Interview
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LIFE

1. Your life is entirely characterized by a continuous interaction between philosophy and science. Why did you develop such twofold interest? Was it natural for you to reconcile these two different approaches to knowledge?

I grew up in Cologne in Germany; I was there till 1938, when the Nazis came to town. And then my family and I, I was just a boy, left for the United States. I picked up a book in a German series which was called *Culture of the Present Time* (I am translating, of course) and it was on philosophy, a general book on philosophy. It started to tell about Aristotle and his idea of four different kinds of causes and so on. And this interested me very much. So I very soon realized that you cannot deal with these questions just by being intelligent; you have to have scientific knowledge, so I decided that I would study both science and philosophy, because you couldn't do the philosophical work that I was interested in doing without it.

In the United States, as you know, you study first for your Bachelor's Degree and then sometimes for a Master's degree and then for a PhD. And I did exactly that. I majored as an undergraduate in both mathematics and philosophy, because I knew that mathematics was important for physics, and I knew that physics was important to understand the structure of the universe. So I took the Bachelor's Degree (Wesleyan University, Middletown, Connecticut) in mathematics and philosophy, the Master's Degree (Yale University) in physics, and then *en route* I took the PhD (Yale University) in philosophy of science. I wrote a PhD thesis on what are known as Zeno's paradoxes. It was published as a book called *Modern Science and Zeno's Paradoxes*. When I was a graduate student I concentrated of course on philosophy of science, and my PhD supervisor was Carl Gustav Hempel, who was a wonderful and eminent teacher. And this was in brief my educational career.

2. You never speak of some conflicts between a humanistic approach that is typical of philosophy and a more rigorous character that is typical of science…

Well, I didn't feel this difficulty because my sympathies were entirely with the scientific approach. I mean I have nothing against what might be called a humanistic approach, provided that good arguments are given for it. And not just what somebody would like to believe.

3. Your work started in the wake of generalized excitement for astonishing scientific discoveries in physics, such as Einstein's theories and quantum mechanics. Certainly, the twentieth century will be remembered for the close contact and interconnection between science and philosophy. Do you think that philosophy still has the same possibility, as it had in the past during scientific revolutions, to give its contribution to the advancement of the scientific knowledge?

Yes I think the job of philosophy in part is to provide careful logical analysis of issues. And that task never stops, because as scientific knowledge develops, new issues arise. So I think the task of philosophy remains to be a kind of logical "watchdog", offering *critical scrutiny*, and that is an ongoing task.

PITTSBURGH

4. You are the main founder of the Center for Philosophy of Science at the University of Pittsburgh, one of the most important academic research institutions where to study and do research in the field of philosophy of science. Could you tell us how you came up with the idea of creating such a place?

I had a very good inspiration. When I was just a beginner in the academic world, there was the first center for philosophy of science in the United States, founded by Herbert Feigl, who was a Viennese originally. He was a wonderful human being in addition to being a good organizer. He invited me as a beginner to come to the center, which was at the University of Minnesota in Minneapolis, and to participate in discussions, workshops and so on. And so I got the idea that I would like to run such a center, not in competition to Feigl's, but as a kind of extension. So when I was invited by the University of Pittsburgh (Pitt) to create, from scratch, a philosophical enterprise, I told the chancellor and the vice-chancellor involved that I would very much like to create a center for philosophy of science. And they said: That's wonderful, we would be happy for you to do it. And they supported me and they said that if I acted as a magnet to attract other people, they would be very happy to appoint them. And I did that, because all kinds of famous people then decided to come to Pittsburgh. It was very exciting and I loved it. I started the Center in 1960. But I also energized the Department of Philosophy at Pitt by recruiting people in several philosophic specialties.

5. What was the philosophical climate before the creation of the Pittsburgh Center for Philosophy of Science in the United States?

It varied. In some places philosophy of science was already appreciated if not cultivated, but at least appreciated. In other places it was almost a cemetery, and so I decided it was a good place to begin. Then of course we started to attract people. And people wanted to become what we call 'visiting fellows' of our Center. We have a program to bring people from all over the world (scholars in philosophy, philosophy of science, psychology, cognitive science) who come and spend a term or a year working with us and connecting up with other fields at the university, such as physics. We have a very strong group in relativity theory in physics, which is one of my main interests in the philosophy of space and time. We have published a series of volumes called the *University of Pittsburgh Series in the Philosophy of Science*.

