Interview with Patrick Suppes. (In Chinese, with English translation).
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Philosophy and Life

Interview with Professor Patrick Suppes, Stanford University

Sumei Cheng

Professor Patrick Suppes from Stanford University, in the U.S., has spent more than 40 years on writing the book, *Representation and Invariance of Scientific Structures*, and a year after this book was published it received the Lakatos Award Prize in 2003. In 2009, this book received the China Social Science Foundation support to be translated into Chinese, before it was published by Shanghai Publishing. In order to allow readers to understand well the thoughts of the author and opinions of myself, I interviewed the author by email regarding the meaningfulness and the main points of the book. This interview reflects the author's thinking and understanding of education and research in the philosophy of science, and the development of contemporary philosophy of science at his present age of 90.

1. **You said that you were less than the age of 40 when you began this book and you finished it in your tenth year of retirement, at the age of 80. This means that this book is an important book in your academic research. What is its contribution to contemporary philosophy of science?**

First, let me deal with the question of my age at the beginning of this book. I do not think there is a definite answer to this question. The original manuscript entitled

"Set-theoretical Structures in Science" began as a very small set of notes distributed to students in my Stanford class on the philosophy of science in the 1950s. I continued to give this course every year, and every year I added more notes. After a decade I began to have a reasonable approximation to a manuscript. This was about 1965. The progress for the next 30 years was more scattered, but whenever I gave a seminar on any of the topics relevant to the final version of the book, e.g., the foundations of probability, I tended to put in the manuscript at least parts of any article I wrote in connection with such a seminar. Because I gave seminars on many different topics, I accumulated rather detailed treatments of a wide variety of subjects from quantum mechanics to the psychology of visual space. But my approach to these many different topics was unified from a formal standpoint, which was, of course, the general set-theoretical viewpoint at the heart of the book.

Concerning the second part of your question about the significance of the book for contemporary philosophy of science, I believe I can give a rather general, but succinct answer to this question. At the heart of much of the dominant philosophy of logical positivism in the earlier years of the 20th century was the effort to give what was then called logical account of the foundations of science. Significant progress was made by such philosophers as Rudolf Carnap and Hans Reichenbach. But for a variety of reasons, their efforts did not go deeply into the complicated structure of modern science in what could be recognized as a fully successful effort. My analysis is that the main reason for this was that the extensive and necessary use of much mathematics in modern physics and more generally in modern probability theory required a formal apparatus that was not easily reduced to pure logic as a framework in any simple or natural way. On the other hand, the mathematical concepts which became prominent in both quantum mechanics and probability, were of a purely set-theoretical nature, yet without the use or reduction of the set-theoretical framework to the axiomatic foundations of set theory. In summary, the reduction to logic in a straightforward way was practically impossible, and on the other hand, the reduction of set theory to an axiomatic theory itself was a very long route that violated the mathematical spirit and practice that pervaded modern mathematics. This practice, that spread really to the whole of pure mathematics, was one of formulating concepts informally but clearly within a purely set-theoretical framework. A primary aim, and the unifying one, was to show that the standard methods of pure mathematics could be extended in a natural way to give the appropriate set-theoretical foundations for almost any precisely formulated scientific theory. There can, of course, the other foundational programs in the philosophy of science, and I am by no means claiming that the route I chose is the only one, but I do feel I was reasonably successful in following it.

2. You think that the topics of representation and invariance of scientific structures are a major part of philosophy of science. Could you summary your main ideas about them?

This question follows naturally as part of my answer to the first question. In that answer I mentioned only the problem of reducing the formulation of scientific theories to the set-theoretical framework widely used in pure mathematics. I did not go on to say anything about the practice in pure mathematics of how such a structure is often used. It was recognized in the latter half of the 19th century by Pasch and Hilbert that the proper approach to the foundations of geometry must be a formal approach, a development I described in Chapter 2 of my book. The next step in this axiomatic foundations of geometry, exemplified well in Hilbert's well-known small treatise Foundations of Geometry published at the end of the 19th century, was to seek a formal answer to the question how do we know that this set of axioms is intuitively correct, and how do we know that it has just the right intuitive strength. Notice that both of these questions, the first of which is the classical problem of representation, and the second of which is the classical problem of invariance, are very different from questions of consistency and independence of axioms, so prominent in logic, but not in geometry or physics. As the 20th century developed, it soon became clear that these questions of representation and invariance could best be formulated in a general set-theoretical framework. An excellent historical account of such an example in this evolution inside mathematics is the detailed book of Wussing (1984) on the genesis of the concept of an abstract group.

