J Gen Philos Sci (2016) 47:1–10
DOI 10.1007/s10838-016-9323-2

OBITUARY

Patrick Suppes: A Profile

Carlos Ulises Moulines[1]

Published online: 12 February 2016

Patrick Suppes was one of the most influential philosophers of science worldwide during the second half of the 20th century and the beginning of the 21st. A number of distinguished contemporary philosophers of science, such as Nancy Cartwright, Michael Friedman, Ian Hacking, Joseph Sneed, Frederick Suppe and Bas van Fraassen, among many others, have publicly and repeatedly expressed their deep intellectual debt to Pat Suppes. Some of the philosophers of science influenced by Pat's work were close acquaintances of his; others (like myself) have had only a casual contact with him; still others might have never met him personally; but, at any rate, there is a huge number of people in the departments of philosophy of science all over the world whose teaching and research would have become very different if they hadn't read and/or heard what Suppes had to tell about the appropriate contents and methods of philosophy of science. But Suppes was not only a great philosopher of science. He also proved to be a very able scientist, even an *experimental* scientist, especially in the fields of psychology and psycholinguistics. And he also was a remarkably successful entrepreneur in the business of computer-assisted education. This is a quite striking combination. As a matter of fact, I know of no other similar case in the 20th century. One should probably look back to the beginnings of the modern age to find other examples of persons successfully combining philosophical originality, experimental skills, and a flair for business. Suppes is famous for having stated on several occasions, in a mixture of pride and self-irony: "I am the only genuinely empirical philosopher I know". I agree with this self-portrait only partially since I would add: "Suppes was the only genuinely empirical philosopher *and* successful businessman I know"…

It should be stressed quite from the beginning that Suppes' work didn't lead to a unified school of thought. There is no "Suppesian system", neither in general philosophy, nor in

✉ Carlos Ulises Moulines
moulines@lrz.uni-muenchen.de

[1] Fakultät für Philosophie, Wissenschaftstheorie, und Religionswissenschaft, Ludwig-Maximilians-Universität München, 80539 Munich, Germany

the foundations of science. At most, one could see a unifying methodological thread in his emphasis on the use of set-theoretical tools when analyzing philosophical and scientific matters. Certainly, there is some kind of intellectual affinity between Suppes and former prominent philosophers of science of the 20th century, in particular the Vienna Circle and its "fellow travellers". This affinity lies in the significance attributed to science for philosophy, in the use of formal methods of analysis, and in the overall striving for clarity and precision. But that's all. Nothing would have been more alien to the Suppesian attitude than the attempt at a conceptual unification of knowledge as in Carnap's *Aufbau*, or at a linguistic unification of science as in Neurath's project of the *Encyclopedia of Unified Science*, or at a methodological unification as in Popper's falsificationism. Suppes emphasized more often than not that scientific knowledge appears as essentially *fragmentary*, even when restricting our attention to one particular discipline like physics. It is fragmentary conceptually, theoretically, and methodologically. It's a *patchwork*—as Nancy Cartwright, would say. And the first duty of the philosopher is to take account of this fact. In sum, Suppes was, in his whole intellectual career, by the scope of his interests and the methods he used, a *pluralist* thinker through and through.

Due to my own limitations (much narrower than those of Pat Suppes), in this memoir I'll concentrate on Pat's contributions to the philosophy of science—the area where I feel more at ease. Nevertheless I'll also try to provide some clues for the appraisal of the other aspects of his work.

