

AN INTERVIEW WITH SÉBASTIEN RIVAT :: JÜRGEN LANDES

Abstract

Jürgen Landes interviews Sébastien Rivat.

Keywords

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Sébastien Rivat is an assistant professor at the MCMP in the Stephan Hartmann group. Like many philosophers of science he initially trained as a scientist. He recently won an ERC grant; congratulations! Before leaving you to the interview, I'd like to warmly thank him for his time. Beyond the exciting research he talks about, he also has some sound advice on writing a successful grant proposal. I thus hope that even readers not (yet!) interested in the philosophy of physics have reasons to read the interview.

JL: Tell us about your academic background

SR: I originally come from mathematics and physics. After finishing my undergraduate studies in France, I moved to Cambridge in the UK to complete my master in theoretical physics. I stayed one more year to complete a MPhil in history and philosophy of science, and I was lucky to be accepted right after that into Columbia University's PhD program in philosophy. It was a wonderful op-

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portunity to explore a variety of philosophical topics. I eventually came to develop a thesis project on the philosophy of relativistic quantum field theory, the theoretical framework that unifies special relativity and quantum mechanics. And this project eventually brought me toward the end of my PhD to develop projects on the history of contemporary physics. After my defense, I spent two years at the Max Planck Institute for the History of Science in Berlin to work on these projects, and I joined the Munich Center for Mathematical Philosophy at LMU Munich in 2022 to develop other philosophical projects. Going back and forth between the two has proved to be really productive in my case.

JL: How did you get into the philosophy of physics?

SR: My master in theoretical physics really helped me to get a first footstep into the nitty-gritty details of physics. But I also got increasingly confused about the structure of more advanced theories and the underlying world picture I thought I was supposed to get out of them. I felt that it would make more sense to try to understand what these theories are all about before going any further. At the time, Jeremy Butterfield was giving a seminar on the foundation of quantum theories at the Center for Mathematical Sciences at Cambridge. I think he played a major role in me switching to philosophy of physics. In hindsight, I also think that the switch was a rather natural one. Master programs in theoretical physics usually require some serious engagement with the details of quantum field theory. But despite its remarkable empirical success, this framework still lacks a proper conceptual and mathematical foundation. Establishing the mathematical existence of realistic quantum field theories is even one of the Millennium Prize Problems selected by the Clay Mathematics Institute in 2000. So, upon encountering quantum field theory, I imagine that many students may also have stumbled on the fundamental question ‘how

on earth is it possible for such a mathematically ill-defined and seemingly inconsistent framework to be so empirically successful at the same time?'. I have been particularly interested in trying to understand how the approximation methods involved in this framework work and what the world must be like for them to work so well. But I should mention that many others have been more interested in the mathematical foundations of quantum field theory per se.

JL: What is hotly debated in the philosophy of physics?

SR: There are obviously many hot debates right now, and my selection will be very partial. The longstanding issues of explaining the emergence of the arrow of time (i.e., why macroscopic phenomena don't seem to go backward in time) and accounting for the metaphysics of quantum mechanics (e.g., what "superposed" quantum states correspond to) are still widely debated. Philosophers more interested in space-time theories have been concerned with issues of theoretical equivalence, reduction, and underdetermination in recent years, to paint them in very broad terms. For instance, there has been a bit of back and forth lately about how to best understand the sense in which the curved spacetimes of classical general relativity reduce to the flat spacetimes of special relativity in sufficiently well-behaved local spacetime patches. On the particle physics side, philosophers have been worried about isolating the sorts of entities and structures that underwrite the success of our best current theories. Taking seriously the idea that these theories are merely approximate raises surprisingly difficult interpretative issues (see, e.g., <https://philsci-archive.pitt.edu/18056/>). And there are of course a number of emerging debates that cut across theories of space-time and matter, including the black hole information paradox, the emergence of space-time in quantum gravity, and the

relative epistemic standing of competing models of cosmology.

JL: You recently won an ERC project with title: "The Scaling Revolution in Physics: Historical and Philosophical Perspectives". Could you tell us what this project is about?

