

## Interview with Stathis Psillos

João Pinheiro

(Centro de Filosofia das Ciências da Universidade de Lisboa)

[joaopinheiro@hotmail.com](mailto:joaopinheiro@hotmail.com)

Stathis Psillos is professor of Philosophy of Science and Metaphysics at the University of Athens, former editor of the Journal Metascience, and one of the foremost proponents of scientific realism. Professor Psillos was a visitor at the Centre for Philosophy of Sciences of the University of Lisbon between the 17<sup>th</sup> and the 19<sup>th</sup> of June, 2015. On the first day of his visit he participated in the International Workshop “Descartes and Newton on Method” (organized by María de Paz) with a lecture entitled “Hypotheses in the two *Principia*”. On the second day he gave a lecture entitled “Why believe in Atoms? Jean Perrin’s argument for realism”. Finally, on the third day of his visit he participated in yet another International Workshop on the subject of “Henri Poincaré’s Philosophy: Conventions and Structural Realism” (also organized by María de Paz), with a lecture with the title “Conventions and Relations in Poincaré’s Philosophy of Science”. The following text is the transcription of an interview recorded at the Faculty of Sciences of the University of Lisbon after his lecture on the 18<sup>th</sup>.

**João Pinheiro:** I read that you started your higher education with a degree in Physics and later on you did a PhD thesis on the subject of scientific realism. What got you into this theme in the first place and why should we be minimally driven to pursue scientific realism?

**Stathis Psillos:** I studied Physics because, although I wanted to study Philosophy for quite a long time in my life, it was very hard to do

Philosophy in Greece in a way that respects Science. Philosophy in Greece was and still is mainly and to a large extent disconnected from Science. My study of Physics led me to pursue philosophical questions like “What entitles us to believe in the scientific image of the world?”, “Why should we take Science seriously as an enterprise that tells us what the world is like?”. These are basically the questions of Scientific Realism. Once I started thinking about these questions it was inevitable for me to go into Scientific Realism. I think the Scientific Realism debate is an extremely interesting philosophical project and I hope I’ve contributed somewhat to understanding what the stakes are. Science is so important to our lives and to our various endeavours as humans that understanding (and defending) its claim to truth is very, very important for Philosophy of Science.

**JP:** Despite partially opposing Structural Realism, you often talk about the structure of nature and even wrote a book entitled *Knowing The Structure of Nature*. What is more, you wrote in *How Science Tracks Truth*, somewhat enigmatically if I may add, that the nature of an entity forms a continuum with its structure. It seems to me such conception makes it hard for us to distinguish what is structure from what is not, which is a usual accusation against structuralists. Are we all discussing different definitions of structure?

**SP:** The title of the book was not meant to say that only structure is known. I think what makes structural realism a distinctive realist position is that it puts a limit on what can be known by claiming that only structure can be known. Now, what is structure? Initially, [John] Worrall and other people thought of structure as opposed to nature and I think there is no such serious distinction there, between what an entity is and what its structure is. But you might think of the structure as the mathematical structure of the theory that represents the World. Again, I think there is no watertight distinction between what an entity is and what role it plays within a structure. So, I’m against Structural Realism when it claims that only structure can be known, that there is a limit to what we can know and that this is the structure of an entity, or the structure of a theory, or the structure of the World. I think this distinction makes no difference if we do not have a clear conception of

what it is for something to be “non-structural” (which is what is not, and cannot be, known according to Structural Realists).

**JP:** You wrote, and I quote, that “if our best science is not our best guide to our ontological commitments, then nothing is”. And why is that? Why not religion, for instance? Is it due to the lack of method and if so, why is methodology so important to ontology?

