Akademiniai maršrutai

AGAINST INTUITIONS IN PHILOSOPHY

Professor Stephen Stich interviewed by Bruno Mölder

Stephen Peter Stich (born 1943) is an influential US philosopher, a Fellow of the American Academy of Arts and Sciences. He is Board of Governors Professor in Philosophy and Cognitive Science at Rutgers University and Honorary Professor at the University of Sheffield. Stich has made important contributions to cognitive science, epistemology, philosophy of mind and psychology. In recent times, he has been involved in the rationality debates, contributed to research on moral psychology and played a seminal role in the birth of experimental philosophy.

Books: From Folk Psychology to Cognitive Science: The Case Against Belief *(The MIT Press, 1983),* The Fragmentation of Reason: Preface to a Pragmatic Theory of Cognitive Evaluation *(The MIT Press, 1990),* Deconstructing the Mind *(The MIT Press, 1996),* Mindreading: An Integrated Account of Pretence, Self-Awareness, and Understanding Other Minds *with Shaun Nichols (Oxford University Press, 2003) and two volumes of collected papers* Volume 1: Mind and Language, 1972–2010 *(Oxford University Press, 2011) and* Volume 2: Knowledge, Rationality, and Morality, 1978–2010 *(Oxford University Press, 2012).*

He was one of the keynote speakers in the conference "The Moral Domain: Conceptual Issues in Moral Psychology" at Vilnius University (9–11 October 2014). The interview was taken after the conference on October 11, 2014.

BM: *Why did you become a philosopher?*

SS: It is actually an interesting story. I am in many ways an accidental philosopher. As an undergraduate between 1960 and 1964, I took a number of philosophy courses and was somewhat interested in philosophy. By the time I was getting ready to graduate, the Vietnam War was heating up. In the United States, at that time, there was conscription for young men and one of the best ways to avoid being drafted was to stay in school; there were exemptions if you went to graduate school. So to avoid being sent to Vietnam I had to go to graduate school in *something*, and the only subject in which I had enough credits to get into graduate school was philosophy. I was interested in philosophy of science and went to Princeton. My goal was to work with Carl Hempel on topics like explanation and confirmation. That was the first accident.

There was a second accident during my first year at Princeton. I got an invitation to go to a series of invitation-only lectures by somebody I had never heard of, named Noam Chomsky. These were the Christian Gauss lectures that Chomsky gave in Princeton that later turned into part of his book, *Cartesian Linguistics*. Only one or two graduate students were invited to Chomsky's lectures. Nobody explained why I was invited. I was not the best graduate student, far from it. My guess is that it was a mistake, that they sent the invitation to the wrong address.

Sitting in the audience as Chomsky began setting out his argument, my jaw dropped. In 1965 in places like Princeton, rationalism was just a quaint historical view. Everybody was an empiricist at that point, when logical positivism was the dominant movement in the very recent past. And here is Chomsky, in his very, very unemotional, dispassionate way, saying that the debate between the rationalists and the empiricists can be put to an empirical test and the evidence overwhelmingly favours rationalism over empiricism. That's when I decided that this I what I want to do: I wanted to bring empirical evidence to traditional philosophical questions. In a certain sense, I have been an interdisciplinarian from the word go. What we were seeing, in Chomsky's work, was the birth of cognitive science, although back in 1965, to the best of my knowledge, the term did not exist yet.

BM: *Chomsky's review of Skinner's book Verbal Behavior has been also described as giving birth to cognitive science. This came out already in 1959.*

SS: I had not read it at the time. Later it became influential in my thinking and in the thinking of everybody else who was working toward a serious mentalistic psychology. That made doing cognitive science relevant for philosophy. The problem with Skinnerian behaviourism is that it is so austere and unconnected with traditional philosophical questions that philosophers tended quite appropriately to ignore it.

BM: *Did you also decide that you become a professional philosopher at that point or did this come later?*

SS: I think it was exposure to Chomsky and what was to become cognitive science that persuaded me that I would become a professional philosopher as opposed to just hiding out in graduate school for a few years so I did not get drafted and sent to Vietnam.

