

Caldwell, Bruce

Working Paper

Remembering Mark Blaug

CHOPE Working Paper, No. 2012-06

Provided in Cooperation with:

Center for the History of Political Economy at Duke University

Suggested Citation: Caldwell, Bruce (2012) : Remembering Mark Blaug, CHOPE Working Paper, No. 2012-06, Duke University, Center for the History of Political Economy (CHOPE), Durham, NC

This Version is available at:

<https://hdl.handle.net/10419/149688>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

REMEMBERING MARK BLAUG

BY BRUCE CALDWELL

CHOPE WORKING PAPER NO. 2012-06

APRIL 2012



Remembering Mark Blaug

Bruce Caldwell

Department of Economics

Duke University

Box 90097

Durham, North Carolina 27708-0097

E-mail: bruce.caldwell@duke.edu

Abstract
of
Remembering Mark Blaug

In the paper I offer some vignettes on my relationship, both professional and personal, with Mark Blaug, and by way of example reflect on his impact on the history of economics.

Keywords: Mark Blaug, history of economic thought, economic methodology, Karl Popper, falsificationism, Imre Lakatos

JEL Codes: B2, B31, B4

Remembering Mark Blaug

Bruce Caldwell

Mark Blaug had a greater impact on my ideas and career than any other person. He was a role model who eventually became a friend. It was a privilege to know him and I will miss him dearly. In what follows I will provide some snapshots of our relationship, which lasted my professional lifetime.

I first “met” Mark Blaug, as many other generations of students had, when I took a graduate course in the history of economics (in my case, in spring 1975 under Vincent Tarascio at the University of North Carolina – Chapel Hill). His *Economic Theory in Retrospect* (we used the revised edition of 1968) was the required text and, basically, our Bible. Mark wrote with a style all his own: he was humorous, opinionated, insightful, and very, very clear. Long before Deirdre McCloskey was writing about rhetoric and the power of persuasion, Mark was providing an exemplar of it. The book sold me on becoming an historian of thought.

It also helped me to become one, in a very specific way. The book contained a number of “Reader’s Guides” in which Mark summarized and commented upon the key elements of canonical texts. These were of course invaluable when I was putting together my own class in the history of economic thought for the first time. I suspect that his book provided the same service for literally scores of teachers of the history of economics.

As important as it was for the development of so many future teachers of the history of economics, the book had, in my estimation, one flaw: its emphasis on “translating” the ideas of

earlier writers into models. It followed from the ultimate economics profession dictum: Real economists talk in models. I resisted this approach when I prepared my class: there is only so much time in a semester, and I preferred instead to try to place the economists we studied in the context of their times. It was not until later that I came to realize that Mark's approach was yet another manifestation of the power of positivist ideas, that scientific attitude that so strongly influenced our profession in the middle of the last century. If economics is a cumulative positive science, then surely we should be able to show how the earlier writers' ideas (or at least, those of any early writer who is any good) can be expressed using the tools of today.

My next encounter with Mark started out very badly. I completed my dissertation, which carried the descriptively accurate but charmless title, "Recent Methodological Thought in Economics: Survey and Critique from a Philosophy of Science Perspective," in spring 1979. The next year I decided to turn it into a book. I sent a copy of the dissertation along with a proposal about ways I might change it to make it more publishable to Cambridge University Press. After what felt like an eternity they sent me a letter turning it down: it was too similar to a book they already had under contract. To my horror I soon found out that my competitor was none other than Mark Blaug. Just my luck: the person from whom I had learned my chosen field, the person whose writing inspired me to become an historian of economics, he whose very book I had so recently and shamelessly cribbed from, had written a text on the same topic as mine.

Mark's *The Methodology of Economics, or, How Economists Explain* (1980) arrived in my departmental mailbox in late November 1980. It sat on my desk for about an hour, as I worked up the courage to open it. Over the course of that hour I almost convinced myself that

though his title would *seem* to indicate that he had written a book on *roughly* the same subject, that surely there would be substantial differences because there are so many ways to approach such a complicated topic.

When I finally opened it, I almost passed out. He had not only reviewed the philosophy of science and then used it to examine the methodological literature in economics (exactly as I had done). *Worse*, he went back further in time in reviewing the methodological literature. *Still worse*, he provided nine chapters in which he offered an assessment of various parts of the neoclassical research program. *And worst of all*, as I already well knew, this guy could write. His first section was titled, “What You Always Wanted to Know about the Philosophy of Science but Were Afraid to Ask.” Look again at the colorless title of my dissertation. I was doomed.

