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Patrick Suppes

Lucie Stern Professor of Philosophy, Emeritus
Stanford University, CA, USA

Why were you initially drawn to formal methods?

Like a great many people interested in formal methods in philosophy, I was drawn already to certain kinds of formality in my early training in mathematics. I mean by "early training" that which I received in elementary and secondary school, especially in secondary school. By far the hardest math course I took as a high-school student was a course what was in those days called "solid geometry", meaning, of course, three-dimensional geometry. This subject is still a difficult one, especially when taught synthetically. Students of today who are progressing more rapidly through the curriculum will almost certainly, in an American high school anyway, have a reasonably thorough course in calculus, but this was not the case when I was a high-school student in the 1930s. The second encounter with formal methods that I remember clearly was a course in calculus I took as a sophomore at the University of Chicago (1940–41). The instructor was someone who was himself then young, but became a very well-known mathematician—the topologist, Norman Steenrod. You would never have guessed from the way he conducted the course, and was not something that I learned until much later, that he was a brilliant mathematician. He seemed rather slow, extremely thorough, very thoughtful, but not very quick to give us answers. He would have long pauses when he wasn't sure of exactly how he wanted to say something. Thinking back upon it, he had a big influence because of the careful, detailed, and patient way he drew out of a group of reasonably bright but naive students the epsilon-delta methods for characterizing limits, instead of plunging right in with the compact notation used then in calculus courses. I was intrigued and involved by the methods he tried to teach us to give us a proper foundation for the calculus.

Before going into the army, I declared myself a physics major. For various reasons at that point, I took a break at the University of Chicago and was spending a little more than a year at the University of Tulsa in Tulsa, Okalahoma, where I was born. I remember in the course in differential equations (spring, 1942) I had as a junior at the University of Tulsa, an extended discussion of the reality of infinitesimals. Several of the students with whom I talked got involved in this matter as well, and we had endless naïve discussions of our own about what was really meant by the reality of infinitesimals. I felt challenged by the question in several different directions, was not able to give, of course, a satisfactory answer of my own, and did not think the instructor gave a very good answer either, but this further awakened my latent interest in formal methods. The next occurrence was again back to the University of Chicago where I was an Army Cadet being trained in meteorology. We were given excellent courses on the dynamical equations of the atmosphere and especially on hydrodynamics (1942–43). I was immediately attracted to the kind of reasoning characteristic of the foundations of hydrodynamics taken from a classical viewpoint, exemplified, for example, in the classic book of Horace Lamb (1932/1945), written now about a hundred years ago (1879). These foundations remain tricky today, as the saying goes. Perhaps the outstanding problem in classical mechanics, still not properly settled from a physical standpoint, is that of turbulence. In any case, I was challenged and fascinated by the attempt to give rigorous physical foundations, rigorous in the physical sense, that is, but still formal in the sense of ending up with the standard differential equations.

After the war, I went to Columbia University in New York City as a graduate student, where my mentor was Ernest Nagel. In particular I learned a great deal from his introductory course in mathematical logic using the well-known textbook of Hilbert and Ackermann (1938/1950). I still remember vividly my finding a slight mistake in one of Nagel's formal recursive statements of the definition of a sentence in first-order logic. He graciously acknowledged my pointing out a minor error. Once again, my interests in formal methods were increased. Another influence were the courses I attended while a graduate student in philosophy. These were the graduate lectures in mathematics by Samuel Eilenberg and one and two other members of the mathematics department. Eilenberg was a mathematician of the Polish school, like Tarski, who influenced me later. His lectures were brilliant and wonderful examples of formal methods at work. I still remember the course in abstract-group theory he gave. A number of my friends, also army veterans, who were graduate students in physics, took the course expecting to learn how to use group theory in the analysis of quantum mechanics. They were quite aghast and disappointed at the extraordinarily abstract level of Eilenberg's course—for me, it was a delight. This was also true of his first graduate course in topology.

