

Of Realist Turns

A conversation with Stathis Psillos

Fabio Gironi

STATHIS PSILLOS IS PROFESSOR OF Philosophy of Science and Metaphysics in the Department of Philosophy and History of Science in the University of Athens, former president of the European Philosophy of Science Association and editor of the review journal *Metascience*. Psillos is one of the most prominent defenders of scientific realism in contemporary philosophy of science, and he formulated his arguments in defense of realism in two important monographs: *Scientific Realism: How Science Tracks Truth* (1999) and *Knowing the Structure of Nature: Essays on Realism and Explanation* (2009). Psillos' investigation begins with the identification of three core theses of scientific realism:¹

- *The Metaphysical Thesis*: the world has a definite and mind-independent natural-kind structure;

¹ See Stathis Psillos, *Scientific Realism: How Science Tracks Truth* (London and New York: Routledge, 1999), xvii and Stathis Psillos, *Knowing the Structure of Nature: Essays on Realism and Explanation* (Basingstoke: Palgrave Macmillan, 2009), 4.

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- *The Semantic Thesis*: scientific theories should be taken at face-value, being truth-conditioned descriptions of their intended domain, both observable and unobservable;
- *The Epistemic Thesis*: mature and predictively successful scientific theories are to be considered well-confirmed and approximately true descriptions of the world.

He proceeds by offering an articulation of the so called “no-miracles argument” for scientific realism as the crucial argument supporting this realist worldview, taking it as an instance of inference to the best explanation and defusing the attacks of vicious circularity moved against it. He has also defended scientific realism from a range of other anti-realist arguments, including Larry Laudan’s pessimistic meta-induction, the argument from underdetermination of theory by evidence (the so-called Quine-Duhem thesis), and the constructive empiricism of Bas Van Fraassen. Psillos’s scientific realism conjoins a positive epistemic attitude towards a fully knowable natural-kind structure of the universe with a robust, non-epistemic conception of truth, constructing a realist stance which is (as famously phrased by Crispin Wright)² both metaphysically modest (there is an external world which is in every way independent from us) and epistemically presumptuous (this world can be known, to a good approximation of truth-likeness by our best epistemic practice, i.e., science).

There is thus no better dialogue partner than Prof. Psillos to discuss realism, especially for those interested in “bridging the gap” between the continental and the analytic philosophical traditions. I take it to be an integral part of the mandate of *Speculations* to promote this cross-contamination: those interested in the resurgence of realist concerns from within the continental tradition ignore the vast analytic philo-

² See Crispin Wright, *Truth and Objectivity* (Cambridge, MA: Harvard University Press, 1992), 1-2.

sophical production on this topic at their peril. An informed understanding of how the realist stance has evolved in the last few decades of philosophical research in philosophy of science in dialectical engagement with a variety of anti-realist positions and how it has worked towards the clarification of concepts like causation, explanation, truth, and reference to unobservable entities, will offer precious conceptual resources for realists of all stripes and backgrounds.³

As readers of *Speculations* will know, in the last few years we have witnessed a return of realist concerns within the continental tradition: this has taken shape in both readings of figures from the history of continental philosophy on the background to the problem of realism and antirealism (often in relationship with their understanding of the natural sciences)⁴ and of formulations of new, original realist positions. Many of these new theoretical orientations have been grouped under the term “speculative realism,” a rather loose category which can be characterized, for brevity’s sake (and indeed in the attempt to find a minimum common de-

³ Some thinkers within the continental realist movement are aware of this necessity, and it is not uncommon, in their work, to find reference to a range of analytic figures including Wilfrid Sellars, John McDowell, Robert Brandom, Nancy Cartwright and Paul and Patricia Churchland.

⁴ See, for example, Lee Braver’s historical narration of antirealism in continental philosophy (Lee Braver, *A Thing of This World: A History of Continental Anti-Realism* [Evanston, IL: Northwestern University Press, 2007]), Iain Hamilton Grant’s defense of a materialist/vitalist Schelling (Iain Hamilton Grant, *Philosophies of Nature After Schelling* [London and New York: Continuum, 2006]), Trish Glazebrook’s account of the evolution of Heidegger’s opinions about science throughout his philosophical career (Trish Glazebrook, *Heidegger’s Philosophy of Science*, [New York: Fordham University Press, 2000]), Manuel DeLanda reconstruction of Gilles Deleuze’s realism through his engagement with complexity theory and differential geometry (Manuel DeLanda, *Intensive Science and Virtual Philosophy* [London and New York: Continuum, 2002]), Martin Hägglund’s and Michael Marder’s materialist/realist reading of Jacques Derrida’s philosophy (Martin Hägglund, *Radical Atheism: Derrida and the Time of Life* [Stanford, CA: Stanford University Press, 2008] and Michael Marder, *The Event of the Thing: Derrida’s Post-Deconstructive Realism* [Toronto, Buffalo, London: University of Toronto Press, 2009]). It is worth noting that many of these texts have the programmatic intent of presenting the work of continental figures in terms appreciable by an analytic audience.

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nominator in a range of often widely different approaches), by a reaction against and rejection of all those boundaries (Kantian-transcendental, phenomenological, cultural relativist, postmodern) posed between the human subject and “things-in-themselves” independent from human epistemic access. The criticized stance can be reduced to what Quentin Meillassoux has christened “correlationism,” the thesis defending the viciously circular impossibility of thinking an entity *x* as independent of thought, a stance which always reinscribes (correlates) the independent dimension of an entity within the limited horizon of a language, of consciousness, or of any other transcendental condition. These new forms of realism share the belief in the possibility of constructing a philosophy which can reclaim the right to deal with things in themselves, but to do so in a “speculative” manner. One should be cautious in defining what “speculative” means here. Roughly, new continental realisms are “speculative” insofar as they either 1) reject the mandatory grounding of a realist metaphysics on purely empirical foundations and thus promote the reactivation of the possibility of a rationalism of a pre-Kantian kind (reclaiming the possibility of “first philosophy” and, to a certain extent, carrying forward the continental ambition of doing *fundamental ontology*) or 2) even when embracing the natural sciences’ results as a starting point (without caricaturizing or simplifying them), intervene precisely where the sciences themselves are unable to find an internal explanation of their results by revising their metaphysical conceptual apparatus. As a general point one can say that it is precisely the negotiation of a new relationship between (continental) philosophy and science which is at stake in speculative realism, and thus that different orientations along this realist “spectrum” are to be distinguished on the basis of their degree of allegiance to the natural sciences or—if we consider the comparative dimension that we will pursue in this interview—the degree to which they reject the strict naturalism which dominates the analytic field.



Stathis Psillos – *Of Realist Turns*

Fabio Gironi: *I would like to begin by asking you how you developed your philosophical interests. You started your academic education in Greece with a degree in Physics. What pushed you to philosophy and specifically to the philosophy of science?*

Stathis Psillos: Part of the reason why I was drawn to the study of the natural sciences and of physics in particular was disillusionment with the way philosophy was conceived of, and practiced, in Greece back in the 1980s (and until not too long ago—perhaps even today in certain traditional circles). Philosophy was taken to be an essentially philological discipline constitutively engaged with the interpretation of the texts of the great dead philosophers (especially the ancient Greeks) and with an attempt at a grand historical narrative of philosophical ideas; as if philosophical ideas were developed in an epistemic vacuum independently of what was going on in science and in general culture. Actually, philosophy was taken to be a discipline which has evolved in opposition to science. Studying philosophy *this way* was extremely unattractive to me (even though, unbeknownst to me back then, there were pockets in a couple of philosophy departments in Greek universities that resisted this conception of philosophy). I was therefore led to physics, but it was quite clear to me from quite early in my studies that I was looking for a window of opportunity to engage with philosophy in a systematic manner. My turn to philosophy of science was a natural outcome of my engagement with physics and my tendency to look for philosophical problems that arise within physics as well as from what physics tells us about the world. I wrote my first degree dissertation on issues in the philosophy of quantum mechanics (trying—in vain, I am afraid—to understand the rich Aristotelian notion of potentiality and its possible relevance to the stochastic conception of the world, as this is depicted in the standard interpretation of quantum mechanics). Back then (in the late 1980s) it was quite hard to find any serious literature in Greece and I was lucky to be given by a teacher of mine the typescript of the

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yet unpublished book of Michael Redhead's *Incompleteness, Non-Locality and Realism*, which excited my interest in realism.

My commitment to realism (admittedly in a naïve and perhaps vague way, and mainly conceived of as materialism) was already there because of my theoretical engagement with Marx. In fact, this engagement kept my philosophical awareness alive throughout my University studies and led me to try to understand both the idealist and the empiricist opposition to realism (perhaps, unwittingly, conflating them back then). Reflection on Marx's second thesis on Feuerbach ("The question whether objective truth can be attributed to human thinking is not a question of theory but is a practical question. Man must prove the truth—i.e. the reality and power, the this-sidedness of his thinking in practice. The dispute over the reality or non-reality of thinking that is isolated from practice is a purely *scholastic* question.") was leading me towards a conception of realism that was meant to enable the task of transforming the world. I was feeling quite satisfied by the fact that this task was meant to be the *proper* mission of philosophers, as Marx, I thought, was claiming in the famous eleventh thesis on Feuerbach: "The philosophers have only interpreted the world, in various ways; the point is to change it." But I soon realised that I was fooling myself. Contrary to Marx's eleventh thesis, the point was *still* to interpret the world—if we are to know what we are doing when we try to change it. In this endeavour to interpret the world, science, I thought, was the bastion of rationality and progress; the *terra firma* upon which one could base all hopes for a better world. I believed back then—and still believe now—that science is the best way we humans have invented to push back the frontiers of ignorance and error, to achieve a deep understanding of the world and of our place in it, and to make the world a better place to live. What I now add is that science is *not* a faultless, value-neutral and interest-free way to understand and change the world.

But science and its claim to truth and knowledge are not immune to criticism; hence, they need justification and de-

fence. To me, looking into the scientific realism debate was no longer optional. It amounted to taking a *standpoint*: the scientific realist standpoint. When I went to King's College London for graduate studies in philosophy of science (having gratefully received a state's scholarship, without which I would have been unable to pursue my philosophical studies in the UK), I came into the scientific realism debate with no neutrality. I wanted to defend scientific realism, along with the objectivity and rationality of science and its method. This was both an intellectual and, I thought, a *political* goal. Back in the 1990s, there was a pervasive thought, especially among left-wing American and continental European intellectuals, that undermining the alleged epistemic authority of science, challenging its claims to objectivity and knowledge, was an act of emancipation from the strangling authority of Reason. I was never persuaded by this rhetoric. It conflated intellectual authority with authoritarianism and, at least to all of us who learned our basic philosophy and politics in the European south, intellectual authority (and objectivity and criticism and the search for truth) were the arch enemies of any kind of authoritarianism.

FG: *Indeed. Considering the paradigm of “charismatic” populist authoritarianism that has been steering politics in my own country in the last decades, I couldn't agree more. I'd like now to introduce readers not acquainted with them to discussions taking place in the analytic philosophy of science (since enthusiasts of the continental “realist turn” often tend to overlook the fact that a similar turn has occurred in the analytic tradition roughly between the 1960s and the 1970s, and has developed vigorously ever since) and, second, to expose you to some recent realist developments in continental philosophy. As for the first point, can you clarify how the “scientific” qualifier differentiates “scientific realism” from the more general “realism” part of philosophical vocabulary since medieval scholasticism? And can you offer a brief historical narrative guiding us from the realist turn which lifted the embargo on the reference to unobservable, theoretical entities—originating in the work of*

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*philosophers such as Wilfrid Sellars, J.C.C. Smart and Richard Boyd—to the present state of the scientific realism debate?*⁵

SP: Historically, realism has been taken to be a doctrine about the independent and distinct reality of universals (*qua* attributes or species). It was opposed to nominalism, viz., the view that only particulars exist. Nominalists argued that general terms and predicates are merely names for classifying particulars in terms of their similarities and differences. Realists—who, historically, came first—claimed that universals are real entities referred to by abstract terms, general names and predicates, and argued that they are necessary for knowledge and for grounding the similarities and differences among particulars. There have been transcendent realists (those who think that universals—*qua* Platonic forms—are apart from, and prior to, the particulars) and immanent realists (those, like Aristotle, who think that though a universal is the one over the many and “imperishable,” it is not apart from the many).