**SP:** Science is the best means invented by us humans to understand and transform the World. If Science cannot reveal to us something, it does not necessarily mean it does not exist. But it is hard for me to think of how else we can come to know that something exists if not by means of Science and its methods. In any case, whatever is true about the World should be consistent with what Science says about the World; it should not be in conflict with what Science tells us there is. So I think Science is the ultimate arbiter of what there is in the World. Metaphysics is a key philosophical enterprise trying to complete the scientific image of the World by theorising about the deep structure of the World, or more specifically about the ontic categories, or the ontic frameworks through which we understand the World. Science and metaphysics should not be in conflict, but in collaboration. It’s not about verification: I don’t think something has to be empirically verified or experienced for it to be true, but I do think Science constrains our conceptions of the World.

**JP:** Brian Ellis reviewed your *How Science Tracks Truth* saying that it does not reveal much about your actual ontological commitments... independently of his critique, I believe we could extend his observation to most if not all of your work: you have not yet made any straightforward and non-negative defence of a specific ontology, but that is actually in accordance with what you say in your latest book, *Knowing the Structure of Nature*, where you do a demarcation of scientific realism from naturalistic ontology. Are they that much independent issues? I find it hard to disassociate one from the other, for how can one agree that a given theory is approximately true of the world and simultaneously reject the ontology the theory seems to imply?

**SP:** That is where the issue lies: what scientific realism has to say about ontology. When we talk about ontology we shouldn't (simply) mean electrons, protons, quarks, atoms or whatever. I understand 'Ontology' in a more strict and rigorous way. It is about the basic categorical framework—the network of categories—by means of which we understand the World. So, ontology is about "what kind of things there are", whether there are properties, whether properties are universals, whether causation is something other than necessary connections in nature, whether natural kinds are individuated by their essences, etc. When I wrote my book on Scientific Realism I believed – and I still believe now – that scientific realism *per se* should be independent of ontological issues such as the above. For you can be a scientific realist even if you are a regularist about causation or if you adopt a Ramsey–Mill–Lewis conception of laws of nature. So I take it that the metaphysics of Scientific Realism is a position about the independence of the World. Science is a discovery game: we discover what the World is like. And the World is largely independent of human minds, epistemic practices, conceptualisations; it is not constructed by us, etc., etc. This conception of independence is important to adopt if you are a scientific realist but in itself it does not dictate whether the ontology of the World is universals, or classes of resembling particulars or tropes. I think these issues are independent of the scientific realism debate. I have my own approach to them which is broadly neo-Humean, that is a kind of a thin metaphysical conception of what is the ontological structure of the World and I try to defend it independently of Scientific Realism.

**JP:** Can we say that, in a sense, there is an "overdetermination of ontology by theory"?

**SP:** That is true and a nice way to put it. In fact, no ontology in the sense I am using the concept is entailed by scientific theories. Most likely the scientific conception of the World underdetermines the ontological structure of the World. This does not necessarily mean that there is no fact of the matter as to what the right ontological structure of the World is or what the ontological structure that best fits our scientific conception of the World is. But clearly you cannot derive the ontological structure of the World from the scientific account of the

World. So what scientific theories give to metaphysics is the conceptual space in which it can operate.

**JP:** You have criticized Nancy Cartwright's account of capacities for necessarily requiring regularities in order to be properly identified. But I cannot see how this is a problem for the proponents of capacities and causal powers in general. The existence of regularities may well be an epistemic condition for the identification of the capacities – the causal powers theorists might concede that and argue that in the *ordo cognoscendi* regularities do come first, but that does not mean they are ontologically more basic than capacities. As such, they will be entitled to say that there are regularities in virtue of there being capacities.

