The Study and Teaching of Philosophy of Science

An Academic Interview of Professor Peter Achinstein

***Liang*:** Could you please share some more details about the reason why you chose philosophy of science as a life-time business? It is said that you were motivated by Hempel when you were studying at Harvard. Is that true?

***Achinstein*:** Well, I didn't choose it at the beginning as a life-time career, but philosophy was always fascinating to me, since it raises fundamental questions that I find very challenging. I chose to do my Ph.D. thesis on probability theory, particularly Carnap's probability theory, and that got me interested in philosophy of science.

The philosopher who had the greatest influence on me as a student was Carl G. Hempel, who came to visit Harvard one year when I was an undergraduate. He taught a course on probability, and one on philosophy of science. These were the most interesting philosophy courses I'd taken at Harvard. When I was a graduate student, I wrote my thesis in the area of probability and induction. This led me to the question: what is evidence, and can it be defined in terms of probability? At the same time, I began attending courses in physics at Harvard and later at Johns Hopkins, which got me further interested the philosophy of science. So, I didn't start out by planning to work in the philosophy of science. When I became a graduate student, I was not like some graduate students who are sure what they want to focus on at the very beginning of their study. You see how well you do, and you see what interests you. At Harvard at that time there was very little individual faculty supervision. You had no advisor as a graduate student. You had to fend for yourself. Many students could not work under those conditions, so they dropped out. It was tough, competitive, and there was nobody telling you how well you were doing. You really had to be motivated to continue. That changed well after I received my Ph.D. Now you have a lot of supervision, and people are encouraging you. At that time when you were a graduate student, you had no idea what you have to do to succeed, you just proceeded one step at a time. And I was good at that, I did not need an advisor. As far as your Ph.D. thesis was concerned, the only feedback you got was at the very end after you submitted it to the department. And when you did you had no idea who was going to examine you on it. In fact, at that time I asked the chairman of the philosophy department if he could appoint W.V. Quine on my examining committee. Quine was the most notable philosopher in the department. But the chairman said, "I can't ask Quine anything."

***Liang*:** So, you didn't have an advisor?

***Achinstein*:** No, as an undergraduate student you had an advisor, whom you could pick. Harvard at that time paid much more attention to undergraduate students than graduate students. If you went for honors, you had to write a thesis. So, I had an advisor for that, Roderick Firth, one of whose specialties was epistemology. My undergraduate thesis examined a debate between Quine and C.I. Lewis, who was very famous at Harvard before Quine. Lewis defended the analytic/synthetic distinction, and Quine notoriously attacked it. In my thesis I defended Lewis against Quine. Quine was on my thesis examining committee. He asked me 30 tough questions, and graded each answer.

***Liang*:** So, as an undergraduate student, you had an advisor, but when you became a graduate student, you didn't have any advisor.

***Achinstein*:** Yes.

***Liang*:** Then how do you know whether you are on the right way or not?

***Achinstein*:** You don't know. It was like submitting an article to a journal now, you have no idea whether the journal is going to accept it or not, or whether they think it is good or bad, or whether they are going to ask you to revise it. It is much tougher from that perspective.

Liang**:** You talked a lot about Quine. Does that mean Quine had the most important influence on you?

Achinstein**:** Well, I took every course Quine gave at Harvard. He was a major figure in philosophy, a superstar in the field. As an undergraduate student I took most of the courses he taught, and as a graduate student, I took the rest of them. And as a graduate student he gave me a special private course in advanced mathematical logic.

Liang**:** Besides Quine, could you talk more about other teachers who had an important influence on you?

Achinstein**:** Well, as I said, Hempel was the major influence on me, although he was a visiting professor for a year when I was an undergraduate. Quine was not a great teacher. He had a quick mind, an interesting mind, but his influence was primarily through his writings rather than his teaching. Whereas Hempel was outstanding at both. He was by far the best teacher that I had in philosophy. He had a very clear mind, and lots of energy. He was also exceptionally nice. He took teaching extremely seriously.

Liang**:** Your Ph.D. dissertation focused on Carnap's probability theory. How did you come up with that idea?