Patrick Colonel Suppes was born on March 17, 1922, in Tulsa, Oklahoma. His father and his grandfather were in the oil business. His mother died when Pat was only 4 years old. He was the only child. Some time later his father remarried and Pat was raised by his stepmother. She took much care of him and, in due time, encouraged him to develop his patent intellectual capacities to get a higher education, in spite of his father's wish that he took over the oil business. (His father was, at any rate, tolerant enough to accept that his only child should try an academic career.) Many years later, when Pat already was a young man, he got a half-brother, George, to whom he developed quite strong ties that lasted their whole life. Soon, it became clear that Pat was quite gifted for physics and mathematics; on the other hand, already in his adolescence he developed a sense for arguments in philosophical matters, apparently due to his stepmother's influence, who was a Christian Science activist. He made his first year college at the University of Oklahoma, in 1939. Since he felt bored there, he decided to continue his studies at the University of Chicago; but, because of family reasons, the next year he came back to Oklahoma, to the University of Tulsa, and eventually obtained his major in physics there. In 1942, he was called up in the Army Reserves. For a while, he was not sent to the front and was allowed to return to the University of Chicago in order to obtain his B.S. in meteorology in 1943. The US Army badly needed competent meteorologists to pursue its war efforts in the Pacific, so that Pat was first sent to a lonely base on the Solomon Islands; there, to fight boredom, he read a lot of Aristotle and learnt French. Afterwards, he was sent to the island of Guam, where his military duties were more time-consuming. He wasn't discharged of the Army until 1946, with the grade of a captain.

By that time, Pat had already decided not to pursue the career of a professional physicist but rather to study philosophy, and more particularly the philosophy of science, and more particularly still, the *formal* philosophy of science. Thus, in 1947 he went to make his doctoral studies at the Columbia University. Ernest Nagel was there and he already was a highly reputed philosopher of science. On several occasions afterwards, Suppes has recognized his great intellectual debt to Nagel, both for the content of his courses but still more for the way Nagel presented the stuff—unpretentious, distanced, and systematic. Pat had envisaged to write a PhD thesis on the formal axiomatization of mechanics under Nagel's supervision. But Nagel persuaded him that, given the profile of the Philosophy Department at Columbia, it appeared "politically safer" to undertake a rather historical kind of investigation. Thus, Pat's dissertation became a partly historical, partly conceptual analysis of the idea of action at distance in Descartes, Newton, and Kant. This may at least in part explain a feature of Pat's interests that accompanied the whole of his intellectual career and that may seem a bit astonishing for someone who is mainly known for his work in formal philosophy and empirical science: his genuine, profound knowledge of the great thinkers of the classical tradition in philosophy, especially Plato, Aristotle, Descartes, and Kant. Suppes obtained his PhD from Columbia University in 1950.

Soon afterwards he got a position as a young lecturer in philosophy of science at Stanford University. And thus began Pat's extremely rapid academic career. Rung after rung he quickly climbed the steps of academic recognition so that, a few years after his arrival at Stanford, he already enjoyed a high reputation in American philosophy and science, and started to attract a number of quite able disciples. The crucial first step for this development was Pat's meeting with John McKinsey in Stanford. McKinsey was a logician, one of Alfred Tarski's most promising disciples, who unfortunately died prematurely a few years after his acquaintance with Suppes. McKinsey shared with Suppes the enthusiasm for formally axiomatizing not only mathematical, but also empirical theories. But he convinced Suppes that the best way to do this was not by applying first-order logic, as so many formal philosophers of science had tried to do before, but to use the tools of ("naïve" or semi-formal) set theory.

As the result of the close collaboration between Suppes and McKinsey, the first adequate set-theoretic axiomatization of a physical theory emerged: "Axiomatic Foundations of Classical Particle Mechanics", which Suppes published together with McKinsey and A. C. Sugar in 1953. This seminal paper represents a milestone in the program of formally reconstructing empirical theories which has been so characteristic of much of formal philosophy of science in the second half of the 20th century. Suppes' well-known slogan "To axiomatize a scientific theory is to define a set-theoretic predicate"—a methodological principle one generation after another of philosophers of science (including myself) have followed—has its roots in his collaboration with McKinsey. Thus was born what came to be known as the "Stanford school in philosophy of science"—a very influential school in the second half of the 20th century, to which a younger generation of philosophers of science like Ernest W. Adams, Nancy Cartwright, Ian Hacking, and others might also be ascribed. As I said at the beginning, one should not expect here the presence of a philosophical school in the usual sense, but rather of a common attitude, a "mentality" if you wish, when confronting philosophical issues—namely a sort of "pluralism" in a wide sense.