6. Do you think that there is a genuine interest of scientists for philosophy?

That varies greatly. For people who have had a European exposure generally the answer is yes. Others are either indifferent or hostile. And the reasons why some are hostile are understandable: they have encountered philosophers who knew nothing about science but told everybody what made the world go round.

And they didn't like that. And so these are people who will understandably stay away from philosophers.

7. In your opinion, what does Pittsburgh represent today, what is its role in the contemporary philosophical world?

Well, there is more in philosophy than philosophy of science. After I had been at Pitt for a decade, by 1971, I created a special department, the department of history and philosophy of science, because the history of science is also very important for philosophy of science. But there is a general philosophy department (for people who are interested in ethics and other fields of philosophy), and they are, of course, perfectly respectable. They are not part of our center, but we collaborate, especially in the training of students. We have lectures and visitors of all sorts, so it's a very active give-and-take between different areas of philosophy, philosophy of science and the general university environment. It's very good.

PHILOSOPHY OF PHYSICS

8. You said that it's important for philosophy to have a connection with the history of science. Do you think that this could be useful for science as well? For physics, for example.

Very important. For this reason: we know, as has been said, that the history of science is the history of abandoned theories. We know superseded theories, such as Aristotle's biology, the phlogiston theory of chemical combustion, and so on.

These developments were very important because you learn from the history of science that theories are replaced by other theories, we hope by better ones, but they are replaced. And some of the ideas that were leading in physics, for example early in the nineteenth century, are not known to most physicists now: they learn contemporary physics. But students of the history of science and of the history of philosophy of science of course study these developments, because it shows them different conceptualizations of similar observational material.

9. First of all, your work was related to questions of philosophy of physics. What was the role of Poincaré's conventionalism in developing your conception concerning the epistemological status of geometrical conventions in physics?

It was very important. I'll say this about Poincaré: he was a great mathematician and he had a very clear mind, but when he wrote on philosophical topics, he sometimes wrote in a very undisciplined manner. For example he wrote a paper on conventionalism, which I read many times over; it was so awfully unclear. It made me really unhappy that this brilliant mathematician was writing like this. I must refer you to my careful clarifying account of Poincaré's geometric conventionalism in my 1973 treatise *Philosophical Problems of Space and Time*, 2nd edition, chapters 1C and 4B. *But* of course I read his *Science and Method* and the three books of his *The Foundations of Science*. Then he introduced a lot of problems in physics and mathematics, so in that sense he was a pioneer also in the philosophy of science. But as I said, you have to be very careful because he is often not very clear.

10. Certainly, within the neo-positivist approach to philosophy of physics, it was Reichenbach's work about the nature of space and time that emphasized the empirical character of the metric.

No, he did *not* emphasize the empirical character of the metric; but the reason for that was complicated because he thought that congruence is a matter of stipulation. Then he said that, once you have defined a standard for congruence, for equality of length and for time intervals, it becomes an empirical question what the actual geometry is. So it was a sophisticated notion, and I was tremendously impressed by Reichenbach. Very early in my studies, when I first studied physics and philosophy, I read his 1928 German book *Philosophie der Raum-Zeit-Lehre*, and later (1957) Maria Reichenbach translated it into English as *Philosophy of Space and Time*. Reichenbach was for me a lodestar, a guiding inspiration.

With the rebuttal of the Synthetic a Priori (typical of the neo-positivists), Reichenbach assumed that the choice concerning a spatial metric is specified by a coordinative definition of congruence.

Yes, that was another way of saying that the statement that such and such will be the standard is a stipulation, it's not discovered but it's conventional.

> And he upheld this thesis also in the context of his distinction between universal and differential forces.

Yet with respect to the so-called universal and differential forces, Reichenbach was not very clear. But first let's say this: gravitation is a universal force in the sense that it affects all things *alike*. But, differential forces are forces that affect different materials to a different degree. For example heat would expand a piece of metal more than a piece of wood, or than a stone. So these are so far the differential forces, but Reichenbach unfortunately also used the notion of universal force as a *metaphor*, as a pictorial device for discussing different standards of congruence. For example if you take a rod, that's one criterion of congruence. But if you say that when the rod is transported, it does not remain self-congruent, Reichenbach describes that by using the term 'universal forces' metaphorically to say that all sorts of solid rods are being deformed *alike* under transport.