**SP:** That's exactly what I want to deny! I want to deny that regularities in the World are grounded in regularity enforcers. In fact, if there are regularity enforcers in the World like powers and capacities, they can be there without anything happening in the World. The capacities and powers can presumably exist without being exercised. What makes something a power and capacity is precisely that it can exist without being manifested. Therefore, there could be all the capacity and power in the World without anything happening. The condition for there being capacities in the World even as explanatory principles is that they are regularly exercising themselves. So by positing irreducible capacities we transfer the problem of regularity from something like a crude fact about the World to the regular exercise of capacities. In this sense, I think it is not just an epistemic condition for knowing capacities, but a conceptual condition on the very nature of capacities to think that they are regularly exercised; if anything, it is in virtue of these regular exertions of the capacities that there is regularity in the World. So, when I criticized Cartwright I said "no regularity in and no regularity out" – no regularity in the exercise of capacities, no regularity in the World. In that sense I think regularity is more fundamental and not just a simple epistemological condition.

**JP:** You understand natural laws as those regularities that are characterised by the unity of a (natural) pattern... as such, can or should they still be understood as *sub specie aeternitatis*?

**SP:** The idea of *sub specie aeternitatis* matters to me because I think that if you are a serious neo-Humean, as I tend to be, you should dissociate the question of what laws of nature are from the evidence we've got for them; for the evidence we have is always inductive. The evidence always concerns the past and the present and we always project the evidence to the future when we say that a regularity holds. But what is a regularity? What kind of entity is it, independently of the evidence we might have for it? A regularity is something in the World which is spatio-temporally extended, so a regularity is not just the evidence we got for it but all the instances that constitute the regularity, whether they are observed or not observed, whether we actually have seen them or they will exist in the future. I thought, however, there is more to regularity than the sum of the spatio-temporal instances and that is because we want to argue that even if an instance were missing the regularity would still hold, or even if one more instance were added, for some reason, the regularity would still hold. So I do not want to identify the regularity with the sum of its instances. And that is where the idea of a natural pattern comes in. So I want to conceive of a regularity as the instances plus the pattern. The pattern is the network of similarities and differences of the instances in virtue of which they belong to this regularity. The pattern unifies the instances, as it were. This is an idea I am still developing. I am fairly confident about this and I think I know what needs to be done, but I want to make room for the robustness of regularities, especially for counterfactual robustness of regularities, without inflating my ontology by regularity enforcers, but by thinking of patterns as unifiers of the instances.

**JP:** Following Alexander Bird's critique of your defence against the argument of overdetermination of theories by evidence why is it that you only count what is observable as evidence?

**SP:** It's not something I very much like, but that's how the debate goes. The Scientific Realism debate starts, especially after [Bas] van Fraassen's work on it, with the assumption that we have epistemic access to what is observable, and then the question is whether we have some kind of access to whatever is not observable; so it's become a kind of condition for this debate that evidence is observational. I never thought that that was the right conception of evidence however, because I always argued in my work that considerations of simplicity, of unification, and of novel predictions are all evidential considerations that bear on the likelihood of a theory being true. And I've argued that an argument might be given in support of these theses by looking at the History of Science, by looking at what happened in the past and at what kind of considerations scientists use as evidential considerations to support the various theories that they adopt as being approximately true.

**JP:** While defending scientific realism against a comment from Bas van Fraassen you have mentioned theoretical knowledge as something that evolves... should we interpret yours and Peter Lipton's commentaries as metaphoric or do you believe there is some truth to Evolutionary Epistemology?

**SP:** There are two ways of thinking in Evolutionary Epistemology: a) as an analogy; b) a literary true theory about how knowledge grows. I don't have strong views on either way. But I think it is mostly an analogy. I don't think we should take evolutionary approaches to knowledge as established. They are serious approaches, with important insights, but I am not yet persuaded by them. When I say that knowledge grows what I mean to say is that how theories change over time justifies us to believe that there is substantial continuity and convergence in Science. Continuity and convergence are not just at the level of empirical observational laws. They happen also at the level of the theoretical claims of the theories, which purport to tell us something about the deep structure of the world, that is, about the kinds of entities that exist, and the kinds of causes and mechanisms that exist. In this sense the growth of knowledge is basically what I conceive as an evolving and growing, scientific image of the World,

where some things are abandoned but new things are added and extend our scientific knowledge of the World.