Finally, when I got to Stanford as a young instructor with a recently received Ph.D. from Columbia in the fall of 1950, I had learned a great deal as a graduate student but not really how to do any formal research. Under the tutelage of J.C.C. McKinsey, who unfortunately died early in 1953, I began in earnest to write formal papers, the first one being a paper on measurement (1951) and the second being the papers on the axiomatic foundations of mechanics that McKinsey and I wrote in the early 1950s. In answering these questions I refer only to dates of papers but without titles. (They can be found in my bibliography on my web site, http://www.stanford.edu/~psuppes/ or in my actual collection of the papers which are also on the web and may be found at the following location, http://suppes-corpus.stanford.edu/index.html.) The completion of my introduction to formal methods, as I think of it in perhaps an artificial way, was, on McKinsey's recommendation, my attending Tarski's graduate seminar at Berkeley (1950–52) to which I went once a week from Stanford. Watching Tarski at work was a lesson of the greatest value in appreciating and understanding at a deeper level what could be accomplished with the highest standards of intellectual clarity.

What example(s) from your work illustrates the role formal methods can play in philosophy?

For me but not for everybody of course, the best way to think about formal methods in philosophy is in providing a foundation for mathematics or for the sciences. I include in the sciences the problem of clarifying the foundations of probability. Much of my philosophical work has been devoted to publishing examples of using formal methods in clarifying, or at least trying to do so, the foundations of various special parts of different branches of science. I'll mention here briefly and give simply the dates of publications, which as I said can be found in my collection of articles on the web. As I mentioned in my answer to the first question, for historical reasons I mentioned first my initial paper on the foundations of measurement (1951) and then the papers with McKinsey on the foundations of classical mechanics (1953), and shortly thereafter a corresponding foundational paper on relativistic particle mechanics with Herman Rubin (1954). In 1956, I studied the role of subjective probability and utility in decision making and gave a set of axioms that I thought were in certain respects superior to those given by Jimmy Savage in his well-known book published in 1954, *Foundations of Statistics*. My paper appeared in 1956 and my claim about the paper as to why it was an improvement from a formal standpoint was the elimination of some unnecessary structural axioms.

I mention next the 1958 paper with Dana Scott on the foundations of theories of measurement in which we proved some formal results on very elementary measurement situations that could not be nicely axiomatized in a finite way. I believe it is fair to say that this paper has had considerable influence over the years.

I next mention various papers I wrote on the foundations of stochastic models of learning in psychology—one of my abiding interests. I particularly like the 1959 paper with Estes on the foundations of linear models and the technical report (1959)we wrote in that same year on a detailed foundational and axiomatic treatment of stimulus-sampling theory, the results of which were not published completely for many years. I also like a paper that has not been much read, published in 1961, on behavioristic foundations of utility in *Econometrica*. This was meant to be a paper to show economists that a much broader and deeper psychological foundation (stimulus-response theory) could be used for deriving the formal properties of stochastic utility functions. I have only recently returned to this work and am now in the process of redoing, more than forty years later, the foundations of utility, again, from this viewpoint, but with a more general set of axioms based upon a wider set of association processes as psychologically fundamental. In 1969, I wrote what was probably my favorite paper in stochastic learning theory, namely, I showed how to derive in a formal way, the theory of finite automata as a basis for regular grammars or, in very good approximation, context-free grammars from stimulus-response theory.

A 1962 article on models of data has had pretty wide circulation. Here I was trying to show how complicated it is to abstract from the details of experiments to a set-theoretical characterization of the data, ready for statistical and other analysis. I remember discussing this with Tarski who felt that there was no real model theory, and, of course in his sense there was none. Data were being prepared, ready for models. But this emphasis on the absolute necessity of abstracting the data, in a very sharp way from endless experimental details, has been a theme that I have returned to continually. This was my first systematic effort at using formal methods on the problem of data.

In 1963, I published a paper I still like on the role of probability in quantum mechanics. I had been studying quantum mechanics ever since I was a graduate student at Columbia. This was the first paper in which I tried to do anything systematic. The formal part of this paper was bringing the role of probability very much to the front, whereas it usually occupies the back seat in most, or at least so in the earlier, foundations of quantum mechanics. A strictly more formal paper from the standpoint of logic was then a 1965 paper on logics appropriate to empirical theories, by which, I really meant quantum mechanics.

In 1970, I published a monograph on probabilistic theory of causality, in which I used things I had been learning for many years about probability, to give what I thought was at least an approximate foundational analysis of many cases of causality. This monograph, which is not very long, has had some influence on what other people have had to say about probabilistic causality.