BM: *Did you also protest against the war?*

SS: Yes, actively. When I got to Michigan, which was my first job, the anti-war movement was growing and I was a member of a group (partly inspired by Chomsky's political writing) called *Call to Resist Illegitimate Authority*. Chomsky was an active supporter of the group and he was one of the unindicted co-conspirators in the trial of the famous baby doctor, Benjamin Spock and others who had initially signed the call. There were petitions that one could sign endorsing the Call, and I immediately signed one. I used to think that I was one of the tens of thousands of people. It turned out many years later that a much smaller number of people had signed, and that all of us immediately got FBI files. So the FBI started keeping a file on me because of my anti-war activities. When I went to the University of Michigan in 1968, I saved more than half of my salary in my first year. There was a really live possibility that I lose my draft deferment, since Nixon started to get rid of the deferments, which were mostly benefiting middle class white kids. The University of Michigan is a one hour drive from the Canadian border, and my plan was that if they ordered me to serve in the military, I would get my car and drive to Canada and become an expatriate, living on my savings until I could find a job in Canada.

BM: *Let's go back to philosophy. In your first book* From Folk Psychology to Cognitive Science *you defended an eliminativist position that common-sense mental notions have no use in cognitive science and hence they should be dispensed with. Then in* Deconstructing the Mind *you report how you became disil-*

lusioned with eliminativism, since you noticed that the eliminativist argument relies upon a doubtful premise on the nature of reference. But what is your current position in philosophy of mind?

SS: In *Deconstructing the Mind* I rejected all the arguments that had persuaded me of eliminativism, but I didn't articulate a clear view about the status of the posits of folk psychology. It was only quite recently, when I was preparing my responses for the *Stich and His Critics* volume, that I tried to address the issue. My colleague, Mary Frances Egan, had written an article challenging me to clarify my view. My current view really is very clear. Despite eliminativist criticisms, the positing of traditional intentional mental states kept going on in cognitive science and cognitive scientists invoking those posits kept producing important and insightful research. One clear example is research on heuristics and biases in reasoning, judgment and decision making. Another is research in developmental psychology which is up to its ears in things like the false belief task and children's acquisition of concepts. The study of autistic individuals is yet another excellent example. And there are many more. On my view, what concepts should be posited by a science gets determined by the people doing good science. If good science is getting done using these common-sense mental state concepts, there is something preposterous about philosophers telling the scientists that they should not do it.

Back when I was writing *From Folk Psychology to Cognitive Science*, there wasn't such a rich body of cognitive psychology invoking the concepts of common-sense psychology. But as cognitive science kept growing and producing important results, it began to look simply ludicrous to argue that all these impressive results are to be dismissed, because they are making some sort of philosophical mistake. In *From Folk Psychology to Cognitive Science*, I argued that folk psychological concepts suffer from various sorts of vagueness and situational sensitivities, which are impediments to doing good science. So, I argued, we should abandon folk psychological concepts and embrace a formal or "syntactic" theory of the mind. But by now, there is overwhelming evidence that the vagueness and situational sensitivities that I emphasized in *Folk Psychology* are *not* impediments to doing good science. It would, I think, be an interesting research project to explore *why* they aren't.

BM: *This presumes that the only criterion for the use of intentional notions is their success in science, but one could take folk psychology at the face value because it works in everyday life, and not to look at its usefulness for science. What do you think of that approach?*

SS: My original eliminativist arguments were all aimed at how cognitive science should develop; they were arguments to the effect that the science of the mind should do without intentionally characterised mental states. Cognitive science was in its infancy, and my main interest was how cognitive science should move forward. That ordinary people find folk psychology useful in everyday life is certainly an interesting fact. It would be good to have an explanation of why and how it works in ordinary life. But that does not tell us anything particularly interesting about how the science of the mind ought to develop, any more than the fact that ordinary people treat the surface of the world as flat tells us anything about how geography should develop. I don't

think there is any particular reason to give priority to the conceptual scheme of ordinary people in building a science. Thus I am not impressed by what Jerry Fodor characterises as the "Grandma uses it all the time" argument. It is true, Grandma does use it all the time, but Grandma also uses folk physics notions all the time and does not understand most of the concepts used in Newtonian physics, let alone the concepts used in quantum physics. But that is no argument for using folk physics notions in physics.

BM: *Is this also an ontological claim that the science determines what there really is?*

SS: Yes, I believe that. Somebody, but I can't remember who, once jokingly described me as the last surviving Quinean. Like Quine, I think that on lots of matters, including what the basic ontology of the universe is, science has the power to make the ultimate decisions, although there is a lot of philosophical work to do, to say what exactly it is that the science is telling us about these questions.