**JP:** Are externalism and naturalism the unavoidable allies of Scientific Realism for the 21<sup>st</sup> century?

**SP:** I used to believe that. I am no longer fully committed to this view. I think that Scientific Realism is perhaps easier to defend if you take an externalist perspective in epistemology because you avoid dealing with the problem of justifying ampliative inferences in so far as they are reliable in producing knowledge of the World. In the last few years, I started to take more seriously the claim that, even if externalism is true, we should give reasons to justify our beliefs. Therefore, we should be able to defend Scientific Realism even if we adopt an internalist epistemology. And it is harder to do that in many ways. You might need to introduce notions which might sound anti-naturalistic to some, e.g. some kind of belief in *a priori* justification; or at least to some kind of *a priori* principles which are constitutive of a framework, though revisable on broader empirical grounds. I am not sure if this is incompatible with naturalism but some people think there is a tension between naturalism and a kind of apriorism about justification. I am still thinking about these issues... I want Scientific Realism to be neutral about the internalism versus externalism epistemological debate and to be a defensible view no matter what stance one may take in the epistemology of justification.

**JP:** Obviously, you believe that science and philosophy are closely related, but both within scientific and philosophical communities there are those who talk poorly of this alliance and, unfortunately, less often, there are even those who find philosophy useless to the advancement of Science. Why do you think this is so and what should be done, if anything, to counter such tendencies?

**SP:** If indeed there is this hostile attitude towards Philosophy from scientists it is kind of a recent thing. It has never been a trend in the History of Philosophy. If you look back, most if not all major philosophers were either scientists as well or they took the understanding of the scientific image of the World as a key part of

their philosophical endeavours. Conversely, scientists were also thinking philosophically, trying to understand issues like the method of Science, the confirmation of scientific theories and things like that. You find that in Newton, you find that in Huygens, you find that in Descartes, you find that in Kant, you find that in Einstein and in Poincaré. Perhaps there was a break during the German idealist time although even then there was quite a bit of concern about Science. The culmination of this interaction between Science and Philosophy was towards the end of the nineteenth century when most serious Philosophy of Science was developed by professional scientists who changed our ways of thinking about Philosophy. I don't know how to explain that kind of tendency in current times. Perhaps, it is partly because Science has become very specialized and Philosophy has become too conceptual and mostly disconnected from Science. But I very much hope there is room for rapprochement and convergence, and that this is an odd phase that will end at some point, and philosophers and scientists will collaborate again and think hard about common conceptual problems and get together to think about Science as a whole.

**JP:** Today you gave a lecture about Jean Perrin's arguments for being realist about atoms. The atom is a fine example of a theoretical term whose meaning has changed throughout the centuries. Though nowadays the majority would not object to the existence of atoms, if we were to recover what the term used to refer, let us say, in the tradition of Leucippus and Democritus, the case might turn out to be different. Nevertheless, atomism – and let me just emphasize that I am not referring to the Atomic Theory – is still alive and well as a paradigm for research in Science. Do you think we have good reasons to accommodate atomism in our metaphysics?

**SP:** That's not an easy question. It depends on what you mean by "accommodating atoms" in our metaphysics. I think of atomism as a scientific conception of the World that says that ultimately matter is granular, matter is particulate, and atoms themselves [the particles] have structure, they are not the ultimate elements. In this sense clearly Leucippus and Democritus were wrong, though they had the right insight about matter. But of course, the concept of atom has changed

meaning over time. I think that is natural: we change our theories, we change our concepts. What I think does not occur ever so often is what sometimes people call radical reference variance, the view that the referent of a concept, the thing that somehow is aimed at when the concept is used, changes. I think there is a lot more referential continuity in theory-change and I don't think incommensurability is a big threat simply because there is also partial overlap in intension. The meanings of the changing concepts of, say, "atom", clearly overlap, especially if you take a broad causal-descriptive theory of reference and focus on the causal agents we aim to describe and to refer to when talking about atoms. So I think atomism is well entrenched as a scientific position. However, things are different if you think of atomism as a metaphysical theory akin to Leibnizian or early Wittgensteinian ways of viewing the World... I'm not sure about that. I should stay agnostic about the use of atomism in metaphysics.