In my enthusiasm for the beauty of this development of structural concepts in mathematics, I was almost certainly too optimistic about the extent to which they would spread to the philosophy of science in any short period of time.

3. You try to use the axiomatic method to investigate problems of representation and invariance in any systematic past or present science and provide various theorems about them based on different subjects. What is your aim?

My primary aim in spending so much time on axiomatic methods in various domains of science has been to clarify the nature of the fundamental concepts of important scientific theories or practices. Many auxiliary questions of great interest on their own arise in such an investigation, but I continue to think of clarification and precise elucidation of concepts as the important general aim in applying axiomatic methods to a given subject matter. Of course, I claim no real originality in having such an aim as this. It is clearly evident in Euclid and in the most important mathematical physics of the 17th century, as can be seen in the works of

Huygens and Newton. For many reasons, as mathematics and physics evolved in the next two centuries after Newton, the two disciplines diverged in their methods, with mathematics becoming ever more rigorous in its systematic approach, and physics becoming much less so. Much of my own early efforts in the philosophy of science have been devoted to trying to revive the use of axiomatic methods in the foundations of the sciences, not just in pure mathematics.

A good example of a set of ideas I would like to reconstruct and put in axiomatic form would be the empirically successful flat-earth assumption of ancient Chinese astronomy, as explained in the *Zhou bi suan jing* (Cullen 1996). As far as I know, this is the only historically important example of the flat-earth hypothesis being put to serious quantitative and empirical use.

4. Is the theory talking about the world or describing the world in your mind when you discuss the representation and invariance of scientific structures?

What scientific structures are meant to refer to depends on the intuitive subject matter whose concepts are being axiomatized. In the case of physics, the subject matter is about the world, and not about my idea of the world. My ideal world is expressed in the axioms, but the subject matter of the axioms is not my own ideas, but the nature of the world. On the other hand, in studies of the mind or brain, we can think of the primary subject matter as being, in one sense, mainly about the subjective experience of persons, even though, in another sense, we can study the brain as a physical system, indeed as an intricate physical computer, whose electromagnetic fields give it a computational power that can rival our best digital computers on certain kinds of problems, such as producing or understanding natural language.

5. Chapter 5 about the Representation of Probability is the longest one in this book. You analysis the developmental history of probability and some kinds of probability, such as subjective probability, objective probability, and the propensity interpretation of probability. What is your point of view?

I like this question, because I have been thinking about it ever since I wrote that chapter and published the book in 2002. My current answer matches what I said at the end of the chapter, but now I say it with greater confidence. My point of view is neither subjective nor objective, but pragmatic. At the end of the chapter, I cited two sources to support my current view, one consisted of quotations from the early prominent thinkers about the foundations of quantum mechanics; these quotations were remarkable for their reserve about the nature of probability as it is used in the theory of quantum mechanics. The second example was from the wonderful book by Mosteller and Wallace (1984), which applied a beautiful array of modern statistical methods to determine the authorship of one of the Federalist papers – important documents in the early political history of the United States. After the work was completed, their Bayesian friends thought their statistical methods were subjective, and their Neyman-Pearson friends thought the opposite, i.e., they were objective. With my deep interest in applications in the philosophy of science, but in particular of probability, I heartily agreed with Mosteller and Wallace, as well as admired their austere attitude of making no commitment, because none was needed, about the nature of probability in order to make use of it computationally. I do not mean something deep by this last remark, but rather when we are doing much scientific work we no more think about the foundations of probability than we do about the foundations of the number system. For many this comparison will not seem appropriate, but I think it is a way of emphasizing the desirability of having a pragmatic view about the nature of probability, because of the enormously diverse ways that probability is used in applications of science to very diverse phenomena.

6. Chapter 8 about Representation of Language is the next to longest in this book. You mention the fields of psychology, cognitive science, and artificial intelligence and so on. What problems are you trying to explain?

