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§1 EDITORIAL

I recently attended the bi-annual conference of the Society for Philosophy of Technology at the University of North Texas. One thing that strikes me about philosophy of technology is, more than the variety of approaches it encompasses, the constant reflexion on its own subject matter (i.e., on what technology is and consequently on what philosophy of technology is), on its actors (i.e., philosophers, sociologists, technologists, ...), and on the arenas in which it operates (i.e., academia, technology labs, public opinion, ...).

Going back home—home being both Europe and philosophy of science—I thought that this activity of reflecting on who we are and what we do needs to be encouraged. I therefore try to take a step in this direction each time I have the chance to do an interview. This time I sent my questions to Stathis Psillos, a remarkable philosopher of science based in Athens. You will see how Stathis sees his own research within the philosophical panorama; you will also hear his views about academia in the middle of economic crisis and in the middle of a process that tends to commercialise everying, including philosophy.

I am coming to believe that reflecting on who we are and what we do—namely, on what kind of academic we want to be—is at least as important as publishing good pieces of research in good venues (be they journals, volumes, or *The Reasoner*). The reason, I think, is quite simple (in theory but not in practice, alas). We cannot make an impact without an idea of what that impact is and of what the impact's target is.

I hope you will find Stathis' words inspiring and thoughtful. I did.

FEDERICA RUSSO
Philosophy, Kent

§2

Features

Interview with Stathis Psillos

Stathis Psillos is Professor of Philosophy of Science and Metaphysics at the University of Athens. He is amongst the founders of EPSA—the European Philosophy of Science Association—and a leading philopher of science.

Federica Russo: Thanks for accepting to be this month's interviewee. You are certainly well-known to philosophers of science, but *The Reasoner* has a much wider audience. Would you like to briefly present your research profile to our readers?

Stathis Psillos: Thank you for your kind invitation. I am a philosopher of science, practising what is nowadays called General philosophy of science. The way I see it, this characterisation is meant to make a statement; to take a stand: there is this thing called *science-in-general* and there are important philosophical issues that crop up in our attempt to understand this general cognitive activity that transcends the bounds—and I would say underpin—the various individual sciences. General philosophy of science is defined by an intellectual tradition which aimed to develop a coherent philosophical view of science, *qua* a part of culture, with distinctive epistemic features and relation to reality. It operates within the broad two-dimensional framework that ancient Greek philosophy—and in particular, Aristotle—bequeathed to posterity; a framework whose contours are shaped by the epistemology of science and the metaphysics of science. Hence, I take it that the constitutive quests of General philosophy of science are the features and methods that make scientific knowledge distinctive and the deeper structure of reality required or suggested by a coherent (and perhaps unified) scientific image of the world. It's difficult to locate yourself within this framework without knowing some individual science, but the challenge is precisely to try to have a philosophical view about science-in-general.

For various reasons that have to do with my own intellectual development, I entered philosophy of science with a philosophical agenda: to defend a realist conception of science. This kind of endeavour has taken up most of my research time. Its highest point so far (and perhaps for good) is my book *Scientific Realism: How Science Tracks Truth*, which appeared in 1999. In it, my main aim was to develop the explanationist defence of realism—roughly the line that a) the reasons that entitle scientists to take some of their theories as (approximately) true are explanatory; and b) that the very claim that some theories are (approximately) true is itself the best explanation of the various empirical and predictive successes that these theories enjoy. Issues such as these led me to try to tackle head-on the famous historical challenge to scientific realism, aka pessimistic induction. My study of the history of science was not with the eyes of a professional historian of science, but I still think that it is *philosophical perspectives* that determine the norms of relevance in the use of the history of science within philosophy of science. In this sense, I am a Duhemian: I take history to play an important role within philosophy of science in warning off both dogmatism and scepticism. My subsequent work on scientific realism (some of which is collected in my *Knowing the Structure of Nature: Essays on Realism and Explanation*, 2009) was meant to tackle three issues that I thought were very important for a coherent realist approach to science: metaphysics, truth and mathematics. The latter is a topic I have been thinking about recently. I am trying to defend an anti-nominalist version of scientific realism, mostly because I think that nominalism is an impoverished approach to science and reality. On the role of truth in scientific realism I am still wavering. I have defended the correspondence theory of truth and have argued that scientific realism is not properly defensible if truth is taken to be broadly epistemic. But I have not yet come up with a settled view on the issue of whether a thin—deflationary—conception of truth is enough for scientific realism. The research issue that has preoccupied me considerably in the last ten years is the metaphysics of scientific realism. Here I go against the realist tide and adopt a broadly Humean conception of reality, which denies necessary connections and regularity-enforcers (such as powers) and takes it that laws of nature are those regularities that play an essential role in a unified theoretical scheme of the world (a version of the Mill-Ramsey-Lewis view). I call my approach Scientific realism with a Humean face'. I am not yet entirely sure it is fully coherent, but this is an issue that I will have to face sooner or later.

