

The work of Argentinean physicist and philosopher Mario Bunge as an exemplary life for the fruitful integration of philosophy and the sciences

Interview conducted by Ricardo Rozzi and Alexandria Poole at McGill University, Montreal, Canada, May 6, 2009¹

Argentinean physicist and philosopher Mario Bunge's life work serves as an exemplary model of the fruitful integration of philosophy and the sciences. As such, we are pleased to present a section of an interview with Professor Bunge, conducted in Montreal on May 6, 2009 by Dr. Ricardo Rozzi and Alexandria Poole.

Born in Buenos Aires on September 21, 1919, Mario Bunge's life was imbued from an early age with the tumultuous political and cultural climate of his native Argentina. In a 2007 interview for the *McGill Reporter*, Bunge remarks that he "learned to read in newspapers, not in school books, which bored [him]." This statement can be seen as a metaphor for Bunge's approach to his synthesis of thought throughout his career. As he approached reading through his interaction with the daily happenings of the world around him, he approached philosophy through his lens as a burgeoning scientist. He became a distinguished and tremendously prolific writer in both physics and philosophy (including the publication of an eight-volume *Treatise on Basic Philosophy*), and neither aspect of his career has existed independently of the other. As he describes in the following interview, Bunge strives to bring the empirical to the realms of theoretical physics, philosophy, and the social sciences (his most recent book, published in 2008, is a treatise on political philosophy). He argues that a philosopher of science is ineffectual when he has not "set foot in a laboratory," and likewise, that science can only blossom within a philosophical framework.

He received his doctorate in physico-mathematical sciences in 1952 from the National University of La Plata, while simultaneously guiding his own studies in philosophy. After serving on the faculty of the University of Buenos Aires in both theoretical physics and philosophy, Bunge immigrated to Montreal in 1966, where he has lived since and currently serves as the Frothingham Professor of Logics and Metaphysics at McGill University.

Ricardo Rozzi: Professor Bunge, first of all, thank you very much. As I told you, this is a program we are working on with the University of Magallanes and the Institute of Ecology and Biodiversity—in the south of Chile—which ties together various Chilean universities. We are also working with Argentinean and Latin American universities, and with the University of North Texas, on a program that integrates the ecological sciences with philosophy, in an intersection that aims to express environmental ethics and biocultural conservation.

Along with our thanks, I want to tell you that this is a series of micro-documentaries that we are making under the concept of exemplary lives. By exemplary lives, we mean a life that we admire, that has inspired us through its work. We would also be pleased to know what your life has been like, what has inspired you to integrate philosophy and the sciences. Please tell us some anecdotes about your life in Magallanes, and from there we will derive other specific questions.

¹ Interview filmed by Kelli Moses, transcribed by Luna Marticorena G. and Alexandria Poole, translated and edited by Charmayne Palomba and Ricardo Rozzi, and revised by Mario Bunge.

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Mario Bunge: I became familiar with Punta Arenas very briefly in 1931. I had just finished the first year of high school. I have very vivid memories of the landscape, and above all of the bluffs² that I do not believe I had seen anywhere else; I think they are also in Australia. Suddenly from a plain arises a bluff, a kind of isolated mountain, and I remember the landscapes as very different from those of Argentina, so nearby. There I really encountered, for the first time, the cordiality of the Chileans. Argentines have a deserved reputation as rude. Chileans, on the other hand—with the exception of Pinochet and the assassins—are known (at least in Argentina) as being a very sweet, very cordial, very friendly, very open, unaggressive, and very welcoming people. The times I have gone to Chile I have been welcomed with great caring and much cordiality, and have very easily become one among friends. That is an issue that has to do with sociology, and not with ecology.

You ask me how I came to philosophy, how I came to science. I was interested in philosophy when I was 16 or 17 years old, reading in a disorganized fashion. I was fascinated, and I was particularly fascinated by the philosophy of physics. At that time, in the year 1937, there were two physicists, very famous English cosmologists—Professor Arthur Eddington and James Jeans,³ who were excellent at writing about the cosmology of that time for the general public, and who, furthermore, were philosophically idealists. Jeans was a Platonist, and said that the universe is a system of mathematical symbols; Eddington, on the other hand, was a Kantian. He did not know it, but he was a Kantian—for him, everything was in the mind: space and time were forms of intuition. Despite the fact that Eddington had made a living and was a very distinguished astronomer, he was not introspective. Rather, he used a telescope, and looked outwards. At that moment, I decided to refute them, because I was a realist and I realized that in order to refute Jeans and Eddington—because I was a realist—I had to know physics because they were vulgarizers. So I decided to study physics, and I did so.