In 1970 I was 48 years old, and what I have described above gives, I think, a reasonable sense of the kinds of things that I had been involved in, and had spent time on, applying formal methods in philosophy. For my work over the next 45 years, which represents more recent research, I have divided into five areas: foundations of measurement, semantics of natural language, quantum-mechanical entanglement, representations of cognition and perception in the brain, and the nature of freedom. I devote a paragraph or so to each of these topics. They represent a wide, —many might well think too wide—, spread but reflect a style I had already adopted in earlier years.

Foundations of measurement. My dominate activity was the three-volume work with this title, co-authored with David Krantz, Duncan Luce, and Amos Tversky. Volume I was published in 1971, after a long lag, Volume II in 1989, and Volume III in 1990. This work represents, I think it is fair to say, the most extensive effort in the modern literature to give a systematic treatment of measurement from many different disciplines. It is by no means complete, but it does cover a lot of concepts and a lot of previous research.

Perhaps its largest influence is to be found in subsequent publications by many different authors from many different countries in the *Journal of Mathematical Psychology.*

Semantics of natural language. Beginning in 1973 with my presidential address to the Pacific Division of the American Philosophical Society, I expressed my skepticism of too narrow a view of meaning and concentrated most of the lecture on setting forth examples and principles of what I call congruence concepts for meaning, as an analogue of the many different concepts of congruence in geometry. The example that I found the most important and also the most neglected was that of paraphrase, which seems to me absolutely essential to memory, to conversation, and even to listening to lectures. When we ask someone what he learned at a lecture, he does not repeat it verbatim, but gives a relatively coarse paraphrase. I have recently returned to paraphrase, making it central to computer-assisted instruction in language arts and writing for young students.

In the same year, 1973, I published my first work on the semantics of context-free languages, which in spite of some problems, remain a dominant principal form of formal grammars. In 1980 I extended this analysis to the individual differences I expect in procedural semantics. Each of us has somewhat different semantics at the particular level of actual procedures of producing or comprehending utterances. As part of this program I also wrote a couple of articles, the first in 1979, on logical inference in English and then in the 1980s and 1990s a long series of articles with various people on semantics for robots, including a number of articles with Michael Boëttner and Lin Liang on robotic learning of natural language. Another main collaborator in this period was Colleen Crangle on both the semantics of English and robotic applications.

Quantum mechanical entanglement. My interest in quantum mechanics was revived by working with Mario Zanotti in the 1980s on the logical and philosophical problems raised by the existence of hidden variables. In 1981 we wrote a joint article on when probabilistic explanations are possible, with the answer being that they are not possible when the variables in question do not have a joint-probability distribution. We proved this is also the case when they do not have a common local hidden variable. We continued this work in a number of additional publications on hidden variables. In 2000 Acacio de Barros, a physicist from Brazil, and I wrote a paper on inequalities for three-particle entanglement experiments, an extension of the two-particle experiments familiar from the Bell inequalities. We also approached this topic from a more detailed angle, formally, in another paper on the same subject in 2001. What I found most enjoyable about the two papers with de Barros is that they turned out to be the natural generalization, from a formal standpoint, of the Bell inequalities, replacing bivariate correlations by the expectation of the product of the spins of three particles.

I also like to stress, quite apart from my own interest, that the quantum entanglement of particles, which can be at great distances from each other, is certainly the most puzzling remaining feature in the whole of classical quantum mechanics. What will be discovered in the years ahead at different levels of energy and different circumstances is another matter, but the entanglement experiments can take place in rather standard circumstances here on Earth. Moreover, the concepts involved in these experiments are very much those involved in building quantum computers. So the interest in entanglement continues, and will most certainly continue for the indefinite future, to be an active research focus.