SR: The project aims to examine the historical development and the philosophical implications of physicists' new way of studying physical systems "scale by scale" and "across scales". To give a simple picture, physicists have been very much concerned at least since the rise of modern science with developing theories of space, time, matter, and motion that could apply in principle everywhere and under any circumstances. Philosophers have also very much relied on this ideal of formulating a final theory to interpret the content of theories that physicists came to formulate along the way. This is particularly clear in my sense today with classical general relativity and non-relativistic quantum mechanics. But it seems that the situation has considerably changed since the 1950s. Physicists now widely recognize that the best theories we currently have work only at certain scales. To put it more precisely, physicists widely recognize that these theories are more reliably formulated as effective theories (on pain of being inconsistent and making unwarranted assumptions about unexplored domains). Physicists also now spend most of their time studying systems scale by scale and analyzing how physical effects depend on each other across scales, even when they work on speculative models of quantum gravity. This more cautious attitude toward the frontiers of physics and this general methodological shift raises two fundamental questions in my sense: (i) How should we adjust our interpretative practice in light of this historical transformation? (ii) What kind of world picture should we extract from this new way of doing physics scale by scale? The project will address these questions by taking two of the most sig-

nificant scale-based theoretical tools developed by physicists from the 1950s onward: namely, effective field theories and renormalization group methods. And we will integrate both historical and philosophical analyses to try to get as clear and precise as we can about the impact of this historical transformation on fundamental issues pertaining to scientific representation, reduction, scientific realism, and the metaphysics of science.

JL: What is it that people should be doing in the philosophy of physics but are not doing?

SR: Let me say at the outset that philosophy of physics is living a very healthy life at the moment. There is a large variety of different approaches on a wide range of topics, from groundbreaking conceptual analyses published in mathematical physics journals to naturalized metaphysical studies published in top-tier generalist journals. But if I had to highlight one important blind spot, it would be the lack of engagement with the fact that all the empirically successful theories that we have so far are “merely” approximative. Concretely, this means that philosophers of physics would benefit from engaging more thoroughly with the variety of approximation methods developed by physicists and thinking further about how to reconcile our favorite all-or-nothing philosophical categories with the approximate and partial character of the systems we wish to study. Kerry McKenzie has recently done some very important work in that respect (see, e.g., <https://philsci-archive.pitt.edu/23033/>, for her latest paper on the topic).

JL: Are you interested in other areas you want to tell us about?

SR: My work in the philosophy of physics has indeed led me to

think about a number of more general issues. For instance, I have been thinking lately about the epistemological status of discretization methods in science (e.g., what are the epistemic benefits gained by using discrete mathematics?). I started to think about this question from the perspective of lattice theories in physics, where the continuous background of standard theories is replaced by a discrete grid made of sites and links. And I quickly realized that there are actually serious theoretical obstacles if we want to put our best current theories on a lattice. The physicist David Tong actually gives a very accessible talk on this topic, suggesting that these obstacles put some pressure on the idea that the actual world could be simulated by a computer. Whether the argument is decisive or not, I highly recommend watching the recording of his talk online (<https://www.youtube.com/watch?v=QPMn7SuiHP8>).

JL: I recently conducted in an interview with editors on writing good papers. As a successful grant writer I'm now turning to you for some tips/tricks/advice for our readers on how to write a good grant.

SR: My experience is very limited. But from what I have seen so far, a good grant proposal starts with a simple and exciting idea that one will eventually be able to summarize in three minutes. This is actually the time given to present one's project in many panels of the ERC grant program. Typically, this simple idea takes the following form in the case of a philosophical project: (i) most philosophers in the field F have assumed (or done) X; (ii) but Y suggests that X is wrong (or wrongheaded); (iii) the project will show how taking Y seriously has a fundamental impact on the field F. Then, to write a good grant proposal, there are three challenges to overcome: (i) finding what is missing in the field while remaining charitable and exhaustive; (ii) finding Y (inter-disciplinarity helps but usually raises additional methodological

issues); (iii) having a detailed and concrete plan for the implementation of the project (which usually requires having already done some work on the impact of Y).

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