The main purpose of this long chapter about language is to explain, or to point out what still needs to be explained, about the deep and diverse ways we can approach the structure and function of language. In the chapter I give a fairly extended discussion of formal syntax, a less extended discussion of semantics, and a still longer discussion of models for learning language. Looked at in a narrow way, these different approaches to language use quite different methods. One purpose of the chapter is to show how these methods can be unified within a general set-theoretical approach. No one of them deals with more than a very small part of the rich and diverse nature of language as it has been developed and used through the centuries in every aspect of human endeavor and thought.

7. **You still lead a team to do research in these fields until now and you go to your office everyday to deal with many problems. What are your research topics? What philosophical problems do you hope to answer?**

In many ways my research topics remain unchanged, but my detailed applications and even my philosophical attitudes to many issues have continued to change. In any case in my actual work, as you put it, and going to the office everyday, is to spend my time on three general topics. The first of the three is my continued work in the use of computers in education, especially now in online education. This topic is scarcely mentioned in the book we are discussing here, but it is a big part of my life and has been so for more than 50 years. The second topic is research on the brain, which I only began after retirement in 1992. Since then I have spent a great deal of time on what I would call system neuroscience, with a particular concentration on language in the brain. I described the beginning of this research on the brain, primarily using EEG recordings of brain activity, in chapter 8 of the book. I will not here describe my work in this area since 2002 when the book appeared, but I just want to note that I continue to spend a large part of my time on this effort. The third area is one that I have kept active, even though not the dominant activity of my intellectual life. That is the continued thinking and writing about broad issues in the philosophy of science, and sometimes in other parts of philosophy.

A recent example is the seminar on free will that I gave at Stanford in the academic year 2009 – 2010. As I listened to students and talked to them about the things I was reading on free will, I came to realize that a concept I considered important in other domains of science kept raising its head. This is the concept of spontaneity, famous in Kant's concept of freedom, the nature of radioactivity in 19th century physics, and now very prominent in political discussions everywhere. Perhaps I should not have said *everywhere*, because for some strange reason the discussion of spontaneity is now almost entirely absence in the modern literature on free will, dominated as it is by concepts of morality and responsibility. Yet in common talk, literary ruminations and many other places, spontaneity is a positive normative concept about behavior. And I am just the kind of philosopher who thinks that spontaneity is an important component of much behavior that we judge positively without any concern for tinges, or more than tinges, of morality. What is also important is that it is a mark of free will at work, apart from questions of moral responsibility.

8. What is your opinion of the developmental trend of philosophy of science?

There are many things I like about recent developments in the philosophy of science. The most important is the much greater emphasis on the nature of experimentation in all aspects of scientific investigation. For too much of the past, philosophers have been almost totally enamored with the language and theory of science, and have cast their ideas of change too much in theoretical terms, without adequate concern for the details of their use in experiments or observational measurements in such disciplines as astronomy. Another change that I view positively and that is certainly important for the future is the way in which philosophers have moved to give considerable attention to the philosophy of biology, which includes, of course, my own main interest now in neuroscience. When I was a graduate student in the 1940s in philosophy, little attention was paid to the philosophy of biology. In fact, I believe that during my entire graduate years and also during my first decade of teaching at Stanford, scarcely a word was said by anybody about teaching the philosophy of biology.

On the other hand, I am not as happy about developments in the foundations of probability and related topics in the philosophy of science. For example, there could be much more close and critical examination of the way statistics are used to interpret and support particular scientific hypotheses or theories. This is a large and complicated subject, and it is certainly not the responsibility professionally of philosophers to be the leading experts, but the subtle and complex issues that arise in the analysis of data, especially statistical, are from the systematic standpoint perhaps our best modern examples of how knowledge is acquired and its quality judged. It is these latter kinds of questions that fit very well into classical philosophical analyses that are mainly epistemological in character. Yet their occurrence in the philosophical literature is still relatively rare, especially in comparison with the torrent of statistical data and results we now have in everyday life and in science about every aspect of human existence.

9. I know that you were in charge of the Education Program for Gifted Youth at Stanford University. So you are a philosophy professor and an educator also. Would you like to say something about that?