It’s an interesting question when and under what circumstances the term “realism” started to acquire philosophical currency. I have not looked into the matter with any seriousness. The term appears in Kant’s first critique (quite late in the text) joined with the qualifiers “transcendental” and “empirical.” Kant contrasts realism to idealism; in particular to his own transcendental idealism. Kant claims that transcendental realism takes the phenomena (outer appearances/objects of the senses) as real and as existing independently of us and our sensibility, thereby taking them as things-in-themselves. It is transcendental realism that he famously denies and to which he opposes his transcendental idealism, viz., the view

⁵ “The Present State of the Scientific Realism Debate” is the title of the first chapter in Psillos’ *Knowing the Structure of Nature*. Readers seeking a fully detailed account of this debate should turn to it or, for an even broader perspective, to Psillos’ exhaustive historical survey of the entirety of twentieth-century philosophy of science in chapter fourteen of Dermot Moran, *The Routledge Companion to Twentieth Century Philosophy* (London and New York: Routledge, 2008).

that the objects of knowledge are not the things-in-themselves, but the phenomena as they are constituted by their epistemic conditions for their knowledge (the categories and the forms of pure intuition). But transcendental idealism, he insists, makes room for *empirical* realism, meaning that the objects of the senses are material things that are to be found in space, even though space (and time) are *a priori* forms of sensible intuition. The fact is that Kant's way to cure the "scandal of philosophy" (recall: "it must still remain a scandal to philosophy and to the general human reason to be obliged to assume, as an article of mere belief, the existence of things external to ourselves...and not to be able to oppose a satisfactory proof to anyone who may call it in question"⁶), created another scandal: the inherent unknowability of things as they are in themselves (by beings like us anyway, who are bounded by sensible intuition).

The Kantian dichotomy between the noumena and the phenomena (an epistemic dichotomy, to be sure) made any robust realist position having to face an uphill struggle: to save the independence of the world from the human mind while avoiding scepticism or agnosticism. Denying the very distinction between the noumena and the phenomena, the Hegelian idealist tradition compromised the independence of reality from thought, thereby securing its knowability. It's not clear to me there were any strong realist voices in the nineteenth century. Perhaps the strongest was Gottlob Frege's who took it that the truths of arithmetic are fully objective, mind-independent and about numbers *qua* abstract objects. Bertrand Russell, in the early twentieth century, developed what came to be known as (a version of) structural realism in an attempt to argue that, given various quite plausible causal assumptions, the structure of the things-in-themselves (that is of the world as-it-is-in-itself) is inferable from, and hence knowable on the basis of, the structure of the phenomena. Rudolf Carnap famously argued that the issue of the reality (and mind-independence) of the world is a pseudo problem,

⁶ Kant, *Critique of Pure Reason*, B xxxix.

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but following Moritz Schlick he made room for empirical (as opposed to metaphysical) realism. The spectre of metaphysical realism, as Schlick put it, was the phantom of a world “somehow standing behind the empirical world, where the word ‘behind’ indicates that it cannot be known in the same sense as the empirical world, that it lies behind a boundary which separates the accessible from the inaccessible.”⁷ It was the specter of the Kantian noumena, perhaps under the illusion that there is a special non-empirical method of knowing them. Rejecting metaphysical realism, Schlick and co. were striving for a position which would leave metaphysics behind, without however abandoning the rich conception of the world, as this is described by the sciences—a world populated by atoms and fields and whatever else our best science tells there is. Science advances by revealing the constituents of things that we encounter in perception and the fact that these are (typically) invisible is no reason to suppose they are not real. Hence, Schlick and co. were aiming to articulate an empiricism-friendly philosophical stance towards science which is distinct from instrumentalism but not committed to a metaphysically-loaded sense of reality.

By the 1920s, the classical Newtonian conception of the world was giving way to a new theoretical framework dominated by Einstein’s theories of Relativity and Quantum Mechanics. What is more, the atomic conception of matter was gaining wide acceptance—it had become the new paradigm. With it, this conception brought the issue of the ontic status of the various invisible entities posited by theories to explain the various observable phenomena. By the turn of the twentieth century, there was a rather heated debate concerning the status of explanatory hypotheses in science—those that posited the existence of unobservable entities. The resistance to explanation-by-postulation was motivated by philosophical arguments, mostly driven by what was taken to be commitment to empiricist theses. One line of resistance had to do

⁷ Moritz Schlick, “Positivismus und Realismus,” *Erkenntnis*, 3, 1-31, 1932. Translated as “Positivism and Realism” in *Logical Positivism*, Alfred J. Ayer, ed. (Glencoe, NY: Free Press, 1960).

with semantics: how can we render language to refer successfully to things that are not given in experience? Another line of resistance had to do with epistemology: how can we possibly come to know anything about the unobservable, if the basis of this knowledge is not rooted in experience? A third line had to do with metaphysics: what exactly is it to be committed to the reality of unobservable entities? Perhaps, a final line was methodological: in trying to understand science as a practice that involves theory and observation, do we need (and do we have) to read theories as if they aim to tell a true story about the unobservable world behind or beyond the phenomena? In practice, these four lines of resistance were mixed and conflated. But the fact is that very many eminent scientists who had philosophical motivation and acumen (from Ernst Mach, to Pierre Duhem, to Henri Poincaré, to Wilhelm Ostwald) took it that there is something deeply problematic with explanation-by-postulation and its promise to take our epistemic grasp beyond the limits of (immediate/sensory) experience. It turns out that the key to shifting scientific opinion in favour of the reality of atoms was Jean Perrin's theoretical and experimental work (roughly around 1910) on the causes of the Brownian motion, which drove home the message that explanatory hypotheses can be highly confirmed by empirical evidence (provided they acquire characteristics that make them definite and testable). It was in this period that the first versions of a major argument for scientific realism were drafted, by the likes of Poincaré, Duhem and Ludwig Boltzmann—viz., that the predictive success of scientific theories cannot be a feat of chance, but that it is best explained by (and hence gives us reason to accept) facts involving unobservable entities which, according to the theories, are causally responsible for the generation of the relevant empirical phenomena. It was also in this period, however, that an important argument against scientific realism started to take shape: the argument from theory-change in science. This is based on the historical fact that there are radical revisions in the scientific image of the world; that past theories were abandoned and replaced by

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substantially different ones. This fact caught the public eye in France, in the beginning of the twentieth century, under the rubric “the bankruptcy of science.” If current theories will have the fate of the past ones (if they too become part of the future history of science books), what is the reason to take them seriously as revealing to us the way the world is? Faced with the problem of radical discontinuity in theory-change, Poincaré and Duhem argued that there is, nonetheless, some substantial continuity at the level of the mathematical equations that represent empirical as well as theoretical relations. From this, they concluded that these retained mathematical equations—together with the retained empirical content—fully capture the objective content of scientific theories. By and large, they thought, the theoretical content of scientific theories is structural: if successful, a theory represents correctly the structure of the world. It is noteworthy that at least in Poincaré’s case, his structuralism had a Kantian origin. He took it that science could never offer knowledge of things as they were in themselves. But he did add to this that their relations could nonetheless be revealed by structurally-convergent scientific theories.

These two major arguments (one from the success of scientific theories and the other from the existence of revolutions in science) were destined to define most of the logical space within which the scientific realism debate would take place later on in the century. Neither of these arguments were at the forefront during the heyday of logical positivism. It was Herbert Feigl’s liberating critique of the main tenets of logical positivism that set the agenda for the realist turn of the 1950s. He argued that the empiricist programme had been a hostage to verificationism for too long. Verificationism runs together two separate issues: the evidential basis for the truth of the assertion and the semantic relation of designation (i.e., reference). It thereby conflates the issue of what constitutes evidence for the truth of an assertion with the issue of what make this assertion true. If theoretical statements cannot be given truth-conditions in an ontology that dispenses with theoretical entities, then a full and just explication of scien-

tific theories simply requires commitment to the irreducible reality of unobservable entities, no less than it requires commitment to observable entities.

Perhaps the first full-blown defence of scientific realism was Jack Smart's *Philosophy and Scientific Realism*—published in 1963, though his key papers on the reality of theoretical entities were published in the middle of 1950s. Smart rebutted various views that treated theoretical entities as fictions or phenomenal constructs or mere concepts. Smart put the defence of scientific realism in proper perspective by arguing that it rests on an abductive argument, *aka* inference to the best explanation. Smart argued against instrumentalists that they must believe in cosmic coincidence. Scientific realism, on the other hand, leaves no space for a cosmic-scale coincidence: it is because theories are true and because the unobservable entities they posit exist that the phenomena are, and are related to one another, the way they are. It is fair to say that the realist turn in the philosophy of science was greatly facilitated by Wilfrid Sellars's attack on the myth of the levels. This myth rested on the following image. There is the bottom level of observable entities. Then, there is the intermediate level of the observational framework, which consists of empirical generalisations about observable entities. And finally, there is yet another (higher) level: the theoretical framework of scientific theories, which posits unobservable entities and laws about them. It is part of this image that while the observational framework is explanatory of observable entities, the theoretical framework explains the inductively established generalisations of the observational framework. But then, Sellars says, the empiricist will rightly protest that the higher level is dispensable. For all the explanatory work vis-à-vis the bottom level is done by the observational framework and its inductive generalisations. Why then posit a higher level in the first place? Sellars's reply was that the unobservables posited by a theory explain directly why (the individual) observable entities behave the way they do and obey the empirical laws they do (to the extent that they do obey such laws). He, therefore, offered an indispensability

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argument for the existence of unobservable entities: they are indispensable elements of scientific explanation of singular observable phenomena.

In his brief review of Smart's book in 1964, Quine exclaimed: "With science dominating our lives and progressing ever faster on even more frontiers, it is strange that such a view [the realistic view of fundamental particles of physics] needs urging. Strange but true." But by then, the tide had started to move the scientific realists' way. Putnam expressed this by his famous slogan, which has become known as the "no miracles argument": "The positive argument for realism is that it is the only philosophy that does not make the success of science a miracle." In his widely circulated and discussed, but still unpublished, manuscript *Realism and Scientific Epistemology*, Richard Boyd tied the defence of scientific realism with the best explanation of the fact that scientific methodology has succeeded in producing predictively reliable theories. Boyd viewed scientific realism as an historical thesis about the "operation of scientific methodology and the relation between scientific theories and the world." As such, realism is not a thesis only about current science; it is also a thesis about the historical record of science: it claims that there has been convergence to a truer image of the world, even though past theories have been known to have been mistaken in some respects. This historical dimension is necessary if the truth (or partial truth, or significant truth) of scientific theories is to be admitted as the best explanation of the predictive reliability of methodology. For, as noted already, unless continuity-in-theory-change and convergence are established, past failures of scientific theories will act as defeaters of the view that current science is currently on the right track. If, however, realism aims to explain an historical truth—viz., that scientific theories have been remarkably successful in the prediction and control of natural phenomena—the defence of scientific realism can only be *a posteriori* and broadly empirical.

Couldn't scientific realism be lightweight? Would it not be enough for someone to accept the reality of unobservable entities without also rendering them mind-independent?

And wouldn't this move bring scientific realism in contact with empirical realism and in freedom from metaphysical realism and/or transcendental realism? Well, a lot depends on how exactly the claim of mind-independence should best be understood. I take it that the sense in which realists claim that the world is independent of theories, beliefs, warrants, epistemic practices, etc. is best captured by admitting the possibility of divergence between what there is in the world and what is issued as existing by an epistemically right theory, which is licensed by the (best or even ideal) evidence or other epistemic criteria. It is precisely for this reason that realists need to rely on a non-epistemic conception of truth (the most popular, and controversial, of which is that truth is correspondence with the facts), which does allow for the foregoing possibility. When truth is attributed to the theory, this is a substantive attribution which is meant to imply that the theory is made true by the world, which, in its turn, is taken to imply that it is logically possible that an accepted and well-confirmed theory might be false simply because the world might not conform to it. A realist non-epistemic conception of truth, and in particular the possibility of divergence, does justice to the hard-won fact of empirical success and convergence of scientific theories. Given that there is no guarantee that science converges to the truth, or that whatever scientists come to accept in the ideal limit of inquiry or under suitably ideal epistemic conditions will (have to) be true, the claim that science does get to the truth (based mostly on explanatory considerations of the sort we have already seen) is quite substantive and highly non-trivial. If, on the other hand, the possibility of divergence is denied, the explanation of the success of science becomes almost trivial: success is guaranteed by a suitably chosen epistemic notion of truth, since—ultimately—science will reach a point in which it will make no sense to even raise the question of whether there is possible gap between the way the world is described by scientific theories and the way the world is.

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FG: Thanks, that was an excellent survey indeed! Now, for the second point. To start with, as a philosopher of science with an analytic background what is your relationship, if any, to the continental tradition? You authored a *Philosophy of Science A-Z* text,⁸ which includes entries on notable philosophers of science: the closest one of these gets to being considered “continental” is perhaps Pierre Duhem, hardly a central figure in the continental canon. I take your choices not as prejudiced or idiosyncratic, but dictated by the necessity of faithfully representing the discipline as it is practiced, with its themes and central figures. Are students trained in the analytic tradition of philosophy of science exposed to any non-analytic material?