But that's just metaphorical talk, and so I suggest to get rid of it, and to talk about universal forces only when we mean the *literal* sense like gravitation. Now, gravitation is *not only* a universal force, *but also* a differential force. For example if you have a bookshelf made of wood and the books are very heavy, you'll see that it will bend. But if you have a bookshelf made of steel, it's almost not going to bend. And that's all because of gravity; so gravity also is a differential force and not only a universal force.

11. Your position is, in some respects, coherent with that of Reichenbach. What are the affinities and the differences between you and the neo-positivist tradition?

I think that logical empiricism, and neo-positivism sometimes, had a good influence in the sense that it called for clarity in discussing issues in philosophy, and from that point of view it was very good. But in other ways I think it was not very good. There is what I call phenomenalist positivism, which essentially says – Ernst Mach said it – that the world is just a bunch of sense-data. But the world is the world and we, because the world affects our bodies, have sense-data, but the world doesn't consist of sense-data. So there has been a lot of confusion about this, and that is so-called phenomenalist positivism, which I never accepted.

12. In some pages of your book *Geometry and Chronometry in Philosophical Perspective* (e.g., p. 250) you try to explain the differences between your sort of geometric conventionalism and what you called "trivial semantic conventionalism". What exactly are the differences between your conception of a coordinative definition of congruence and a mere semantic stance? If this is not a position concerning only semantic values, what motivates the choice of one definition of metric instead of another?

Everybody knows that words, which are noises, can be used to refer to things. And that's true for words in English, in German, and so on. It's trivial because everybody knows it. But in theory of measurement, the question arose (and was asked already by Ernst Mach, the Austrian physicist): what evidence would you give that, if you applied the meter stick here and if you then applied it there, it remained congruent to itself under transport? And that is not a matter of fact, but this is a matter of convention, though not in a very obvious or trivial sense. This requires a little reflection.

> Do you mean that a coordinative definition of congruence when we talk about a stick involves a reflection about physical theory, so self-congruence under transport is not a trivial semantic definition? If this is not a position concerning only semantic values, what motivates the choice of one definition of metric instead of another?

The famous German mathematician Bernhard Riemann, when he became a professor, wrote his Habilitation Lecture *On the Hypotheses which Lie at the Foundation of* Geometry. There he discussed the question of congruence under transport, and here is what he said: The definition that the meter stick remains congruent to itself depends for its *consistency* on a physical fact that is not a matter of definition.

And that fact is the following: If you have two coinciding meter sticks and you bring them to another place via *different* routes, they will still remain equal to each other. If they didn't do that, it would become inconsistent to use them, depending upon *which one* you use. It would be chaos. We would never have developed the theory of solid bodies the way we did.

So that was Riemann's point, and it was a recognition that a convention is made possible by certain facts of the behavior of bodies. It was very insightful, and I discussed it at the beginning of my treatise *Philosophical Problems of Space and Time*.

13. Besides philosophy of space and time, your interest has also been concerned with broad epistemological questions, such as the Duhem-Quine thesis about the limits of confirmation and discreditation of individual scientific hypotheses. Contrasting Duhem and Quine, in one of your famous articles, *The Falsifiability of Theories*: Total or Partial? A Contemporary Evaluation of the Duhem-Quine Thesis, you claim that it is possible to realize genuine crucial experiments, that is, to produce a direct falsification also of individual hypotheses. This position seems to support a kind of falsificationist epistemology as proposed by Popper. What distinguishes your view from that of the Austrian philosopher?

He just talked about it in general terms. He didn't discuss these concrete questions, and when he did, it was very unclear. Let's start with the very simple example of *modus tollens*: If A then B, not B therefore not A. But Duhem looked at the confirmation and at the refutation of scientific theories. He noted that when you take a hypothesis and you want to see whether it's well supported by the evidence or refuted by the evidence, you do not test only this hypothesis in isolation from everything else, but this hypothesis in conjunction with other auxiliary hypotheses. The combination of them gives you a prediction. And then you check that prediction, whether it's correct or not, and if it isn't, then it is not clear where the trouble is. And that makes it difficult; therefore Duhem said that there are no crucial falsifying experiments and also no crucial verifying experiments.

