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An Interview with Henry Hazlitt

Henry Hazlitt

AEN: When did your interest in economics begin?

Hazlitt: Oh, it began very early, when I was about 18 or 19. My original interests were in psychology. I was a great admirer of William James, and my ambition was to go to Harvard and study psychology and be a professor of psychology and philosophy, but I had to go out and get a job instead to support my mother.

So then I decided that I'd like to get on a newspaper, and the only way I found I could do that was to join the *Wall Street Journal* as secretary to the managing editor. After I got on I began to try to study the stock market. I began to realize there was such a thing as economics, that it was a science, and as worthy of my attention as psychology. I began to read in economics. I'm not sure whether I had good taste or good luck, but the first book I read was a small volume on *Outlines of Political Economy* (1911) by S. J. Chapman, a disciple of Alfred Marshall. Then I found out that the most popular college textbooks were those by E.R.A. Seligman and by Henry R. Seager. So I chose Seligman, who was pretty much of a socialist, though I was somehow on guard aginst him; I was almost born a free enterpriser. (I did not learn about F. W. Taussig till later.) Anyway, I picked up my economics, not by taking any course in it, but by reading economics books. As I say, I read the book by Alfred Marshall's disciple Chapman. I never took much to Alfred Marshall. I didn't like his compromising attitude.

Then I stumbled across a book in the library that fascinated me. That was Philip H. Wicksteed's *The Common Sense of Political Economy*. (I had never heard of it, nor had nearly anybody else at that time. It was published in 1910 and I think I found the book only a couple of years after it was published.) That had a tremendous influence on me. Wicksteed was one of the first expounders of "marginal" economics, a disciple of Jevons, and his work was very close to Austrian economics. Mainly through him, I got interested in theoretical economic questions.

In 1920 I became financial editor of *The New York Evening Mail*. I did a daily signed column on finance and economics. I decided I would study one commodity market at a time and learn that market. I continued this to learn both about individual commodities and markets in general.

AEN: You mentioned that you were born a free enterpriser.

Hazlitt: Almost. I think one early reason was because one summer, when I was a kid, I was with my uncle who was an electrician for an amusement park show called *The Galveston Flood*. The owners were always hoping for customers. No one was coming in. But they were looking forward to the Fourth of July. Then people would flock in. But on the Fourth of July it poured. Nobody even came to the entertainment park. I acquired a deep empathy with entrepreneurs. I didn't think of them as people who make obscene profits but people who could make unforseen losses. That experience put me on the free enterprise side. I learned that it took a great deal of luck as well as judgment to make a profit.

AEN: Since your interest in economics obviously grew very suddenly, was there ever a point where you thought of abandoning journalism and pursuing an academic career?

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Hazlitt: I couldn't; I had no choice. I had been going for a time to City College, because there was no tuition charge, but I had to give that up within a year in order to take a job earning money. So I could only have gotten an academic job by getting a college and post-graduate education first.

AEN: When did you first become acquainted with the Austrian school?

Hazlitt: That's hard to say. Reading Wicksteed of course was close. I was very lucky in my friendships and lucky in the books I chose. I read a book by Benjamin M. Anderson, whom I later got to know. This was his 1917 book *The Value of Money*. He was an acute critic of nearly all other writers on money, and especially of Irving Fisher and his mechanical quantity theory of money. Mac Anderson read German, and discussed many German writers on money. He referred to the German edition of Ludwig von Mises' *Theory of Money and Credit* and wrote: "In von Mises there seem to me to be very noteworthy clarity and power. His *Theorie des Geldes und der Umlaufsmittel* is an exceptionally excellent book." That impressed me.

When in 1936 Jonathan Cape in England brought out the English translation of Mises' *Socialism* I read a review of it in *The Economist*, I requested a copy from Cape and eventually received one. I reviewed it in the *New York Times*. I was terrifically enthusiastic about the book and called it "a classic written in our time." I mailed a copy of my review to Mises. (I found out from Cape that he was then living in Geneva.) Lu answered me. That was the beginning of our correspondence. Sometime in 1940 I got a telephone call. The voice on the other end said "This is Mises speaking." As I've told many of my friends since, it was as if someone had called and said "This is John Stuart Mill speaking." I had referred to Mises as "a classic," and you don't expect a classic to call you on the telephone! Anyway, that led to our acquaintance. That was my formal introduction to Austrian economics, although I had read, much earlier, Bohm-Bawerk's *Karl Marx and the Close of His System*.

AEN: Mises came to the U.S. in 1940, and you were a close friend of his. What was his situation like then?