**JP:** Is it the most positive thing scientists can say about the theories that have empirical success that "look, our theory enjoys truth-like constituent theoretical claims"? Sounds to me Scientific Realism is becoming rather modest, which is probably a good thing.

**SP:** Look, the issue is whether you want to take the History of Science very seriously or not. If you want to learn from the History of Science, you got to come to terms with the fact that scientific theories of the past were shown to be false and abandoned on the grounds that they have been contradicted by experience. Therefore, not everything past scientific theories said were true; some of it or most of it was false. Now, if you stop there you might take it as a reason for pessimism about our current theories - the pessimistic meta-induction would say something like "precisely because most past scientific theories were false, despite the fact of their being successful, current theories are likely to be false". On the one hand, you've got an extreme pessimism, whereby current theories are false - period -, there is no continuity in theory change; on the other hand, you've got something like an extreme optimism: "whatever current science says is true". We know we should settle for somewhere in the middle. We know past theories enjoyed successes, we know lots of the constituents or the components of past theories which contributed to

the successes have been retained in subsequent theories, and we know that this is likely to happen to some if not to most of the constituents of our current scientific theories. I am not sure whether the glass is half-empty or half-full, whichever way you look at things there seems to be an important lesson for Scientific Realism from the History of Science. I think it's not an "all or nothing" matter, either all true or all false, but a growing and developing scientific image of the World.

**JP:** You have said that in principle everything is knowable, and I guess it follows from this that in principle absolute truth is possible, but instead of asking you how, which would be a nuisance, I would like to ask you what motivates this epistemic principle. It is a very strong claim.

**SP:** Again, you should see that in context. I say that in my book *Knowing the Structure of Nature* and in this book the key message is that after van Fraassen's critique of Scientific Realism – recall that he drew the line between observables and unobservables and he wanted to show that somehow knowledge of unobservables is impossible, suspicious, irrelevant, derogatory or whatever – the debate over Scientific Realism has shifted into drawing the line of the possible limits of scientific knowledge within unobservables. So, most rivals of scientific realism nowadays would say "we are not realists about some aspects of the unobservable"; Worrall would say – Structural Realism would say – "we are realists about the structure, we know nothing about the non-structural", [Ian] Hacking would say "we know about entities, but not the [fundamental] theories about these entities", [Anjan] Chakravartty would say "we know the detection properties, but not the auxiliary properties", [Jody] Azzouni would say "we know whatever we have thick [experimental] epistemic access to, and not all those for which you get access via the confirmation of theories", etc., etc. So they tried to impose a dichotomy within the unobservable: some part of the unobservable is not knowable and some part of it is. What I claim is that there is no natural way to draw this distinction and in this sense everything is knowable. That is an audacious claim... I'm not saying we will know everything, but I'm resisting to the temptation to draw a line within the unobservables such that some part of it is

inherently unknowable and some part of it can be known. That is an open issue and in principle everything can be known in this sense.

**JP:** Am I right to assume that you believe that scientists should be worried about such things as the unification of physical theories, for instance, or about the unification of science as a whole, hence why you say that a successful unification is a non-empirical virtue of a theory and as such counts as evidence for its truth?