SP: A lot depends on how we should understand the so-called *continental* tradition. As you have seen from my previous answer, I have been influenced by many continental thinkers, though they are not in the canon of what is called *continental philosophy*. But what exactly is continental philosophy? Are we thinking in terms of the Franco-German tradition in contradistinction to the Anglo-American one? But let us not forget that analytic philosophy, let alone analytic philosophy of science, would be nowhere if it were not for certain strands within the Franco-German tradition: from Frege, to the neo-Kantians, to Wittgenstein, to the French conventionalists, to the Logical Positivists. When I try to picture the so-called continental tradition, I see some schools of philosophy, like phenomenology, existentialism, structuralism, hermeneutics and post-modernism. Is there anything that unifies them into a single tradition? Perhaps it is that they are subject-centered; perhaps it is that they are based on narratives rather than rigorous arguments and conceptual analysis; perhaps it is that they are anti-science (in the sense that they bracket—to say the least—the scientific image of the world and are indifferent to the possible relevance of scientific findings to

⁸ Stathis Psillos, *Philosophy of Science A-Z* (Edinburgh: Edinburgh University Press, 2007).

philosophy and its methods); perhaps it is they take the key task of philosophy to be to unravel how the subject is related to the world of experience and what categories constitute this relation; perhaps it is the thought that there are no external (non-subjective, non-textual, non-what-have-you) standards of correctness of philosophical theory; perhaps it is all (or some) of the above in various blends. I do not think this kind of search (for the blueprint of *continental* philosophy) is either profitable or interesting. I prefer to look into individual thinkers and schools (with some order of preference—I would never bother much with Heidegger!), and to try to find out whether what they say, or argue for, can help us better to understand some philosophical problem. I am deeply impressed, for instance, by Hegel's critique of mechanism and I have argued that the key problem he raised, viz., that mechanisms are individuated functionally and hence that their boundaries and composition are relative to the function they are taken to perform, is significant for the current debate about mechanisms in the philosophy of science. Or take Husserl's *The Crisis of the European Sciences and Transcendental Phenomenology*. This is a really significant piece. Husserl was very critical of the "bottomless theorising" that characterised the exact sciences. His criticism of the modern (post-Galilean) science and of the mathematisation of nature on which it was basing its search for objectivity is that in this process, science lost contact with the world of subjective experience. He took as the task of his own philosophy to rehabilitate subjectivity. He then urged that scientific objectivism be bracketed and that philosophy (that is, his own phenomenology) focuses on the life-world; the "actually intuited, actually experienced and experienceable world."⁹ I happen to disagree with the way Husserl prioritises the life-world. But the problem he raises—the relation between the world as it is described by science and the world as we experience it—is profound and you can find variations of it both Carnap's *The Logical Structure*

⁹ Edmund Husserl, *The Crisis of European Sciences and Transcendental Phenomenology* (Evanston, IL: Northwestern University Press, 1970), 50.

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of the World and in Sellars's famous discussion of the relation between the scientific image and the manifest image—where the category of “person” is ineliminable. Here we are talking about three different perspectives on the same philosophical issue and the classification of these perspectives in the categories “continental” and “analytic” would simply distort their significance. Or take Althusserian Marxism and its insistence on the structure over the subject as well as the need for science to break free from ideology (though, as Althusser himself admitted, his early distinction was too theoretical). This is not the place to go into details, but my view is that modern structuralist tendencies in the philosophy of science have a lot to learn from the French structuralist tradition (especially when it comes to the social world and the social sciences).

It is true, however, that there is little communication between analytic philosophers and continental philosophers and that this is partly due to the fact that philosophical training has been identified with the immersion within a tradition and its own ways to raise and to articulate philosophical problems and to determine what counts as the right approach or answer to them. I would not surprise anyone if I said that I simply cannot get a grip on what some “continental” philosophers say, though I can more easily associate with them when what they argue is translated (perhaps by someone conversant in both traditions) into the language of the philosophical conceptual framework I relate with.

In recent years, there have been systematic attempts by various “analytic” philosophers to immerse themselves into the views of the continental thinkers—and this is quite heartening, if only because, if you think of it, the split between the so-called analytic and the so-called continental philosophy is a historical event that took place *within* a single philosophical framework. It is related (to some extent at least) to the split of Kantianism into two neo-Kantian schools who disagreed as to how best they were supposed to develop the key Kantian points after the collapse of the neat way in which Kant's described how knowledge is possible. Those in Marburg

took mathematics and the natural sciences as the models of objectivity and knowledge and aimed to remove all intuition from knowledge, while those in Baden focused on values and their role in knowledge, turning their attention to history and the human sciences and aiming to unveil their peculiarities vis-à-vis the natural sciences. Whichever way to look at it, both the analytic and the continental traditions are heirs to the network of problems, concepts, methods and theories that constitute the lore of philosophy from Plato to old Kant.

When it comes to philosophy of science in particular, it is significant that analytic philosophers of science have started to take notice of the tradition of historical epistemology—what is simply called “epistemology” in many continental countries—which is a genuinely historical and contextual approach to conceptual and philosophical problems in the sciences. This encounter should ideally lead to a new synthesis between historical approaches to science and philosophy of science.

FG: Right, let's pursue this further. I feel that it's still reasonable to say that this “neat” disciplinary division in the philosophy of science can perhaps be traced along two lines. First, as you just mentioned, the importance (or lack thereof) attributed to historical concerns. This might be a sweeping statement if we consider the analytic tradition as a whole, but it seems to be fair if we consider the philosophy of science (moreover, I think that there is some truth in the claim that historical interests in analytic philosophy, while not absent, tend to be located on the meta-philosophical level rather than organic parts of the construction of an argument). This is arguably a consequence of the logical empiricist collapsing of the traditional disciplinary distinction between Naturwissenschaften and Geisteswissenschaften (essentially in favour of the former), one which took shape in the Carnapian “unity of science” program and which strictly confined “cultural objects” outside of the mandate of science. Little more than a decade later Edmund Husserl laments precisely this positivistic reduction of philosophy (and science itself, both somewhat subsumed in the German term Wissenschaft) to a narrow concern with a factual objectivity ex-

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punged of the concern for “human questions” and, in a memorable line, claims that “Positivism, in a manner of speaking, decapitates philosophy.”¹⁰ He goes on to denounce the “naïvete through which objectivist science takes what it calls the objective world for the universe of all that is, without noticing that no objective science can do justice to the [very] subjectivity which accomplishes science.”¹¹ Scientific objectivity, an ethical imperative to be reached for the Husserl of the *Crisis*, is ultimately grounded in a lifeworld (*Lebenswelt*) of intersubjectively, historically constituted cultural formations. Even outside the Husserlian phenomenological legacy, continental philosophy of science, in particular the French epistemological tradition running (roughly) from Emile Meyerson to Michel Foucault through Leon Brunschvicg, Gaston Bachelard and Georges Canguilhem, was composed by thinkers with a scientific background who put a premium on a philosophico-historical analysis that would emphasize the discontinuities of science. These would be often caused by those psychological, (inter-)subjective preconceptions (“epistemological obstacles” as Bachelard named them) which are to be accounted for if we are to offer an account of science as actually practiced by human subjects. A far cry from Carnap’s antipsychologism guiding, in the *Aufbau*, his “rational reconstruction [rationale Nachkonstruktion] of the concepts of all fields of knowledge on the basis of concepts that refer to the immediately given.”¹² Canguilhem well synthesizes the spirit of French *épistémologie* in one paragraph:

The history of sciences is not the progress of sciences in reverse, i.e. the putting into perspective of outmoded stages whose truth is today on the point of disappearing. It is an effort to enquire into and give an understanding of the extent to which outmoded notions or attitudes or methods were, in their time, successful; and consequently of the respect in which the outmoded past remains the past of an activity for which it is necessary to retain the term

¹⁰ Edmund Husserl, *The Crisis of European Sciences and Transcendental Phenomenology* (Evanston, IL: Northwestern University Press, 1970), 9.

¹¹ *Ibid.*, 294-295.

¹² Rudolf Carnap, *The Logical Structure of the World and Pseudoproblems in Philosophy* (Chicago and La Salle, IL: Open Court, 2003), v.

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“scientific.” To understand what gave instruction in its time is as important as exposing the reasons for its destruction by what followed.¹³

Of course, Thomas Kuhn acknowledged Meyerson among his key influences, but the Kuhn-inspired historical turn seems to have de-legitimized itself (in the eyes of most philosophers of science) with what were perceived as post-Kuhnian relativist excesses (from Paul Feyerabend’s methodological anarchism to David Bloor and Barry Barnes’ “strong programme” in the sociology of scientific knowledge) with the result that today mainstream philosophy of science remains well insulated from those projects of “science studies” that aim at placing science in its historical (but also gendered and social) context. I personally think this is for the worse, and I see much value in the recent, more regulated, return to a merging of history and philosophy of science (HPS) in the so called “Integrated HPS” (or &HPS) projects,¹⁴ (in which I think you are personally involved, being among the organizers of the 4th international Integrated HPS Conference, which was held in Athens last March). HPS can help re-conceptualize episodes and concepts from the history of science from being the province of antiquarian interest to the living field of original philosophical work. As Hasok Chang recently put it “history-writing can be a very effective method of philosophical discovery.”¹⁵ What is your position regarding this split along historicist lines? Does the HPS trend hold the promise to effectively integrate analytic philosophy of science with historical research, and could this be an occasion for rapprochement between the two traditions?

SP: It’s obvious from what I said above that we agree on quite a bit. But I disagree with Husserl’s judgement on Positivism.

¹³ Georges Canguilhem in Gary Gutting, *Continental Philosophy of Science* (Oxford: Blackwell, 2005), 201.

¹⁴ For an overview of this project, see Seymour Mauskopf and Tad Schmaltz, *Integrating History and Philosophy of Science. Problems and Prospects* (Dordrecht, Heidelberg, London, New York: Springer, 2011) and the issue of *Isis* (199:1, 2008) with a focus on “Changing Directions in History and Philosophy of Science.”

¹⁵ Hasok Chang in *Integrating History and Philosophy of Science*, 111.

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Recall that his claim was against positivism as the dominant *ideology* for doing science: science is only concerned with experience and with getting the facts right. I am not sure any serious philosopher (not even Comte himself) held this view. Clearly this was *not* the view of the Logical Positivists and Husserl was aware of this. So if we take the “in a manner of speaking” seriously in his dictum, he might well be making a good point! But he too felt that the Logical Positivists’ approach was a weapon against irrationalism. Their criticism of traditional speculative metaphysics was meant to reshape philosophy in such a way that it is brought (again) in contact with science and rigorous conceptual tools and methods (broadly borrowed from logic and mathematics). So I’d say that positivism, in a manner of speaking, liberated philosophy. It’s true though that the Logical Positivists had had little time for history (though not for subjectivity and its place in the theory of knowledge). This is somewhat ironic since, at least until they were forced, by the rise of the Nazi’s in power, to leave the Continent (Schlick, as is well known, was assassinated in the staircase of the University of Vienna), they were the true heirs of the philosophies of science of Poincaré, Duhem and Mach; philosophies of science which were deeply immersed in history. But the insensitivity to history was, in a sense, necessary for what the logical positivists took as their immediate task, which is this: how to reconcile the emerging new scientific image of the world with the collapse of the Kantian theory of knowledge, without at the same time jettisoning the Kantian idea of the spontaneity of understanding. Fulfilling this task requires an orchestrated philosophical act, one key element of which is clarifying the conceptual foundations of the new scientific theories (so that what they say of the world—their factual content—becomes as clear as possible), the other key element being the need to reformulate and reshape the standard philosophical categories by means of which the analysis and criticism of knowledge is effected. In this process, the very idea of intuition and of synthetic *a priori* knowledge of the world had to go; better: synthetic *a priori* principles were reconceived as analytic and yet revisable

framework-dependent principles. It was in this context that Schlick attacked Husserl's *Wesensschau* (intuition of essences). He thought that empiricism could accommodate subjectivity without having recourse to sense-intuition or to substantive synthetic *a priori* principles. No special intuition of essences was necessary for knowing the structure of experience. The so-called "phenomenological propositions," far from being part of the structure of the life-world, were analytic principles having to do with the structure of language. However, the very idea that the remnant of the Kantian spontaneity of understanding was to be found in framework-dependent and hence revisable general principles had a deep (if implicit) historical motivation, viz. the presence of revolutions in science. The synchronic logical analysis of the language and concepts of science that the positivists pursued was predicated on the thought that the form of the scientific method (*aka* inductive logic) is diachronic (and hence, essentially historically invariant), while its content is historically variable.