A week later I finally got up the nerve to begin reading it. Slowly but surely I realized that Mark and I differed in one key respect: he placed much greater weight on the methodological prescriptions of Karl Popper and Imre Lakatos than I did. Product differentiation was possible! I first began formulating my criticisms of Mark’s approach in a book review that though generally laudatory also included the line that Mark was “too fierce of a falsificationist” (Caldwell 1981, 245). When I published my own book the next year (one that carried the catchy title of *Beyond Positivism*) I included a chapter in which I criticized falsificationism and endorsed “methodological pluralism.”

Mark reviewed the book and savaged the idea of methodological pluralism. As he put it, in good Blaugian style:

...he advocates 'methodological pluralism,' or 'let a hundred flowers bloom.' To me this seems to be tantamount to the abandonment of all standards, indeed, the abandonment of methodology itself as a discipline of study.

... 'Methodological pluralism' is a sham, an excuse for never making any final judgments about competing theories. I do not believe that Bruce Caldwell really means what he says. Still, he has discovered a very effective rhetorical device for making even methodological 'dogmatists' like myself think again. His book will set a cat among pigeons (Blaug 1983, 3, 5).

It is a testimony to Mark's even-handedness that even as he laid out his case against pluralism and in favor of the importance of testing and falsificationism, he praised my book for its treatment of the philosophy of science, which was somewhat more detailed than his had been. This was the basis for the incredibly generous line in his review that "if one is only ever going to read one book on the philosophy of economic science, this is the book to read" (Blaug 1983, 1). This magnanimous gesture toward a junior scholar really made an impression on me.

I finally met Mark Blaug in the flesh when Bob Coats introduced us at the History of Economics Society meetings in Pittsburgh in May, 1984. He turned to me and said, "So you're Bruce Caldwell," then leaned back as if to throw a mean right hook at me. It was totally unexpected and I laughed out loud, as he smiled broadly. Like the girl in *Jerry McQuire*, he had me at hello.

Mark and I corresponded about twice a year over the next several years, exchanging papers and hashing out our positions. His letters were always handwritten. He pulled no punches when offering his opinions, on my work or those of others. About one prominent methodologist he said simply, “B. is crazy” and about another’s position he said, “My view is: yes, but so what?” (Blaug to Caldwell, 12 Oct. 1983). Then there was his reaction to the man who occasionally buttonholed people at meetings:

I am not surprised to hear about your experiences with S. He sells himself to all comers and his writings are always impossible to follow. He is a sort of charlatan but possibly not altogether a fraud but simply someone who cannot communicate. I love the way his wife traipses along wherever he goes, adoring him non-stop. If only we had such uncritical admirers! (Blaug to Caldwell, 18 June 1984)

Regarding my benighted views, this opening barrage is a favorite:

I am glad to see that you are willing to admit past mistakes in your “Clarifying Popper” but you go on making new mistakes, so that I sometimes wonder whether I will ever catch up with you. Still, I will never abandon the task of converting you to SANITY, that is, FALSIFICATIONISM (Blaug to Caldwell, 9 Feb 1990).

We also frequently met at meetings, two of them expressly on the themes so dear to his heart: a December 1985 meeting in Amsterdam on “The Popperian Legacy in Economics... and Beyond” and an October 1989 meeting in Capri on the legacy of Lakatos, each of them giving rise to a conference volume (De Marchi 1987; De Marchi and Blaug, 1991). An incident at the 1987 History of Economics Society meetings is the one I most often enjoy recounting. The

meetings that year were at Harvard and were very well-attended with many international participants. At one point I was approached by someone who said that there were a couple of Japanese scholars who wanted to meet me. He made the introductions, and after we shook hands all around I quickly realized that their command of English was equivalent to mine of Japanese, that is, zero. We stood there uncomfortably for a moment, when suddenly another gentleman ran up to us, excitedly repeating a phrase over and over to his friends in Japanese. The scholars I had just met looked at me apologetically, as if to say, well, under the circumstances we *must* go, sorry, and they themselves were now murmuring the phrase. As they left I suddenly realized that the phrase was “Mark Blaug”! Their friend had spotted Mark and they were now rushing over to shake his hand.

In my opening remarks I alluded to Mark’s influence on my ideas and career. I have already mentioned how important his textbook was for me. But even more important was the extended debate we had over the merits of Popper’s ideas, and where they led me. A key encounter took place at a conference on interpretation in economics that was held at Wellesley College immediately preceding the Harvard 1987 History of Economics Society meetings. As I was later to recount:

Wellesley is one of the Seven Sisters and the men’s room in the building in which we met reflected it. Since the male-female ratio among economists interested in interpretation mirrors that of the profession as a whole, there was a line in the men’s bathroom at coffee break time, and it was close quarters to boot. Behind me stood Mark Blaug, and hoping to pass the time a little less uncomfortably, I asked him an impossibly big question. I said,

“Mark, you and I have been arguing about the merits and limitations of Popper’s falsificationist methodology for nearly a decade now. Where do you think our argument stands?” After about a three-second pause he said, “We both think falsificationism is hard to put in effect in economics. You say we should abandon it, and I say we should try harder.” I was stunned. Ten years’ worth of work, and he had summed it up, under duress, in two sentences (Caldwell, 1994, pp. 137-38).