I have already mentioned in answer to previous questions that I have strong educational interests, but I am happy to reiterate here that from a general perspective my involvement in education has been the most practical part of my life. By this I mean I have been deeply involved in education at all levels, but especially in the actual practice of teaching and evaluation of its success or failure, especially for elementary-school students. In the 1950s in the era of Sputnik, I became concerned about the teaching of mathematics in elementary school, especially because I also had a strong involvement in my first child's learning of mathematics as it was then taught in the first grade. So I confess that much of my initial response to Sputnik was highly personal. I have recorded this incident in some detail in my intellectual autobiography that you can read on my website suppes-corpus.stanford.edu. This early interest rapidly developed into a much broader concern about the teaching of mathematics and other subjects in our public schools, so now for more than 50 years I have in each year, I am sure, devoted a serious amount of time to my interests in education, and these have lived happily together with my other scientific and philosophical interests. In fact, my many efforts to conduct experiments on learning and to evaluate the success of my own or other educational programs has overlapped in many different ways with my interests in the foundations of probability and statistical inference.

This last remark leads naturally into a good way of closing in answering these many questions about my philosophical attitudes. Someone who knows me well recently said to me "I have come to understand your life better as I realize that your natural tendency is to convert any aspect of your life into a research project." This is the best simple explanation I know of my tendency to think about and to work on many different kinds of things. I am not saying that having this tendency as developed as it is in me is a good thing, but it is accurate to say I have it.

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哲学与人生

——对斯坦福大学苏裴斯教授的专访

成素梅

《科学结构的表征与不变性》(Representation and Invariance of Scientific Structures)一书是美国斯坦福大学的苏裴斯(Patrick Suppes)教授撰写了四十多年的一部著作,出版不久便荣获了 2003 年的拉卡托斯奖。在本书的中译版受 2009 年度国家社科基金的后期资助并即将由上海译文出版社出版之前,为了让读者能够更准确地理解本书作者的主要观点和当前本人的一些想法,我通过电子邮件与作者就本书的要点与意义作了进一步的专访。这次专访的内容透射了作者在近 90 岁高龄的今天回顾他在科学哲学的教学与研究中的一些感悟与体会,以及对当代科学哲学发展趋势的看法。

问:您在《科学结构的表征与不变性》一书的序言中说,当您开始撰写此书时,您还不到 40 岁,而在您退休 10 年之后的 80 岁才完成。这意味着,在您的学术生涯中,这本书是一本重要之作。那么,它对当代科学哲学的贡献是什么呢?

答:首先,我在本书的一开始研究的是当时的时代问题。我认为,对这个问题没有一个明确的答案。原手稿的标题是《科学中的集合论》,它开始于 20 世纪 50 年代我在斯坦福大学的科学哲学课上发给学生的一些笔记。我每年都连续上这门课,并每年都新增很多笔记。10 年之后, 我开始有了一个近似合理的手稿。这大约是在 1965 年。接下来的 30 年的进展比较零散,不过,每当我做学术报告时,只要报告主题与本书的最终版本相关(比如,概率的基础),我往往都会把相关内容增加到手稿里。因为我做过许多不同论题的学术报告,所以,我从量子力学到心理学的很多论题积累了相当详细的论述。然而,我的进路是从一种形式的立场来统一许多不同的论题,这种立场当然是成为本书核心的一般集合论的观点。

就本书对当代科学哲学的意义而言,我相信,我能对这个问题给出一个相当一般的但言简意赅的回答。在 20 世纪初,占有统治地位的逻辑实证主义的哲学核心主要是努力给出所谓的科学基础的逻辑解释。有意义的进步是由像鲁道夫·卡尔纳普(Rudolf Carnap)和汉斯·赖欣巴哈(Hans Reichenbach)那样的哲学家取得的。但由于种种原因,他们的努力并没有深入到能被认为是很成功努力的现代科学的复杂结构

