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REFLECTING ON *BEYOND POSITIVISM* AT THIRTY-FIVE

BY

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Abstract

Reflecting on *Beyond Positivism* at Thirty-Five

In 1982 my book *Beyond Positivism: Economic Methodology in the Twentieth Century* was published. At the 2017 History of Economics society meeting, a session was held to mark the 35th anniversary of that event. Papers by Wade Hands, Kevin Hoover, Tony Lawson, and the trio Peter Boettke, Solomon Stein and Virgil Storr were prepared. In this paper, I respond by reflecting on how I came to write *Beyond Positivism* and on the state of the field of economic methodology at the time, and then commenting briefly on each of the papers noted above.

Keywords: positivism, methodology of economics, philosophy of science, methodological pluralism, instrumentalism, theory choice, Karl Popper, Friedrich Hayek, complex phenomena, basic economic reasoning

JEL Codes: B2, B25, B4, B41, B53

Reflecting on *Beyond Positivism* at Thirty-Five

Bruce Caldwell

I will begin by thanking everyone who has taken part in this symposium – the editors for suggesting it, the participants who took the time to consider once more the arguments contained in this ancient book, and the people who attended the session at the 2017 History of Economics Society where the initial papers were presented.

I have to confess that when Scott Scheall first proposed a symposium to me, aside from the usual emotions of sheer astonishment and of being deeply flattered, I also felt a certain ambivalence. Though the book was published in 1982, it was, after all, a work based on a dissertation, parts of which dated to the mid-1970s, a piece of juvenilia, a faded snapshot from a long ago time. It also reminded me that I am no longer a contributor to this literature, having become much more fully a historian of economics. And finally, it is a reminder that I am not getting any younger. I suspect that the decision to celebrate a 35th anniversary – an odd year – was at least in part due to the realization that were one to wait until the 50th anniversary there would be little guarantee that anyone who had read the book when it first appeared, much less the author, would be alive or, if so, sentient.

Any apprehensions that I may have had were quickly dispelled when I read the four lovely essays that were prepared for this symposium. I will respond briefly to each one through the course of this piece. But first I will take a little space, in good historian style, to tell some stories, specifically, stories about how I came to write the book, about its reception, and about the sorts of things that were going on at that time in the then re-emerging field of economic

methodology. I say re-emerging because interest in methodology never disappears altogether, but it does wax and wane. At the time I wrote it was beginning to take off again.

Part of the motivation to write such a book was my reaction to my experience of graduate education in economics at UNC in the 1970s. As an undergraduate I took a lot of courses in philosophy, religion, and history because I found their subject matter fascinating, but I ended majoring in economics, partly for practical reasons – I thought it would be easier to get a real job with such a degree – and partly because I thought that it provided interesting insights into how the world worked. I graduated in 1974, right in the middle of a recession when job prospects were few, so I decided to further my education. Graduate school was a bit of a shock. Everything I liked about economics was absent. In its place was modeling and metrics, neither of which I was good at or found interesting. When we got to applied fields like IO, there were lots of empirical studies, but as my professor told us on the first day, none of them had established much in the way of robust results. Why spend time learning sophisticated techniques if they do not lead to any results, I wondered. Aside from these sorts of doubts, I did not understand why economics in graduate school was so **different** from my undergraduate education. None of my professors or colleagues could explain that to me, or even seemed able to make any sense of why someone would ask such a question.

I should point out that those were troubled times for the economy as a whole, what with stagflation, wage-price controls, oil embargoes, stop-go policy and the like, so that some people certainly **were** asking questions about how we were doing economics. But they tended to be radicals of various sorts – institutionalists, Marxists, post-Keynesians and the like (at this point I had never heard of the Austrians). I thought myself a radical at the time – I believe I even

joined URPE (the Union for Radical Political Economy) for a year, but I was put off when, at my first American Economic Association meeting, I saw some of my fellow radicals following Milton Friedman around, chanting outside whatever room he was speaking in. I thought it rude, of course, but worse, anti-intellectual: one should debate, not chant. Anyway, anyone who held alternative views were dismissed by my mainstream professors as ideologues, or better, as **not real scientists**. This suggested that my professors knew what real science was, but they still could not explain why what we were doing, which seemed strange to me, qualified.