This episode led me to read or reread everything I could find about Popper’s position, to see if Mark was right. As I did so I came to a much better understanding, as well as appreciation, of Popper’s critical rationalism, and to recognize that the methodological pluralism I had long endorsed shared many similarities with that doctrine. In any event, after I had exchanged a few rather lengthy letters with Mark, I wrote a paper on Popper that, in its first draft, read like an open letter to Mark. After I changed it into a more traditional paper, it was eventually published in the *Journal of Economic Literature* as “Clarifying Popper” (Caldwell 1991).

Getting a paper on economic methodology published in such a prestigious outlet certainly helped my career. But equally important, in the course of doing research for the paper, I wrote to a Popperian philosopher of science, Bill Bartley. We exchanged letters, and I met him in California while giving a paper on Hayek’s methodology there. Bartley was the General Editor of the book series, *The Collected Works of F. A. Hayek*. Bartley died, after a brief illness, in February 1990, and I dedicated “Clarifying Popper” to him. My having met him, however briefly, was nonetheless instrumental in my becoming the editor of a couple of volumes in that *Collected Works* series, and ultimately in my succeeding Stephen Kresge as the General Editor in 2002. My debates with Mark Blaug led me, by way of Popper and then Bartley, much deeper into Hayek, my research home for the past two decades.

I would also like to think that my growing enthusiasm for the methodological ideas of Hayek and the Austrians rubbed off a little on Mark. For this I have but one slender bit of evidence, something he said in his Afterword to the volume on Lakatos:

Modern Austrian economists go so far as to suggest that the Walrasian approach to the problem of multi-market equilibrium is a *cul de sac*: if we want to understand the *process* of competition, rather than the equilibrium *end-state* achieved by competition, we must begin by discarding such static reasoning as is implied by Walrasian GE theory. I have come slowly and extremely reluctantly to the view that they are right and that we have all been wrong (Blaug 1991, 508).

I last saw Mark in June 2003 when, with my wife and two children, I visited him and Ruth in their lovely farmhouse in Tavistock, Devon. We managed to arrive just as Nate Rosenberg and his wife did, but Mark and Ruth handled the overloaded house with aplomb and good cheer. At dinner my famously finicky eater of a son Sam, aged 11, picked at his food. Good indulgent American parents that we are, we had a jar of peanut butter at the ready, to slather on a piece of bread as we had done a number of times before so that he wouldn't starve his way across England. Mark took another approach. First, he told him how good the food was, and that he was really missing something. Then he tried bribery. "Sam," he said, "what if I paid you to eat that piece of meat before you? I'll give you five pounds to do it." Sam's eyes became very, very wide. He knew five pounds was a lot of money.

Ultimately, Sam still turned him down. But he never forgot his encounter with this strange, intense, adult human being. Nor would anyone who was fortunate enough to know him.

References

- Blaug, Mark. 1968. *Economic Theory in Retrospect*. Homewood, IL: Richard D. Irwin. 5th ed. 1996.
- _____. 1980. *The Methodology of Economics, or How Economists Explain*. Cambridge: Cambridge University Press. 2nd ed. 1992.
- _____. 1991. "Afterword." In Neil De Marchi and Mark Blaug, eds. *Appraising Economic Theories: Studies in the Methodology of Research Programs*. Aldershot: Edward Elgar, pp. 499-512.
- Caldwell, Bruce. 1981. "Book Review: Mark Blaug, *The Methodology of Economics, or How Economists Explain*." *Southern Economic Journal*, vol. 48, July, pp. 242-45.
- _____. 1982. *Beyond Positivism: Economic Methodology in the Twentieth Century*. London: Allen and Unwin. Reprinted with a new preface by Routledge, 1996.
- _____. 1991. "Clarifying Popper," *Journal of Economic Literature*, vol. 29, March, pp. 1-33.
- _____. 1994. "Two Proposals for the Recovery of Economic Practice." In Roger Backhouse, ed. *New Directions in Economic Methodology*. London: Routledge, pp. 137-53.
- De Marchi, Neil, ed. 1988. *The Popperian Legacy in Economics: Papers Presented at a Symposium in Amsterdam, December 1985*. Cambridge: Cambridge University Press.
- De Marchi, Neil and Blaug, Mark, eds. 1991. *Appraising Economic Theories: Studies in the Methodology of Research Programs*. Aldershot: Edward Elgar.