I began my studies of physics at the University of la Plata, the only university that had laboratories. At that time, the University of la Plata was perhaps the most progressive in Latin America, having been founded at the beginning of the 20th century by a very exceptional man, , Joaquín V. González, a greatly cultured man, and one who had a modern vision of the university. Instead of beginning with the professional schools—medicine, law and engineering—he started with the basic sciences, with the natural sciences, with the Museum of Natural Sciences, the Astronomical Observatory and the Institute of Physics. The Institute of Physics then started the Engineering program, and that is where we physicists and engineers studied. The physics courses were taught by the few physicists that there were at that time. Then everyone did practical work, unlike what was happening in the rest of Latin America, where the teaching of physics was purely verbal, and practical work was not done. At that time, the only ones doing practical work were the chemists; if they knew how to work in laboratories.

² Mario Bunge is referring to the “drumlins” that create small, elongated hills, in the shape of whales, formed by the movement of the glaciers, which abound in the Pampas located to the north of Punta Arenas on the outskirts of the Strait of Magellan.

³ Sir James Hopwood James (1877–1946) and Arthur Stanley Eddington (1882–1944) were British physicists and astronomers who wrote very popular books that discussed the philosophical and religious implications of the new physics.

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In any case, I studied physics very enthusiastically, and at the same time, on my own, I studied philosophy. In the Philosophy departments, idealist philosophers prevailed. In Buenos Aires, the neo-Hegelian idealism of Gentile, the collaborator of Mussolini, prevailed, and in La Plata the German historical-cultural school prevailed. In any case, there were no realists, no materialists, not one dissident voice. I studied more or less by myself, because the libraries were very poor, and I had to look for philosophy books in secondhand bookstores, used books, and little by little I was putting together a library and reading what I could.

No one read philosophy journals, no one read physics journals; the only ones that read physics journals were students. We formed two seminars: one at the University of La Plata and another at the University of Buenos Aires, and we met every week to comment on and discuss articles published in recent issues of the *Physical Review* or *Reviews of Modern Physics*. Our professors did not read journals, despite the fact that they had come from the best universities in Europe—one in Göttingen, another in Berlin; one of them had done his doctoral thesis with Max Planck. They devoted themselves to teaching because they thought that they could not do research in Argentina.

Fortunately, and by surprise, in 1943 an Austrian physicist named Guido Beck arrived. He had been the assistant of Heisenberg, one of the fathers of quantum mechanics. He immediately took me as a student, and gave me work in nuclear physics. In the meantime, I continued my studies in philosophy, and in 1944 I founded the journal of philosophy *Minerva*, which had a precarious existence of only one year. The goal was to fight irrationalism, in particular the existentialism, which had already begun to be felt in Argentina, of course, along with the political pressure of a semi-fascist regime. There was a semi-fascist coup d'état in 1943, which even directly led to the power of Perón.

There was no way of publishing, there were no publications; all journals of philosophy were from the universities, and were published by a few university professors, without revision and without control. To me, it seems like a mistake that the universities published their own journals without peer review, because that rather promotes a profession of “local celebrities.”

Then I published, and my first article was published at the age of 20. There were two very pretentious articles in philosophy. My first article in physics appeared in 1944. I published various articles on physics, and some texts in philosophy in North American and English journals. It is very difficult to publish in journals at the international level when one lives in a Third World country. One sends an article and the directors say, “ah, Buenos Aires, where is that? Ah, in Brazil!” they know immediately that it is in Brazil—that is, if they know. Then, in addition, my articles were not very well written despite the fact that I was the one responsible for revising the English of my colleagues in physics. My English was better than theirs, but it was still inaccurate. Anyway, I published in British and North American philosophy journals.

Later, in 1955, I went to Chile to teach a physics seminar and a short course on the problems of causality, something that had concerned me for many years because it was understood at that time that causality had died—that the world was not causal but that it was indeterministic. First the

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positivists and then the quantum physicists seemed to be determined. I wrote a book, *Causality*, which vindicated the role of causal laws. That book came out of the lectures I gave at the University of Chile. It was published by Harvard University Press, and later translated into eight languages.

The University of Buenos Aires had expelled me for not joining the Peronist party and for not contributing monthly to Eva Perón's foundation—two very grave offenses. At the end of 1952, I was fired from the School of the Sciences, and in 1956, after the fall of Perón, I was reinstated as a professor there. I taught quantum mechanics, and later I won a contest of philosophy of science in the Philosophy department. Going back to 1950, I was considered a physicist and an amateur philosopher, and spent most of my time earning a living and doing quantum physics. I was in favor of the official interpretation of quantum mechanics, called the Copenhagen school of interpretation,⁴ until I realized that it was false. I realized it with a very simple example: according to the Copenhagen interpretation, physical theories refer to observations and measurements, and not to reality. Then, all the variables that figure into a physics theory can be measured, and they all refer to objects that are being subject to observation and experimentation. But the simplest example is the free thing, which is not subject to any external action, and which in particular is not being subjected to operations of observation and experimentation: a free particle or the free photon, or an isolated thermodynamic system, etc.