In the first few weeks of my microeconomic theory class the professor, Bill Pfouts, whom I later thanked in the preface of the book, had us read the Friedman and Samuelson pieces on methodology. We never returned to this in his class, but at least I learned that there existed a literature that discussed how to do economics scientifically. Then I took Vincent Tarascio's two courses, the first on the history of thought, and the second on methodology. The reason that the second class was on methodology was that Vince had done his own dissertation on Vilfredo Pareto's methodological thought. Pareto helped introduce positivism into economics, and in the book that came out of his dissertation Vince assessed Pareto's contributions, and in the process reviewed some philosophy of science from the 1950s. As I wanted to write in the field, and given my concerns about economics, we decided that I could update the treatment of the philosophy of science, then review various debates in economic methodology. I hoped, somewhat naively, that this would help me to understand why economics turned out the way it did in the twentieth century, and perhaps to justify the practice I had encountered in graduate school.

I was unsuccessful, and indeed, my findings were bizarre. The philosophy of science that had been dominant in the twentieth century had been some form of positivism. It had been developed with the physical sciences in mind, but was in eclipse. When they wrote about methodology, those economists who made any reference to the philosophy of science usually invoked some variant of positivism, but they often were not careful in their borrowing. In short, economists writing about how to practice economics properly tended to borrow, badly, from a defunct philosophical position. The few exceptions were heterodox economists, who shared a dislike of both mainstream economics and positivism, and often criticized them on methodological grounds. I was no closer to understanding why economics was practiced the way that it was, but I did have some raw material for some articles and an eventual book.

Of course, in those days there were no journals like the *Journal of Economic Methodology* or *Economics and Philosophy*. But there was the *Journal of Economic Issues*, edited by Warren Samuels. Samuels was an institutionalist, but he was also a self-described pluralist, and that was about the only place outside of the *South African Journal of Economics* (where Larry Boland sometimes published articles) where someone could publish methodology. As it happened, Warren was dedicating a special issue of the journal to economic methodology, set for publication in December 1979, and sent out a call for papers. He got a number of takers – probably a good sign – and the special issue grew into two. I published two pieces in the double issue, one on positivism and the methodology of economics, the other co-authored with Vince on the problem of theory choice in economics (Tarascio and Caldwell, 1979; Caldwell, 1980a). I should note that Warren was ultimately ousted as the editor of the journal for his proclivity to publish such eclectic and disparate, non-institutionalist stuff. He went on to edit

the hardback annual *Research in the History of Economic Thought and Methodology*, which is now publishing this symposium.

I have two mildly amusing stories about my next steps. In spring 1979 I submitted a paper proposal for the Southern Economic Association meetings, “a Critique of Friedman’s Methodological Instrumentalism.” The proposal was accepted that summer. Soon thereafter, Larry Boland’s paper, “A Critique of Friedman’s Critics” appeared in the *Journal of Economic Literature*. I had been scooped, in a prominent journal, by one of the biggest names in methodology. Thinking fast, I decided to transform my paper into a critique of Boland’s position. A few weeks before the meeting I got a letter from Larry Boland saying that he was coming to the Southern meetings (this could not have been anticipated – he was coming all the way from British Columbia, for God’s sake), would be in the audience, and would be very interested in hearing what I had to say. I was petrified.

At the session my discussant was Dudley Dillard, who ended up being my guardian angel. In his comments he criticized my paper, but he prefaced his remarks by saying, “Though I have problems with this paper, it is much better than the one by Boland, which was terrible.” When it came to my time to respond, I said, “I believe Larry Boland is here, so perhaps he might want to offer a few words?” Boland of course went after Dillard, and as these two big names debated one another, I was quietly left off the hook. Pure serendipity! Boland ended up giving me some excellent comments, and the paper was ultimately published in the *Southern Economic Journal* (Caldwell 1980b). I cannot imagine that the fact that Vince Tarascio was editor of the journal at the time had anything to do with my ability to publish there.