Philosophy abhors vacuum, so the historical method that Duhem and Poincaré (as well as Mach) had followed in their philosophies of science was picked up by the French epistemologists of the school of Gaston Bachelard. But I take it that there was a lot of uncertainty as to how exactly history should be an integral part to philosophy of science. Back in 1906, Duhem was quite clear about the importance of the historical method:

The legitimate, sure, and fruitful method of preparing a student to receive a physical hypothesis is the historical method. To retrace the transformations through which the empirical matter accrued while the theoretical form was first sketched; to describe the long collaboration by means of which common sense and deductive logic analysed this matter and modelled that form until one was exactly adapted to the other; that is the best way, surely even the only way, to give to those studying physics a correct and clear view of the very complex and living organization of this science.¹⁶

¹⁶ Duhem, Pierre, *The aim and structure of physical theory*, trans. P. Wiener

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The historical method—the historical investigation of the conceptual processes that led to an adaptation between matter (empirical laws) and form (mathematics)—was taken to be an essential way to do philosophy of science. This is because the historical point-of-view unravels the constitutive interplay between empirical-factual investigations and mathematical-formal frameworks in the development of scientific theories. Admittedly, Duhem tied his historical turn to a certain historiography of science, viz. one that stressed the elements of continuity and rejected the view of theory-change as the way Athena emerging fully armed from Zeus's head. Hence, he was using history as a guide to the *future*: as a way to show how there can be revolutions without incommensurability; how the physics of each epoch “is nourished” by past physics and “is pregnant with the physics of the future.”

The view of the role of history shaped by the French epistemologists seems to me to be far more radical than Duhem's. I think its forebear is Emile Boutroux, who argued for the presence of “genuine irreducible contingency” in the world and took it that according to this doctrine “it is erroneous and chimerical to attempt to reduce history to a simple deduction.” Furthermore, he argued that “it is not...the nature of things that should be the final object of our scientific investigations, it is their history,”¹⁷ which, incidentally, he took it to be the locus of objectivity. The French epistemologists extended these ideas to the very nature of science, arguing that science is essentially historical (no core themes, methods, etc.), the object of science (and concomitantly) the object of philosophy of science being *historically variable*. This way to view science leads to particularism, and particularism (when fully developed) is self-defeating. Unless all these activities that are classified under science have some general and shareable characteristics, it is hard to see what makes them science; what unites them under a common rubric?

(Princeton: Princeton University Press, 1906), 268-269. It is clear from the context that Duhem meant it as a general method for the study of science.

¹⁷ Emile Boutroux, *The Contingency of the Laws of Nature*. (Chicago and London: Open Court, 1920), 166, 167.

When Thomas Kuhn pleaded for “a role of history” in the introductory chapter of *The Structure of Scientific Revolutions*, he was fully aware that history did already have a role—especially among the French *epistemologists*. So, his plea was for a *new* role for history, and in particular one that was based on the rejection of the cumulative-developmental model of science. There is, certainly, a way in which history was assigned a *new* role within general philosophy of science and this was related to the structure and the testing of the macro-models of scientific growth that became popular in the 1960s and 1970s. Models of scientific growth, such as Kuhn’s and Lakatos’s, presented the unit of scientific appraisal (the scientific paradigm, the scientific research programme) as an evolving dynamic structure that follows a rather tight historical pattern. Kuhn emphasised both the element of historical tradition that characterises normal science (seen primarily as a rule-governed—or exemplar governed—activity) as well as the element of change that characterises revolutionary episodes (seen primarily as an abrupt change not-fully-accounted-for in terms of reason and evidence). Lakatos stressed the element of continuity and looked for clear-cut criteria of progressiveness in the transition from one research programme to another, which could underpin a notion of developmental rationality of science. But both took issue with a conception of science in general which had taken it to be subject to rules by means of which theories are appraised (e.g., a formal system of inductive logic and degrees of confirmation). And both took it that their macro-models of science reflected—and hence were licensed by—the actual *historical* development and succession of scientific theories.

The genie of history was out of the bottle but I feel there still a lot of uncertainty—among philosophers of science—as to what wishes to make. If we were to think of the matter a bit abstractly, we could distinguish the following ways in which history of science and philosophy of science can be related. (1) Philosophy of science is an essentially ahistorical discipline dealing with the logical analysis of the structure and concepts of science. If there is any role for history of science,

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it is merely its role as the *past of science*: it is either a narrative as to how concepts evolve or a source of examples. (2) Philosophy of science is the theory of historically individuated macro-models of theory development. History of science is then conceived of as the domain of application (and testing) of these models. (3) Philosophy of science involves a historical dimension in searching (in an *a posteriori* fashion) for the forms and justification of general rules and methods of science—what came to be known as methodological naturalism. (4) Philosophy of science is the rational reconstruction of the history of science and as such it relies on the history of science for warranted descriptions of how past scientists have actually practised science. I am not claiming that this list is exhaustive. Nor it is the case that these four points of view are totally independent from each other (especially the approaches 2 to 4). But what they all have in common is that they promote a kind of philosophy-infested history of science; that is, a reading of the history of science in which that criteria of relevance are fixed by philosophical considerations.

It's time for a renegotiation and re-appraisal of the relations between the history of science and the philosophy of science. It's not the case that there should be just one correct way in which history of science should be related to philosophy of science and a lot of insight will be gained by exploring the various ways in which philosophy of science and history of science could interact. I have tried to clear some of the ground for a renegotiation of the relation between philosophy of science and history of science in a very recent piece of mine called "What is General Philosophy of Science?," which appeared in a special issue of the *Journal for General Philosophy of Science*. I would recommend a New Deal. The model I would promote is based, roughly, on the dipole idealisation/de-idealisation. Much of philosophy of science involves idealisations—what Alexander Koyré aptly called "structural schemata." This is inevitable if a general view about science, its structure, methods and concepts is to be had. It is inevitable if we move beyond particularism and have a view of *science-in-general*. This is the proper subject matter

of philosophy of science. But this drive towards idealization and abstraction, towards an idealized view of science, is essentially incomplete; it leaves out of the picture a lot of the fine structure of science. An important way to reveal this fine structure, I think, is to use history of science as a de-idealiser, thereby getting a more accurate representation of the cluster of activities (and the various determinants) that constitute science. To put it bluntly, idealized (philosophical) models explain but do not represent; while de-idealised (historical) models represent but do not explain. Ideally, we need a new balanced relation. When you do philosophy of science, it is inevitable that the reading of history will be based, ultimately, on philosophical criteria of relevance. But this does not entail that a proper understanding of the history of science—one licensed by historical methods—will leave our philosophical conception of science intact. Integrated HPS is certainly on the right track. I feel, however, that it has not yet managed to mobilise historians of science to the extent that it is necessary for a partnership of equals to get off the ground.

FG: *I guess that from a more properly philosophical standpoint the question is: to what extent, if at all, does historical awareness in philosophy of science undermine our faith in the correctness of our theories, the reliability of our methods or even in our theories' ability to refer to an external, theory-independent world? Does such an historical reconstruction inevitably lead into a Laudan-like pessimistic meta-induction and ultimately to some form of anti-realism?*

SP: This is a good guess! Note, though, that things were not like that in the beginning of the twentieth century, when what should be properly called *historical philosophy of science* was formed. I have spoken already about the “bankruptcy of science” debate and how Poincaré and Duhem were trying to restore some warranted belief in scientific rationality and progress. The point is that the study of the history of science does not necessarily undermine the philosophical view that as science advances there is convergence to a stable network

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of principles and theories about the deep structure of the world; to truer theories, as I would put it. In fact, a proper appreciation of the history of science delivers a mixed message: there is change and continuity; rupture and stability. This is no news, of course. Already in 1900, Boltzmann addressed the “historical principle” employed by the phenomenologists, viz., that hypotheses are essentially insecure because they tend to be abandoned and replaced by other, “totally different” ones. Against this “historical principle,” he argued that despite the presence of “revolutions” in science, there is enough continuity in theory change to warrant the claim that some “achievements may possibly remain the possession of science for all time.”¹⁸ To be sure, we realists need to do a bit more work here. Two moves are really important. The first is to make the claim of convergence plausible, viz., to show that there is continuity in theory-change and that this is not merely empirical continuity; substantive theoretical claims that featured in past theories and played a key role in their successes (especially novel predictions) have been incorporated in subsequent theories and continue to play an important role in making them empirically successful. But making this first move does not establish that the convergence is to the truth. For this claim to be made plausible a second move is needed, viz., that the emergence of this stable network of theoretical assertions is best explained by the assumption that it is, by and large, approximately true. This is, roughly put, the role of the no-miracles argument. In doing all this, current theories constitute the vantage point from which we examine old ones—could there be any other vantage point? Yet, the identification of the sources of success of past theories need not be performed from this vantage point.

Note that those who think that the history of science will necessarily lead to a pessimistic conclusion, viz., that current theories too are likely to be false and abandoned, rely on various illicit philosophical assumptions that can be unearthed

¹⁸ Ludwig Boltzmann “The Recent Development of Method in Theoretical Physics,” *The Monist* 11 (1900): 253.

and challenged. One of them is an uncompromising holism regarding the confirmation of theories; another is a theory of meaning and reference that leaves no room for semantic bridges between distinct theories. The point that I am trying to make is that in this debate there is no neutral use of the history of science—the history of science does not speak with the voice of an angel. I take seriously Canguilhem’s dictum that “Without epistemology, it would thus be impossible to distinguish two kinds of history of science, that of superseded knowledge and that of sanctioned, that is, still actual because acting, knowledge.”

FG: *Back to the division between the two traditions. I think that a second split line can be traced back to the notorious Carnap-Heidegger controversy about the role that modern logic should play in the development of future philosophy, about the legitimate employment of language (and arguably, about the political nature of the social reform that both perceived as necessary) but mostly about what the overcoming/abandonment of metaphysics really should amount to.¹⁹ Even after the abandonment of the logical empiricist program, and the consequent rehabilitation of a range of metaphysical concerns, analytic philosophy still presents an hostility (or indifference) to that tradition of fundamental ontology, that kind of Aristotelian “first philosophy” concerned with being qua being, that came back to the fore in the wake of Heidegger’s project of answering “the question of the meaning of Being.” Today’s analytic metaphysics is organized around the problems of modality, of defining space and time, of causation, personal identity and free will, and hardly address the issue of “Being” (indeed, I think that a rough but efficient rule of thumb to distinguish a piece of analytic philosophy from a continental one is to count the occurrences of*

¹⁹ The classic reference for this debate remains Michael Friedman, *A Parting of Ways: Carnap, Cassirer and Heidegger* (Chicago and La Salle, IL: Open Court, 1990), but Abraham Stone recently proposed a slightly different take on the disagreement between the two philosophers, downplaying their disagreement over issues of logical consistency and emphasizing those regarding the allowed uses of language in his “Heidegger and Carnap on the Overcoming of Metaphysics” in *Martin Heidegger*, ed. Stephen Mulhall (Aldershot: Ashgate, 2006).

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“Being” as a noun). In Heidegger’s eyes, what contemporary philosophy of science refers to as “metaphysical commitments” would amount to a mere ontic project of identifying existent entities, rather than a properly ontological inquiry of Being itself. On the other hand, post-Heideggerian continental philosophy has kept referring to “Being” in its ontological (but post-metaphysical) projects, especially in the work of “realist” thinkers such as Gilles Deleuze and Alain Badiou, the former reactivating a tradition of “univocity of Being” which runs back to Duns Scotus, the latter reformulating the question of being in mathematical terms. This disagreement regarding the possibility of ontology can be seen as rooted in a different relationship with the natural sciences. From your standpoint, does it make any sense, today, to pursue the question of what “Being” is or means over and above what current best science tells us about the fundamental constituents of the universe, or is such a question a vestigial problem, a relic of medieval scholasticism or a “Heideggerian hangover?”²⁰

SP: I would not trust Heidegger too much! And I doubt he should be given too much credit anyway. If one were to answer the question “what is metaphysics?” by trying to read Heidegger’s homonymous lecture, one would get a very distorted and perplexing idea of what it is all about. I’d say: if you want to do metaphysics (and to see metaphysics at its best) start straight from its source: Aristotle. The question of being is central to his *Metaphysics*. But more importantly, Aristotle suggests that there are two questions to be asked. One is what kinds of things there are (what kinds of being are), while the other is what it is for something to be: what is being. It might well turn out that these two questions are interconnected. But their conceptual separation makes metaphysics possible as a distinct and distinctive enterprise. For the second question can be asked only *within* metaphysics; it arises from a genuine metaphysical *aporia*. It transcends the

²⁰ I borrow this expression from Adrian Johnston, “Hume’s Revenge: À Diex Meillassoux?” in *The Speculative Turn: Continental Materialism and Realism* edited by Levi Bryant, Nick Srnicek and Graham Harman (Melbourne: Re.Press, 2010), 110.

bounds of the individual sciences, since the latter investigate the being-under-a-description, and hence some part of it, say the physical or the biological world (1003a22-26). Metaphysics is the science of essence; of being *qua* being. But Aristotle wanted to put metaphysics in the service of science—what he called *episteme*. The fundamental structure of reality (ultimately comprising primary substances, essences (or essential properties *qua* universals) and accidental properties (*symvevikota*) grounded the possibility of *episteme* and made *episteme* a distinctive kind of knowing (*qua* general, explanatory and necessary). His account of scientific knowledge (in *Posterior Analytics*) goes hand in hand with his account of the fundamental structure or being (in *Metaphysics*). If we take Aristotle seriously, adding the adjective “analytic” to metaphysics is a pleonasm.