Well, what I did was look at some examples in physical geometry and I found an example – but that didn't prove anything in general – in which you could isolate a hypothesis epistemologically.

So that's when I got involved with Quine: I tried to make use of the idea of what he called the holistic character of knowledge in his general philosophy. But I wrote to him (and I published the letter), and he acknowledged that he didn't mean to say something as strong as he had been taken to claim.

Well, do you think that a scientist has a rational criterion to define what is the hypothesis that is responsible of the failure of a theory? Or is it something like an intuition?

Much more the latter, because they focus psychologically on particular hypotheses. Those are the ones that they worry about, the others they take along as baggage. And that's how they work, but that's ok. Duhem was the one who said: just wait a minute, think about this more carefully. And he certainly did.

PHILOSOPHY OF PSYCHOANALYSIS

14. The reference to Popper gives us the opportunity to introduce another aspect of your work. Popper, like you, has dedicated part of his efforts to criticize the methodology adopted in the field of psychoanalysis. Even if both you and Popper are not admirers of the psychoanalytical framework, your opinions diverge. Can you clarify in what consists the difference between your critique to psychoanalysis and Popper's?

I learned a great deal from thinking about what Popper had said. But I don't think he was a very good philosopher of science. A lot of what he wrote was very poor, and he is greatly overrated I believe. First, he knew almost nothing substantive about Freud's psychoanalysis. But he wrote about it. And he grew up in Vienna. After all, that's where Freud was working. Popper's work on psychoanalysis was extremely uninformed. For example his statement that it's an unfalsifiable theory. Well, I have shown easily that it's falsifiable by reference to Freud's etiology of paranoia as set forth in his paper on a case of paranoia, which runs counter to the psychoanalytic theory of the disease. There Freud discusses what sort of evidence would have shown that his theory of paranoia was incorrect. And Freud had published this in 1915, two years before Popper said that psychoanalysis is unfalsifiable, which is wrong.

Secondly, it's wrong to say that falsifiability is *the* criterion for the rational acceptability of scientific theory. It's certainly relevant to it, but to say that falsifiability *is* scientificity, that's sloppy and wrong. I wrote an essay in which I show in detail all the mistakes Popper made re psychoanalysis, but he got me interested in psychoanalytic theory, so I owe him that I started to study it, thinking that there must be something wrong with what he had said.

And then the psychoanalysts started to get very nervous about what I was doing because they saw that I was raising serious questions, and some of them were very encouraging, others thought that I was a dangerous man.

15. Do you think it is possible and desirable to find, for Freud's theory, more extra-clinical confirmations that are different from those obtained through the normal psychoanalytical setting?

Yes I do. You see, the trouble is, psychoanalysis began as a clinical theory based on working with patients. That's totally understandable. But the problem was that Freud and his followers then decided that it's from the clinical situation between a therapist and a patient that all the evidential material comes from, and has to come from there. So the first thing they did not consider was running experiments which are much better controlled than to sit there and talk to their patients. Therefore the epistemology of the theory is much more complicated than they recognized.

And also Freud himself recognized that in looking at the clinical responses of patients, you have to realize what the power of suggestion is. If I am your therapist and you are my patient, and I lead you to say that certain things bother you and other things mean a great deal to you emotionally and so on, I am very often leading you by suggestion to produce compliant responses, responses in which you will do what I expect, trying to please your therapist, because you depend on me for emotional health and support. And that could be very misleading.

This is what happens of course, I believe, in religious counseling when people go to priests or rabbis or ministers or whatever: They are their telephone wire to god. So I said to the psychoanalysts: How do you know that what your patient is producing is a real response or just an attempt to please you? And you know, Freud had a very important colleague, Wilhelm Fliess, who made him very angry because he asked Freud: How do you know that you're reading your patient's mind instead of reading your own mind?