It may be asked what is so revolutionary in Suppes' set-theoretical approach to the reconstruction of theories. The answer is twofold: On the one hand, it leads to a completely new conception of the "essence" of scientific theories; on the other, it allows a clear and precise analysis of two pivotal notions in the foundations of science: *representation* (of one structure by another) and *invariance* (of structures). I'll deal with the second aspect later on. Here, I'll say some words on the first aspect. For centuries (actually since the Ancient Greeks), philosophers and scientists, when asked what makes the identity of a theory, would typically answer "a set of propositions". And if you want to be more precise, you have to list a number of axioms expressed in a given language, preferably a formal language, and more preferably still the language of first-order logic. This was the common view more or less tacitly presupposed by all (formal) philosophers of science during the first half of the 20th century. Suppes radically departs from this point of view. He doesn't say that the formal-language, propositional approach to theories is completely wrong, but he makes two fundamental methodological objections to it: First, formalizing theories in first-order logic (or second-order logic, for that matter) is such a cumbersome enterprise that it is a practically impossible endeavour for theories with a minimal degree of complexity (like those of physics and other empirical sciences); second, it obscures many essential aspects of empirical theories (in particular representation and invariance). For Suppes, the adequate way to conceive of theories is not as linguistic entities (a series of statements or propositions), but rather as non-linguistic, model-theoretic, structural entities: A theory is a set of models. To be sure, such models allow us to formulate statements about nature, but this is a different issue: It doesn't belong to the identity of the theory. Suppose someone asks what is the "essence" of a theory like classical particle mechanics (CPM). The Suppesian answer to this question is this: This theory consists of a (potentially infinite) number of models (structures) determined by the set-theoretical predicate "*x* is a system of classical particle mechanics". This predicate may precisely be defined by exclusively using notions coming from (naïve) set theory. The objects to which this predicate applies (the theory's models) are structures of the kind "*<P,T,s,m,f>*", where *P* is a finite set of objects (called "particles"), *T* is an interval in the real numbers (whose elements are called "instants"), *s* is a twice differentiable vector function (called "position"), *m* is a one-valued function (called "mass"), and *f* is a three-valued function (called "force"). A structure of this kind Suppes calls "a possible realization" (of mechanics); it becomes a full model of CPM when me make sure that it moreover satisfies Newton's laws. It immediately follows from this way of conceiving theories that, normally, there will be a big array of genuinely different (non-isomorphic) models for a given theory. In the case of CPM we have one model representing the solar system, another one for the system Earth-Moon, another one for an oscillating pendulum, another one for a projectile, etc. It also naturally follows from this way of looking at theories that a further interesting question might be what relationships these models have between them—a question scarcely analyzable within the standard, "linguistic" approach to theories.

This way of looking at theories had a great impact on a younger generation of philosophers of science—not only in Stanford but elsewhere. It led to what since the 1970s has often been called "the Semantic View of Theories": Theories should not be conceived of as linguistic, but rather as model-theoretic (set-theoretically defined) entities. Actually, "*the* Semantic View of Theories" is something of a misnomer since, under this label, a number of quite different approaches in the philosophy of science have been subsumed, such as those of Bas van Fraassen, Ronald Giere, Frederick Suppe, and the structuralist reconstruction program initiated by Joe Sneed and Wolfgang Stegmüller, to which I myself have contributed. However, it is true that, in spite of the great differences of approach between these authors, they have a kind of "common denominator": Theories are not sets of statements but sets of models (or of more complex structures ultimately consisting of models). This common denominator has its roots in Suppes' work.

