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Contents

§1 Editorial

I am delighted to be the guest editor of this issue of *The Reasoner* and I wish to thank Jon Williamson and Federica Russo for the invitation. I will open this issue with an interview with Theo A. F. Kuipers. Theo is Professor of Philosophy of Science at the University of Groningen, where he taught till this academic year a number of courses firmly entrenched in the analytical tradition of "formal" philosophy of science.

As he made immediately clear in our conversation, Theo disagrees with the idea that the "classical" tradition of Carnap, Hempel and Nagel has been definitively superseded by the more recent trends in the "new" philosophy of science and in the sociology of scientific research. Logical analysis and "rational reconstruction"—or "explication", as Theo prefers to call it—are still important tools for the philosopher of science. This is a main message of his *Structures in Sciences* (Kluwer A.P., 2001), an "advanced textbook" in what Theo has dubbed "neo-classical philosophy of science". His previous book, *From Instrumentalism to Constructive Realism* (Kluwer A.P., 2000), is an outstanding essay of the neo-classical approach, and includes Theo's main results concerning confirmation, empirical progress and truth approximation (also known as *verisimilitude* or *truthlikeness*). In his career, Theo has worked on an impressive variety of philosophical problems, such as inductive logic, explanation, reduction and the structuralist view of scientific theories. His results on these topics are probably best illustrated by the papers collected in the two volumes of *Essays in Debate with Theo Kuipers* (Rodopi, 2005), edited by two of Theo's former PhD-students, Roberto Festa and Jeanne Peijnenburg, and a former post-doc, Atocha Aliseda. Thirty seven philosophers and scientists from all over the world comment on his work, and Theo replies to each of them in a lively debate which is the best evidence of the importance of his philosophical activity. Theo's last edited book is *General Philosophy of Science:*

Focal Issues (Elsevier, 2007), the first volume of the *Handbook of the Philosophy of Science* edited by Dov Gabbay, Paul Thagard and John Woods. The book collects a number of articles by leading scholars on the central topics in (neo-classical) philosophy of science and it is likely to become a standard reference for the scholars in this field.

I'm keeping this editorial short, in order to let Theo tell you about his intellectual history and his philosophical views. This preamble was to say that Theo is one of the few persons I know who can answer such "big questions" like: What is philosophy of science? What is its proper method? What has science to do with truth? As the reader will see, in our conversation we touched upon each of these problems.

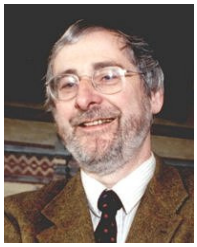
Gustavo Cevolani
Philosophy, Bologna

§2
Features

Interview with Theo Kuipers

Gustavo Cevolani: First of all, thank you for agreeing to be this month's interviewee. As far as I know, you studied mathematics, and you only later became interested in philosophy. Can you start by telling us how you first got into logic and philosophy of science as an area of research? Are there some "big names" in philosophy of science that, more than others, influenced your way of thinking and doing research?

Theo Kuipers: Looking for the nearest non-Catholic academic institution to study mathematics, I went in 1964 to the *Technische Hogeschool* (a polytechnic) in Eindhoven, where I got my kandidaats (B.Sc.) in 1967. However, I disliked the instrumentalist way of learning mathematics. Since I wanted to understand the working of, e.g., differentiation and integration tricks, I enrolled in philosophy of mathematics at the University of Amsterdam. I liked the course on the Foundations of Mathematics by Haskell B. Curry, the successor of Evert W. Beth, and other foundational courses by Kees Doets. Moreover, in my extended minor in mathematics I learned algebra, topology and probability theory. This was the kind of mathematics that I would have liked to have had from the beginning, which would have been the case had I started immediately at a "normal" university. In that case, however, it is unlikely that I would have switched to philosophy. As a matter of fact, I became even more attracted by areas taught by former students and co-workers of Beth, notably philosophical logic (Else Barth) and philosophy of science (Hans Mooij, Peter Wesly). Finally, I profited a lot from the courses in analytical philosophy of Gabriel Nuchelmans in Leiden. Carnap, Hempel and Nagel on the one hand and Popper on the other attracted me the most, the first three for their style and method, Popper for his ideas. These four represent what I like to call "classical philosophy of science". By the way, my false start in a technical science institution explains my lifelong interest in design science, an area that used to be neglected in philosophy of science.



GC: Your textbook *Structures in Science* (2001) is a manifesto of what you call the "neo-classical" approach to philosophy of science. Here, you complain that "the philosophy of science seems to have lost its self-confidence" and propose the neo-classical approach to overcome this "crisis". Which are the essential ideas underlying this approach?