BM: *You have become fairly critical about philosophy, or at least of a certain kind of philosophy. Please elaborate your reasons.*

SS: My scepticism is focused on one element of philosophical methodology. It is a methodology that has been around for a long time. The examples go back to Plato. In the 20th and 21st centuries, this so-called "method of cases" has been very widely used. This method relies on thought experiments (typically recounting hypothetical cases) and philosophers' intuitive reactions to them. After setting out the hypothetical case, philosophers ask whether a person in the thought experiment has knowledge, or whether an action described in the thought experiment is morally acceptable, and so on. Philosophers' intuitive reactions to these questions have been used as data for two different sorts of projects. One project draws conclusions about objective questions, like is the action described in the thought experiment really morally acceptable, or does the protagonist in the thought experiment really have knowledge? In the other project, philosophers are using intuitive responses to hypothetical cases as evidence about concepts. Though it is sometimes unclear which of these two projects a philosopher is pursuing, they are really very different: analysing a person's or a group's concept of causation, for example, is not the same as saying what causation is.

I have been very critical about the use of this method in both projects, although the critique has been slightly different for the two different projects. It is easier to make the case against people using intuitive evidence for claims about objective matters. The objection is quite straightforward. The assumption of the method is that the content of the intuition people have in response to a thought experiment is likely to be true. But if there are demographic variations in intuitions – if, for example, Asians have one intuition and Westerners have another intuition – then they cannot both be right. And if there are order effects and framing effects or environmental effects – if intuitions are influenced by the presence of dirty pizza boxes or the presence of fart spray, that casts serious doubt on the assumption that the content of the intuition is true. It really is as simple as that.

BM: *Could you also say what intuitions are? It's a philosophical term of art after all.*

SS: As I use the term, intuitions are relatively fast, spontaneous judgements made with little or no conscious reasoning. In cases like those I alluded to, an intuition is just a

spontaneous judgment about whether the protagonist in the thought experiment really has knowledge, or whether the action described is morally permissible. Those intuitions are very much like the syntactic intuitions that play an important role in Chomskian linguistics. I give you a sequence of words in a language that you are fluent in and ask you questions like "Is that a grammatical sentence?" For overwhelmingly many cases you have an immediate inclination to judge "Yes, it is" or "No, it is not". You don't engage in any conscious reasoning, and you have no idea where that judgement came from.

BM: *Is your point that since intuitions are varying on non-philosophical grounds, we cannot rely upon them as evidence for philosophical claims?*

SS: I would prefer to say the intuitions are being influenced by factors that are *irrelevant* to the philosophical issue at hand. Since there is independent evidence that intuitions vary across demographic groups, that they are sensitive to the order in which questions are asked, that they are sensitive to minor differences in wording, which clearly don't affect the philosophical question at hand, you cannot make the standard philosophical assumption that the content of the intuition is likely to be true. The method of cases works in this way: you want a theory of knowledge, so you look at a bunch of cases, Gettier cases, fake barn cases, etc. and you make judgements about whether these are cases of knowledge, and then you try to construct a theory that captures your intuitions. You assume that most of your intuitions are true, and you try to give a theory that accords with those intuitions. But if there are major demographic differences in people's intuitions about philosophical thought experiments, then the intuitions cannot all be true.

BM: *But is it really the case? Recently, some people have argued that there really is not that much variance in intuitions as experimental philosophers assume. Others have found problems in the methodology of the experiments.*

SS: That's an important question. Before answering it, let me sketch a bit of history. Without relying on any empirical evidence, I have been sceptical about the use of intuitions for a long time. Back in 1988, I wrote a paper called "Analytic epistemology, reflective equilibrium and the problem of cognitive diversity" where I pointed out that just imaginary cognitive diversity would be problematic. A standard response to this was always: "This may be logically possible, but it does not actually happen, so philosophers should not be worried about it." As soon as we started to publish the experimental stuff, people started taking the possibility very seriously; they clearly found it a lot more worrisome. And you are right, there have been lots of critiques of the work of experimental philosophers, many of which I think are perfectly appropriate and demand responses. People say that the methodology is flawed and in some cases they are right. When we started doing this, we had a few very good psychologists such as Richard Nisbett as advisors, but there was an enormous amount we had to learn about experimental methodology. Some of our early papers are methodologically flawed. There are also claims that some of the results do not replicate and that has to be looked at further. These are empirical objections to empirical claims, so they are entirely within the boundaries of what should go on in scientific debate. Has it been established "beyond reasonable doubt," to use the standard that Ernie Sosa proposes, that there are demographic differences in philosophical intuitions? No, absolutely not, it is

still an open empirical question. I am inclined to think that the body of evidence currently available favours an affirmative answer. But lots more work is needed. However, I think that we *have* made a compelling case that experimental philosophy has an important role to play in philosophy. It has to have a place at the table, because sitting in your armchair you will never know whether there are important demographic differences in intuitions. If you agree that *if* there is demographic variation, or *if* there are irrelevant environmental factors that affect our intuitions, then it is a problem, then you have to have experimental philosophy as a part of the methodology of philosophy.