FR: You started your career in the UK (MSc, PhD, and a postdoctoral position) and then you moved back to Greece. What brought you to the UK in the first place? And what brought you back to Greece afterwards? The UK and Greece must be very different working (and social) environments, I believe. What do you think Greek academics should learn from the Brits, and vice-versa?

SP: Back in 1989, there were no Master Programmes in Greece and when it came to the philosophy of science, there was a then very young PhD programme in the National Technical University of Athens run by a visionary group of scientists who had a sustained interest in philosophy of science. I was associated with this

group but I was lucky enough to get a state scholarship to study contemporary philosophy abroad. The UK—and London in particular—was an obvious choice back then. I was offered a place by the then Dept of History and Philosophy of Science at King's College London and attended the University of London MSc in History and Philosophy of Science and Mathematics (run jointly by various London Colleges). The London philosophy of science community was very much in transition back then. When David Papineau joined the King's HPS dept in 1990, he admitted me as his PhD student and this was a great window of opportunity for me. David's eye for the broader philosophical issues was a revelation to me. At the same time, I had had the opportunity to live through the transition of the LSE from the Lakatosian tradition (which I still value) to the post-Lakatosian one. John Worrall had just published his seminal paper on structural realism and this gave me a lot of food for thought. David and John created a dipole which very much shaped the way I do philosophy of science. But a key influence for me was the work of Richard Boyd and his insistence that the defense of realism (and of the objectivity of science) has had a political dimension as well: human emancipation.

I spent almost 9 years in the extremely stimulating and innovative London intellectual environment; at King's for my dissertation on scientific realism and at the LSE with a British Academy Postdoctoral fellowship. If I have managed to achieve anything in my intellectual life, it is mostly due to these years in London. But in the end, I was never good enough to be offered a job in London (or the UK for that matter) and when I was thinking about my future after the BA fellowship, I was approached by the then newly established dept of Philosophy and History of Science in the University of Athens with an offer to join it. Personal circumstances back then, including the fact that I had still to do my military service (I was a draft-dodger for three years, unable to visit my family in Greece), led me to accept this offer and go back home. This is not something I regret (at least not most of the time).

There is no doubt that there was (and still is) a huge difference between academic departments in Greece and the UK. When, as a junior member of staff, I asked a secretary of the dept in Athens to prepare a flyer for a talk that Wes Salmon would give to the dept, she forcefully explained to me that this was not her job. When, later on, I was trying to persuade a senior Library officer of the University of Athens that we need to subscribe to the JStor, he was looking at me with amazement. When I have academic visitors from abroad, I still have to do all the arrangements for their hospitality. But one can look (back) at all this with a smile. The serious difference is in the research culture. I am focusing on the humanities and especially on philosophy. Things have changed in the research culture in philosophy in Greece, but with a slower pace than I had hoped. There is a younger generation of philosophers—colleagues with solid philosophical training mostly, but not exclusively in the UK and a generation of home-grown PhDs—who take seriously the issue of publication in refereed international journals. But there is still a lot of resistance to the idea that the practice and appraisal of philosophy in Greece should be governed by the same standards and criteria as in virtually the rest of the world. A case has still to be made for the point that publishing in established journals is (among other obvious things) an antidote to nepotism and favouritism.

Given this, it might sound ironic that I also think that an advantage of the Greek academic environment is that you do not perish (even) if you do not publish! There is more tolerance and less pressure to 'produce'. This means that there is more time to let ideas mature. The recent 'impact-factor' onslaught in the UK is, to my mind, the logical conclusion of not building enough resistance as a community earlier on towards the uncontrolled entrance of the market forces and market standards in academia. So although we still have a lot of hard work to do in Greece to raise the standards of academic research in philosophy, this (hopefully) can be done in a way that resists treating philosophy as yet another product on the supermarket shelves whose value is governed by the law of supply and demand. The balance is delicate; there might not be, in the end, enough momentum to change for good the prevailing research culture in philosophy in Greece. But in the endeavour to resist the commercialisation of philosophy, we are in the same boat with all or most other professional philosophers in the world—at least I hope so.

FR: I am interested in learning about the academic situation in various countries, so this is a question that I often ask to my interviewees. What is being an academic in Greece these days? How did the economic crisis affect Greek academia?