Of course, by arguing for a "set-theoretic preference", Suppes didn't want to imply that first-order logic was useless for the study of philosophy in general, nor for philosophy of science in particular. For him, as for any other serious philosopher today (so I hope), the first thing a student of philosophy has to do is to learn the essentials of formal logic. As a matter of fact, a few years later, in 1957, Suppes published one of the best logic textbooks that are still on the market: *An Introduction to Logic*. I even dare to say that, from a didactic point of view, this is the best elementary logic textbook I know; and I've used it for many years in my introductory courses in philosophy of science. But it is revealing of

Suppes' stance that the Second Part of this book is not about logic as such but rather about elementary set theory, including the methodology of axiomatization by means of set-theoretic predicates. That is, logic is the necessary but by no means sufficient first step to analyze formally issues in philosophy and empirical science. The universal language, the Esperanto for philosophy and for the foundations of science is not logic but rather set theory. I once heard Suppes say (I don't know whether there is some written record of this), paraphrasing the Wittgenstein of the *Tractatus*: "What can be said at all can be said set-theoretically". Of course, this should not be taken too seriously, but it should be taken as seriously as possible, at least in philosophical and scientific contexts.

Suppes was not only interested in the methodological application of set theory to matters outside mathematics; he also dealt with the foundations of set theory itself. The result of this interest was *Axiomatic Set Theory* (1960), another excellent textbook. Suppes' exposition of set theory here is based on the axiomatic system of Zermelo/Fraenkel, but it modifies it to a certain extent to make the construction of concepts more intuitive and better applicable to areas outside pure mathematics. The book is more than half a century old by now, but it continues to be a very good introduction to the subject for students of philosophy, mathematics and empirical science. In spite of his various and changing interests in many other areas of philosophy and science in subsequent years, Suppes remained essentially true to his "set-theoretical faith", as is shown by the compilation of much of his set-theoretical writings in the anthology *Models and Methods in the Philosophy of Science* (1993), as well as the latter *Representation and Invariance of Scientific Structures* (2002).

Besides publishing (mostly in co-authorship) some other articles on the axiomatic foundations of physics, in particular the special theory of relativity and quantum mechanics, in the mid-1950s Suppes began his studies on the foundations of the theory of measurement, a hitherto much neglected field of philosophy of science. Suppes rightly considered measurement theory as crucial to analyse the empirical foundations of theoretical science. Later on, on several occasions, he expressed his astonishment about the apparent lack of interest of most philosophers of science for the theory of measurement since this is exactly the kind of field where philosophers of science could do some work that would be philosophically relevant for general epistemology and at the same useful to the practising empirical scientists. The (philosophically crucial) question addressed by a foundational theory of measurement is this: How is it possible (and what does it mean) that pure mathematics is applicable to empirical, "qualitative" facts? The Suppesian answer is astonishingly simple: It means that there is a *homomorphism* from the qualitative structure considered into a numerical structure. A very simple example is this: Why is it that (rational) numbers can be applied to express the height of people, let's say a group of schoolchildren? Well, because there is a homomorphism from the structure $<C,T>$ (where C is the group of children and "xTy" means "x is as tall or taller than y") into the structure $<Q,\geq>$, where Q is the set of rational numbers. The (provable) statement that such a homomorphism exists is called "a representation theorem". Of course, in this example the representation theorem is very easy to prove; but in other cases the task of proving such theorems may be much more demanding. Now, the aim of the general theory of measurement is to find out the diverse (empirical) conditions under which such theorems may be proved. Suppes' work in this area addressed not only the general issues of measurement but contributed in a quite substantial way to clarifying the possibilities of fundamental measurement in different areas of psychology and economics which *prima facie* seemed not to be amenable to a (direct) quantitative treatment. For all these investigations he consistently made use of the set-theoretical methods of axiomatization he already had applied in the foundations of physics. In this research area he collaborated with many people, but above all with three other scholars associated with him for a long period of time: David Krantz, R. Duncan Luce and Amos Tversky. As a result of this long and close collaboration the three volumes of *Foundations of Measurement* were published—an immense, impressive work that has since become the "Bible" for anybody (philosopher or scientist) seriously interested in the vexing but most important issue of the "conditions of possibility of measurement" (to take up a Kantian mode of speech). The first volume of this path-breaking work appeared in 1971; the next two volumes appeared much later, in 1989 and 1990, respectively. The two later volumes are more technical and difficult to digest for anybody who is not a full specialist on the matter. But the first volume of 1971, though not being completely easy to read, may be assimilated with much gain by students of philosophy or science interested in the quantitative foundations of empirical science—in any discipline whatsoever. The book contains many simple, as well as not so simple, examples from physics, psychology, and economics. I have often used this book for some of my more or less advanced courses in the philosophy of science, and my students have become as fond of it as myself.