I take it that the immediate rival to “analytic” metaphysicians (would it not be better to be called “metaphysicists?”) is the metaphysics-free tradition within analytic philosophy that was associated with Humean empiricism and later on with logical positivism. Could it then be that the addition of “analytic” is meant to make (pre-Kantian) metaphysics more palatable? Metaphysics is inevitable—the only question is: how much of it is necessary? Now, one may ask: necessary for what? To put it poetically, metaphysics fills the cracks of the scientific image of the world (in its totality and interconnectedness). To put it more theoretically, metaphysics secures the coherence of the scientific image of the world. I very much doubt that it makes sense to do metaphysics in complete isolation from what science tells us about the world, but I also think that science does *not* dictate a unique conception of the metaphysical structure of the world; of the kinds of beings there are; of the kinds of connections there are among them; of the basic characteristics that they have to have in order for the world to have unity and coherence. Science goes a long way, but not all the way (ultimately, it cannot settle the question of being *qua* being). Think of the question of what, and how many distinct, categories of being need to be presupposed by a coherent conception of

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reality—this is the problem of nominalism versus realism about universals. Or think of the question of whether there is *sui generis* power in the world which grounds and explains the regularity there is in it, or whether it is regularities all the way down, as I am fond of saying—this is the problem of the nature of causation. Or think of the question of whether some kinds of properties are constitutive of the kind of being something that there is *is* or whether all properties are on a par—this is the question of essentialism. These are typically metaphysical questions whose answer should certainly be constrained by what we know of the world via science; but they are clearly underdetermined by what science tells us about the world.

If you think of it, this situation is not terribly odd or unfamiliar. Scientific theories themselves are underdetermined by the empirical evidence and yet there are plausible criteria to break ties of empirical equivalence: empirical equivalence does not entail epistemic equivalence. The situation is essentially the same with metaphysics: the name of the game is “inference to the best explanation.” Metaphysical hypotheses about the structure of the world might not explain in precisely the same way in which scientific hypotheses about unobservables explain, but they do play an important explanatory role by enhancing the unity and coherence of the scientific image of the world. When the logical positivists attacked metaphysics, they were not in the business of taking explanatory criteria as decisive. A.J. Ayer famously took it that what’s wrong with metaphysics is that it promises knowledge of reality which transcended the world of experience. He was right that there is no special non-empirical method of acquiring knowledge of the world. But he was wrong to restrict the empirical methods of science to those allowed by verificationism. Be that as it may, verificationism was a natural (if exaggerated) reaction to the speculative metaphysics of German idealism and its successors. Heidegger, for instance, thought that the inquiry about what he called *THE* nothing (the non-being) is a central preoccupation of metaphysics, which sets it apart from science (of which Heidegger said that it “wishes to know

nothing about the nothing”). Carnap was fully justified to take on *this* conception of metaphysics and to argue that it fails to express genuine propositions. Here again, Carnap was taking metaphysics to be an endeavor to “discover and formulate a kind of knowledge which is not accessible to empirical science,” perhaps by means of *special inferences* that may begin from experience but transcending experience. This is something that Heidegger and co. may well have been fond of. But explanatory methods (which are legitimately employed in science) might well take us beyond experience without transcending it (at least in the technical philosophical sense of “transcendence”). In 1957, when Carnap added some remarks to the English translation of “The Elimination of Metaphysics through the Logical Analysis of Language” he noted that his early reactions to metaphysics did not apply to attempts “towards a synthesis and generalization of the results of the various sciences.” When philosophers like Quine (and Sellars) made room for explanation, metaphysics (properly understood as not relying on *sui generis* methods and inferences) started to become legitimate again. Quine was sharply critical of Carnap’s point that ontological questions could be asked in two distinct ways: as external questions and as internal ones. Carnap, famously, excluded *external theoretical questions*: questions about the reality of a general *type* (or category) of entity which are supposed to be settled by looking for (empirical) evidence for the reality of this type or by insight into the metaphysical structure of the world. Questions concerning the reality of a type of entity, Carnap argued, are legitimate and have content, but only if they are taken to be *either* external *practical* questions concerning the benefits of adopting a certain framework which includes this type of entity in its basis ontic inventory *or* as *internal theoretical* questions concerning the evidence there is for (or other reasons for accepting the reality of) certain tokens of this type, but only after a framework has been adopted. Despite his trenchant criticism of Carnap’s dichotomy, Quine did agree with Carnap on a fundamental point, viz., that there is no theory-free standpoint from which what there is can be

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viewed. But he took this denial of a theory-free vantage point to imply that there is no sharp line between theoretical issues (or questions) and practical ones. Ontological questions (questions about what there is) are theoretical questions *as well as* practical ones: they are answered by our best theory and there is no extra-theoretical court of appeal. Already in *Two Dogmas of Empiricism*, Quine had argued for the “blurring of the supposed boundary between speculative metaphysics and natural science.”

If explanation-based metaphysics is allowed, where does one stop? Should, for example, a scientific realist adopt neo-Aristotelianism simply on the basis that it is the best explanation of, say, the neo-Humean account of the world? My own view on this matter comes to this. We should certainly take IBE seriously, but it can be contested that neo-Aristotelianism does indeed meet the best explanation test. One particularly acute problem is that all the denizens of the neo-Aristotelian world (powers, metaphysical necessities, dispositional essences and the like) are themselves unexplained explainers. Though everyone should accept *some* unexplained explainers, in this particular case, they are more poorly understood than the Humean facts that they are supposed to explain. Another problem, noted above, is that it is not clear at all how all these heavy metaphysical commitments are related to current scientific theories. The fact is that this kind of neo-Aristotelianism—and its commitment to heavy-duty metaphysics—has become a major force in current analytic metaphysics. And it also true that it is being developed (to a large extent at least) in close connection with science. Unfortunately, not all current analytic metaphysics is in contact with current science. This raises a serious issue: what are the criteria of success in metaphysical theorising? It cannot be merely internal consistency; the metaphysical theory must also be plausible. Since there is no *a priori* insight into plausibility, I think the plausibility ranking must be based on the ordinary defeasible criteria that are used in science to rank and evaluate competing theories. If all this sounds too shaky a ground for metaphysics, so be it!

FG: I largely agree with you here, even though I think that some forms of rationalism or a priori forms of reasoning can be salvaged if articulated within a Darwinian framework, defending a kind of naturalized rationalism which in my opinion is the most interesting path of inquiry taken by up by some “continental naturalizers.” However, going back to your indictment of neo-Aristotelian metaphysical options like powers or dispositional essences (and in general your scepticism towards any sort of “crowded” metaphysics) I would like to probe your opinions a little deeper with a “limit case.” How do you react to the recent renaissance of panpsychism (seen both as an approach to the “hard problem” of consciousness²¹ but also as a respectable general metaphysical option²² for a description of reality as a whole)? On the one hand, what I find interesting is that it seems to be an option which cuts transversally across the “two traditions” drawing in metaphysicians of both purely analytic breed and those inspired by German idealism or phenomenology. On the other, it seems to me to be a hopelessly wrongheaded stance, one that fails your test of plausibility as being the best explanation, and that makes a rather odd use of otherwise correct anti-anthropocentric guidelines—it’s alleged to be a sign of human-centered narcissism to assume that humans are the only entities in the universe endowed with “mind” or some form of intentionality. The most famous argument here is the Galen Strawson thesis that “real physicalism” (as opposed to a reductionist, dogmatically scientific “physicSalism”) actually implies panpsychism.²³ Your “scientific realism with a Humean face” is open-minded enough to not be a dogmatically, “old-fashioned” physicalist one (or indeed invested in any other strong metaphysical commitment) because it is defensible independently from naturalism, but isn’t panpsychism a prime example of

²¹ See Michael Blamauer, *The Mental as Fundamental: New Perspectives on Panpsychism* (Heusenstamm: Ontos Verlag, 2011) and David Chalmers, *The Conscious Mind: In Search of a Fundamental Theory* (Oxford: Oxford University Press, 1996), 297-299.

²² See *Mind that Abides: Panpsychism in the New Millennium* edited by David Skrbina (Amsterdam and Philadelphia: John Benjamins Publishing Company, 2009).

²³ See Galen Strawson “Realistic Monism: why Physicalism entails Panpsychism,” in *Mind that Abides: Panpsychism in the New Millennium*.

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an explanans which is far less clear than the explanandum? Is this not a clear case where “empirical equivalence does not entail epistemic equivalence?” Personally, I am particularly interested in the metaphysical clashes behind the science vs. religion debates, and to take as an example another path-breaking panpsychism-friendly philosopher—Thomas Nagel—I think a clear point can be made regarding all this. The argumentum ad ignorantiam that proceeds from our sketchy understanding of consciousness to the plausibility of some form of conscious activity in non-human entities, essentially defended in his 1979 essay,²⁴ seems to me to lead directly to the theses he puts forward in a later essay called “Secular Philosophy and the Religious Temperament.” Here, he seeks a “secular alternative” to reductive naturalism and identifies it in a kind of natural teleological process wherein “each of us...is a part of the lengthy process of the universe gradually waking up”²⁵ How far does your Humean/empiricist outlook allow you to go in the refutation of a thesis like this, which seems to fly in the face of some central, historically hard-won, steps towards the goal of a full(er) scientific knowledge of nature (here, the rejection of Aristotelian teleology and the physics it produced)?

SP: There are endless possibilities in philosophy, given time and world enough. I have not followed the literature on panpsychism (at least the recent one, since a form of it is supposed to be present in Spinoza), but I feel there are two readings of it, one weaker (and relatively plausible) and another stronger (and I think implausible). The weaker reading, I take it, is an attempt to dethrone the human mind from the centre of the universe, opposing the Protagorean idea that the human being is the measure of everything. In this sense, panpsychism would say that the mind and the mental life is not the prerogative of the human animals. But note well: this conception does not entail a special view about the soul

²⁴ Thomas Nagel, *Mortal Questions* (Cambridge: Cambridge University Press, 1979), Chapter 13.

²⁵ Thomas Nagel, *Secular Philosophy and the Religious Temperament* (Oxford: Oxford University Press, 2010), 16,17.

or the mind, or the spirit. In the history of philosophy, these have been the various candidates (typically, but not invariably, taken to be the same “thing”) for the uniqueness of the humans among the “created” beings. This individuating factor has been taken to be imperishable, in constant motion, the locus of thought and mental activity, the subject of salvation and others. Weak panpsychism need not be committed to all this and is consistent with the scientific image of the world (if we take it to imply that the mental life is not uniquely human). The stronger version of panpsychism, in my view, would be committed to the implausible hypostatisation of the soul, albeit extending it to other animals (or even to non-animals). Why is this view implausible? Precisely because it does not sit well with what we know about the mind and its functions. It feels good to believe that there is an immortal soul; that the mind is a substance; that there is a set of non-natural properties that constitute the mental economy. It gives reassurance. But does it do good? I see no intellectual benefit in accepting this view. I still endorse non-reductive physicalism (though it is not entailed by scientific realism) which is essentially the same as naturalism. Sober—that is non-eliminative—naturalism puts a pressure on everything that is (supposed to be) non-natural to show that it has what it takes to be included in the natural world. So, all *prima facie sui generis* entities (or states, or attitudes) that are needed to explain Moorean facts (which include facts about colours and epistemic norms and evaluative attitudes and beliefs and pains) need to *earn their right* to be included in the natural world. They don’t earn this right automatically (by featuring, say, in potential explanations—cf. animistic or vitalistic explanations). Nor do they enter the natural world autonomously. And to earn this right is, a naturalist would say, to be suitably dependent on the natural. There are notorious problems with this notion of dependence. But the central characterisation, I think, should be in terms of physical constitution. Naturalism need not be imperialistic, but it has to be *elitist*. Even so, it’s *not* arrogant elitism that characterises it. Anything that is *prima facie sui generis* can earn the right for inclusion in the

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elite club, but they have to do some work to achieve that. It is a contingent fact about the world that all spatio-temporal entities are physically constituted. This does not, on its own, exclude the possibility of a property-dualism (or, better, property-pluralism.) But perhaps all that is needed to be added is that given the physical constitution of all spatio-temporal objects, whatever properties they have—and whatever causal powers they endow these objects with—are controlled from *within* “and are not imposed upon them from without” (cf. Dewey, Hook & Nagel 1945, 109).²⁶

Naturalism excludes supernaturalism. Perhaps, Dewey, Hook and Nagel (1945, 116) can help here too. The *horror supernaturalae* is indeed the horror of naturalists. But this horror is the expression of a methodological policy: it is the firm refusal to accept that for which there is no evidence (or, in some cases, that for which there is overwhelming evidence against). In a certain sense, the naturalists’ *horror supernaturalae* is the outcome of the following principle: if something is not acceptable, then it should be avoided, which is the contrapositive of the sound principle: What cannot be avoided, is to be accepted.