The other story has to do with turning the dissertation into a book. I began that process in 1979 by submitting a proposal to Cambridge University Press. After what seemed to me to be a suspiciously long period of time they finally sent me a rejection, noting that they already had a manuscript on a similar topic. This turned out to be Mark Blaug's *The Methodology of Economics, or How Economists Explain* (Blaug [1980] 1992). Scooped again, this time by the biggest name in the history of economic thought! (Blaug's *Economic Theory in Retrospect* [1962] 1996 had been my graduate textbook in the field.)

This was not a promising development. I held out hope, though, that his book would differ from mine. It did, but not in a good way. I still remember when his book arrived in my mailbox at UNC-Greensboro in November 1980. It sat on my desk, unopened, for quite a while. When I finally opened it I nearly passed out. Like me, he reviewed the philosophy of science and assessed various contributions to the methodological literature in economics. But in addition, he then applied his insights to a number of research areas within economics. Game, set, match, I figured.

This turned out to be an overreaction. Mark Blaug did all of us who have an interest in methodology a great service by simply writing the volume, because when a well-known and well-regarded economist decides to devote a book to a topic, people pay attention. And luckily for me, there was still space for me to make some sort of contribution, because Blaug had taken a strong Popperian/Lakatosian prescriptivist approach to his topic. I had come away from my dissertation with no clear conclusion other than that, because positivism was dead within the philosophy of science and the theory choice problem seemed insoluble, we should be open to exploring alternative paths, embracing both more novelty and more alternative forms

of criticism, ideas that I came to call methodological pluralism. I emphasized these aspects in the published version. My call for pluralism ended up resonating with a number of people, and disquieting others, but for me it was a pragmatic response to the disarray I found in the literature on methodology in economics, where no one seemed interested in explaining why we did what we did, but everyone was sure that what the non-mainstream groups were doing was not scientific.

I wrote the book while doing a post-doc with the Austrians at New York University during the 1981-82 academic year. I had by then become curious about heterodox groups within economics who actually paid attention to methodology, especially the post-Keynesians and the Austrians, and here was an opportunity to study with one of them. Indeed, I hoped that by studying the criticisms offered by those who took alternative approaches I would get a better sense of what the mainstream was all about. They provided an alternative approach into the subject.

The time at NYU was very stimulating intellectually. In addition to meeting a number of people in the Austrian camp whom I would know for the rest of my life, it was also there that I first came in contact with Wade Hands. Wade must have read one of my pieces in Warren's journal, because in January 1982 I got a short letter from him with a paper enclosed paper that was forthcoming in *Philosophy of the Social Sciences* titled "Blaug's Economic Methodology" (Hands 1984). I was over the moon: there was someone else of my own generation in the profession who was interested in methodology, and not only that, the person shared my criticisms of Mark Blaug's approach! Here is an excerpt from my ecstatic response:

Dear Professor Hands, I read your paper while having lunch in a local restaurant, and had I not been in New York, I would have been considered strange (since everything is bizarre in New York, no behavior is considered strange): I kept exclaiming “exactly,” “precisely,” and other such words while reading. Yours is literally the first paper I have read in methodology with which I was in total agreement. I have enclosed a copy of my review of Blaug’s book which appeared in the *Southern Economic Journal* last summer; as you can see, we agree point for point Caldwell to Hands, 4 February 1982).

My book was published in 1982, and as I noted above, not everyone was pleased with it. Kevin Hoover’s critical commentary thus provokes in me a distinct sense of déjà vu. When he presented his paper at the History of Economics Society meetings, he argued as he does here that my embrace of pluralism was a function of me being too much a product of the hippy-dippy time period in which I was writing, and showed a sartorially challenged picture of me to underline his point. In turn, I teased him for offering such “small-minded, mean-spirited, and ungenerous” comments about an obvious piece of juvenilia, invoking Joseph Nye Welch’s comments during the Army-McCarthy hearings, “Have you no sense of decency, sir? At long last, have you left no sense of decency?”