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当中。我分析,关于这一点的主要原因是,在现代物理学中,更一般地说,在现代概率论中,对许多数学的广泛而必要的运用,要求一种形式方法(formal apparatus),这种形式方法不容易以任何简单方式或自然方式还原为作为一个框架的纯逻辑。另一方面,在量子力学和概率中很重要的数学概念,具有纯集合论的本性,但还没有把集合论框架用于或还原为集合论的公理基础。总之,以直接的方式还原为逻辑在实践中是不可能的。另一方面,把集合论还原为公理理论本身是一条违背了充满现代数学的数学精神与实践的漫长之路。真正遍布于整个纯数学的实践是在纯集合论的框架内对概念进行确切的形式表述。一个首要目标也是一致的目标是表明,我们能够以一种自然的方式把纯数学的标准方法扩展到为几乎任何得到精确表述的科学理论提供适当的集合论基础。在科学哲学中当然可能有其他的基本方案,而且,我也绝不声称,我选择的路是唯一的,但我确实觉得,我在这条路上相当成功。

问:您认为,科学结构的表征和不变性的论题是科学哲学的主要部分。您能总结一下您关于这两个论题的主要观点吗?

答:这个问题是在我回答了第一个问题之后自然会接着提出的问题。在我对第一个问题的回答中,我只提到了把科学理论的表述还原为在纯数学中得到广泛运用的集合论的框架。我没有在关于这种结构通常用法的纯数学中继续对这种实践说什么。19 世纪下半叶,帕斯(Pasch)和希尔伯特(Hilbert)承认,几何基础的恰当进路一定是一种形式的进路,即我在本书的第 2 章描述的一种进展。在希尔伯特于 19 世纪末出版的名著《几何基础》中很好地举例说明了,这种几何的公理基础的下一步是寻找对下列问题的回答:我们如何知道,这个公理集合在直觉上是正确的,而且,我们如何知道,它恰好有正确的直觉力量(intuitive strength)。 请注意,在这两个问题中,第一个问题是经典的表征(representation)问题,第二个问题是经典的不变性(invariance)问题,它们完全不同于一致性(consistency)问题,也与公理无关,因此,在逻辑学中是突出的,但在几何或物理学中并非如此。随着 20 世纪的发展,很快就看清楚,表征与不变性的这些问题在一般的集合论框架中能够得到最佳表述。在内在于数学的这种演化中对这样一个事例的出色的历史说明是武辛(Wussing)的著作①。在这本著作中,他详细地记述了抽象群概念的起源。

我出于对数学中结构概念的这种发展美(beauty)的热情,我对这些结构概念在短期内延伸到科学哲学的程度,近乎肯定太乐观。

问:您试图在过去或现在的科学中,运用公理化方法研究表征和不变性问题,并根据不同的学科,提出关于它们的不同定理。您的目的是什么呢?

答:我在不同的学科领域内花费如此多时间的首要目标是为了澄清重要的科学理论或实践的基本概念的本性。这样一种研究产生了许多兴趣浓厚的附带问题,但我仍然把概念的澄清和精确表述看成是在把公理方法应用于已知问题时的重要的

① Wussing, H., The Genesis of the Abstract Group Concept, Cambridge, MA: MIT Press, 1984.

总目标。当然,我断言,具有诸如此类的目标,没有任何创造性,正如在惠更斯(Huygens)和牛顿的著作中所能看到的那样,这个目标在欧几里得几何和17世纪最重要的数学物理学中是很明显的。由于许多原因,随着数学和物理学在牛顿之后的两个世纪的演化,这两门学科在它们的方法上分道扬镳了,数学以系统化的进路变得越来越严格,而物理学则变得越来越不太严格。我自己在科学哲学方面的早期努力热衷于在这些学科的基础中,不只是在纯数学中,试图恢复使用公理方法。

我乐意重构并以公理形式提出的一组观念的一个很好的例子,是在经验上成功的古代中国天文学的平面地球假设,就像周髀算经①中所说明的那样。据我所知,这是历史上严格定量地在经验上使用平面地球假设的唯一重要的事例。

问:当您讨论科学结构的表征与不变性时,在您的想法中,理论是在谈论世界呢,还是在描述世界呢?