SP: These are extraordinary days! The western world has started to come out of a very deep economic crisis, which will leave big scars on social institutions, the universities included. Greece is in a terrible mess and no end of the crisis is in sight. The story is complex and interesting, but my own view—or the bottom line of it—is that in Greece we live through a massive attack on the welfare state as this was built and developed after the collapse of the military junta in 1974. The standards of living of the majority of the population—which, admittedly, rose over the last two decades but mostly due to really hard work—are being squeezed; unemployment is rising beyond control (especially among the youth) and at the same time (despite, or because of, the crazy austerity programmes) the economy has gone into a deep depression. There will be philosophical lessons to be drawn from what has now been happening in Greece, I am sure. The universities suffer no less. The bud-

get has been slashed to the extent that there is a serious chance that there won't be enough money to see the year through; there are about 800 young academics (and some talented philosophers among them) that have been elected to junior university posts but are not being appointed by the state; there is a lot of to-ing and fro-ing concerning the promotion and the tenure cases of many university teachers; there will be huge reductions to the temporary staff that the universities employ to do teaching; the government is about to impose a massive reform of the structure of higher education, which might lead to mergers of universities and the closing down of departments as well as to the appointment of unelected governors to run the universities; most of the research funds (including EU funded projects) are frozen. There is a real danger that the Greek universities will be devalued and that a whole academic generation—and one with better education and research profile as a rule—will be lost for good. This is the setting (not to mention the cutbacks of about 15% of our annual salary with more to come) within which we are invited to do our academic job, to 'intensify' our research output and to create centres of excellence. Apart from any political action anyone sees fit, I believe that 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 fight against this assault, by example and intellectual mobilization.

FR: You have been the first president of the European Philosophy of Science Association. How did you like it? Do you think there is a 'European' way of doing philosophy of science, as opposed to . . . ?

SP: The creation of the EPSA was a lot of hard work and would not have happened if it were not for the vision and contribution of Bengt Hansson, Stephan Hartmann, Mauro Dorato and Mauricio Suárez. I was deeply honoured to have been its first president. It was an experience that I enjoyed quite a lot, though it was not always plain sailing. Still, the EPSA has now an official journal which has already produced the first two issues and an important biennial international conference. As you know, the third EPSA conference will be hosted by the University of Athens from the 5th to the 8th of October 2011. The response to the call for papers was great—almost 400 contributions, of which about 180 have been accepted for presentation in the conference. I wish we had space to accommodate more parallel sessions, since I am sure a lot of good papers and symposia proposals had to be rejected in the end. EPSA is now firmly on the philosophical map. It is there to build bridges and to promote collaboration and exchange of ideas among philosophers of science in Europe and the rest of the world. In a sense, EPSA is yet another professional philosophical association, where the adjective "European" marks the place of its headquarters and the location of the conference. I'd like to think however that there is a legitimate task to look for a European perspective in doing philosophy of science. This is *not* to imply an opposition to supposedly non-European perspectives. Rather, I take it to bring into focus the need to revive, refresh and perhaps integrate the various traditions within philosophy of science in Europe: the currently dominant analytic tradition, with the perspective of historical epistemology, the various formal approaches to conceptual analysis that were mostly developed in the northern and eastern Europe, the various tendencies to see science as a social and perhaps political phenomenon. Working in a country that is considered to be in the 'research periphery' of Europe, I take it that EPSA should create a space for the advancement of capacities and the building of intellectual abilities of all those philosophers of science who—for reasons that we need to discuss at some point or other—do not have the opportunity to be and work in elite institutions and places. There is plenty of quality in European philosophy of science—what is sometimes lacked is opportunity.

FR: In philosophy of science, you made a notable contribution to the debates on laws, explanation, causation, and, recently, mechanisms too. Do you consider your achievements just 'conceptual' or are there consequences for scientific practice?

SP: I am not sure how philosophy of science can have consequences for scientific practice. There might be unintended consequences, but the idea that philosophy of science should aim or try to advise scientists how to practise science does not appeal to me. I study science philosophically because I value science and try to understand its epistemological and metaphysical presuppositions/implications. But I do it because I want to have a coherent philosophical view about science *simpliciter* and not because I hope that this view has consequences for scientific practice. There is no philosophically neutral approach to science. There is no philosophically detached description of 'the' scientific practice. Even when scientists themselves describe their practice, they do it from a philosophical point of view (occasionally spontaneous and perhaps confused or even incoherent). However, I do not believe that philosophy of science—at least the way I would like to practice it—is just 'conceptual' analysis. I do not look for definitions. Concepts are immersed in practices (which, occasionally, are predicated on a network of concepts) and in history; concepts *have* a history and a repertoire of applications which constrain the way we philosophers ought to think about them.