Achinstein**:** In the course on probability Hempel gave when I was an undergraduate student, he spent a lot of time on Carnap. He knew Carnap, who was one of the greatest living philosophers. Carnap wrote a very important book called *Logical Foundations of Probability,* published in 1950. Like Quine, Carnap had big ideas, and he could spell them out very carefully, he was technically talented, in terms of doing the mathematics behind the probability theory. He was not just out to prove theorems, he wanted to find the foundation of probability that made good logical sense. So Carnap developed a theory which is not that popular now, but it was a major theory at the time. It is called logical probability. Carnap wanted to do for probability what people had done for formal logic. He wanted to produce a system in which a purely formal calculation can determine how probable a scientific theory is on the basis of the available evidence. Such a system would help resolve scientific disputes. For example, one of the most controversial theories in physics today is string theory, which says that everything is composed of strings vibrating in 10-dimensions of spacetime. Some physicists say that the probability of such a theory, given everything we know, is practically zero, others disagree. Carnap wanted to present a precise, formal way of settling such a dispute. It was a very ambitious, exciting project.

Hempel lectured on Carnap's system in his course on probability. This got me interested in the question of how the thing is supposed to work.

So, in my Ph.D. thesis, I proved some theorems about Carnap's system, indicating that it couldn't work in certain areas. For example, suppose you have a sequence of coin tosses, and approximately half the time the coin had landed heads. But in addition suppose that on every tenth toss the coin has landed heads. Now Carnap could develop a system only to take into account the relative frequency of heads, not the order in which heads appears. So, for him the probability for getting heads on the 100th toss would be ½, since that is the observed relative frequency of heads at that point. But, look, you say every tenth toss the coin has landed heads, so the probability of its landing heads on the 100th toss should be much higher than ½. I proved formally that Carnap's system could not take into account the idea of periodicity in the evidence. I sent my article to a journal called *Philosophy of Science*. And they sent it to Carnap who wrote a reply. Of course, that got me very interested. So, there is my paper, and next to it is Carnap's reply. Before these were published Carnap wrote me a letter saying "Your theorem is right, but I have another theorem which shows that I can do all these ideas in a different way." I said, "Professor Carnap, thank you very much, it is a great honor, could you give me the proof?" He said, "I can't give you the proof, because I have lost it, and I can't reconstruct it." So, when he published his article, following mine, he put in a footnote, "The proof is too complex to give here." So, it is an unproven theorem. It took me a day or two to figure out what his theorem even meant. But I decided not to reply in print or in private to Carnap. The fact that we gave dueling theorems to the world was enough.

Liang: Do you have other similar exciting experiences?

Achinstein: When I was a graduate student, Harvard gave me a fellowship to travel anywhere I would like for a year. The really exciting place to be abroad at that time was Oxford University. Oxford had more than 70 people in philosophy at that time. I went to see one of the most famous of them, Gilbert Ryle. He wrote a book called *The Concept of Mind*. And he liked meeting foreign students. Many English professors at that time were not particularly friendly to foreigners. But somehow Ryle liked meeting new people. Ryle said, "You tell me you are working on induction. Do you know I have written an article about induction recently?" I said, "No, please tell me about it." And he told me about it, and gave me an article he had just published. So, I took the opportunity to write a reply. I wrote a paper critical of Ryle that was published in the British journal *Analysis*, to which Ryle replied in the same issue. Having big guns such as Carnap and Ryle reply to your work really gets you motivated.

Liang: What would be your suggestion on selecting/choosing research topics to Ph.D. students, considering that many of them are having a hard time choosing one.

Achinstein: Well (laugh), you are looking at the wrong guy. When I was a graduate student you chose your own topic. You were on your own.

Liang: Because you have been teaching for many years, I think you must be very good at suggesting topics to your students.

Achinstein: You know, my best students have selected, their own topic, and then I have suggested various directions they might take. Look, you must be interested in the topic, and you must be good at it. Not all the students are both. Some students are interested in a topic but are not good at it, others are very good but they are not interested in it.

You must be both. You must have motivation and talent. Most students bite off too much, they can't do it. You have to narrow it down, you have to raise specific kinds of question. But you don't know until you start thinking about it and seeing where it is going to go. And that is normal. So, when they write up a thesis proposal and are examined on it many students don't do as well as they would like and get frustrated. They haven't read all the material on the subject or they don't know enough about where the thesis will take them.

Liang**:** One of my classmates is studying William Whewell, a 19th century British scientist and philosopher of science, and I was told that Laura Snyder, a former student of yours, is one of the best scholars to focus on Whewell. I think maybe my classmate would like to hear about Snyder.

Achinstein**:** Well, she came to Hopkins to study the history of ideas, but the professor in that field died just before she arrived. So she concentrated in philosophy and took some of my courses in the philosophy of science. I would talk about the debate between John Stuart Mill and William Whewell, over the nature of scientific reasoning, because it was such an important debate. That inspired her and she wrote her Ph.D. thesis about that. After completing her Ph.D., she was one of the first to enter a new field called History of the Philosophy of Science, and became a founder of a journal by that name, with the initials HOPOS. She has now left academic philosophy to devote full time to writing about a variety of historical and philosophical topics.

