are linearly related, without specifying the exact linear function, the model has two adjustable parameters. We have strong statistical results that, other things being equal, the fewer adjustable parameters a model has, the likelier it is to be true and the more accurate its predictions will be. Simplicity understood as paucity of adjustable parameters is linked to truth, predictive accuracy, and (as I argue in my latest work) explanatory power. Now, to your question: simplicity alone is not sufficient. But I believe (though this is controversial) that the combination of simplicity and consistency is sufficient. My view is that all other theoretical virtues can be reduced to these two. So yes, I regard simplicity as a highly reliable indicator of both truth and predictive success. That doesn't mean that truth is more likely to be simple. It means that, other things being equal, the simpler theory is the more promising candidate.

How do you see mathematical elegance regarding this issue? Is it possible that physics theories with mathematical elegance to be superior to ones without?

The case of elegance is a bit tricky. Typically, there is a correlation between mathematical elegance and simplicity understood as paucity of parameters. In that sense, elegance can serve as a proxy indicator of truth: elegant theories tend to be simpler, and simpler theories tend to be more reliable. But I don't think elegance has any *independent* or *sui generis* epistemic value. Its role is derivative: elegance is valuable when, and because, it signals simplicity.

Does the simplicity work the same for different scientific fields? For instance, does the complex nature of biology differ from physics, a more fundamental science? Is it applicable to social sciences?

The statistical results suggesting that simplicity is a reliable indicator of truth and predictive accuracy rely on minimal assumptions that, I suspect, hold across almost all scientific fields. If a field studies more complex causal relations, as the social sciences do, then the competing theories will themselves be more complex. What matters is not simplicity in some absolute sense but relative simplicity. To illustrate: other things being equal, the difference in predictive accuracy between two polynomials of degree 98 and 100 is equal to the difference between polynomials of degree 1 (linear) and degree 3 (cubic). So the fact that a field deals with more intricate phenomena doesn't change the basic epistemic role of simplicity. That said, complexity does make a difference in practice. Human beings have limits: there's only so much complexity we can handle. Often, we simply don't consider highly complex relations at all. So if the truth in a given domain happens to be very complex, we may never arrive at a theory that captures it with the elegance and power of, say, Newtonian mechanics in its proper domain. One of my hopes is that advances in AI will help us overcome this limitation, by vastly expanding the range of candidate theories we can generate and by making it possible to work with complex functions as easily as we now handle simple ones.

Do you belıeve that simplicity-focused research promise a solution to the crisis physics finds itself in? Do you believe that a theory of everything is possible?

I'm afraid my research on simplicity has little to offer here. The present crisis in physics is not philosophical; it is data-driven. The problem is that we currently lack a unified theory that can adequately accommodate the data we have about the physical universe. Only the discovery of such a theory can resolve the crisis. But there is no guarantee that such a theory exists, let alone that we will find it. Nor is there any philosophical guarantee that a "theory of everything" is possible. One of the lessons that the history of science teaches us is that our intuitions are not always reliable guides to truth. As much as we may wish the universe to have a single, elegant story behind it, that hope could turn out to be unfounded. For all we know, we may live in a universe that keeps its deepest secrets from epistemic agents like us indefinitely. Whether that is the case is an empirical question, not one philosophy can

decide in advance. This is not to say we should despair. A few months before Heisenberg published his paper on quantum mechanics, Pauli wrote to a friend that physics seemed so hopeless that he wished he had become a comedian and had "never heard of physics". In less than five months, when Heisenberg's new ideas appeared, Pauli thought it was once again possible to "march forward." Our current moment may be similar. Who knows?

What makes philosophy of science tempting to you?
Are there any more questions you are interested in philosophy of science?

I'm drawn to philosophy of science for two main reasons. First, science itself is endlessly fascinating. The sheer scope of what it allows us to do is astonishing. Although because we live with it every day, it can be hard to fully appreciate just how remarkable a phenomenon it is. Second, I think philosophy of science is the best kind of epistemology. Science is, epistemically speaking, the best humanity has to offer. So if we want to study our relation to truth – whether it is within our reach and how we can approach it – the best way is to study how and why science works.

There are many questions in philosophy of science that interest me, but one I'm especially focused on now is the relation between explanation and simplicity. As I mentioned earlier, I'm not sure whether science will ever produce a theory of everything. But I endorse the ambitious idea that in philosophy of science we might have a theory of everything. I suspect that the key lies in a proper understanding of simplicity and its role in theory choice. At present, explanatory power is the last major theoretic virtue that hasn't been reduced to simplicity and consistency. But I think ideas from information theory point toward a way of doing exactly that.

Another topic, which might seem a bit futuristic, is the possible bifurcation of science into predictive and explanatory branches. With the rise of powerful predictive tools (such as neural networks) that can generate highly accurate predictions without relying on explicit theories, we now face the prospect of a "predictive science" that operates without explanation. If that happens, we might need a distinct form of science devoted to explanation, one concerned with trying to give us an understanding of the world. Whether this bifurcation is realistic, and what such a division of labor would mean for our understanding of science, is a question I've been thinking about recently.

Why did you choose Bilkent as a step on your academic career?
Do you see a future for philosophy of science here?

When I joined Bilkent, its philosophy department struck me as very impressive (and it has only grown stronger since). The fact that Turkey is both geographically and culturally close to my home country, Iran, was also a major factor in my decision. As for philosophy of science, I do think Bilkent has the potential to become a real center of strength in the field, even though we are far from there yet. The key fundamental factor is having students with strong technical or scientific backgrounds who are also drawn to philosophy of science. And Bilkent has quite a few of them. That makes me optimistic about the future here.