From the mid-1950s on, while not abandoning his previous interests in the foundations of physics and mathematics, Suppes began to be more and more devoted to the foundations of the social sciences, especially psychology and economics. This led him to found, together with Kenneth Arrow, the *Institute for Mathematical Studies in the Social Sciences* (IMSSS)—an institution that soon was to become the paradigm for genuine, well-founded interdisciplinary research. It substantially contributed to the world-wide recognition of Stanford University as a unique centre for interdisciplinary studies. Suppes headed the IMSSS for a very long period of time—from 1959 to 1992. In a sense—and without wanting to underrate the value of the contributions of many other distinguished scholars to this center, one may well say that the IMSSS was the creation and at the same time the intellectual home for Pat Suppes. He left his direction only under the "hard blow" of his academic retirement.

Now, Suppes proved to be not only a brilliant theoretician and scientific administrator in the foundations of the social sciences. More or less during this same period (the 1950s and 1960s), he also engaged in a long series of purely empirical investigations in psychology and economics. He carried out many real-life experiments in these areas and translated the results into a convenient mathematical framework. Two prominent examples of this line of research were his experimental studies in utility and subjective probability in cooperation with Donald Davidson (a Stanford colleague of his at the time), and the application of stimulus sampling theory to learning theory in cooperation with William Estes, where experiments (and their mathematical interpretation) also played a great role. The analysis of mathematical concept formation in children as well as language learning constitute two later examples of this kind of endeavour in experimental-mathematical psychology. As a matter of fact, if we examine the Suppes bibliography from the mid-1950s to the mid-1960s we find out that the majority of his publications are not on philosophy of science in general, not even on philosophy of physics, but on down-to-earth experimental psychology.

But apparently Pat Suppes felt a bit bored in devoting his efforts "solely" to philosophy of science and (experimental-mathematical) psychology: Beginning with the 1960s he perceived the great future that lay ahead in the implementation of computers for all kinds of tasks, both practical and theoretical. He had already been interested in didactic issues for a while, and so he was probably the first to recognize the immense value computers could have in education. Already in 1962 he inaugurated a computer-based laboratory to investigate the best way to teach and learn logic. And a bit later, in 1967, he founded, together with Richard Atkinson, a company, the "Computer Curriculum Corporation", to produce computer-assisted instruction. The company's first years were financially difficult because most people still didn't perceive the potential of this kind of computer application. Suppes was just ahead of his time. But, gradually, the company became a great success and Pat made a lot of money from it… In 1966 he made a now famous prediction: "In a few more years, millions of schoolchildren will have access to what Philip of Macedon's son Alexander enjoyed as a royal prerogative: the personal services of a tutor as well informed and as responsive as Aristotle." At the time, most people thought this was a notorious piece of wishful thinking. We don't believe this anymore. Already in the 1970s, the teaching of elementary logic at Stanford University had become computerized and Suppes sold his programs to other institutions. I well remember my first visit to Pat in 1978: After a very pleasant conversation during lunch-time, he told me he wanted to show me the thing he was most proud of. We went together to an immense hall where dozens of computers were installed on a row. "Here students learn logic", he told me, and added with his characteristic malicious smile: "Very soon, we logic teachers will have become completely obsolete…". As a result of his didactic motivations, Suppes also founded during this time the *Education Program for Gifted Youth* at Stanford University, which he headed until 2010.