Liang**:** You are a founder and director of Johns Hopkins Center for History and Philosophy of Science. Why did you decide to build such a research center?

Achinstein**:** That was around 1969. In the History of Science Department at Hopkins there were some faculty members who were interested in philosophy of science, and some in the Philosophy Department interested in the history of science. One important stimulus for this mutual interest was a book published in the early 1960s called *The*

Structure of Scientific Revolutions. The author was Thomas Kuhn, a physicist by training. The book had a powerful impact on philosophers of science and historians of science, since it provided a new way to view both fields, and view them together as one. If you want to understand the conceptual ideas behind science, and how they develop and change over time, you really need to study both fields.

Kuhn introduced the idea of a "paradigm," a set of basic concepts, assumptions, and methods in terms of which the physical world is to be understood, as well as a set of problems to be solved and guides to how the solutions are to proceed. "Normal science," as he calls it proceeds for a long time by reference to the paradigm and the problems it sets. Very occasionally, however, there are "paradigm shifts" as a result of revolutionary new ways of seeing the world. These give rise to a new and entirely different set of concepts, assumptions, and problems to be solved. Well, this book became enormously influential, even if, as with many such books, it was controversial. It was widely read by people in the natural and social sciences, but the ones he influenced the most were philosophers. Whether they loved him or hated him, they all read him and reacted. Whatever else he accomplished, he reinforced the importance of the idea of engaging in both history and philosophy of science. A few academic centers for the study of both sprang up. I liked the idea and asked, "Why don't we have one at Hopkins?" So, I went to the chairman of the History of Science Department, who liked the idea. And we went to the dean, and said let's go to raise some money for this. The dean said, "All right, we will go to one of the big international companies in Baltimore," both of whose founders and directors were graduates of Johns Hopkins. We thought they will give us money, but they didn't. So, we went back to the dean, who said, "All right, I tell you what, I will give you some money." And he just gave us enough to invite four outside speakers a year and to organize conferences every few years. The papers from these conferences were published as books to which both historians of science and philosophers of science contributed. One of the volumes was on the topic "Physics since Kelvin." (Lord Kelvin, a famous British scientist in the 19th century, gave lectures at Hopkins in the 1880s on the wave theory of light.) Another volume was on scientific evidence, a topic on which I have a special interest.

Liang**:** Some scholars say that induction is a central issue of philosophy of science. Do you agree with that?

Achinstein**:** Yes, sure, it is one of the issues, because many consider it to be the bedrock of science. In the 18^{th} century, the great Scottish philosopher David Hume made the very controversial claim that induction is not justified. But, you know, that sounds crazy. In the 17^{th} century, Hume's idol, Isaac Newton, said, "Of course, induction is justified, we use it all the time, in fact I can't get my law of gravity without induction." Newton listed induction as the third of his four methodological rules of reasoning he proposes in order to prove his law of gravity. The idea is that if all observed bodies satisfy a given law then you can infer that all observable bodies do. And from this you can generalize and say that all bodies in the universe do, whether or not they are observable. But Hume said, "Of course, you can generalize, but there is no justification for it. You can't defend induction deductively, nor can you do so inductively, because that would be circular. So, inductive reasoning is irrational, even though this is what human beings, including scientists, engage in all the time. It is just a habit of the mind, unjustified, but one we are stuck with." Philosophers since Hume have been trying to solve Hume's problem: how can a rule of induction such as Newton's Rule 3, be justified? And as usually happens in philosophy, there are a lot of different answers proposed, including two at the extreme. One of the latter, offered by Karl Popper, is that good scientists don't use induction, only deduction. Another offered by the contemporary philosopher John Norton – one which I think is right – is that there is no universal rule of induction to justify. Each inductive inference, each generalization, is justified by reference to particular empirical facts which differ in each case, depending on the generalization. Particular inductions are justified locally, not by appealing to some global principle of induction, such as Newton's Rule 3.

Liang**:** You are very famous in the field of scientific evidence theory. Would you like to share the idea that why you are focused on evidence theory?