**SP:** Unification has always been something that Science has striven for... It was actually Pierre Duhem who noted that, among other things, unification should be taken as a primitive aim of Science even if you are an instrumentalist about Science, in that you'd like to have a theory which, even if you do not believe it to be true, it unifies as many phenomena as possible under a single cover. So, it's interesting that unification became a trend even among instrumentalists like Mach and Duhem, and of course it became very fashionable among realists because unless you believe the world is unified why should you try to remove contradictions among conflicting models of theories? You could – to put it very crudely – use a theory a few days of the week and another theory a few other days of the week and be home and dry. Removing the conflicts between the theories we accept and trying to unify theories methodologically has always been a sign of progress. We've seen it in Electromagnetism and Optics, in Darwinian synthesis, etc. It's a non-empirical virtue of a theory in that if we take empirical evidence to come from observations and experiments, unification is not something like that. But it is a virtue nonetheless and I think there are good reasons to believe, especially if you look at the past of science, that it is a truth-conducive virtue, a virtue that increases the probability of a theory being true.

**JP:** Do you agree with Lakatos *dictum* that Philosophy of Science without History of Science is empty and history of science without philosophy of science is blind? And are there no exceptions?

**SP:** I think Lakatos was right. Philosophy of Science and History of Science are distinct disciplines in many ways: they have different professional standards, different methods, different audiences,

different questions to ask. But it seems to me they have to work hand-in-hand in understanding Science. I tend to think of Philosophy of Science as being more abstract than what one could call History and Philosophy of Science, which aims to device more abstract models about issues like scientific method, theory change, theory choice, relations between theory and evidence, and things like that. And these, *qua* abstractions, have to be concretised and the only way to be concretised is to actually give them some content by actual Science and actual History of Science. This is not to say that History itself should play a secondary role, subservient to Philosophy of Science. On the contrary, History is important in its own right. It's just that sometimes when you do History of Science for the purpose of philosophical questions you're not interested in exactly the same questions as when you do History of Science for historical purposes. I want to keep them distinct, but I think they should both work together in understanding how Science works.

**JP:** On several places of your works I found explicit opposition to Social Constructivism, but in nowhere I could find a systematic assessment of constructivist arguments. So why do you have repeatedly neglected constructivism, if it really is the case that you do... I might have missed something.

**SP:** That is a fair point. I have not argued against constructivism very explicitly. I've argued against constructivism here and there, and I have got a kind of an argument I've presented in my book *Knowing the Structure of Nature* which would be roughly that: constructivist or extreme constructivist views of science, that is views which would say something like the world itself is constituted by the scientific practices, by the scientific concepts, by the scientific paradigms, fail to accommodate the fact that scientific theories are in friction with the World; that there are anomalies, especially diachronic and persistent anomalies in our scientific theories. If we construct the World and if our paradigms carve up the World, or shape the World, or make the World what it is, why should the world resist our attempt to construct it in a certain way? The very idea that there is some resistance or friction between the World and our theories suggests to me that the

World cannot be our construction; that the World must have already some kind of independent structure such that it resists our attempts to construct it in a certain way. That is an argument I've put forward in my book even though I did not defend it in great detail, but it seems to me that this idea that the World resists our attempts to conceptualize it is a good argument against constructivism and neutralizes other constructivist arguments against the independence of the World.

**JP:** In one of your talks you refer to *l'enfant terrible* Édouard Le Roy as a social constructivist. How does Le Roy's social constructivism differ and resemble positions like the ones held today by Bruno Latour and other sociologists of scientific knowledge?

**SP:** I haven't studied Le Roy as much as I've wanted to actually... I'm in middle of a project now, trying to understand his position. Le Roy was rejecting the idea that there is an objective fact of the matter about more or less anything in the World. Even elementary scientific facts like the melting point of phosphorus or the boiling point of water he thought were constructed and they were constructed because basically we're using concepts and various techniques to make the measurements and to understand the phenomena. This position was a popular position in France in the end of the nineteenth and the beginning of the twentieth century, and actually it's interesting that constructivism was taken to be akin to Poincaré's conventionalism. This made Poincaré retract some of his more extreme conventionalist views and talk about the existence of brute facts which are independent of us and about the so-called scientific facts which, though constructed, somehow capture parts of the brute facts, the empirical facts that there are in the World. Part of his argument for continuity in theory change was an attempt to block these constructivist views of Science. I haven't studied constructivism in the late twentieth century as much as I'd hope, but it seems to me that it rests on the same idea, more or less. And if we were to go back to this debate between Poincaré and Le Roy we would probably find arguments that are good enough against current social constructivism.