There is an issue I want to touch upon and this is the role of *a priori* within naturalism. Philosophical tradition has wavered between two conceptions of the *a priori*: the absolute conception and the absolute rejection. The absolute conception is exemplified in Kant. According to the Kantian conception, the possibility of human knowledge requires placing *a priori* restrictions on the admissible models of the experienced world—only those models are admissible that conform to a set of synthetic *a priori* principles. This captures a sense of *constitutive a priori*: some principles are necessary presuppositions for knowledge (and for doing science)—necessary in the sense of being *sine qua non* for understanding the world. Since those principles that are necessary for experi-

²⁶ John Dewey, Sidney Hook & Ernst Nagel “Are Naturalists Materialists?” *Journal of Philosophy*, 42 (1945), 515-30, reprinted in *American Philosophical Naturalism in the Twentieth Century*, edited by J. Ryder (New York: Prometheus Books, 1994).

ence precede experience, they cannot be defeated by it; they are permanent and *unrevisable*; they are *necessarily true*. Kant thought that these two senses of being necessary—necessary presuppositions for doing science and necessary as permanent and unrevisable—ought to *coincide* if some principles properly were taken to be independent of experience. This coincidence is the kernel of the absolute conception. According to the Millian-Quinean absolute rejection of the *a priori*, there cannot be any justification independently of experience. Mill's chief point was that *all* justification, even justification of the laws of arithmetic, is inductive. Quine's chief point was that *everything* can be revised or abandoned in light of experience. Since, according to the absolute conception, statements that are supposed to be *a priori* are unrevisable, Quine drew the conclusion that there are simply no *a priori* principles. The logical empiricists (capitalizing on an empiricist tradition that arguably goes back to Locke and Hume) thought that there is a middle way: some truths (notably the truths of logic and maths) were meaning-constitutive analytic truths; hence they tried to secure the *a priori* by tying it to analyticity (and to necessity, by implication, since all and only analytic truths were supposed to be necessary). Quine's arguments against analyticity have conclusively shown that there is no non-circular way to characterise analyticity. This, of course, does not show that there are no analytic truths—but it does question that we have a coherent idea of what we attribute to them when we call them analytic.

There is another way to defend a middle position between the absolute conception and the absolute rejection, without being committed to analyticity. This is to drive a wedge between the elements of *a priori* knowledge: constitutivity and necessity. The *locus classicus* of driving this wedge is found in Hans Reichenbach's *The Theory of Relativity and A Priori Knowledge* (1921). He drew a distinction between two elements in the Kantian conception: *a priori* principles are meant to be necessarily true; and they are meant to be constitutive of the object of knowledge. Reichenbach accepted the second dimension but denied that *a priori* principles were

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necessarily true and unrevisable—rather, being framework-dependent, they are abandoned when the framework they are constitutive of is abandoned. I have tried to develop this middle ground in joint work with my ex-student and current colleague Demetra Christopoulou.²⁷ The point I want to make now is that this relativised conception of the *a priori* seems compatible with a broader naturalistic perspective in the sense that naturalism does not obliterate the spontaneity of the understanding; nor is it committed to the rejection of the view that some principles are constitutive of the object of knowledge. In a rather marvelous passage, Poincaré drew a fine distinction between contradiction and condemnation. He was quite firm in that no experiments can ever contradict a constitutive principle (what he called “conventions”). For no experiment can conclusively *refute* such a principle. Yet, experiments can *condemn* a constitutive principle, or even a whole framework, in that persistent failure to account for new facts renders a particular principle or a whole framework no longer convenient. What a *realist* naturalist should retain at all costs is obviously the possibility of friction between our conceptual schemes and the world, which friction (making itself present in persistent and recurring anomalies) is (to a large extent) responsible for the replacement of conceptual frameworks by others.

FG: *The analytic/continental divide is active on several dimensions: professional, stylistic, methodological and thematic. I take the first to have little of philosophical merit, boiling down to a matter of safeguarding one's own academic turf. Are the other dimensions crystallized enough to impede hopes of reconciliation, and is reconciliation a desirable outcome to start with? I think there can be three possible approaches: 1) bridging the gap, possibly through an interpretative work aimed at demonstrating how behind different methods and styles there can be identified*

²⁷ Stathis Psillos and Demetra Christopoulou “The A Priori: Between conventions and implicit definitions.” In N. Kompa, C. Nimtz, and C. Suhm (eds.), *The a priori and Its Role in Philosophy* (Paderborn: Mentis Verlag, 2009).

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common concerns;²⁸ 2) preserving the gap, in the name of either the preservation of “essential traits” (analytic virtues of problem-solving and clarity vs. continental “breadth of vision” and “existential relevance”) whose disappearance is deemed dangerous²⁹ or in view of an inherent value of a fragmentation of viewpoints and approaches, or 3) ignoring it. What would this latter option amount to? I take it to be a real possibility that the divide will gradually vanish with generational change: as the “old guard” dies out, a new generation of philosophers will achieve intellectual maturity having ignored institutional divisions and having simply read-and thought through-the work of philosophers from both camps. Here I agree with part of Richard Rorty’s diagnosis, identifying the institutional origin of the split at the “graduate student level.” Rorty argued that

graduate students trying to shape themselves into plausible job candidates for teaching positions in philosophy only have time to read so much. They can please only so many potential employers....No matter how much intellectual curiosity a student has...there just is not enough time. So if she develops am-

²⁸ One can think of the work of Christopher Norris and (more recently) of Samuel Wheeler to present Derridean deconstruction (and, in Norris’ case, Alain Badiou’s philosophy) in terms understandable to analytic philosophers, or of Lee Braver’s and Jeff Malpas’s comparison of the work of Davidson with Heidegger’s and Gadamer’s (see Christopher Norris, *Language, Logic and Epistemology: A Modal-Realist Approach* [Basingstoke: Palgrave, 2004]; Christopher Norris, “Tractatus Mathematicus-Politicus: on Alain Badiou’s *Being and Event*” in *Speculations II* (2011), 7-48; Samuel Wheeler, *Deconstruction as Analytic Philosophy* [Stanford: Stanford University Press, 2000]; Braver, *A Thing of This World*, and *Dialogues with Davidson: Acting, Interpreting, Understanding*, edited by Jeff Malpas [Cambridge, MA and London: MIT Press, 2011]. Also several chapters of *A House Divided: Comparing Analytic and Continental Philosophy* edited by G.C. Prado, (Amherst, NY, Humanity Books, 2003) seem to adopt this “contrast and compare” strategy over selected philosophical themes.

²⁹ An example from the analytic camp is Timothy Williamson, who in the appendix of his *The Philosophy of Philosophy* (Oxford: Blackwell, 2007), 289, recommended that contemporary analytic philosophy as a whole should “do better,” guilty, in his eyes, of forsaking its mandate of argumentative clarity, rigor and precision and indulging in “ugly, convoluted, ramshackle definitions of concepts and theses.”

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bidexterity [Rorty's term for proficiency in both traditions], it will usually be in later life—usually after she gets tenure. Then she can afford to start following her nose rather than pleasing interviewers or senior colleagues.³⁰

However, I think that we are witnessing today the emergence of a significant minority of graduate students reckless enough to take the risk and attempt to develop “ambidexterity.” Do you see some form of reconciliation as necessary, and would you encourage students to ignore traditional boundaries? And would you say that a realist philosophy of science can be at the forefront of such reconciliation, the two traditions having, so to speak, to be judged equally by the standards of an external reality independent of the philosophical style one uses to examine it?

SP: Hume used to say that philosophy arises out of intellectual curiosity and that the philosophical problems will keep cropping up and boggle the investigative mind even if we try to lay them to rest by an appeal to common sense. There are different ways to address the very same philosophical problems; there are different prioritizations of their urgency; and, ultimately, there are different problems for which philosophers are curious about. This, schematically put, explains the dichotomy between the two traditions, but also highlights that they are traditions within *the very same intellectual enterprise*. As I noted in my reply to an earlier question, what we call “the two traditions” have emerged from the very same womb and they share a common ancestry. In practice, things are more complicated of course, and no-one should be oblivious to this. There is a certain philosophical ideology associated with each tradition and until fairly recently there have been important linguistic, stylistic and methodological hurdles that had to be jumped if one were to immerse oneself in both traditions. Even nowadays, it's hard to understand a thinker from the “opposite tradition” unless you read stuff that ex-

³⁰ Rorty in Prado *A House Divided: Comparing Analytic and Continental Philosophy*, 18.

plains what they were supposed to be doing in the language of the tradition you are duly immersed. Immersion is part of the philosophical training and it depends, at least partly, on contingent factors. Given this, I doubt that the matter has to do with an “old guard” and its resistance to rapprochement. In the European continent, where the “continental tradition” was dominant for decades, there is a younger and very dynamic generation of philosophers which conscientiously inscribe themselves within the analytic tradition and pursue analytic themes vigorously and with flair. There are vibrant societies for analytic philosophy and plenty of congresses and workshops. This might be ironic since it happens in an age in which the original divide tends to fade away in the Anglo-american philosophical community. This might well have to do with the fact that the history of philosophy has become a hot topic in the analytic tradition in the English-speaking world. But on the European continent, analytic philosophy still plays the role of an identity-maker among young academic philosophers. To promote analytic philosophy is to make a statement about what philosophy is; what philosophical problems are important; what methods pertain to philosophy; how philosophy is connected with science, etc. I am part of this tradition in my own country, even though I understand its limitations. Reconciliation will take time. Developing a *rapprochement* is much more manageable and welcome. The form that this will take is hard to tell. I would encourage philosophy students to engage with the writings of the major thinkers of the twentieth century and to try to identify the problems they were grappling with and how these problems re-appear and are re-shaped in the work of various past and present philosophers of various schools and traditions.

Can realism facilitate this rapprochement? The very issue of realism and its rivals is constitutive of philosophy and present in both traditions, perhaps in different forms. In this sense, it could provide a platform for thinking that philosophy is ultimately one and its fundamental problems the same for all. I was very glad to see (in the material you sent me and in the claims made in your questions) that there is a “realist

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turn” happening presently in the continental tradition. It was even more heartening to learn that this turn is a self-conscious attempt to reclaim the realist ground and to recoil from the dominant neo-idealist and anti-realist tendencies within this tradition. I feel that what you call “correlationism” (the view that the only thing that can be accessed is the relation between thought and being and not the relata in isolation of this relation) has had a strong grip on the tradition that obscured the fact that a) the relata can be posited (and get their identity) independently of their relation; and b) if the (cognitive) access to reality were independent of thought, language, concepts, etc. we would not *need* thought, language, concepts, etc. to access reality. It is precisely because the cognitive access to the *independent* reality is mediated by epistemic categories, that the very question of the conditions and credentials of this access becomes philosophically exciting and pressing. If there is a problem that “correlationism” points to, (the problem of how thought is related to reality), it is *not* solved by collapsing the two relata to one. It cannot be solved by making thought spinning in the void. There are various issues that can provide a fertile ground for the growth of the discussion between the various realist tendencies in both traditions; to name but a few: the relation between realism and materialism, the issue of reductionism, the role and function of mathematics and the question of truth.