The recent mechanistic revival in philosophy of science is a good example. I am a critic of this revival not because I think that the concept of mechanism has no content. Rather, the opposite is the case: it has too rich a content to be taken for granted *philosophically*.

It is true that scientists—especially in biology, cognitive sciences etc.—look for and identify mechanisms. What then is the philosophical task vis-à-vis this practice? I say: use philosophical tools to study it. I take it there are two major tendencies currently. The one that I do not prefer amounts to a sophisticated commentary on the practice of looking for and identifying mechanisms. The one I am friendlier with aims at explicating the metaphysical and epistemological role that mechanisms are supposed to play in the scientists' give-and-take with the world. I start my own critique of mechanism (in the paper you accepted in the recent OUP volume on causation that you edited with Jon [Williamson] and Phyllis [Illari]) by noting that there are at least two general ideas of mechanism and that neither of them (but for different reasons) can lead us to draw interesting metaphysical conclusions from the recent mechanistic craze. Where do I get these two concepts? From history, of course. But reconstructed history—hence, what I call 'conceptual history'. The mechanistic conception of mechanism (associated with the mechanical philosophy and a conception of mechanics as the foundation of science) is kind of too easily satisfiable to be useful (this is what I call Poincaré's problem). The other conception—a non-mechanical conception of mechanism—associates mechanisms with the task of explaining the behaviour of a whole in virtue of the behaviour of the parts plus structural constraints. But this conception requires a prior understanding/identification of the whole and its function and this implies that anything that can be this whole and perform this function is an appropriate mechanism (this is what I call Hegel's problem).

The case of laws of nature is different. Here we have a rich philosophical concept which is at the very core of the metaphysics of nature. It turns out that it is extremely difficult, if possible at all, to have a coherent view about laws of nature without thinking of the relevant concept as part of a network of concepts such as causation and counterfactuals. Hence, the problem of laws of nature is a distinctively philosophical problem and it remains interesting and important even if it makes no difference to scientific practice. In this case, it is like having one equation with three unknowns and there is no way forward but to try to create the two missing equations; that is, to rely on intuitions and established usage (history and practice!) so that we get at least a partial hold on the concepts involved. I take a broadly Millian approach to laws, but this means that I need a story about counterfactuals, and all I can offer at the moment is based on (arguably vague) intuitions about modal force.

Causation makes things more complicated, because I think that if we take the history, usage and overall role of the concept seriously, we are entitled to draw the conclusion that perhaps it is an accident that the very same concept is supposed to cover the cases in which we think there is a productive relation between cause and effect and the cases in which we think there is a relation a robust dependence of the effect on the cause. Hence, causal pluralism seems quite appropriate. To cut a long story short, I think nothing of what I have ever said will be useful to a practising scientist—except by accident. But this does not mean that it is of no value to a practising scientist. I view philosophy as the laboratory of theoretical abstraction: philosophy supplies the abstract form and history, science, practise (in other words, the activities that engage the world directly) provide the matter. Philosophical abstraction without (scientific, historical, practical) concretisation is empty; (scientific, historical, practical) concretisation without philosophical abstraction is blind.

FR: In the light of the big changes that the academic world is facing, what do you think is the (new) role of philosophers? Is there an 'impact' we can really make on society? If so, what does it amount to?

SP: I went into philosophy because I wanted to change the world and I did not know how (else) to do it. After twenty five or so years of philosophical endeavours, I still do not know how to change the world. But I do know a lot more! I know that philosophy is an enterprise conducive to human intellectual and moral flourishing. It sets free human reason and puts it to the service of truth (and virtue). It is this conception of philosophy that I identify with. Philosophy is not about solving problems—though problem-solving is very welcome. Philosophy is about freeing the human mind from ideological fetters; it is about having a view about how it is best to go about having a view of the world; it is the cement that holds together (and makes possible in the first place) a solid and coherent image of the world. And that's why philosophy and science are intimately connected, even though they are independent enterprises.