Beginning with the 1970s, another important shift in Suppes' philosophical evolution took place: the development of a new, highly original approach to probability in connection with causality. His first essay on this line of research was *A Probabilistic Theory of Causality* (1970); a more systematic treatise is *Probabilistic Metaphysics*, of 1974, which grew out of a series of lectures held in Uppsala. Suppes' contribution to this area of philosophy of science (and philosophy in general) is also path-breaking and has strongly influenced discussions on causality up to the present-day. For a long time, philosophers and scientists had taken as indisputable evidence that causality and determinism go hand in hand: causes necessarily determine their effects. A world where events are causally connected with each other cannot but be a deterministic world. Now, with his incorruptible sense for analyzing philosophical issues without dogmatic preconceptions, Suppes convincingly argues that there is no good reason for closely tying causalism with determinism: Causal talk, whether in common sense or in developed scientific theories (like physics), makes perfectly good sense, even if we are not committed to a deterministic worldview. In fact, Suppes thinks that causalism is highly plausible, whereas determinism is highly implausible: It is highly plausible to say that John's careless driving will sooner or later *cause* a car accident, though the connection between careless driving and accidents is only probabilistic in nature. The same goes for the more sophisticated areas of empirical science, like biology or physics. This is so not only in the case of quantum physics, where the disconnection between the idea of one event causing another and the idea of a deterministic law connecting both is apparent, but also in classical physics where predictions based on, say, the laws of classical mechanics can only be made more or less probable. In short, causalism is good metaphysics, determinism is bad metaphysics. To analyze the general notion of a cause, Suppes draws on Hume's classical definition (causes and their effects are contiguous in space and time, causes precede their effects, and causes are regularly followed by their effects), but to avoid intuitive counter-examples (like the well-known barometer fall "causing" the ensuing storm), he takes recourse to probability in a rather simple though convincing way; in his own words: "one event is the cause of another if the appearance of the first event is followed with a high probability by the appearance of the second, and there is no third event that we can use to factor out the probability relationship between the first and second events". This simple idea continues to be the point of departure for present-day developments (surely more sophisticated technically) in the probabilistic theory of causality. It is also interesting to note in this context that Suppes was the first consistently to construct a *qualitative* notion of probability. This proves to be very useful for many areas (be they common-sensical or scientific), where there is no prospect of defining an appropriate (quantitative) measure of probability.

The culmination of Suppes' lifework may be seen in the publication in 2002 of his treatise *Representation and Invariance of Scientific Structures*. Here we have a systematic, detailed, and unified exposition of practically all topics Suppes dealt with since the 1950s. A previous draft of this work in a mimeographed version had been circulating among interested scholars since the beginning of the 1970s under the title *Set-theoretic Structures in Science*. (I myself had the privilege to get a copy of it from Pat himself—a copy I often used for my own courses). However, it took more than thirty years until he decided that the time was ripe for publication. The book can be divided in two big parts. The first is on the general philosophical topics to which Suppes devoted his life: the advantages of the set-theoretical approach for analyzing scientific theories, the notion of a model, the foundations of measurement, and, above all, the great significance that the notions of representation and invariance have for analyzing the content of scientific knowledge. These notions had already played a more or less prominent role in Suppes' previous writings, but now they become absolutely central (hence the book's title). When discussing *Foundations of Measurement* we have already seen what it means to say that an empirical structure may be represented by a numerical one. But now the notion becomes generalized: Given certain conditions, a particular kind of structure (whether numerical or not) may be used to represent another particular kind of structure. And the proof that this is possible (a fundamental task for the philosopher of science according to Suppes) is precisely a representation theorem. The general idea is this. Suppose you have a non-categorical theory, i.e. a theory where the models are generally not isomorphic to each other (which is the case for practically all empirical theories). Then, an important task for the philosopher of science is to show, by means of a representation theorem, that there is a distinguished subset of models such that any other of the theory's models stands in the relation of isomorphism (or, more generally and more realistically, of homomorphism) with some model of the distinguished subset. Then, in a sense, one may say that this distinguished subset is the theory's real content (its "world").