***Achinstein*:** Evidence is behind everything in science. The question is: what exactly is it? How should it be defined? Some physicists say that string theory is not science, because there is no evidence at all. What are they looking for? In 1978, I wrote my first article on evidence, and it was published in a British journal called *Mind*. In it I criticized a standard theory that defines evidence for a hypothesis as anything that increases the probability of the hypothesis. I developed a new definition of evidence in that article and more extensively in a book published in 2001 called "The Book of Evidence." There were lots of responses to my ideas, especially by those who wanted to defend the standard theory. I proceeded to reply to a number of them, thereby continuing the discussion of this fascinating topic, one that has important implications about what scientists should be looking for when they look for evidence.

A few years ago a conference on string theory was organized in Munich, Germany to which physicists and some philosophers of science were invited, myself included. The physicists were worried because there are no experiments providing evidence for their theory. The guy who organized the conference, Richard Dawid, said, "Yeah, we do have evidence, but it is indirect evidence, it is not experimental." So, I said, "Ok, what counts indirect evidence?" And he started giving me his theory of indirect evidence, which includes facts like "it is the only theory so far developed to explain the phenomena in question (it is the "only game in town"), it is a theory of a type that has worked well in other areas, it is a simple theory." I reject these ideas because none of them provides a good reason for believing that the theory in question – in this case string theory – is true. Only experimental evidence will do that.

***Liang*:** I think, judging by common sense, the concept of evidence is very vague, it is not clear.

***Achinstein*:** **One of the problems here is that a lot of those who think about what evidence means want to include too much in that concept. For example, above I mentioned simplicity: If a theory is simple that should count as some evidence that it is true.** In a book I published in 2018, called "Speculation," I devote two chapters to simplicity. Is the fact that a theory is simple evidence that the theory is true? Does that fact enhance our belief in the theory? Newton says that it does, and Einstein agrees. So, I take on Newton and Einstein. I say: give me the argument. Newton gives no argument, but just says, “Nature is simple.” That is the reason you can use induction. By using observations from telescopes you establish that the law of gravity works in our solar system. Then by induction you say since it works in our solar system it probable works everywhere. Why? Because nature is simple. A world in which the same laws hold everywhere is simpler than a world in which different laws work for different parts of the world. To which I say: how do you know that the world is in fact simple in this way? In his published writings Newton does not say. But in unpublished letters he claims that God works in simple ways. He doesn’t say how he justifies that claim. Einstein doesn’t invoke God. For him, simple theories have worked in the past, so they are likely to work in the future. My response to that is: some simple theories have worked, some haven’t. Simplicity is not a sign of scientific truth, so it is not evidence for a theory. Moreover, you can’t justify induction by saying that nature is simple, and then go on to justify the claim that nature is simple by saying that simple theories have worked better in the past than complex ones. That reasoning is circular. It uses induction to justify induction.

Liang**:** I noticed that both you and other teachers of Hopkins are using original works when teaching. Do you think that reading first hand materials is the best way to study philosophy?

Achinstein**: Yes.** Look, these are works of some of the greatest thinkers. They were writing at a time when science was not so specialized as it is now, so more people can understand them. They wrote books educated people could read. And if educated people can read them, then undergraduate students can read them. In my course called Philosophy and Science: An Introduction to Both*,* I have the students read original scientific works from the 17th century to the beginning of the 20th century. We start with Descartes’ rules for how to do science, and then turn to his physics, in particular, his laws of motion. which people who study philosophy very rarely read. They think of

Descartes only as a philosopher. But, of course, he was also a mathematician, and a physicist. You know, he was the first guy who got a version of the law of conservation of momentum. And he got Newton's law of inertia before Newton did it. So, we read parts of Descartes's physics, as well as his philosophy of science. Now, Descartes is what is called a "rationalist." He thought that, just as in mathematics, reason, not experimentation, is the final arbiter of whether some proposition is in science is true. When undergraduate students read this, they think he is crazy. Following Isaac Newton, Descartes' most important scientific opponent, they think that experimentation and observation are the final arbiters of truth in science. Newton says that in science you must start with what he calls "phenomena" – facts determined to be true by experiment and observation. Then you proceed to make causal and inductive arguments from these facts to your theory. Only then will you determine what is true. To understand this more modern-sounding Newtonian view of science properly, you need to understand views Newton was fighting against, most particularly Descartes' philosophy of science. That's why I start with Descartes in my course before I turn to Newton's rules for doing science and his use of those rules in his argument for the law of gravity. In any case, Descartes is such a good writer. He writes much better than philosophers and scientists do nowadays. And he raises fundamental questions about science in a way that hadn't been raised before. Students get excited by these questions. So I am happy to start the course with him.

Liang**:** I am very surprised to see that so many students – over 50 of them – take your logic course. Do you think that all the students majoring in philosophy or science are supposed to master logic as a basic skill?