16. More than a rebuttal of psychoanalytic theory, your work is also a critique of the hermeneutical interpretation given to it by authors such as Habermas, Ricoeur and Klein. Do you think it is possible that

psychoanalysis regains a naturalistic stance as hoped by Freud in an early version of his thought?

Freud said: psychoanalysis is a natural science, and asked what else could it be? Yes, he thought that, certainly. And he thought that the method of free association can be used to discover the causes of people's behavior. But, in my writings, I have offered a careful critique of the claim that free association is a *causally probative* method of investigation.

So do you think that psychoanalysis can work in the manner of natural sciences? How?

Yes, but I think it has to be done carefully, and people have to use good criteria of evidence, as I have tried to show in my writings.

17. Do you think that the development of a 'scientific' theory of mind and of the unconscious is possible, without however subscribing to a physicalist reductionism?

Yes. I think it's a prejudice that if a claim is scientific, meaning that it's based on evidence and good reasoning, then it has to be reductive physically or biologically. That's just a sloppy mistake. Not primarily a mistake by those who believe in the scientific approach to cognition, but in the minds of those who are opposed to it.

18. Do you think that today it is still reasonable to work at defining a sharp distinction between what has to be conceived as pure science and what doesn't?

No I don't, and I'll tell you why. Because such a sharp separation *would* provide a criterion of demarcation, but there is no general criterion of demarcation. There is a very valuable paper by my former colleague Larry Laudan called "The Demise of the Demarcation Problem" (1983, *Physics, Philosophy and Psychoanalysis; Essays in Honor of Adolf Grünbaum*, R.S. Cohen and L. Laudan Eds.). There he discusses the different attempts to provide a general formula of demarcation. I don't think there is a general formula.

THEISM

19. Over the last twenty years your works have also been dedicated to an epistemological analysis of the theistic stance. In one of your most relevant articles about this topic you consider Leibniz's critical question *Why is there something contingent at all, rather than nothing contingent?* with the aim of showing that no theistic stance is able to provide an epistemologically sound answer.

But why *should* there be just nothing? That has to be shown, and they haven't shown that. So they assume something just to get the question going and then they demand an answer to it. But that procedure begged the question.

20. What kind of reader did you have in mind when you started writing about it?

Well, for one thing I believe that all claims to knowledge have to be examined, and that includes of course stances about the whole world and what caused it and so on. Some people think scientific skeptics like me believe that they know it all. That's not so. It's the religionists who claim that they know how the world came into being and what makes it go around. They are the ones who think they know it all. And then I ask them: show me. I think the arguments for the existence of god are very poor.

Do you think that your analytic argumentation could somehow matter to believers?

Yes, i.e., to those who don't think that it's just a matter of faith, but who think that there are arguments in so-called natural theology. They believe that arguments can be given. They and I agree that the matter has to be discussed in terms of arguments. Where we part company is when they think the theistic argument is good but I think the argument is very bad.

What feedbacks have you received from them?

All kinds. There is Richard Swinburne with whom I debated publicly, and there is Craig who has written about this. I have replied polemically to both of them.

21. Do you consider your work, in some respects, a continuation of the neo-positivist battle against metaphysics?

Well, I would say that it's too general. I don't think there is a sharp divide between metaphysics and philosophy of science at all. I think there is continuity. I think that the great merit of the logical empiricist movement was to raise carefully questions such as: *What do you mean by this?*, *How do you know it's true?*, *Why should we think it's true?*. These are all constructive questions, and I think any good epistemologist would welcome this. Kant was obviously interested in epistemology and he would have thought of the positivists as continuing his tradition. So I think to say that metaphysics is bad is a mistake. That's just name calling and it seems to me to be unhelpful. The thing to do is to ask *What do you mean?* and *How do you know?*. And those are good questions. And if positivists ask them, I say good for them! And if religious people ask them, I say good for them!

22. In which way do you think science can influence a religious way of thought? We know that a direct conflict between science and religion is not the best the way to deal with the situation. So maybe science needs a different strategy, maybe it should work, like religion, from inside the society?

I think that we should, not only in scientific pursuits, but also in discussing religious beliefs, do our best to ask ourselves, *What is the evidence?*, *What do you mean?*. And if it's clear what they mean, then: *How do you know?* These are the questions that I think should always be asked.