

Michael Polanyi: A Man For All Times

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ABSTRACT Key Words: moral inversion, potential theory of adsorption, epistemology, Sovietology, economics, Karl Marx, Magda Polanyi, paradigm, Tillich chair.

This article is a response to the Scott and Moleski biography of Michael Polanyi by one of Polanyi's last students.

Bill Scott's biography of Michael Polanyi has been a lifetime in coming. When I first met Michael in 1961, I was 22 years old. He was 71. Not then, nor in later years, did Michael ever strike me as being an old man. Michael's intellectual dynamism came from the power of his mind to produce new insights that cast new meaning on so many subjects. Michael was the most exciting person I ever encountered. Now 45 years later and 30 years after his death, I have been asked by *Tradition and Discovery* to provide my views of the long awaited biography.

I approach this task from memory alone. Somewhere I have boxes with copies of Michael's manuscripts and our correspondence. If I had time to go through them, it might sharpen my memory and provide interesting anecdotes of those long ago times. However, I do not believe that it would alter my response to Bill Scott's labors, which were put into final form by Martin Moleski.

It is, perhaps, useful to the reader to know of my association with Michael. In 1961 the Thomas Jefferson Center for Political Economy, a unit of the economics department at the University of Virginia in which I was a graduate student, invited Polanyi to give a seminar to graduate students and the lectures, "History and Hope."

The following year I was a graduate student in economics at the University of California, Berkeley. I discovered that Polanyi was at the Center for Advanced Study in the Behavioral Sciences in Palo Alto. A sympathetic Berkeley political science professor accepted me for a 4-credit hour seminar and released me to Polanyi. The following year I returned to the University of Virginia. The year after that, the University of Virginia economics department released me to Merton College, Oxford, to do my Ph.D. dissertation under Polanyi during the 1964-65 academic year.

My dissertation topic was the theory of economic planning. Whereas I did a tremendous amount of reading in the Bodleian on my subject, I spent my many hours with Michael discussing his philosophy and neglected the dissertation.

In 1966 I was appointed assistant professor of Economics at Virginia Tech. I finally wrote my Ph.D. dissertation and received the degree from the University of Virginia in 1967. That summer found me back in Oxford. I was again in Oxford for the fall and winter terms of 1968-69.

I think 1968 was the year that Michael and I were appointed visiting professors in the chemical engineering department at the University of California, Berkeley, to give a course on his epistemology (Scott says it was 1967). Michael backed out—I think because he felt pressed to get more of his ideas down on paper.

I obtained leave from Virginia Tech and grants from the American Philosophical Society and Earhart Foundation and went back to Oxford. I was invited to give a Special University Lecture on my Soviet economic work, which was beginning to appear in scholarly journals.

Through 1969 I was thoroughly versed with Michael's philosophical work. I read his manuscripts as he developed his thoughts and took his ideas into more fields. Michael was driven to overthrow a misunderstanding of science and an epistemology that he saw as destructive of civilized life.

I saw it also and was moved with the same spirit. Perhaps if we had given the course at Berkeley, I might have found an appointment at a college or university interested in Polanyi's thought, left economics, and taken up his philosophy and projects that he was never able to undertake, such as an explanation of 20th century history in terms of moral inversion.

When the Berkeley opportunity passed, I turned to the task of putting straight Soviet economic experience. My book, *Alienation and the Soviet Economy*, published in 1971, was a fundamental challenge to Sovietology. I explained the Soviet economy as the outcome of a struggle between inordinate aspirations and a refractory reality. The Soviet economy was not centrally planned, but its institutions, such as material supply, reflected the original aspirations to establish a non-commodity mode of production. My work was indebted to Michael's insights, and I dedicated the book to Michael. Two decades later with the collapse of the Soviet Union, my book was republished without a word changed.

In 1973 my book, *Marx's Theory of Exchange, Alienation, and Crisis* (co-authored with M.A. Stephenson) was published. This work established a central thesis of *Alienation and the Soviet Economy*—that Marx's critique of capitalism was due to its commodity character and that the purpose of central planning was to eliminate exchange relationships. The work also showed that Marx regarded violence as the effective force in history and that Marx most certainly was no humanist, thus establishing Michael's disputed contention that Marx left violence alone as the effective force in history.

In 1969 I married an English girl. Our frequent trips to England kept me in touch with Michael and Magda. After Michael died, we continued to visit Magda. Magda loved to be taken to the Trout Inn for lunch. Magda was never one not to speak her mind, and she complained about the lack of progress with the biography, expressing her wish that I had undertaken the task.

There had been some discussion about me doing the biography. Michael thought it would be unwise as it would be difficult for me to establish myself as a scholar in my field on the basis of a biography of a scientist and philosopher.

Reading Scott's biography, I realize it would have been unwise for another reason. Scott has provided what must be a masterful account of Michael's scientific work. All I knew of Michael's work as physical chemist came from reading "My Time with X-Rays and Crystals" and "The Potential Theory of Adsorption" and from stories he told me. All I would have been able to do would have been to connect his experiences as a distinguished scientist with his insights into scientific organization and authority, discovery, and the nature of knowledge. Scott provides a detailed account of Michael's discoveries, research teams, and colleagues that is marvelous.