FG: Let us pursue the theme of the continental “realist turn” then. As I’ve tried to sketch in the opening remarks, a common trait of these new continental approaches to realism is the insistence on considering reality in-itself as not reducible to our cognitive capacities and to our metaphysical categories. Their return to realism associates a rejection of Kantian dichotomies between the humanly knowable and the unknowable (or even of the postmodern, relativist, or linguistic denial of any noumenal reality) with informed allegiance to contemporary science. These philosophers are especially interested in the counter-intuitiveness and irreducible character of the reality presented to us in the Sellarsian “scientific image.” Consequently, metaphysics is not seen as limited

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to Strawsonian “descriptive” tasks but allowed to be thoroughly “speculative,” i.e. legitimately operating on a purely philosophical ground to offer an account of those consequences and presuppositions which science cannot account for intrascientifically. Adrian Johnston (a critic of some “speculative realist” positions, but part of the broader resurgence of continental realism nonetheless) summarizes this spirit when he argues, referring back to Hegel, that “the sciences produce out of themselves, on their own grounds, an internal delimitation of their explanatory jurisdictions”³¹ while a physicist like Gabriel Catren proposes a “speculative physics” aimed at deducing the rational necessity of scientific theories.³² As I noted above, an economical way to say this is that these kinds of “speculative” realisms offer only a conditional submission to naturalism: the natural sciences are the most reliable epistemic enterprise which humans have managed to come up with, but there are real features of the world which a method regulated by strict empiricist scruples cannot fully account for. Herein lies the subtle but crucial divergence between the continental and the analytic realist stance. You are strongly against what you call “principled epistemic divisions” between what can be known and what cannot, and indeed claim that it is possible to know the structure of nature (that is what current best science offers us), but in your Humean-flavoured realism you have a naturalist skepticism of those inflationary neo-Aristotelian metaphysics which postulate natural kinds, powers, metaphysically necessary laws and so on, since they rely too much on a priori postulation of what the world must be like. How do you feel about attempts to reintroduce some forms of rational speculation in the context of our scientific worldview, a philosophy that, starting from the natural sciences, attempts to employ their results as speculative opportunities for a reconceptualization of our metaphysical categories, included those which were employed by science in the first place? To focus this further, you wrote that “only science can tell us what the world

³¹ Adrian Johnston, “Materialism, Subjectivity and the Outcome of French Philosophy,” interview by Michael Burns and Brian Smith in *Cosmos and History*, 7:1 (2011), 177.

³² Gabriel Catren “A Throw of the Quantum Dice Will Never Abolish the Copernican Revolution,” *Collapse V* (2009), 459.

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is like. Philosophy can only raise some principled challenges to the ability of science to tell us what the world is like.”³³ How far, in your view, can these challenges go? Are there reasons to place some boundaries on the epistemic audacity of science, and its ability to answer, without philosophical aid, meta-scientific questions about science’s own foundational assumptions or are these questions to be considered (in positivist fashion) meaningless?

SP: Science is far from sacrosanct! But it is also by far the best way we humans have invented to know the world. This does not mean that philosophy is the handmaiden of science; nor does it imply that the scientific image of the world is free from deep and controversial philosophical assumptions. In my most recent book, *Knowing the Structure of Nature*, I indeed argued against the view that there is a principled epistemic division between what can be known of nature and what cannot; hence that there is a principled limit to the scientific knowledge of the world. This limit is different in the assorted positions that I argued against, but it is supposed always to be *principled*, *definite* and drawn by philosophical reflection and argument. I am not claiming that science will discover everything there is to know. Science might, in the end, not reveal us what the world is like. It might be able to disclose only part of the structure and furniture of the world. But this is as it should be. It would be a totally different matter if there were good reasons—mostly drawn by philosophical reflection on science, its methods and its limits—to believe that we *qua* cognitive beings, or science *qua* an epistemic enterprise, are cognitively closed to some aspects of the unobservable world. What I do claim is that though there might be parts of nature that science might never be able to map out, these do not fall nicely within a conceptual category which captures one side of a sharp epistemic dichotomy (the unknown *x*: the things in themselves; the unobservable; the non-structure; the intrinsic properties, or what have you).

³³ Psillos in *The Continuum Companion to the Philosophy of Science*, edited by Steven French and Juha Saatsi, (London and New York: Continuum, 2011), 88.

Naturally, there are significant philosophical motivations for raising these epistemic barriers that science is supposed to be unable to cross. It might be ironic but one important recent motivation is that (a form of) realism is best defended if it lowers its epistemic optimism. Hence, there are weaker versions of realism on the market such as structural realism or semirealism. The challenges to realism come from various sources, but perhaps the most significant (as we have already seen) comes from the history of science, and has the form of the pessimistic induction. Another challenge (with some empiricist credentials) comes from the claim that the explanation by reference to unobservable entities and mechanisms (what I call explanation-by-postulation) leads to inflationary metaphysics. There is a sense in which this is obviously true: realism takes science to proceed by positing further entities that are meant to *explain* the life-world and its (typically non-strict) laws. But in another sense, the inflation is metaphysically harmless. For, if you think of it, science proceeds by positing micro-constituents of macro-objects, whose main difference from them is that they are, typically, unobservable. That a putative entity is unobservable is, if anything, a relational property of this entity and has to do with the presence of observers with certain sensory modalities (of the kind we have) and not others. No interesting metaphysical conclusions follow from this fact; nor any seriously controversial ontological inflation.

As I have noted already above, the attempt to marry realism with a neo-Aristotelian conception of the metaphysical structure of the world is a different matter. There I side with neo-Humeanism, which I take to involve the following three negative theses:

- A There are no necessary connections between distinct existences (No necessity enforcers).
- B There are no universals as distinct from classes of resembling particulars (No resemblance enforcers).
- C There are no powers as distinct from their manifestations (No regularity enforcers).

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It might be thought that neo-Humeanism is anti-metaphysics altogether, but this is wrong. As I said already, metaphysics—that is, a view about the deep structure of reality and its fundamental constituents—is not optional. The only serious issue, I believe, is how *deeply* this view should be digging; how rich the conception of the fundamental structure of reality ought to be. Neo-Humeanism promotes a rather thin—or sparse—view of the fundamental structure of reality, according to which there are irreducible regularities in nature (regularities all the way down, so to speak) which involve patterns of dependence among members of natural classes (natural properties) and which underpin the causal and generally modal relations there are between them. But buying into the idea that the world is characterised by regular patterns of co-existence and succession of property-instances is metaphysics enough!

Does science need the help of philosophy? Clearly yes! There are certain issues that can be raised only within a proper philosophical perspective on science. These include the status of first principles in science; the relation of science to reality; the epistemic credentials of scientific theories; the fabric of the deep structure of the world as it is described by science; and the very possibility of a unified (but not necessarily reductive) account of it. Actually, these are issues that cannot be successfully dealt with at the level of individual sciences. We have to look at *science as such*. The individual sciences, as well as their philosophies, lack the conceptual resources and the power of abstraction that are required for a more global perspective on reality—for seeing the whole picture. They are limited by the fact that they focus on aspects or layers of reality. Putting together the scientific image of the world, looking at the various interconnections among the “partial” images generated by the individual sciences, and clearing up tensions and conflicts is precisely the kind of job that philosophy of science is meant to do. To put it in Sellarsian terms, philosophy of science offers the space in which the various images of the world provided by the individual sciences are fused together into a stereoscopic view of reality.

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FG: A similar question but put in terms of truth rather than ontology. A stance defended by some recent figures of continental realist philosophy, inspired by the work of Alain Badiou, is that we need to reconceive the concept of truth as that which by definition breaks the boundaries of our current-best knowledge and that which introduces radical novelty in our worldview. A wedge must be firmly put between knowledge and truth, since the latter will have the power to completely rearrange the structure of the former. You think that a verification-transcendent conception of truth is a cardinal pillar of the realist position, and that true assertions have truth-makers which are independent of our current opinion (or lack thereof) about them and that we shouldn't be shackled by the epistemic criteria of warranted assertability or trapped in our linguistic horizon. Yet I suspect you would be cautious of claiming that new truths can be discovered through purely rational, logico-deductive means rather than by ampliative inference grounded on empirical observations. Do you consider it possible for our truth-tracking enterprises to go, to use Graham Priest's formula, "beyond the limits of thought,"³⁴ to those boundaries that cannot be crossed, and yet are crossed? And do you consider the total set of truth-makers in the universe (the known and the unknown existents) to be a closed totality that doesn't allow for novelty? Or is this, once again, a meta-scientific question that the empirically-minded realist can refuse to answer?

SP: As I noted above, I take a non-epistemic conception of truth to be an essential realist commitment because this is the best way to capture the standard realist assertion that the world is mind-independent. Traditionally, the opponents of realism (idealism and phenomenalism) expressed their antagonism to realism by claiming that there is only mental stuff in the world. So the realist declaration of independence might be seen as a commitment to the view that there is material stuff in the world and, in particular, that the entities posited by scientific theories are non-mental (material). I do not think this

³⁴ See Graham Priest, *Beyond the Limits of Thought* (Cambridge: Cambridge University Press, 1995).

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is a useful way to think of the realism issue any more. There is an anti-realist tradition which argues for something more complicated and interesting. It centres on the conditions that must be in place for legitimate commitment to the existence of whatever entities are said to make up the world. According to this long anti-realist philosophical tradition, it makes no sense to be committed to the existence (or reality) of some entities unless this commitment is understood as implying (and being predicated on) the fulfilment of certain *epistemic/conceptual* conditions, the most popular of which is Michael Dummett's warranted assertibility. Very much like realism, this tradition opposes idealism and phenomenalism. But it does render the world (or a set of entities) mind-dependent, albeit in a subtler sense: it forges a logical-conceptual link between what there is in the world and what is licensed as existing on the basis of the satisfaction of suitable epistemic conditions; hence, this kind of anti-realism renders what there is (whatever kind of stuff it may consist in) exhaustible by what can be known in principle (verified, warrantably asserted and the like) to exist. Opposing this kind of mind-dependence, the realist claim of mind-independence should be understood as logical or conceptual independence: what the world is like does not logically or conceptually depend on the epistemic means and conceptualisations used to get to know it.

As I stressed above, this commits realism to the possibility of a *divergence* between what there is in the world and what is licensed as existing by a suitable set of conceptualisations and epistemic conditions. Modern anti-realism (let's call it verificationist anti-realism) precludes (*a priori*) this possibility of divergence by adopting an epistemic conception of truth. What, ultimately, is at stake in the scientific realism debate is a robust sense of objectivity, according to which the world as it is independently of our changing and evolving conceptualisations of it is the final arbiter of their correctness. Verificationist anti-realism cannot, however, dissociate objectivity completely from the obtaining of some or other (however idealised and inter-subjective) epistemic condition. The result is that the final arbiter of the correctness of our

conceptualisations is not the world but the fact, if it is a fact, that some but not other conceptualisations satisfy certain epistemic conditions and therefore are licensed by them.

Some care is needed here, however. The claim that truth is evidence-transcendent is a claim about the nature of truth; a claim about what makes a truth true. It is not an epistemic claim about the knowledge of truth; it does not lead to scepticism (though it does leave its possibility open). Attaining truth very much depends on our truth-tracking methods and their reliability. These are ampliative and hence defeasible. Their success requires epistemic luck, but it is *not* due to luck; it requires (and gives us evidence for) a co-operative world.

I would certainly not contrast truth to knowledge. Truth is required for knowledge, but there may well be unknown truths. Whatever else it is, truth is something that has no expiry date. Unlike dairy products, truth cannot go off. If a belief is *true* now, it is true atemporally: it has been true in the past and will stay true in the future. In this sense, truth cannot be equated with acceptance or kindred epistemic notions. Nor can it be equated with what communities or individuals agree on, or with what the present evidence licences. If we made these equations, truth would not be a stable property of beliefs. It could come and go all too easily. Besides, if we made these equations, we would end up with a thoroughly relativised conception of truth. But relativism about truth, viz., the claim that truth ascriptions are always relative to a person or a community, is ugly and self-refuting, anyway. Even those who think that truth is, ultimately, an evaluative concept, have to think of the norms that govern its use as objective (or ideal). Similarly for knowledge. Knowledge is not something that can be lost in the sense that something can be *known* now but not known tomorrow. Sometimes we use the term “knowledge” colloquially, equating it with whatever we have evidence to accept or whatever we believe today. Then we say that our knowledge of the world has changed; or that what it was known in the past is considered false today. This is loose (and incorrect) talk. Once possessed, knowledge is not lost.