TK: From the seventies on it became fashionable in international philosophical circles, and even more in Dutch circles, to suggest that authors like Kuhn and Feyerabend successfully abolished the insights of the classics, not to speak of the upcoming relativist sociologists of science. However, many cute babies were thrown away with the bathwater, such as the following. It was wrongly concluded that the distinction between observational laws and (genuine) theories depended on the assumption of a theory-free observational language. As argued by Lakatos, Kuhn's global analysis of scientific behaviour could well be reinterpreted as illustrating the rationality of science. Refinement of Nagel's analysis of reduction was perfectly possible, as shown by Nickles, Schaffner and Sklar. Finally, "concept explication" could well remain the main method, and in fact this happened in analytical philosophy of science, although it was seldom acknowledged. In sum, as a rule, the worthwhile insights of the critics of classical philosophy of science could and should be used for refinements, leading to neo-classical philosophy of science. Let me mention the example about which I claim to improve upon Lakatos: non-falsificationist behaviour of the instrumentalist type, as documented by Kuhn and Lakatos, is perfectly rational, because it is more efficient for truth approximation than straightforward falsificationist behaviour.

GC: A fundamental theme of your research has been the concept of verisimilitude or truthlikeness, and its applications. Can you explain in a few words what verisimilitude is and why it is important for philosophers of science? A curious impression one may gain exploring the literature is that verisimilitude is mainly a "European affair": is this impression misleading? Moreover, it isn't difficult to find discussions of scientific progress or realism that don't even mention truth-

approximation: what are the reasons for such a lack of interest?

TK: Let me first amplify this element of surprise. An important success of the first decades of (constructive) analytic philosophy was the discovery, notably by Russell, Carnap, Hempel, Beth and Barth, that the recognition of the relational character of concepts can be an important means in the solution of age old philosophical problems. This pertains not in the least to asymmetric relations that are constitutive for comparative concepts like "longer than", "caused by", etc. Similarly for the concept "better than", and hence for "improvement" and "progress". Even more than European ones, American contributors to the realism-antirealism debate seem to be unaware of the possible relevance of this insight. One continues to talk in classificatory terms: "true" versus "false" theories and reference claims on the realist side, and "empirically adequate" versus "inadequate" theories on the empiricist side. The weakening to "(not) approximately true theories" does not help, for it remains non-comparative and can explicate "progress" at most in a simplified, arbitrary way. Compare this with "(not) more or less long" to explicate growth. From the relational point of view it is rather plausible to think in terms of "empirically more successful" and "closer to the truth", the latter being the crucial notion behind "verisimilitude" (or truthlikeness). In terms of my favorite example, it may well be that Einstein's theory is false, it may even be far from the truth, but we have good empirical reasons to assume that it is closer to the truth than Newton's. In general, a false theory may or may not be close to the truth, but in both cases it may be closer to the truth than another one. The latter is more easy to assess, however provisional, than the former, notably by comparison of empirical problems and successes.

GC: A striking aspect of your work is your "formal" approach to philosophical problems, which is very different from the informal and "narrative" approaches so popular in contemporary philosophy of science. What are the advantages of a formal approach to philosophy of science and, more particularly, what is the role of "theorems" in such discipline? As an example, a central result of your *From Instrumentalism to Constructive Realism* (2000) is the so called "Success theorem". Can you explain in a few words the intuitive content and methodological importance of this result?

TK: As a rule, one engages in the explication of one or more concepts in order to explicate intuitions or to dissolve paradoxes in which these concepts are crucial. In case of intuition explication, the subsequent task is to prove a theorem to the effect that the intuition, if reformulated in explicated terms, becomes either justified, demystified or undermined, whatever the case may be. In case of dissolving a paradox, it has to be shown that it can no longer be construed in the explicated terms. One example is the (qualitative) explication of the intuition that empirical progress is functional for truth approximation, by proving first of all the "success theorem", according to which (actual, but not directly assessable) truth approximation entails assessable empirical progress. The methodological importance of this (simple) theorem stems from the fact that empirical progress can best be achieved by the instrumentalist methodology, according to which a falsified theory remains in the game as long as it is more successful than other (falsified) theories, whereas the falsificationist methodology is supposed to disqualify such theories altogether.

GC: Arthur Fine has famously claimed that "realism is dead", and many philosophers seem to agree with him. You have devoted much effort to defending a fairly strong form of scientific realism, "constructive realism". How healthy is realism today, in your view?

TK: Arthur Fine and his fellow diehard empiricists remain to take only hardnosed realism into account. Realist responses in the literature to the antirealist charges, such as Laudan's famous pessimistic meta-induction, usually are retreats of realism of a non-comparative and a non-constructive nature. In both respects my kind of realism, being constructive and comparative, is weak, but it is still a serious kind of realism (see my "Comparative realism as the best response to antirealism", to appear in *Logic, Methodology and Philosophy of Science. Proceedings of the Thirteenth International Congress*, Clark Glymour, Wang Wei and Dag Westertahl (eds.), Beijng, 2007). That is, although I neither believe in some kind of essentialism, leading to an ideal vocabulary fitting the natural world, nor in the idea that most of our most successful theories are true, I believe in two realist convictions. First, science can construct, by profiting from empirical findings, more and more suitable vocabularies for domains of the natural world, all of which have an unknown strongest true theory, that is, the truth about a given domain in a given vocabulary, and, second, by searching empirically more successful theories we approach that truth, as a rule. Without such a refined kind of realism there remain two mysteries. For the short term dynamics of theories it would be a miracle why certain theories remain more successful than other ones; this is a variant of Putnam's no-miracles argument. Moreover, there would be no basis for the long term, clearly successful, dynamics of science, according to which, for the time being, not just the most successful theories, but only extremely successful ones get accepted as (approximately) true. The important consequence of this 'theoretical induction' is that their theoretical terms can be added to the observational vocabulary, in the sense that they become applicable, that is, it becomes determinable whether they apply or not. However, for practicing scientists there is no compelling reason to become a constructive comparative realist. As