Philosophy does not always make itself visible; it goes unnoticed in the 'large scheme of things'. But its broader significance is made evident in periods of crisis, or major conceptual shifts (like the one in the beginning of the twentieth century) where the (scientific and social) image of the world has to be re-built. This is not something decision-makers, who are keen to save money, improve resource-management and protect the interests of the 'stakeholders', are able to see. This, ultimately emancipatory, function of philosophy has rendered it an invaluable part of an intellectual heritage that swept the centuries and shaped the ways universities have worked and flourished. Perhaps, this conception of the role of philosophy and, by implication, of the humanities in our intellectual endeavours, is a happy accident that we (collectively) owe to our Greek forebears. Perhaps, if the university education was designed (or invented) from scratch fifty or twenty years ago by the

contemporary politicians, their advisors and their funding bodies, philosophy would be no part of what is required for a balanced and rounded education. In the super-market conception of the universities that tends to preoccupy the minds of the so-called 'economic rationalists', there would be no need for philosophy to be on the shelves for sale; else, it would be simply on offer in a discounted price in the past-their-best-before-date products.

The fact is, however, that philosophy is a precious part of our intellectual culture and we should try to preserve it and cultivate its fruits. But we should see it and defend it as such: as a part of our intellectual culture; of our collective heritage and self-esteem; of the tremendous achievements of human reason; of our collective insurance against authoritarianism and conceptual vacua. If philosophy goes into the supermarket, it will become obsolete. But it won't go into the supermarket, if the argument is won—and this has to be a philosophical argument—that not everything has an exchange value; not everything is a commodity.

There is a tendency, in various countries including mine, to view the role of philosophers as public intellectuals. This attitude is fostered by some philosophers too! They couldn't be more wrong! The opinion of a philosopher about X—where X is something he/she is not an expert about—is of no more value than the opinion of anyone else about X. A public intellectual with an opinion about everything is nothing more than a well-informed journalist—there is nothing wrong with this, except that it is not philosophy! But there is the other extreme too: the full professional detachment, where philosophers do not volunteer their view on X unless they are asked—and they are rarely asked! I am a modest interventionist and I think philosophy should be more present in the public domain without being corrupted by the limelight and the marketplace. This is not to encourage loose thinking. Rather, it is to try to set some higher intellectual standards in the public sphere.

There are important areas in science policy, risk analysis, the public understanding of science, the role of religion in education and in society, the management of disagreement and consensus formation, as well as ethical and political issues in science and the science education in general on which philosophers of science can have an important impact. But it should never be forgotten that whatever impact philosophy of science has on these and other issues, it is the product of the fact that philosophy of science (and philosophy in general) was left alone by society (and we thank society for this!) to develop its own themes, approaches and strategies. The intellectual and institutional autonomy of philosophy is a prerequisite for its playing—via a slow and occasionally unnoticed process of maturation—a broader beneficial role in society. It might sound cheeky to appeal to the etymology of "philosophy": the love of wisdom. Wisdom, if it is ever achieved, requires and takes time. But we all know from experience that once it is available, it makes a difference!

Truth and Success: Reply to Held

In a recent paper (Truth Does Not Explain Predictive Success, *Analysis* 71, 232-234), Carsten Held attempts to offer a new take on the traditional issue of scientific realism versus antirealism.

Held begins by reminding us of the (allegedly) high number of predictively successful theories that were at some point in the history of science abandoned and replaced by other theories, and are therefore regarded as false now. This pessimistic induction threatens scientific realism, as it seems to sever the link between predictive success and truth [from now on, the qualifiers 'predictively', 'predictive' and 'scientific' will be dropped]. Next, Held indicates the most common realist reaction to this: to insist that all past successful theories got at least *something* right about the world, so being at least *partly true* in spite of their *overall falsity* (this can be understood here as the claim that at least some, but not all, the statements contained in those theories described reality as it objectively is—no need to deal with the thorny issues surrounding truthlikeness). This, Held thinks, means that

> the debate on scientific realism hinges on whether there really exists an entirely false theory [i.e., a theory such that none of its statements describes reality as it objectively is] making true predictions (232).

On this basis, to avoid dealing with empirical questions, Held suggests considering the mere *possibility* of success in spite of full-blown falsity. Thus, he asks us to consider a hypothetical theory that is successful. He first points out that the inference from success to truth is non-deductive. This means that there is a logically possible world where the theory under consideration is entirely false. Since any further constraints one may postulate cannot in any case make it necessary that a successful theory is at least partly true in any given world, Held continues, the foregoing means that *any successful theory could be entirely false in the actual world*. Held concludes that realism is undermined, as for any successful theory there is nothing that grounds the belief that it is (at least partly) true.

Is Held's reasoning compelling? I think not.

First of all, Held makes *two* claims. He states (a) that (for scientific theories) 'truth does not explain predictive success' (the title of his paper); and (b) that "an explanation of any scientific theory's predictive success must be compatible with the assumption that this theory is false" (234). But (a) and (b) are only equivalent if the former is understood as the claim that a theory is