答:所谓科学结构是依把其概念公理化的直观主题(the intuitive subject matter)而定的。在物理学的情况下,主题是关于世界的,不是关于我对世界的观念。我的理想世界用公理来表示,但这些公理的主题不是我自己的观念,而是世界的本性。另一方面,在心灵或大脑的研究中,我们能够把首要的主题在一种意义上看成是主要关于主观经验的,即使在另一种意义上,我们能够把大脑研究作为一个物理系统来研究,确实,作为一个复杂的"物理计算机",它的电磁场为它提供能与我们关于这类问题的最好的数字计算机相竞争的计算能力,例如,产生或理解自然语言。

问:关于表征与概率的第5章是本书最长的一章。您分析了概念的历史发展和某些类型的概率,比如,主观概率、客观概率和概率的倾向性解释。您的观点是什么呢?

答:我喜欢这个问题。因为自从我写完这一章并于2002年出版了此书之后,我一直在考虑这个问题。我当前的答案与我在这一章结束时所说的相一致,但现在我更有自信这么说。我的观点既不是主观的,也不是客观的,而是实用的。在这一章的最后,我引证了两种资料来支持我当前的观点,一种是由早期杰出的思想家关于量子力学基础的引文组成的,这些引文令人注目地保留了量子力学中所运用的概率的本性;第二个例子来自莫斯特勒(Mosteller)和华莱士(Wallace)的佳作,这本著作运用一系列漂亮的现代统计方法确定《联邦党人文集》(The Federalist Papers,即美国早期政治史上的重要文件)的一位作者的身份。这种工作完成之后,他们的贝叶斯的朋友(Bayesian friends)认为他们的统计方法是主观的,他们的内曼—皮尔逊(Neyman-Pearson friends)的朋友认为正相反,即他们是客观的。由于我除了对特殊概率感兴趣之外,还对在科学哲学中的应用感兴趣,所以,我非常同意莫斯特勒和华莱士的看法,也钦佩他们不做出任何承诺的严肃态度,因为为了在计算上运用概率,根本不需要关于概率的本性。我不是意指最后这句话有更深的意义,而是意指,当我们正在进行许多科学工作时,我们对概率基础的考虑不比我们对数字系统基础的考虑更多。

① Cullen, C., Astronomy and Mathematics in Ancient China: The Zhou Bi Suan Jing, Cambridge: Cambridge University Press, 1996.

就许多方面来说,这种比较似乎是不适当的,但我认为,这是强调具有关于概率本性的实用性观点的一种可取方式,因为在把科学应用于非常广泛的现象中,运用概率的方式是极其多样的。

问:关于语言表征的第 8 章是本书接下来最长的一章。您在这一章里涉及到心理学、认知科学和人工智能等领域。您试图要说明什么问题呢?

答:关于语言的较长的这一章的目标是为了说明或指出,我们仍然有必要说明,我们能够逼近语言的结构与功能的深度和多种方式。在这一章,我对形式句法进行了相当广泛的讨论,较少讨论语义学,更多讨论学习语言的模型。从狭义的方面来看,这些不同的语言进路运用了相当不同的方法。本章的一个目标是表明,这些方法如何能够被统一在一般的集合论进路中。正如几个世纪以来在人类努力和思想的每个方面所发展与使用的那样,这些方法只研究了丰富而多样的语言本性的很少的部分。

问:您直到现在还领导着一个小组从事这些领域的研究,而且,您每天都到办公室处理许多问题。您目前研究的论题是什么呢?您希望回答什么样的哲学问题呢?

答:在许多方面,我目前的研究论题仍然没有改变,但我对许多问题的详细应用甚至我的哲学态度一直在不断地变化。正如你所指出的那样,无论如何,在我的实际工作中和我每天在办公室里把我的时间花费在三个一般的主题上。第一个主题是我继续从事网络教育,特别是现在的在线教育。这个主题在我们这里讨论的书中很少提到,但这是我生活的很大部分,而且到目前为止已有 50 多年。第二个主题是研究大脑,这是我在 1992 年退休后才开始的。从那时起,由于专注于大脑中的语言,我已经在我所说的系统神经科学方面花费了大量的时间。我在本书的第 8 章描述了早期的大脑研究,即,主要运用 EEG 记录大脑活动。在本书出版时,我在这里没有描述我在这个领域内自 2002 年以来的工作,但我只希望指出,我还会花大量的时间继续这种努力。第三个领域是我一直保持活动的领域,即使不是我精神生活的主导活动。那就是关于科学哲学有时是其他哲学部分的广泛论题的思考与写作。