long as they aim at improving their theories they serve the purpose of truth approximation. But philosophers of science that remain unconditional empiricists persist in a kind of (indeed, strictly speaking, unrefutable) skepticism that neglects the task of trying to understand the very possibility of successful scientific practice, leaving that a double mystery. By the way, regarding more versus less successful theories we can only apply the rule of inference to the best one, that is, the most successful one, as the closest to the truth, a plausible correction of so-called inference to the best explanation (as the true theory).

GC: The fruitful interaction of philosophy of science with logic and Artificial Intelligence has recently produced a number of new methodological research programs: which are, in your opinion, the most interesting and promising ones? Are there any particular topics that you would recommend to philosophy graduate students starting out today?

TK: My favorite example would be "computational philosophy" in general and "computational philosophy of science", as initiated and developed by Herbert Simon, Pat Langley, Paul Thagard, and several others, in particular. In the latter, one tries to solve classical problems in the philosophy of science with means that have been particularly developed in cognitive psychology and artificial intelligence research. The kind of results aimed at are computer programs that enable certain cognitive tasks, or at least to simulate them, such as, discovering laws from data, designing hypotheses, evaluation and revision, concept formation, proposing experiments, etc. To be sure, the possibility for the computational philosophy of science to be of considerable practical relevance is still far away. However, in principle the perspective of more or less standard computer assisted discovery, evaluation and revision need not remain science fiction.

GC: Before, I asked you about your "intellectual models". Now, I would also like to ask you about the "bad examples" in philosophy of science. I was surprised, but also amused, to find an article where you criticised "the Pavarottis of analytical philosophy". I couldn't read it, since it was in Dutch, but perhaps you may tell us something about its contents?

TK: Without denying that philosophers such as Wittgenstein, Quine, Putnam, Davidson and Rorty have also written clear, original and defensible papers, about which analytical philosophy can be proud of, they frequently write so vaguely, unclearly and incomprehensibly that they can easily compete with those continental philosophers that are denounced for their obscurity. As with the latter, the messages of the former usually make some sense, but when understood it is clear that they could have been presented "claire et distinct" in a constructive analytical way. Now the writings of both groups too often function temporarily or permanently as intellectual prisons.

GC: Let me conclude this interview with a completely different and more general question. The fate of philosophy and that of liberty are strictly intertwined. The Netherlands has been the home of freedom of speech since the time of Spinoza. Nowadays, however, this glorious tradition seems to be under attack. The violent deaths of Pim Fortuyn and Theo van Gogh and the frightening threats to Ayaan Hirsi Ali and Geert Wilders immediately spring to my mind. The so called "Van der Horst affaire"—a recent case of self-censorship at Utrecht University, where Prof. van der Horst delivered an expurgated version of his retirement lecture, skipping any reference to Islamic antisemitism—suggests that even academic freedom is at risk. Can you share with us your feelings about the present situation of intellectual and political freedom in your country?

TK: That you, abroad, have heard of this unhappy Utrecht affair surprises me. A short answer to your question is almost impossible. Any violence of a fundamentalist nature, be it of ecological (in case of Fortuyn), Islamic (in case of Van Gogh), Jewish (in case of Rabin), or Christian (in case of the abortion doctor George Tiller) nature, should of course severely be condemned. Moreover, it should be possible to utter any criticism of whatever nature and subject. Assuming some mature prudence, based on understanding of educational limitations of many among us, the intellectual and political freedom in our country is still very impressive. As a philosophical addendum, I would like to conclude with the claim that debates about "respect" in this context frequently are at cross-purposes. We still have to learn the conceptual distinction between two kinds of respect, viz. mere tolerance and serious appreciation.

Can Nature Make an Argument?

The American philosopher C.S. Peirce (1839-1914) claimed that arguments, and more generally, "processes of reasoning," should not be looked at as a strictly human affair. Processes of reasoning, Peirce argued, are indicative of "mind" and he believed that mind is found throughout the whole of nature—not just within the human intellect. An argument, defined as "a process of inference leading to a conclusion," thus comes to cover a wide array of cosmic expressions on Peirce's view (Peirce, 1931: *Collected Papers of Charles Sanders Peirce*, Harvard University Press, 6.456.) Peirce's thesis was that the universe displays various processes of reasoning and that these processes are evidenced in the world's phenomena, most apparently through the evolutionary development that led to human beings who explicitly state arguments as such.

Peirce thought that the universe tends to behave "reasonably," yet he also thought that cosmic rationality allows for statistical variation from established law. Na-