Brain representation of cognition and perception. Having heard in 1996 an excellent lecture on the new technology of magnetoencephalography (MEG), the extension to the magnetic field of the classical electroencephalography (EEG) for the electric field, I became enthusiastic, undoubtedly overly so, on the possibility of using MEG to recognize the representation of words in the brain, with the research being pursued very much on the model of the computer recognition of human speech, a subject on which great progress has been made in the last several decades. That initial enthusiasm was converted within a year into undertaking actual experiments using EEG as well as MEG. I now have published several papers on such representations. This is not the place to enter into the details, but I do want to emphasize a couple of points. First, it seems to me that philosophically it is going to be much more fruitful, especially when it comes to things like language and visual images, to reach for a deeper understanding of brain representations than of mental representations. Think about it. What is your mental representation of the word *anachronism*? Well, my own view is that we have a rather shallow concept of the mental representation of words and, in fact, we are not very conscious of such representations. On the other hand, it is obviously a task worthy of quite considerable research effort to understand both conceptually and empirically how the brain represents language. So what are the brain representations of words and other constituents of languages? This also applies to visual images. How actually do we represent color? If we look at brain signals, can we tell from the brain signal whether you saw in a given context, red or blue? One of the natural ways to show that you have appropriate brain representations, in the spirit of formal methods, is to establish a structural isomorphism between the physical or perceptual representation and the brain representation. So, one of the things to look for is such a structural isomorphism, in the mapping from the perceptual or physical representation to the brain representation of spoken language or visual images. Such structural isomorphisms have not yet been investigated in any very deep way. It is a sign of the still rudimentary character of such research that this is the situation. No doubt much more will be learned in the next few decades. Also, as we come to understand, for example, the brain representations of language and related cognitive matters, I would expect the philosophy of mind to undergo some radical change as well.

In a more general way, my interest in brain representations, has moved me to a broader thesis about philosophy. I have now written a series of papers in the last three or four years centered around what I call the neuropsychological foundations of philosophy. One paper, 2003, with Jean-Yeves Béziau focuses on the computation of truth for elementary empirical sentences, another with the psychological source of Bayesian priors (In Press-a), a third with the role of habits and free associations in rational choice and decision making (2003), and a fourth in the psychological nature of verification of informal mathematical proofs (In Press-b). The basic line of attack is that all of these philosophical matters must be regarded as empirical in character and thus subject to empirical investigation, regimented by psychological and other kinds of empirical theories available. It doesn't mean that we will understand everything about the phenomena, but we will understand more that we did in the past. Certainly, I am critical of Fregean kinds of effort to give a theory of meaning absent of any psychological considerations. It is hard to imagine, from the viewpoint from which I am operating, where else one could find concepts to determine the meanings of ordinary words and sentences, even those of informal sentences about pure mathematics.

Nature and measurement of freedom. Beginning in the last decade, I have written a series of papers on the nature and measurement of freedom, which is in fact the title of the first paper I published on the subject in 1996. I have also offered a course on freedom to undergraduates three or four times during this same period. There are only two points I will stress here. The methodology of my approach to freedom is very much in the spirit of the formal methods already discussed. To begin with, there is emphasis on measurement, and secondly there is an emphasis on what I think is the most missing concept in standard philosophical discussions of freedom, namely, the presence of uncertainty. This is particularly true in discussing freedom in political or social contexts, but I also think it holds for the fundamental psychology of choice of a single individual.

What is the proper role of philosophy in relation to other disciplines?

I have written my answer to this question in several places over many years. Philosophy has no special methodology or special foundational basis to separate it from mathematics and the sciences. The empirical methodology of the sciences must dominate philosophy as well. To understand the mind, for example, there is no privileged way by which philosophers have access to mental representations. In so far as there is access to mental representations of language, for example, it must be found by the methods of empirical psychology and empirical neuroscience. The introspections of the past of many philosophers, among whom I would place Aristotle and Hume as the most important, have certainly contributed a great deal to our intellectual understanding of our mental activities and mental structures, but future progress will depend upon detailed, technical, and disciplined empirical inquiries. I cannot see making much progress by the methods of Aristotle and Hume, and also William James, brilliant and important as they were, they have more or less been exhausted as approaches for finding new results.

What marks philosophers is not having special insight, in particular, what was often thought of as having a special methodology. It is rather that philosophers by taste, and often by training, concentrate on foundational questions. So, for example, investigations in the foundations of mathematics, by and large, use mathematical methods to provide new insight into mathematics. There is not a special brand of methodology called *philosophical methodology* for investigating the foundations of mathematics. What is characteristic, and understood by everybody familiar with the details, is that the methodology for investigating such foundations is just the methodology of mathematics itself. Philosophers who study such foundations are operating in a mathematical way, and they are to be distinguished from mathematicians only by their interest in the particular subject of foundations, just as among other mathematicians we distinguish between individuals interested in geometry, algebra or analysis.