一个最近的例子是我在 2009 到 2010 的学年中在斯坦福大学开设的关于自由意志的讨论课。当我听学生们讨论和与他们讨论关于自由意志的读物时,我开始意识到,我在其他学科领域内认为重要的概念仍然会出现。那就是在康德的自由观中很著名的自发性(spontaneity)概念、在 19 世纪的物理学中和现在到处的政治讨论中很突出的放射性的本质。也许,我不应该说到处(everywhere),因为某种奇怪的理由,在关于自由意志的现代文献中,现在几乎相当缺乏对自发性的讨论,事实上,关于道义和责任概念的讨论占居主导。然而,在常见的讨论、文献研究等许多方面,自发性无疑是关于行为的一个规范的概念。而且,哲学家认为,自发性是在不关注或不只关注责任之意的前提下我们作出肯定判断的许多行为的重要成份,我恰好就是这类哲学家。除了道德责任问题之外,同样重要的是,自由意志的表现是起作用的。

问:您对科学哲学的发展趋势有什么看法呢?

答:在科学哲学的近期发展中有许多我喜欢的方面。最重要是,极其强调关于科学研究的所有方面的实验本性。因为过去在很大程度上,哲学家几乎全部倾心于科学语言与理论,并过分地用理论术语来表达他们的变化观念,没有适当地关注他们在像天文学之类的学科中的实验或观察测量中用到的细节。我无疑看到的和未来很重要的另一种变化是,哲学家已经转向对生物学哲学给予相当的关注,当然,其中包括我自己现在主要对神经科学感兴趣。当我在 20 世纪 40 年代是一名哲学硕士生时,很少关注生物学哲学。事实上,我相信,在我的整个硕士生阶段以及我在斯坦福大学从事教学的头十年里,没有人说要教生物学哲学。

另一方面,我对概率基础和科学哲学的相关论题方面的发展深感不满。例如,运用统计学解释和支持特殊的科学假设或理论时,可能有更严密的和更关键的审视方法。这是一个大而复杂的主题,这无疑也不是作为权威专家的哲学家的职业责任,但从系统的观点来看,数据分析特别是统计分析产生的敏感而复杂的问题,也许是我们如何获得知识和如何判断知识好坏的最杰出的现代例子。后面这些问题很符合主要是认识论特征的经典哲学分析。然而,尤其与在日常生活中和关于人类生存的每个方面的科学中我们现在具有的统计数据和结果的洪流(torrent)相比,这些问题出现在哲学文献中,还是相对少有的。

问:我知道,您负责斯坦福大学的天才青年教育项目(Education Program for Gifted Youth)。因此,您既是一名哲学家也是一名教育学家。您愿意对此说点什么吗?

答:我在回答前面的问题时已经提到我对教育有浓厚的兴趣,但我高兴在这里重申,从一般的视角来看,我卷入教育已经成为我生活的最实际的部分。我的意思是说,除了实施教学和对教学的成败(特别对小学生来说)的评价之外,我已经深深地卷入了所有层次的教育。在人造卫星成功发射的 20 世纪 50 年代,我开始关注小学的数学教学,尤其因为我也已经坚定地参与到我的第一个孩子当时一年级的数学中。因此,我承认,我对人造卫星的第一反映是十分个人的。在我的网页(suppes-corpus.stanford.edu)上你所能读到的我的学术自传里,我已经详细地记载了某些事情经过。早期的这种兴趣很快发展到广泛关注我们的公立学校里的数学等学科的教学,因此,现在 50 多年来,我确信我每年都把大量的时间花在我对教育的兴趣上,而且,这些已经与我的其他科学与哲学兴趣一起幸福地实践着。事实上,我指导学习的实验和评价我自己或其他教育项目的许多努力,在许多不同方面,已经与我对概率基础和统计推理的兴趣重叠在一起。

最后这句话自然导向了结束回答关于我的哲学态度的许多问题的一种好方式。认识我的某个人最近对我说:"当我意识到,您的自然趋势是思考和致力于许多不同类型的事情时,我已经开始更好地理解您的生活。"这是我所知道的对我思考和致力于许多不同类型事情的最简单的说明。我不是说,像我这样的发展是一件好事,但准确地说,我拥有它。

(责任编辑:张帆)