Hazlitt: He was having great financial problems. He never told me a word about them, but Margit, his wife, did. I had very few close acquaintances in the academic world. But I knew that Alvin Johnson, who was then running the New School for Social Research in New York, was giving a lot of German refugees jobs as teachers there. So I gave a dinner at my home to which I invited Lu and Margit, Alvin Johnson and a few others. I took Johnson aside and asked him whether he could possibly offer a job to Mises. Johnson's only answer was: "He's pretty much to the right, isn't he." Then I realized, of course, that those jobs that Alvin Johnson had given out were all to German socialists. But I did finally get the Yale University Press to publish Mises' *Omnipotent Government* and, later, his *Human Action*, both of which I edited at the publisher's request, for English idiom.

AEN: Was there a sudden transition of opinion away from the advocacy of the free market toward interventionism during your career?

Hazlitt: Only a gradual one. It wasn't a conscious transition at the beginning. There were a lot of socialistic ideas, for example, in Seligman, and other popular text-book writers. Seligman himself probably didn't realize the extent of his own socialistic tendencies. Socialism did rather suddenly come in with the New Deal, beginning in 1933, but not under the socialist name.

AEN: Did a person with very free market views have an easier time finding an audience years ago, and didn't there come a point in time where he could no longer be too explicit about his beliefs?

Hazlitt: Most economists moved unconsciously with the political tide. When I wrote my *Economics in One Lesson* in 1946 I didn't quite realize how socialistic most economic writing had already become. This was a fortunate thing for me because my book was not angry at all; it just spoofed the controllers and the price-fixers. I think that's one of the things that attracted readers — there was little anger in the

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book. It just treated the interventionists as if they didn't really know what they were talking about.

AEN: Let's turn to the subject of the Bretton Woods agreement of 1944. Obviously you and Benjamin Anderson were among the few who had some idea that trouble was lurking in that agreement. How do you account for the fact that your warnings had no noticeable influence on policy in those times?

Hazlitt: I just don't know. I'm writing a book now called *From Bretton Woods to World Inflation*. At the time of the Bretton Woods conference, 1944 and 1945, I was writing all the editorials in the *New York Times* on the Bretton Woods decisions as they were being made. As you will remember the guiding spirit at that conference was John Maynard Keynes. The delegates were making inflationary decisions every day. I was tirelessly pointing out that these decisions *were* inflationary. Nobody else seemed to be pointing this out and nobody paid any attention to what I was saying. In fact, I think an awful lot of people were astonished that the *New York Times* was taking this strange eccentric position. When the 43 nations represented all signed the agreement adopting the Bretton Woods program, Arthur Sulzberger, then publisher of the *Times*, called me into his office and said, "Look, Henry, I've been letting you write these things, although I had misgivings about them, but now that 43 nations have agreed to accept the agreement, I don't see how the *Times* can continue to oppose it." I replied "Mr. Sulzberger, if you feel that way, I can't continue to write any more editorials in the *New York Times* on the agreement; I believe it is too harmful." Well, I did continue to write on it but only in my signed column on the financial page, but there was a strained relationship between myself and the ownership of the *Times* because of that.

AEN: A little bit on the side, but I'm very curious about Benjamin Anderson. He fought so vigorously against this kind of inflation right through the twenties and then much afterward. Was he a bitter person because his advice was never heeded?

Hazlitt: Yes, he did become embittered. I remember he was at my house when Landon was running for President against FDR. As the radio returns came rolling in, Mac shook his head and said "This is the mob." He was very depressed, but I don't think his writing was ever bitter. It remained analytical and objective.

AEN: What about your views on monetary reform? Have they changed much since your book on inflation?

Hazlitt: I'm still a believer in the gold standard. I still believe that central banking — the Fed — is responsible for more and more inflation. And because of inflationist legislation over the past number of years, there is not only a quantitative increase in money and credit but a constant lowering of the quality of credit. We are headed toward an increasingly dangerous inflation.

AEN: Should the government have any role whatsoever in a monetary system?

Hazlitt: It already has a tremendous role. The political difficulties of locking it *where it is* are tremendous. The question in my mind is: Should we be discussing what should be done or should we be discussing what's likely to be done? Are we being very unrealistic when we ask what the government should do? Is there any real likelihood that we're going to get a sound money?

AEN: But for purposes of argument, is a system that provides sound money more likely to be achieved without government regulation of any sort? Or do you think that there has to be, even in an ideal system, some oversight?