The same viewpoint applies to the sciences. Philosophers who study the foundations of quantum mechanics are not bringing to that study of foundations a new apparatus of a technical nature for studying quantum mechanics. They are using the methods of quantum mechanics augmented by methods brought in from other sciences such as mathematics. A good example in the case of quantum mechanics would be the contribution on the one side of probability theory and on the other of logic. Many of the questions about quantum entanglement entail problems in the foundations of probability. Here is a good example. The Bell inequalities have the interesting feature that when applied just to the standard experiments, they give necessary and sufficient conditions for there to exist a joint probability distribution of the four random variables being observed, those random variables being two on one side of an apparatus say A and B and two on the other side, symmetrically located, A' and B'. What is observed is one variable from one side and one from another at the same time and the correlation between these two is computed. So for example, we compute the correlation between A and B' over a succession of observations or trials. This is only an example of a probability question in quantum mechanics. There are many more.

The same kind of thing applies to the efforts now to build a quantum computer. The rather neglected subject of quantum logic has come to life again and is now an important component of work on the practical problem of constructing quantum computers. Special forms of logic are required for much of what is done in quantum computing, but those special forms of logic are developed using the methods that have been developed by logicians of both philosophical and mathematical bent over the past half century.

So, when asked about the relation of philosophy to other disciplines, my answer is now, and has been in the past, that philosophers are concerned with foundations. They are concerned with the concentrating on the concepts that are fundamental to a given discipline whether it be mathematics, physics, economics, or psychology. The methods they use for these investigations are pretty much the same methods in broad terms that are used by the scientists working in a given discipline. They just use these methods to apply a very focused analysis on the foundations, and of course, it is also correct to emphasize that in many cases they bring to such study concepts used in an another discipline but ordinarily not concepts that have been developed only by philosophers.

There is an important contrast in the current state of philosophy worth noting. Philosophers of physics do not expect to go off on their own in speculations about physics. They understand that the discussion of foundations of quantum mechanics or other parts of physics requires thorough and knowledgeable use of what is known and has been experimentally and theoretically studied by physicists. The situation is very different in the philosophy of mind. It is rather like having a philosophy of physics that is still concentrated on Aristotelian ideas. The philosophy of mind has not yet fully endorsed and adapted itself to what has been learned scientifically about the mind by psychologists and neuroscientists. It is for this reason I predict that extensive changes in philosophy will occur in this century in the philosophy of mind.

What do you consider the most neglected topics and/or contributions in late 20th century philosophy?

My answer here is partly what I said at the end of question 3. What has been neglected in the philosophy of mind are the contributions of psychology and neuroscience to the understanding of the mind and the brain in the last several decades of the twentieth century. This is why I am predicting an intellectual revolution during this century in the philosophy of mind, comparable to what happened in the philosophy of physics in the twentieth century. It sounds almost paradoxical to say that in the twentieth century the philosophy of mind has been neglected, given how much has been written about it, but what has been neglected is the viewpoint that derives from the relevant scientific work.

What are the most important open problems in philosophy and what are the prospects for progress?

I think there are many open problems that will be of great interest in this century, but I cite three that are especially important and in terms of which great progress will be made. The first concerns our conception of the physical universe, the second is about our understanding of how the brain represents the world around it, and the third is the topic of free will, prominent for centuries in philosophical discussion.

Nature of the physical universe. Because of recent research in many different directions in physics, from experiments in quantum entanglement, which challenge the correctness of the special theory of relativity because of the ability to affect, faster than the velocity of light, the behavior of entangled particles separated by indefinitely large distances, to a variety of findings in astrophysics, we will undoubtedly develop a new set of ideas for thinking about the structure of the universe. New concepts concerning space-time and the nature of matter and energy will cause, in many ways, changes in our conceptual thinking as large as any that took place in the twentieth century. If this prediction is correct, our philosophical conceptions of the physical world in which we live will be changed in the kind of fundamental way they changed with the displacement of the Aristotelian and Ptolemaic conception of the heavens, which dominated our world view for fifteen hundred years, to that of Newtonian mechanics, which itself lasted until special relativity and quantum mechanics were developed in the twentieth century. We are now ready for another such conceptual set of changes, and we will find them at first equally mysterious.