BM: *In a recent book Philosophy without Intuitions Herman Cappelen argues that philosophers do not really use intuitions as evidence. They have the bad habit of using the term, but this does not play the role of evidence. Such a usage merely stresses the point that some assumptions are thought to be common or that some claims are not sufficiently reflected upon. If that's true, does not this undermine your criticism of armchair philosophy?*

SS: Yes, if that is true, it does. But I think that it says something about the desperation of people trying to respond to the experimental philosophy critique that they take Cappelen's book seriously. Cappelen's ploy is to articulate an account of what is required to be an intuition that is absurdly strong and absurdly demanding. And on that demanding account of what an intuition is, it turns out that nobody is using intuitions in philosophical argument. But there is now a growing consensus in the literature that Cappelen's account of intuition is completely implausible. As a referee on a paper of mine (who was clearly no a fan of experimental philosophy) recently said, Cappelen is attacking "a straw man" and his account of intuition is "zany and extreme." The fact that Cappelen's book has got a lot of respectful attention speaks badly of the philosophical profession. Some people are really feeling threatened by experimental philosophy.

BM: *You and Wesley Buckwalter have written about the lack of gender balance in philosophy and defended the striking claim that the reason why there are so few women in philosophy lies in the manner philosophy is practised nowadays. What exactly is this reason?*

SS: First of all, please don't attribute to us a view stronger than the view we defended. We suggested that difference in philosophical intuitions between men and women (in North America) might be *a* reason, why women are underrepresented in philosophy, not that it is *the* reason. We said that as explicitly as we knew how, because we were well aware that we were dealing with a sensitive topic and that the phenomenon of the underrepresentation of women in academic philosophy is a complicated one and has many causes. We wanted to explore one possible cause.

As we discussed earlier, I have long been interested in demographic differences in philosophical intuitions, and one of the most obvious demographic differences is gender. Buckwalter and I found some evidence that there are gender differences in philosophical intuitions. How could that contribute to gender imbalance? Well, if philosophers are using intuitions as evidence for philosophical claims, then if a student does not have the same intuitions as the leading people in the field, or the same intuitions as her mentors, then she is at a real disadvantage. She has a different

perception of what the evidence is. So the thought was that since women have different intuitions from men, and since the majority of leading people in the field are male, as are the majority of philosophy professors, women are going to be at a disadvantage.

It was very controversial, in part because some people interpreted our claim in a way that was very different from the way I intended it to be interpreted. My intended interpretation was: "Look! If philosophical intuitions are influenced by gender, then the method that relies on intuitions as evidence is a biased method." Whereas a lot of people said: "What you are doing is saying 'Women are no good in philosophy!'" But that's not what we were claiming. You only get the conclusion that women are no good in philosophy if you assume that when men's intuitions and women's intuitions differ, men have the right intuitions! Whereas if the intuition process is itself influenced by gender, then it is the method of using intuitions as evidence that is challenged. Gender differences in intuition suggest that the method leads to unconscious bias against women.

Whether or not it is true that there are gender differences in philosophical intuition, I don't think we know. A lot more work needs to be done. Unfortunately, I don't think it is going to be done, because it has become such a political hot potato. I typically only do experimental philosophy research in collaboration with other researchers who are almost always a lot younger than I am and who are usually methodologically and mathematically more sophisticated than I am. But I will not work with a younger investigator on this topic anymore because it is so politically charged. I am beyond the point where my career can be damaged by anything that gets published with my name on it. But a young researcher who tries to investigate gender differences and who comes up with a positive answer is putting his or her career at risk. It is too dangerous to do the work.

BM: *Where does the danger come from, exactly?*

SS: If you assume, as some people do, that you already know all the important factors leading to the underrepresentation of women in philosophy, namely that it is the result of sexism and sexual harassment – and I do not deny that these play a role – then you know in advance that the intuition difference hypothesis is mistaken. It is also viewed as a distraction: it takes people's focus away from the bad things that are happening with sexist behaviour and discrimination. I think that this has motivated a number of people to attack this sort of work quite vitriolically.