Then I realized that physics was becoming—real physics was turning out to be—the operationalist, positivist, semi-subjectivist Copenhagen interpretation.⁵ Now then, what do we put in its place? What do we replace it with? David Bohm believed that the theory had to be changed. I believed him at that time, and went to Sao Paulo on a post-doctorate for six months. But nothing came of that except my decision to write books about the problem of determinism in general, and in particular, about causality, problems of determination; these were ontological, metaphysical problems.

At first, I was very enthusiastic about Bohm's theory, and I taught it first in Buenos Aires and later in the United States—the idea of hidden variables—but suddenly I realized that it wasn't good for anything. Causal theory wasn't like Bohm thought it was, because he did not deduce the distribution of probabilities. Rather, he supposed it was the same as the Copenhagen theory, and I realized further that new solutions or experiments did not arise. Then I changed the focus of my work and decided to formulate standard quantum mechanics in purely objective terms, in terms of physical things regardless of observations. That took me a couple of years. I had the fortune of obtaining a Humboldt fellowship and spent a very productive year in Freiburg, in southern Germany, writing my book *Foundations of Physics*, in which I compose axioms, schematic sciences, and a lot of basic theories, among them the realist quantum mechanics. And earlier, in Buenos Aires, I had begun writing my book *La investigación científica, su estrategia y su filosofía* (*Scientific Research*), which was published in 1967, the same year that *Foundations of Physics*

⁴ Giovanni Gentile (1875–1944), known as the “philosopher of fascism,” revitalized Hegel's thinking in the 20th century.

⁵ The Copenhagen interpretation refers to the interpretation of quantum physics primarily developed by the physicists Max Born, Werner Heisenberg and Niels Bohr (who studied and worked at the University of Copenhagen until 1943, when he fled from Nazi persecution and settled in the United States to work on the Los Alamos National Laboratory and the Manhattan Project).

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came out. When I wrote them, I had not found any manual of the philosophy of science that satisfied me. I found that the existing manuals had been written by philosophers that had never set foot in a laboratory, who were not interested in anything but physics theory, that were not interested in experiments because they had never done an experiment. Almost all of them were positivists, empiricists, but only verbally. Because an authentic empiricist has to be basically interested in scientific operations, such as observations of the earth.

RR: That is interesting. It is something we have discussed quite a lot, in the phenomenological approach to ecology and environmental ethics. Paradoxically, oftentimes people that are promoting, defending and even developing a phenomenological theory, and have supported our proposal about the importance of field ecology or field environmental philosophy, are academics that do not work in the field. Hence, yours commentary reminds me of a situation that we are faced with today.

MB: The strange thing is that it is not realized, but this is the case. My book *Scientific Research* is the first book on the philosophy of science in which there are chapters on observation, measurement, and experimentation. There is still much that must be done in the philosophy of experimental science. I had a Brazilian student, certainly very good, who did a master's thesis here with me about the philosophy of experimental physics. The problem is that he returned to Brazil and did not do his doctorate.

RR: You said that at the time that you were debating with the positivists, but without involving yourself, how would the revolution of this integration between the theoretical and empirical approaches be brought about?

MB: Well, I continued occupying myself with physics, and published an article now and again, the last of which was in 2001, but never on experimental physics. Experimental physics, as the great mathematician David Hilbert once said, is much more difficult than theoretical physics. It is necessary to have experimental ingenuity, and it is very time-consuming. In any case, I stopped doing physics intensely when I came here to live in 1966. In Montreal, I finally had the opportunity to write a philosophical treatise from a new point of view, reconsidering the previously proposed ideas, and integrating science and philosophy subjecting philosophical theses to scientific proof. A philosophical thesis is plausible if it is in agreement with current science; it is better still if it suggests some new ideas in science. I believe that science only flourishes in a philosophical matrix, and if it is absent, science disappears. If the philosophical matrix is bad, for example, if scientists let themselves be influenced by the irrationalism of phenomenologists, existentialists, intuitionists, those called postmodernists, who are irrationalists and anti-scientists. Yet a matrix still exists, a philosophical nest that allows the normal development of "the chicks." For me, this nest is composed of (i) a materialist ontology, (ii) a realist theory of knowledge, (iii) a humanistic ethics, particularly in the case of the social sciences, and (iv) a scientific methodology, which affirms that the scientific method is the best way to get closer to truth.

RR: We had been thinking of asking exactly about advice for students that would attempt integration between philosophy and science today, with a step towards ethics.

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MB: I think that students are naturally interested in the history of science. Every time I have made a historical reference in my physics courses, the students question me, they want to know more. As for me, when I studied physics, chemistry and mathematics, I always read books or articles about historical aspects. People like to know where all that comes from.

RR: How beautiful, the methodological value of the history of ideas for the integration of philosophy and the sciences.