For Suppes, the notion of representation, and the associated notion of a representation theorem, is (not always but usually) connected to another central metatheoretical notion, that of invariance (and of an invariance theorem associated with it). Invariance makes explicit what remains preserved when representing one structure by another. To take again our simple example of representing the height of a group of people by means of the ordering of rational numbers: We can represent that height in terms of centimeters or in terms of inches; the numerical values will be different, but the numerical ordering (representing the qualitative ordering) remains the same, i.e. it remains invariant through changing units. The system's property of being ordered in a certain way is thus preserved. Again, in this example, the proof that this is so is very simple; but in other cases, especially in physics and in more complicated cases of measurement, to find out which properties of the system remain invariant and to prove that this is so, may show to be a quite demanding task.

The second part of Suppes' last book applies the general ideas developed in the first part to the analysis of different empirical disciplines, especially physics, psychology, and psycholinguistics. I cannot go into the details here. Let me only note that a striking feature of this book is the fact that all chapters, both in the general and the special parts, are accompanied by sometimes quite lengthy historical comments on the issues dealt with in the systematic portion of the chapter. Here, Suppes shows his vast scholarship in the history of science and the history of philosophy.

In the last years of his life, Pat was increasingly involved in experimental work (together with a team of collaborators) in psycholinguistics, more particularly in investigating the correlations between the use of words and brain activity. He undertook this kind of research not just for having "experimental fun" but also for genuinely philosophical reasons: He wanted to understand what the empirical basis for semantics is, and how it works in real-life situations. A couple of weeks before his death he was still devising experiments in this area.

During his long and fruitful life, Patrick Suppes was honoured by numerous awards and distinctions. It is impossible here to list them all. I'll just mention that, at one point or another, he was made Fellow of the American Association for the Advancement of Science, of the National Academy of Science, and President of the Western Division of the American Philosophical Association. In 1990, he got the National Medal of Science awarded by President George H. Bush. After his retirement, he became Luce Stern Professor of Philosophy Emeritus and Professor Emeritus in the Department of Statistics, in the Department of Psychology, and in the School of Education of Stanford University.

In March 2012, to celebrate Pat's 90th birthday, a Symposium took place to discuss his heritage in a great number of philosophical and scientific areas. The Proceedings of the Symposium will soon appear under the title *Foundations and Methods from Mathematics to Neuroscience: Essays Inspired by Patrick Suppes*, edited by Colleen Crangle, Adolfo García de la Sienra and Helen Longino.

Suppes made almost all of his academic career in Stanford. For 64 years he remained true to its university. He received many offers from other prestigious universities and he travelled a lot from one corner of the world to another, giving talks and courses, attending conferences, helping other institutions to develop new didactic methods, and so on. Actually, he travelled more, and more frequently, than any other philosopher I know. (He once told me that he had written most of his papers while flying or waiting in some airport.) But eventually he would always come back to his Ithaca—Stanford.