**JP:** In the past two years, you have presented some lectures on the topic of the «Bankruptcy of science» and the polemic around it by the end of 19<sup>th</sup> century in France. First of all, if you dare spoiling us, I would like to know about the book you are writing on the topic and when it will be released. Secondly, I would like to ask you if you think that there are parallelisms with current debates on the epistemic status of scientific knowledge.

**SP:** The book is a project that will take longer than I have hoped because other projects got in the way, but it is certainly something I am committed to and I want to pursue and to finish at some point. I don't know when, but for the time being there is a talk that I give and various things that I have written here and there. This is a very interesting period because science became very prominent in the Third Republic and its prominence and its way to understand the world were questioned by various public figures and public intellectuals on religious and non-religious grounds. I find it extremely interesting that the debate in France was intertwined with the debates on Atomism, debates about various social issues, the Dreyfus affair, and the scope and aim of Literature. And what is more important for me is that this particular case shows how various philosophical positions that we discuss nowadays, like Poincaré's relationalism and conventionalism, or Duhem's critique of various aspects of realism, were shaped at least partly by social and political concerns that were prominent back then. I think there are parallels between this debate and current attacks on Science from religious and constructivist points of view, but I'm still pursuing them so I shouldn't commit myself to saying what exactly they are.

**JP:** In Portugal we have listened to our prime minister basically stating that scientific research should be at the service of Portuguese companies. Well... that goes a long way from a more modest claim about directing scientific research toward the satisfaction of the interests of society. But even then, do you agree that scientific research should be in any way directed and is this a problem concerning values?

**SP:** Scientific research should be free. It should be supported by the State, because being supported by the State means it is being, ultimately, supported by the Public. It should not be at the service of particular social or political or economic interests. And in this sense I think the Portuguese prime-minister and lots of other people who say this are very wrong. Scientific knowledge is enmeshed with epistemic values and social values. It's important not to hide this fact. It's important to unravel this fact, to make use of it. It's a very important element of feminist critiques of science which showed how social values are part of scientific research and practice. And I strongly believe that scientific research should be done for the benefit of society as a whole and the interests of society as a whole, that scientific evidence should be looked for and valued and that without free Science and without a scientific understanding of the World in a way that is uncensored, free, and promotes the rights and the interests of people there is no progress in Science.

**JP:** And now for something completely different. A last question. In July 2011, in an interview with Federica Russo, you talked about the economic crisis in Greece and said that "Greece is in a terrible mess and no end of the crisis is in sight". Is this still the case and do you believe the current state of affairs is at least partially a sign of a weak European Union?

**SP:** I think we are in exactly the same state in Greece. It's interesting that I've said that in 2011 and it's now 2015. I think we're probably in the same situation as we were in 2011, perhaps even worse! Because despite the fact that Greeks have suffered a hell of a lot and relentless austerity policies have been followed for many years, there is no prospect for recovery in the foreseeable future. Actually it seems as though there is no prospect for getting out of this mess forever unless some radical measures are taken. If I may say so, unless part of the Greek debt is relieved, there is no way in which there will be recovery of the economy. You see that in the universities in Greece and that is a pretty sad story because there is a danger, if not a fact now, that a whole generation of people has been actually lost for the Greek university. Great people, a younger generation who would eventually be able to change the fate of the Greek universities for the

better, are unemployed and with no prospects of being employed. With the same policies being followed, or being dictated, or being demanded, I think the prospects for the Greek recovery are very, very slim, and the policies that have been followed, despite the very success stories that have been told, have actually lead to a negative result.