But in fact Kevin was in pretty good company. A couple of philosophically astute commentators early on pointed out, as Kevin does, that I had mischaracterized Friedman’s position: I should not have said that Friedman thought that theories were instruments that could not be considered either true or false. I corrected this in a later paper, where I said that Friedman thought that the assumptions of our theories were false, but that this did not matter, only predictive adequacy matters, a position that is more in line with Kevin’s realist reading

(Caldwell 1992). Complaints about my endorsement of pluralism were equally severe. Tony Lawson once told me that he came to know of my book when Terence Hutchison in giving a talk had held the book up and declared that it was dangerous. (Tony said that as a result he immediately wanted to read it.) Mark Blaug even used the same metaphor as Kevin, saying that methodological pluralism was equivalent to saying “let a hundred flowers bloom” and was “tantamount to the abandonment of all standards, indeed, the abandonment of methodology itself as a discipline of study” (Blaug 1983, p. 3). Strong stuff.¹

Both Hutchison and Blaug criticized pluralism from slightly different but compatible Popperian/Lakatosian frameworks, and for the rest of the decade there was considerable debate as to whether this was a good framework for understanding (or for correcting) economic practice. Meanwhile, I realized that I needed to offer a more careful statement of my own position. I did so at a conference organized in Amsterdam in December 1985 by Neil De Marchi. The conference volume that resulted contains a paper by Hutchison titled “The Case for Falsification” and one by me called “The Case for Pluralism” (Caldwell 1988, Hutchison 1988). For those who are interested in a nice juxtaposition of the two viewpoints, and a more mature statement of my position, I suggest that they consult this volume.²

I mentioned Tony Lawson above; though we corresponded beforehand, I believe I first met him at a conference in Toronto in 1988, and later at one in Malvern that had been

¹ Mark Blaug ended up becoming a mentor and close personal friend: see Caldwell 2013 for a fond reminiscence.

² Some of the section titles of my paper suggest the criticisms that I tried to address: “Does Pluralism Lead to Anarchy?”, “If Everyone Was a Pluralist, What Positions Would Be Left to Criticize?”, and “Will Pluralism Lead to the Discovery of True Theories?” For those who wish to pursue some of the debates of this period further, there was a second conference, organized by Neil and Mark Blaug in Capri in 1989, which dealt specifically with whether the Lakatosian framework was useful in describing or assessing the practice of economics (De Marchi and Blaug, eds., 1991).

organized by a somewhat curious (and now deceased) character named John Pheby, who was then the editor of *Review of Political Economy*, another journal that was amenable to publishing methodological work. I came to know Tony better when he invited me to spend a term at Clare Hall, Cambridge, in the spring of 1994. Tony too has been critical of positivism, but at first I did not understand his position. I thought that, given that philosophers had shown that no science had ever really followed its strictures, it was no longer necessary to criticize positivism, but he insisted that mainstream economics continued to practice it, as opposed to mouthing its rhetoric. As his paper for this symposium shows, he was in fact addressing a different set of doctrines from me when he used the word positivism, namely deductivism, which is indeed alive and well within economics.

His major point, if I understand him correctly, is that the deductivist framework employed by mainstream economists is inappropriate when dealing with open systems, and many economic phenomena are in fact open systems. As someone who studies Friedrich Hayek, I certainly am wholly sympathetic to Tony's ontological claim that we often study open systems, and to his insistence that ontology is important. I will not be able to take him up on his challenge to redo *Beyond Positivism* as if ontology mattered, but I certainly hope someone will.³ I also would ask Tony – he has a much more sophisticated understanding of mathematics than I do – whether there may be some sort of mathematical framework out there that is better at capturing the essential openness of economic phenomena that both of us think is so important.

³ Though he does not take up ontology in the way that Tony would like, Hands 2001 is an admirable critical survey of developments in economic methodology since the publication of *Beyond Positivism*. Hands there describes in detail the naturalist turn, mentioned in passing in his contribution to this symposium, a turn that is part of the reason that Lawson is upset with methodologists, who tend to take the practice of economists as given.