The only thing I can say about Scott's account of Michael's scientific life is that the potential theory of adsorption played a bigger role in Michael's mind than in the biography. Scott provides a detailed account of Michael's work on his theory and its progress, but Scott's account struck me as a smoother and less controversial experience than I remember from Michael. I received the impression from Michael that his theory was potentially threatening to his career, that he could not get fellow scientists to reconsider it even after he had completely proven it, and that he was not permitted to teach it at Manchester. The impression I received from Michael is that his theory was essentially forgotten until two scientists rediscovered it around 1960. When they published their results, they received a lot of favorable attention until someone declared it to be Polanyi's old error. They stuck up for their work, and the theory was finally accepted. I remember this being the reason the editors of *Science* asked Polanyi to write his account published in 1963 as "The Potential Theory of Adsorption: Authority in Science Has Its Uses and Dangers." Perhaps Polanyi's son, John, could clear up this matter.

Polanyi's potential theory of adsorption is a case that illustrates that science is a belief system in which explanations at odds with reigning paradigms can be rejected even though they are correct. Polanyi's explanation of science has powerful implications for the reliability of information in society. Scientific information is the most reliable as it is subject to evidence and the overlapping competences of authorities of integrity who are committed to finding the truth. Social science, news, and public opinion have weaker factual bases and are subject to emotion, ideology, and propagandistic manipulation. For example, misinformation about weapons of mass destruction was used to manipulate Americans into accepting the invasion of Iraq. According to Paul Krugman (*New York Times*, May 29, 2006), NASA climatologist Dr. James Hansen's early warnings about global warming were misrepresented by academics in the pay of the energy industry. Supply-side economics, an important development in macroeconomic economic theory, was misrepresented as a claim that tax cuts pay for themselves. As Polanyi understood, knowing is a perilous undertaking.

Family history and Michael's work as a scientist comprise most of the biography. As the rigors of my own career pried me away from Michael's philosophy, I was unable to keep track of those who turned to Polanyi or to know the extent of his influence. It has been my impression that Polanyi's influence is mainly among scholars in religious studies. This is hardly surprising as Polanyi's epistemology restores meaning to religion, but wipes out the human capital of academic philosophers. The paradigm change is, perhaps, too massive.

What I find missing in the biography is mention of Michael being offered the "Tillich chair" at Harvard. I don't remember whether Michael was offered the chair or asked about his interest. I do remember that I argued with him to take the chair, as it would give him graduate students to carry on his work. I remember him replying that he was not going to help philosophers get rid of him by accepting an appointment in religion. I don't remember the year that Michael was approached about the chair, but someone besides myself must remember it, perhaps Richard Gelwick.

The last 55 pages of the biography, which covers the period that corresponds with my time with Michael, passes too lightly over Michael's concept of moral inversion. This concept was important to his thought and was a subject to which he intended to return. Michael described moral inversion as the consequence of the incompatibility between the demand for moral progress and the skepticism of morality. This inconsistency restricted moral passions to accusations of immorality and to fierce hatred of accused groups and social institutions. Moral inversion was at the core of Michael's understanding of the violence of the 20th century. He hoped to explain, in insightful outline, 20th century history in terms of moral inversion.

If memory serves, as far as Michael got with this project was his essay, “Why Did We Destroy Europe?” I have always wanted to undertake this task, but at 67 years of age it is an unlikely one for me. If anyone has taken it up, I would be pleased to know of it and to respond to it as best as I can.

Bill Scott and Martin Moleski have done a great service both to scholarship and to those interested in the life, times, work and thought of Michael Polanyi. A polymath presents an enormous challenge to biographers. To deliver so much of Polanyi in 300 pages is a remarkable achievement. The biography’s strength is its detailed chronology of the subjects to which Polanyi turned his attention and the people with whom he worked. What the biography needs is a section on Polanyi’s epistemology, contrasting it with other approaches, and explaining how Polanyi’s explanation of the nature of knowledge came from his experience seeking truth as a physical scientist. Those interested in Polanyi the economist can find my account of Michael’s seminal work as an organizational theorist and as a macroeconomic theorist in *Emotion, Reason and Tradition*, edited by Struan Jacobs and R.T. Allen.

Michael was an idealist without any illusions. The portrait that Scott and Moleski give in the last few pages of the biography is consistent with my own impression of Michael. He believed that it was our duty to find and to defend truth. He understood that thought was a dangerous enterprise and could end in French and Bolshevik revolutions. Michael was uniquely gifted, and he did his best to share his gifts with the rest of us.

In Michael’s time many areas of scholarship were affected by emotionalism resulting from political polarization. Michael’s view of progress as a piecemeal accretion within a cultivated set of beliefs did not satisfy those who believed improvement required more radical change. For Polanyi, even revolution is grounded in a prior set of beliefs.

Scott records the uncharitable response of a Hungarian-born Balliol don to Polanyi’s appointment to Merton. I remember attending this don’s lectures. He had recently served in a ministerial capacity in a Labour government. His lecture began inauspiciously. He turned to the blackboard to write the marginal conditions for economic efficiency, no doubt with a view to an attack upon them, but couldn’t produce the simple equation. After several attempts, he threw down the chalk and said, “Efficiency! Bah! What’s important is people.” People, of course, had not proved to be very important to revolutionary socialist governments.

As a final thought, people responded to Michael’s insights long before he put them clearly on paper. How often has a university changed a professor’s chair from chemistry to social studies, much less before publication of a major work in the new field that would justify such a change? Michael’s problem was that he knew more than he could tell, because he did not know the academic jargon of the fields that he invaded. *Full Employment and Free Trade* is a highly seminal work in economic theory, but it was ignored because it was not written in the economists’ language. His academic friends tried to help him by demanding clarity of language, that is, the academic jargon of their respective fields. Michael needed to address his varied audiences in language that they understood, but there is a clarity in Michael’s writings that conveys information that the literal-minded cannot absorb. Sovietologists and economists did not understand what Michael was talking about, but I and others did. I believe that today philosophers still don’t know what Michael is talking about.