Before I finish this short memoir, let me add some personal anecdotes that might shed some light on Patrick Suppes' character—at least so far as it appeared to me. I met Pat several times in the course of several decades. The period I met him most often was during my stay as Visiting Professor at the University of California at Santa Cruz in the academic year 1978–1979. I had gotten Pat's phone number in Stanford from Joe Sneed. I called him to see whether I could visit him. His response was immediately positive, but my first meeting with him might easily have turned into a disaster if Pat wouldn't have been the kind of person he was. We had agreed on a certain meeting point on the Stanford campus. But it was my first time there, I misunderstood something, and consequently I got lost. I finally arrived at the meeting point but with a delay of almost half an hour. I had the most bitter grievances against myself: "How can you dare to let such an important person wait for you for such a long time!?". I expected he wouldn't be on the meeting point anymore, or, if at all, with a face of profound disgust. But nothing of this happened: I encountered a frankly smiling Pat, who welcomed me with genuine warmth, didn't pay much attention to my excuses, and was ready to spend with me a couple of hours of stimulating conversation, eventually showing me, as I mentioned above, his computer room for logic teaching. Moreover, he invited me to attend his advanced seminar at the IMSSS, which took place every Friday afternoon. Since the Santa Cruz campus was less than an hour drive from Stanford, this was rather easy for me. I attended the seminar as often as I could, though it sometimes was very difficult for me to follow the highly technical papers on the foundations of measurement given there. Nevertheless, Pat didn't seem to be annoyed at all by the fact that my own contributions (if any) to the discussions often were of a quite modest content…

I continued to meet Pat now and then on the occasion of some conferences. The last time I met him was in Munich, on a symposium again. He was already about 90 years old but as lively as I had experienced him all the time before. Apparently, the long trip from Stanford to Munich was nothing that would frighten him.

In personal relationships, Pat was a very affable, open, generous human being, with a robust sense of humour. But in discussions within an academic setting, another face of Patrick Suppes could emerge—an extremely serious, hard, uncompromising disputant. I remember I attended once a Congress in San Francisco (I think it was a meeting of the American Philosophical Association). A speaker there (I won't reveal his name) laid out his reflections on the mind–body problem and on the possibility of reducing the mental to the physical. When the talk was finished, Pat stood immediately up and asked: "You have been talking almost an hour about the reduction of the mental to the physical. But you have told us nothing about the particular psychological theory, the particular neurophysiological theory and the particular kind of reduction relation you have in mind". A pained silence followed, and the speaker only managed to mumble some incomprehensible words. This experience was very important for me. It made once and for all clear to me that it makes no sense philosophically to speculate around issues that actually belong to the empirical sciences, and that can best be treated within scientific theories. This kind of philosophy that ignores real science is not only idle, it is even ridiculous. To have been led to understand this, is just one of the many ways in which I'm deeply indebted to Pat.

Patrick Suppes peacefully passed away in Stanford on November 29, 2014, attended by his wife Michelle Nguyen, his five children, his five grandchildren, and his half-brother George.

References

Writings by Patrick Suppes Cited in this Article

Krantz, D. H., Luce, R. D., & Tversky, A. (1971). *Foundations of measurement, vol. I: Additive and polynomial representations*. New York: Academic Press.
Krantz, D. H., Luce, R. D., & Tversky, A. (1989). *Foundations of measurement, vol. II: Geometrical, threshold, and probabilistic representations*. New York: Academic Press.
Krantz, D. H., Luce, R. D., & Tversky, A. (1990). *Foundations of measurement, vol. III: Representation, axiomatization and invariance*. New York: Academic Press.
McKinsey, J. C. C., & Sugar, A. C. (1953). Axiomatic foundations of classical particle mechanics. *Journal of Rational Mechanics and Analysis, 2*, 253–272.
Suppes, P. (1957). *Introduction to logic*. Princeton: Van Nostrand.
Suppes, P. (1960). *Axiomatic set theory*. Princeton: Van Nostrand.
Suppes, P. (1970). *A probabilistic theory of causality*. Amsterdam: North-Holland.
Suppes, P. (1974). *Probabilistic metaphysics* (p. 1974). Uppsala: University of Uppsala.
Suppes, P. (1993). *Models and methods in the philosophy of science: Selected essays*. Dordrecht: Kluwer.
Suppes, P. (2002). *Representation and invariance of scientific structures*. Stanford: CSLI Publications.