At the end of his paper Tony chides me gently for defending what I have elsewhere called basic economic reasoning (e.g., Caldwell 2004, chapter 15; 2009). As most readers will know, as my career has progressed I have become increasingly obsessed with the ideas of Hayek, but what is perhaps not so evident is that my initial attraction to him was to his methodology. Hayek too was critical of positivism, or of scientism. (He could have used the first word: he was from Vienna, and for a time thought about attending the meetings of the Vienna Circle, but was apparently put off, like Mises, by Otto Neurath.) But Hayek was also confident that economics, properly understood, offers some valuable tools for understanding how the world works. In his “Scientism and the Study of Society” essay he criticized social scientists who, in an effort to show that they were **real scientists**, tried to follow what they took to be the methods of the natural sciences (Hayek [1942-44] 2010). In the 1950s, though, Hayek came to realize that the real problem was not the effort to mimic natural scientists, but that economists were trying to apply tools that were appropriate for the study of simple phenomena to the study of complex phenomena, that is, to the study of structured, adaptive complex orders, or spontaneous orders for short. He argued that when confronted by such phenomena, we can still study them scientifically, but what we can achieve is much more limited. Often the best we can do is to offer explanations of the principle by which they operate. He also concluded that we will rarely be able to offer point predictions, that pattern predictions was all that was possible when dealing with spontaneous orders.

Now while Hayek is often somewhat vague in his formulation of these ideas, what he said struck me as a reasonable description of sort of thing that I found most valuable in economics, that is, the basic economic reasoning that attracted me to economics as an

undergraduate, that I taught as a professor, and that I defended in the final chapter of my book *Hayek's Challenge* (Caldwell 2004). Hayek's view also made sense of the limitations that economists face in terms of prediction that had troubled me when I took my IO class. Indeed, taking Hayek seriously helped me answer a lot of the questions I once had about why economics turned out the way it had: the discipline was constantly struggling to figure out ways how to study complex phenomena of various sorts.

Of course, Hayek was no philosopher of science, and I long ago stopped doing work on economic methodology proper, so that was about as far as I got. The reader might then imagine my pleasure in reading Wade Hands's excellent contribution, one that I can perhaps be forgiven for interpreting as saying that the notion of pattern prediction that Hayek developed and that I found compelling has at least some support in the philosophical literature. He even manages to illustrate the case with a piece of basic economic reasoning! Wade as always states his argument very carefully and does not leap to conclusions, but I am willing to take what he says as confirming an intuition that I could not articulate, that Hayek was in fact on to something quite profound in his writings about methodology.

Not everyone was critical of my defense of pluralism in *Beyond Positivism*. People who belonged to heterodox groups often welcomed it, because it pushed back against the idea that there was one true method for doing scientific economics, and so gave them cover to pursue their own chosen paths. Of course, the group that I became closest to is the Austrians, represented in the symposium by Boettke, Stein, and Storr. I first met Pete Boettke when Don Lavoie invited me to George Mason University to give a talk on Hayek's methodology. After my presentation was over I was converged upon by three sharp, engaged graduate students who

wanted to straighten out my misconceptions about Mises and hermeneutics, if I recall correctly. These were Boettke, Steve Horwitz, and Dave Prychitko. I met both Virgil Storr and Solomon Stein through the vast network that has grown up around George Mason: the student-mentor relationship has always been, and continues to be, strong amongst the Austrians!

Anyway, their paper on the attempts by Mises, Hayek, and Lavoie to convince three different audiences about the flaws of socialism, and the frustrations that each encountered, is a brilliant demonstration that, just as was true for me in the 1970s, people often turn to methodology when they seek an answer to the problem: why are we doing economics in the way that we are, and what are alternatives? What is the right way to understand, and to argue about, economic phenomena? How do we match up our theories with the world out there? In trying to answer such questions, the word science is usually invoked, so that a background question that always lurks is, who is the real scientist here? I am not sure that we as a profession are any further along in answering that than when I first wondered about it so many years ago, even if in my own mind I have settled on an answer that I find convincing enough.

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