Achinstein**:** Well, who knows. First of all, it carries two kinds of academic credits. It carries a humanities credit, and it carries a quantitative credit. So, suppose you are a quantitative science student, like a math student, or a physics student, and you are really not interested in taking a literature course, but you want to take a course that gives you a credit in the humanities. Then, you take a course in logic, since it is just like math.

Personally, I would rather see such students take a course in Shakespeare, but they don't see it that way. The same goes for some students in the humanities. They see that it is taught in the philosophy department and think it will be an easy way to satisfy a quantitative requirement. Some of these students don't do as well as they would like. But most of the students who take the course do so for the right reasons, and they do quite well. Besides, logic is fun!

Liang**:** What are the differences between philosophy and science, as far as teaching is concerned?

Achinstein**:** Well, in the sciences teaching is more structured than in the humanities. Students begin with certain basic courses and progress by taking courses in a prescribed order. This sort of structure is much less prevalent in philosophy and other humanities areas. So, in general, professors in the humanities can teach pretty much want they want without concern that they must conform to some pre-set structure and order. In some universities in the United States, there is a university academic curriculum committee to which professors must submit a course plan and get approval for a course syllabus which indicates what exactly will be taught in the course and the order in which it will be taught. We don't have this at Johns Hopkins. But even though there is no curriculum committee, the administration is still encouraging faculty members to provide the students with a detailed plan for the course in advance. Here is what I tell students in my courses: I will give you a list of readings for the course, and tell you how many exams and papers are required. But how many of these reading we will cover, and when, I try to leave open. Philosophy thrives on discussion. I like to pose questions to my students in lectures and discuss the answers they give. That makes them think, which is the most important thing to do in a philosophy class. Sometimes students don't volunteer any questions or engage in discussion. Sometimes they do, and that is unpredictable. But when it happens often it is the most exciting part of the course. This doesn't work if you have a fixed program in which everything must be covered in a certain order.

Fu**:** You claim that there is no absolute answer for philosophy questions, so how do you judge students?

Achinstein**:** By the originality of their answers and how well they are defended. In philosophy we need arguments, not just claims, and you can evaluate an argument even if it is not decisive. Philosophy is not mathematics. It is more like science at those periods when there is no mathematical or empirical proof of the claims being made. Still arguments in favor of a theory can be, and must be, given, even if they are not sufficient to decide the issue. In defending your philosophical (or scientific) theory, tell me what exactly it is supposed to explain and how it does so. Has it been developed with sufficient depth and precision so that it can be examined by others? How does it address this problem or that problem? What are its difficulties? In science you aim for experimental proof, something you don't get in philosophy. But in both you get arguments, some of which are much better than others.

Liang**:** What are the differences between undergraduate education and Ph.D. education?

Achinstein**:** Well, that is a broad question. Graduate education is much more specialized, even within a single field such as philosophy. Many graduate students come here saying "I want to work in this subfield on this problem or that." Yes, there's a certain distribution requirement you have to take even as a graduate student but it's minimal. For example, there are people who come here and take no philosophy of science. They don't want to do it, whether it is because they have no interest, or because it is too tough, or because I am too tough, I don't know. That didn't used to be the case. When I first came here, graduate students were forced to take a variety of different courses. You had to take courses in many different areas of philosophy. Now you don't. There used to be four general exams, called Qualifying Exams you had to take at the end of your second year, in various areas in philosophy. You had to know enough in these areas to teach basic courses in them. Over the years this distribution requirement was modified and finally dropped. So, whether this is for the best or not, graduate education has become much narrower and more specialized.

By contrast, undergraduate education is still fairly broad. You can't just take all physics or all philosophy courses. Generally speaking, the undergraduate students here are quite bright. It's become harder and harder to get admitted to Hopkins. That doesn't necessarily translate into being good at philosophy. That takes not only being smart, but a certain quirkiness of mind and imagination. You have to be willing to consider strange questions and to be happy with receiving answers that you can argue for or against without being able to establish their truth or falsity conclusively. You have to think differently.

***Liang*: ** I admire your life style so much. Many students, including me of course, are impressed by your sense of humor and great teaching skills. And it also amazes us that you published a new book in 2018 at the age of 83, when most people are already retired. What keeps you working so hard?

***Achinstein*: ** Well, first of all, you've got to be healthy enough to do it, you've got to love doing it, and you've got to be good enough to do it, or at least think you are. It is like people who play golf into their 80s or later. They keep playing because they can still do it, they love doing it, and they are good at it or at least have convinced themselves that they are. I think those are pretty convincing reasons.