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(Clearly, knowledge can be “lost” in the unproblematic sense that some kind of truth that was known in the past has not survived in what is known today.) So truth and knowledge are intimately connected. There is simply no guarantee that all truths are knowable; and in any case, realism allows for the *possibility* that there are unknowable truths. Suppose that, as a matter of fact, all truths are knowable and that there is a coincidence between whatever is licensed by an epistemically right theory of the world (that is, a theory that satisfies certain epistemic conditions) and what *really* exists in the world. This need not compromise the realist commitment to the mind-independence of the world. Nor, of course, does it commit realism to an epistemic account of truth. All the realist needs to claim is that there is a certain direction of fit or order of dependence. This can be made plain by being put in terms of a Socratic Euthyphro-type contrast. Suppose there is a coincidence between what there is in the world and what is licensed as existing by an epistemically right theory (that is, a theory that meets certain epistemic conditions). Is the world what it is because it is described as thus-and-so by an epistemically right theory *or* is a theory epistemically right because the world is the way it is? Scientific realists can and should go for the second disjunct, while verificationist anti-realism goes, ultimately, for the first.

I am not quite sure how to understand your request for novelty. The world is the totality of what there is; part of what there is is known and part of it is (and may remain) unknown. If the request for novelty were the request for a kind of openness, I would agree. The world is *transformed* by human action (for better or for worse) and not just by human action, so new things are brought into existence and other cease to exist. Truth-makers come and go. There has been a traditional worry about the independence of the world: how can it be interfered with (known, manipulated etc.) if it is independent of the subject? To this worry I juxtapose another one: what worth would the interference have if the world was not independent of us?

FG: But can there be novelty independently from human interference? I suppose I'm asking if in your view it makes any sense to claim that there is some incompleteness at the ontological, mind-independent level. This is probably what you would call speculative metaphysics, but is there any necessary principle regulating the actual totality of all that is the case (the known and the unknown) to remain the same, or modally constraining the possible to a limited set of configurations? I am hinting here towards worldviews that admit (or require) some form of ontological contingency. You mentioned Émile Boutroux, who, in his *The Contingency of the Laws of Nature*,³⁵ defended the thesis that modern science, in its reliance on fixed laws of nature, offers only a partial understanding of the universe, that limited part where stability reigns, since the latter is really governed by a "principle of creation" and "permeated by contingency."³⁶ Similar arguments were offered by other philosophers and scientists after him, including at least C.S. Pierce, A.N. Whitehead, and J.A. Wheeler. These theses are often (certainly in Boutroux's case) motivated by theological/spiritualist leanings, and yet today it is not uncommon, in "continental" circles, to see "contingency" defended as a secular notion, indeed a radicalization of "Hume's problem" which rejects any metaphysical necessity, ontological unity and universal laws (but I suppose we could at least tangentially include someone like Nancy Cartwright's work in this trend).³⁷ Is this idea of contingency and ontological novelty something that, from your own Humean perspective, cannot be excluded or does it undermine the reliability of scientific knowledge in a way that forces us to discard it? To phrase it differently, how far is your Humean "regularities all the way down" from Bas Van Fraassen's claims that "[t]here are no necessary connections in nature, no laws of nature, no real natural bounds on possibility" and

³⁵ Émile Boutroux, *The Contingency of the Laws of Nature* (Chicago and London: Open Court, 1920)

³⁶ *Ibid.*, 160.

³⁷ See Nancy Cartwright, *How the Laws of Physics Lie* (Oxford: Clarendon Press, 1983) and Nancy Cartwright, *The Dappled World* (Cambridge: Cambridge University Press, 1999).

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that “[r]eally, nothing is necessary, and everything is possible”?³⁸
Can one be a realist rejecting any natural necessity?

SP: I agree that contingency should be defended as a secular notion and part of the reason for my adherence to it is that I believe there is no external (super-natural) law-maker and law-giver. But this does not mean there are no laws! I defend the view that laws are a species of regularity and I deny the claim that there are regularity-enforcers of a distinct metaphysical kind. I have commented on this issue in a previous answer, so what I want to add here is that denying the existence of enforcers (metaphysical entailment; universals; powers) does not imply that anything goes! It does not imply that there are no non-trivial actual relations between the regularities there are in the world; that there are no actual objective relations of similarity and difference in the objects in the world. Denying the existence of enforcers implies that these relations are not metaphysically necessary; they do not have a grounding in metaphysically distinct layers of facts. Those philosophers who think that laws are contingent necessitating relations among universals (David Armstrong, Michael Tooley and Fred Dretske) are right in claiming contingency, but I think we do not have a clue as to what exactly this necessitating relation is; hence it is an extra burden in our attempt to understand the presence of regularity in nature. But the price of metaphysical necessity is even heavier, especially if it's taken together with the currently very popular dispositional essentialism (and power realism). On this view, it is not clear any more whether there are laws! They are either summaries of the potencies of related powers or nothing at all. Worst, there can be all the power in the world and nothing happening in it. I have recently tried to do some work on the notion of pattern, in order to explain the presence of regularity and to differentiate laws from accidentally true generalisations.

³⁸ Bas Van Fraassen in Jan Hilgevoord, *Physics and Our View of the World* (Cambridge: Cambridge University Press, 1994), 124.

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My views have not matured yet (I must say it is really hard to do any serious philosophical thinking under the present situation in Greece). But in broad outline the idea is that a pattern is a repeatable and recurring network of differences and similarities among entities and that those regularities are laws that are characterised by the unity of a (natural) pattern. Patterns seem to have the following advantages: a) they can be characterised in terms of their naturalness; b) they may occur within other patterns; hence they may form networks and c) there need not be a pattern-enforcer (of distinct metaphysical type). I do hope that when this work matures it will show how there are non-trivial actual relations between the regularities there are in the world and hence that the contingency of the laws of nature is far from being a threat to the objectivity of scientific knowledge. This commitment to necessity in nature is, to paraphrase Elizabeth Anscombe, the dogmatic slumbers of the day.

FG: Whatever the details and the arguments employed to defend one's position, what is at stake in being a realist? Both positivists and postmodernists coated their (differently motivated) rejection of realism with ethical concerns. As we've seen, Bas Van Fraassen—arguably the most prominent critic of scientific realism today—still argues along these lines when he claims that metaphysical realists are deluding themselves, guided by a naïve metaphysical reassurance given by “deep” explanations going beyond the phenomenal surface, and presents his own “empirical stance” as the only intellectually responsible, truly “disenchanted” one to assume.³⁹ On the other hand, a number of philosophers have defended realism precisely against the moral dangers of a reduction of reality to opinion (be it individual or collective, be it about scientific entities or political events)—Richard Boyd, for example was equally engaged in the defence of both scientific and moral realism, while Christopher Norris has attacked the postmodern suspension of belief in reality in the wake of very real events like the first Gulf War. Today, think-

³⁹ See *Ibid.*, and Bas Van Fraassen, *The Empirical Stance* (New Haven and London: Yale University Press, 2002).

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ers within the continental tradition are mobilizing continental sources (from Hegel to Lacan, from Deleuze to Derrida) to build more or less direct bridges between a renewed materialism and leftist emancipatory politics (Slavoj Žižek probably being the most prominent figure). Are there ethico-political grounds on which you embrace and defend your realist stance?

SP: This takes us back to the first question. Of course there are ethico-political grounds for realism (at least the kind of realism I want to defend). To be a realist, in my book, is to occupy a certain standpoint according to which there are objective criteria of rightness and wrongness and external facts-of-matter as to what is right to believe and what not. This, to be sure, is an external constraint on our belief systems and in very many typical cases, we might not be able to say or warrantably assert that we know these external facts-of-the-matter or the grounds of objectivity in judgement. This predicament—the human predicament—does not invalidate the role (sometimes, the regulative role) of this standpoint. The realist standpoint and its commitment to objectivity need not (and should not) be confused with a claim that there is a royal road to truth and that some already possess it. Well, science is the best road we have invented so far and we should be quite confident that it tends to lead to truths (though not to the whole truth and nothing but the truth)! But even there, truth emerges from theoretical pluralism, failed theories and defeasible methods. In my mind, the realist standpoint makes possible the battle against relativism. Relativism should not be confused with pluralism and open-mindedness. It is as ugly as its opposite: authoritarianism. It's hard to see how relativism can be avoided without having external standards of objectivity and rightness. It's even harder to think how one can oppose oppression and war and injustice without taking an anti-relativist stance. I cannot go into this now, but a robust realist stance in ethics and society (one that takes it that an underlying social reality grounds social appearances and that ethical conduct has an objective—though not necessarily abstract and ideal—ground) can help human emancipation.

The realist standpoint need not be associated with the impossible view from nowhere. Representation is always perspectival, but the represented is not. In fact, it can emerge as the invariant element in various representations. Nor should we confuse the lack of certainty in knowledge with the lack of objectivity of our knowledge of the world. Objectivity without certainty is possible!

FG: That's a slogan to keep in mind! So, to conclude, I would like to ask you about the repercussions on academia of the current economic situation in Greece. Are you encountering problems when it comes to funding for students or for the organization of academic events? Is the country losing a generation of scholars, emigrating elsewhere in the hope of finding more promising prospects for an employment? Have you or your colleagues considered moving abroad after the radical cuts to the salary of academic staff?

SP: Greece is in a terrible mess currently and will be like this for quite a while. The causes of the crisis is a matter of dispute (there is, broadly, a right-wing and a left-wing account of them), but the working people of Greece—who have heavily suffered from the unprecedented wave of austerity—are not among the causes. We are living through the dismantling of welfare state in Greece—a state that was built slowly but steadily (and not without deep structural problems and deficiencies) after the collapse of the military junta in 1974 and especially in the early 1980s. Deep and persistent recession; rising unemployment (dangerously high among the youth); more than 30% reduction of the annual income of civil servants and most other employees (including the University teachers); slashing of all pensions and benefits; high prices and mounting inflation; one capital tax piling upon another; disappearance of state investments; slashing of the budgets of hospitals, schools, universities, the police... This is Greece nowadays. And on top of it, there is a growing recognition of the obvious: that the recipe prescribed by the IMF (taken from its outdated rulebook) for getting Greece back on track was simply disastrous; a non-starter. After two

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years of ruthless policies that were supposed to take Greece out of the zone of bankruptcy (predicated on the thought that the welfare state is too costly to maintain and that an internal devaluation of “human capital” would make Greece competitive), Greece is still on the brink of default—things have gone worse; almost out of control. But the fiscal deficit (and the crazy policy of diminishing it whilst economy is in massive contraction) is the tip of the iceberg; the social deficit that the relentless austerity has created is far more serious and dangerous. There is an increasing number of dispossessed and disaffected especially in the big cities; there are families with no parent in employment; there is a rise in crime and violence; even in the number of suicides. Poverty and desolation are visible in the streets and the neighbourhoods of Athens. A whole generation will be lost. There is a democratic deficit too, which puts the role of the democratic institutions at stake; but this is a different (and ugly) story.

And as if all this was not enough, the previous government decided to reform the universities, passing a bill which will render them less democratic and more authoritarian institutions. The new ideology of “excellence” is implemented from above and in an environment in which austerity and cutbacks have almost brought the universities to their knees. Disintegrating infrastructure is left to its own devices. Research funds have become scarce. Research grants that have been awarded after a national competition (one of them to my group; the only one in philosophy, I must say) have been frozen and are drowning in a wave of delays and redtape. More than 800 junior members of staff that have been elected in university positions were on the waiting list to be officially appointed; 300 of them were appointed recently after an almost three-year wait, but the prospects for the remaining 500 are not good. An increasing number of students have to look for some kind of part-time job to support themselves. The really sad thing is that the Greek universities are on the brink of stagnation—Greece’s intellectual capital will be wasted. It’s not uncommon that academics or PhDs look for employment abroad; the prospects of intellectual flourishing

in Greece are dim.

My colleagues and I took pride in that, in precisely this atmosphere, we successfully organized in October 2011 in Athens the third conference of the European Philosophy of Science Association. It was an act of intellectual defiance and we were deeply moved by the determination of philosophers of science to come to Athens for the conference, despite the fact that a strike of the air-traffic controllers hit Greece on the first day of the conference. My feeling—call me a pessimist—is that Greece won't make it in the end; really dark days lie ahead of us. The Greek academics (and philosophers in particular) who have contributed to the advancement and the rising international standing of the Greek universities have an intellectual obligation to resist all this; to make values prominent and to show that human beings and their prosperity are above profits.

FG: Thanks a lot for your time, I believe that we covered quite a lot of material and readers of the journal will surely enjoy our conversation. As a parting gift, could you just whet our appetite with a quick description of the book you are working on at the moment? I believe you are preparing something on empiricism, trying to rediscover a certain line of realism-friendly thinkers from within the Logical Positivist movement—is that correct?

SP: Yes, I want to reclaim a tradition within empiricism which took it that the critique of metaphysics should leave intact the world as this is described by science; a world populated by atoms, and fields and DNA molecules, but also by natural kinds and social classes. If time and energy permit, I want to write a book about the history of the philosophy of science in the twentieth century focusing on the transition from views that allowed a priori principles to play a role in the constitution of the object of scientific knowledge to more naturalistic views.