Brain representations of the world. There is a long history of speculative ideas about mental representation, but in mental representations we have the kind of freedom we do not have in dealing with the actual processes of the brain that must embody such mental representations. The deep problems we have only begun to touch are the identification of the kinds of structural isomorphisms that exist between our brain representations of our perceptions or our cognitive patterns of dealing with the world. These structural isomorphisms, as we discover them, should show us in detail how the structure, including the processing structure, of the universe as we perceive it, is represented in our brains. As yet we have barely begun the research required to have a detailed understanding of such structural isomorphisms, but as we do, it seems very likely our conception of our own mental lives will change as well. Let me give just two examples. A widely accepted thesis about the structure of spoken language is that sentences are not understood holistically, but are understood by recognizing the words which are their constituents. No one, as far as I know, defends the thesis that we have a purely holistic understanding of sentences. Our conception of how we compute the meaning or truth of even the simplest sentences depends on some kind of process of recognizing the individual constituents that make up sentences. The corresponding question for the brain is how are these constituents isomorphically mapped into the representation of sentences in the brain. The word or phrase constituents of sentences must be mapped into individual representations in the brain, in order for us to compute, in a feasible way, the meaning and truth of sentences. We surely do not do this computation in some completely holistic way.

A second and related example of structural isomorphism is understanding how we recognize or represent in the brain the various natural properties of visual perceptions. Such structural analysis is also needed for perception using the other senses as well. But to take a simple example, consider the perception of a red circle. This simple abstract figure has two properties, one of color and one of shape. How is each of these properties represented in the brain? How do we disentangle the structural isomorphic representation of color from that of shape in the electromagnetic brain waves or signals that reach the cortex and permit us cognitively to recognize and to express that we recognize what we see? The problem when formulated this way in some sense seems simple, but scientifically it is apparently far from being simple. The important point that I want to make is that it is the relation between the constituents, or the parts, and the whole that must be reflected in the structural isomorphism. Finding these isomorphic mappings is a task that is only barely begun, and until greater progress is made our detailed understanding of what the mental, embodied in the brain, really consists of will be poorly understood.

Free will. For me the most important open scandal in philosophy is the problem of free will. I can understand why Hume gave the solution he did to the problem, because he felt he must have an answer that was consistent with the necessary views that held sway, at the time, of the laws of nature. When he said "necessary" he meant in most respects what we mean by the laws being deterministic. We now have a much deeper and more sophisticated understanding of the physics of matter. Determinism is not an idea that has the sway it once did, but we have not properly absorbed what I think is the conversion of the problem of free will from being a traditional philosophical problem to a scientific one. The scientific problem is to give a detailed account of how it is that matter can be intentional in character. In some sense, we all recognize that matter can be animate. This is the great lesson of modern biology. We also need wide dissemination of a thorough analysis of the intentional character of matter. Even though many details are missing, just as they are in case of the evolution of life from inanimate matter, the outlines of the story are pretty clear. The scientific and philosophical mistake was to believe, as Hume, Kant and other great philosophers of the past did, that in some sense the laws of the universe are necessary and deterministic in character. Kant's antinomies were the apex of this tradition. The great problem for philosophy in this area is to disentangle itself from all the arguments of the past that were mistaken and to develop a view of free will that is consistent with modern conceptions of matter, how matter can be intentional, and how it is realized in some very simple intentional processes like elementary conditioning in biologically primitive animals with a relatively small number of neurons.

There is another side to this story, also important from a different scientific angle. This is the realization that there in not really a strong empirical distinction between deterministic and stochastic behavior. This is a new kind of invariance not well enough recognized in scientific or philosophical circles. The main results are beautiful and surprising ones that come out of ergodic theory, which I shall not try to describe here, but just mention some of the consequences that follow from them. An excellent account is to be found in Ornstein and Weiss (1991). I will end with this example, because of its beauty and profundity at the same time. Consider that hallmark of determinism of the past, an ideal billiard table with an ideal ball moving about it periodically and endlessly in exactly the same endless pattern. Now put on that billiard table a convex object in the middle, off of which the ball must be reflected in its course, just as it is from the sides of the table. There must be errors of measurement, —one of the great truths of modern experimental physics. And these errors of measurement must be bounded away from zero. Then no matter how many observations we may take of the behavior of this idealized ball, which could be a photon "on a billiard table of mirrors," we cannot decide between the correctness of a stochastic theory of the motion, and that of the classical deterministic theory. The two mathematically formulated theories, one stochastic and the other deterministic, are mutually inconsistent, but due to the existence of errors in the measurement, it is impossible empirically to distinguish one from being more correct than the other. This is a new invariance undoubtedly present in many complex physical systems and of great conceptual importance in the proper view of intentionality and freedom. (A view I defended in a 1993 article on the transcendental character of determinism.)

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