BM: *What do you think can be done to achieve gender balance in philosophy?*

SS: Are there sexist attitudes and sexual harassment? Absolutely! Does it discourage women from going into philosophy? Almost certainly, yes. Should efforts be made to prevent that from happening? Yes. All of those things are appropriate and important. If the hypothesis about gender and intuition that we broached is true, then one of the central methods of philosophy may be biased against women. That is harder to address, because the only obvious way to deal with the problem is to reduce philosophers' reliance on the use of intuitions as evidence.

BM: *Is there anything left for philosophy to do in your view?*

SS: My critical focus has been on the use of intuitions as evidence and the method of cases. There are all sorts of philosophical

projects where that method plays little or no role at all. Think of the philosophy of science, in particular, philosophy of physics, philosophy of biology, and philosophy of the social sciences, where the goal is to look carefully at what scientists actually do and how scientific debates and discoveries actually unfold, and to articulate what their methodology is much more explicitly and critically than the scientists do themselves – because they are not at that line of work. These projects in the philosophy of science are completely untouched by the sorts of criticisms I have been urging. Some of the best of that work, for example, the work in philosophy of biology by Elliot Sober, not only contributes to understanding scientific methodology by looking at what scientists do, it also makes suggestions that can improve the practise of the science.

BM: *Do you see any positive role for more traditionally inclined fields like aprioristic metaphysics, epistemology and philosophy of mind?*

SS: Are there rich and interesting parts of metaphysics, epistemology or philosophy of mind that do not use the method of cases and do not rely on intuitions as evidence? Yes. I have no criticism of those parts. With respect to the question of what remains after you have agreed not to use this method, the devil is in the details. We have got to look at particular projects and ask "Can it be moved forward productively without intuition mongering?"

BM: *You already mentioned that philosophy is very close to cognitive science, but what is your view on the relationship between philosophy and science in general? Is there any essential distinction to be made between the two?*

SS: When I was described as Quinean, the person who said that had in mind the view that most influenced me in Quine, the view that philosophy and science are continuous. Where to draw the boundary between philosophy and the sciences is a problem for the dean; it is not an intellectual problem. Theoretical issues in many of the sciences merge more or less seamlessly with philosophy and vice versa.

BM: *Some say that continental philosophy or phenomenology is more relevant for non-philosophers than analytic philosophy. It enriches their lives, it is deep and interesting. Do you think that the sort of philosophy which is close to the sciences can have wider relevance for our lives?*

SS: A lot of scientifically informed philosophy is fantastically interesting and important to many people. Consider for instance the rationality debates. Daniel Kahneman's book *Thinking, Fast and Slow* has been on the New York Times bestseller list for over two years, which is a very good indication that a lot of people are interested in it. The book deals with the debates about human rationality, and philosophers have played a central role in those debates. Normative ethics and philosophy of law are also enormously relevant to people's lives. Let me give you an example. My colleague Douglas Husak does wonderful work on the philosophy of criminal law, analysing and clarifying the relationship between empirical findings and legal principles. His scientifically informed work on what is reasonable and unreasonable policy for laws governing recreational drugs and other illicit substances has had a major impact on the revision of the laws in a number of countries in Europe. What could be more engaging than that? Are we doing foolish things in regulating recreational drugs, are our laws ill-suited to do what we want to

do? Those are philosophical issues, and they can't be addressed seriously without drawing on the relevant science.

BM: *Finally, can you give some advice for young philosophers who are looking for research topics on which to spend years of their life: what are the worthy topics?*

SS: There is a wide range of topics where the empirical sciences are interacting productively with philosophical issues, and much of this work is of enormous interest and importance. Recent work in moral psychology is an excellent example. In that area, philosophers, psychologists and neuroscientists are really interacting productively. They are trying to figure out how the mind works when it is making moral judgments, and why we have minds that work that way. And as Joshua Greene argues in his recent book, *Moral Tribes*, this work has profound implications for public policy. In the philosophy of biology, the work done by Peter Godfrey-Smith, Kim Sterelny, Elliot Sober and others illustrates the ways in which philosophical questions can bear on and shape debates in theoretical biology. That sort of work is bound to grow more important.

BM: *What is required for this is that philosophers and scientists find a common language or that there were translations from one theoretical perspective to another.*

SS: That is absolutely right! I have been engaged in attempts at interdisciplinary work from the beginning of my career, and learning to talk to scientists and scholars in other disciplines the hardest thing to do. There has been progress on lot of fronts. In cognitive science, thanks to the wonderful work of Jerry Fodor and Daniel Dennett and lots of other people, philosophers and cognitive scientists have learned at least a partially overlapping language that makes it possible for them to contribute to each other's endeavors.