Hazlitt: It's a difficult question. As long as government is in the picture the tendency will be toward further inflation. That's what the political forces dictate. However, I am in doubt about whether all controls of banking should be suddenly removed. While the Federal Reserve is an engine of inflation, it's hard to know what would have happened if the Federal Reserve System hadn't been developed. I'm of two minds about this. I know most of my friends in the Austrian school are in favor of removing all government controls of banking, but I can't view that situation quite with equanimity. I wish I had more firm convictions than I do that monetary problems would solve themselves if government completely got out of the picture. I know Mises believed in free banking, and I have much respect for his judgment, but I still have reservations about removing *all* government controls.

AEN: Is it the defenders of the market that bear the burden of proof to show that the market can work, or is this burden on the interventionists?

Hazlitt: I'm not sure which group bears this burden. But I just frankly admit that I am personally in doubt about what ought to be the *precise* limits on the role of government. Perhaps a good rule of thumb might be: When in doubt, don't control. The real question, though, is when that doubt becomes legitimate. Even where we admit the government has a right to step in, it's probable that government will do the wrong thing anyway. I'm just admitting areas of doubt rather than feeling sure that I have the exact solution about just where the line should be drawn against intervention. It would take a long time to explain the basis of my doubts regarding just where to draw the line.

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AEN: You have also, of course, been a long-time critic of Keynesianism, and your book *The Failure of the New Economics* was a landmark in that regard. Some people say that you, in your analysis, were really not grasping the subtleties of Keynes, that you really missed the point. How do you respond to that?

Hazlitt: If I was missing the point then I'm still missing the point now. In fact, I just reviewed for *Reason* a newly translated book by Mises entitled *Nation, State, and Economy* along with Keynes' *Economic Consequences of the Peace*, because there were a great many similarities between these two books. First of all, they both appeared in 1919. (Mises wrote his book about three months before Keynes wrote *The Economic Consequences*.) Despite the similarities, however, Keynes couldn't prevent himself even then from taking sarcastic jabs at capitalism. He was at that time a disciple of Alfred Marshall, he was what we might call a 'near-classicist' of the Cambridge School. In 1919 both Mises and Keynes deplored the size of the reparations burden put on Germany.

AEN: But you have not accepted the view that Keynes was actually a very profound writer.

Hazlitt: No. *The Economic Consequences* contains one of the most eloquent denunciations of inflation that has ever been written. Yet the same man turned around in 1936 and wrote the *General Theory* which recommended inflation as the principal remedy for unemployment. Keynes probably didn't even recognize that inflation was really what he was recommending; he thought he was just recommending government borrowing and credit expansion. Of course, the same logic that led him to believe that such a course would create jobs, should have also led him to conclude that ever paying off the debt and reducing the credit expansion would restore the unemployment. Only never-ending inflation would keep his full employment going.

AEN: But wasn't Keynes a very brilliant man?

Hazlitt: A very brilliant man, indeed, a very brilliant writer, a very *witty* writer. But being a brilliant writer was confused with being a brilliant economist. He wasn't. We should never confuse wit with profundity.

AEN: Why don't you consider the type of monetarism espoused by Friedman and Meltzer to be an adequate response to Keynesianism?

Hazlitt: The monetarist outlook, as proposed by Milton Friedman is, in fact, a mechanical quantity theory of money. He assumes that "the" price level — that is to say, an average of prices — will rise proportionately to the amount of paper money that is issued. That *can* happen for a certain period, but the value of money is not determined mechanically and proportionately with the amount issued. It is determined by public psychological forces. A panic can break out when people suddenly expect the value of money to collapse. That was illustrated in the German inflation of 1920 to 1923. Prices rose for a time roughly proportional with the amount of money issued. But suddenly prices soared far faster than the money supply because the public got panicky. It is psychologic forces that determine the value of money as of other commodities, although influenced, of course, by quantitative considerations.

AEN: You criticize Keynes for resorting to block-thinking and falsely precise mathematical presentations of his ideas, but isn't there a sense in which the monetarists have decided to play according to the Keynesian rules and methods in their work?

Hazlitt: Precisely. Keynes more or less accepted proportional monetarism, also.

AEN: What is your opinion about the present state of economics? Do you see a tendency to return to sounder thinking or would you say the tendency is just the opposite?

Hazlitt: The Austrian school, which is happily getting more numerous, represents a tendency to return to sounder and more precise thinking. But popular and political thinking is more and more drifting toward interventionism and socialism.

AEN: You don't see any change from that trend at all?

Hazlitt: It's very hard to predict what the next trend will be. But the present trend has been toward interventionism. If there is a trend toward free-enterprise advocacy, except by the Austrian school, I'm certainly not confident about it.

AEN: Are you pleased with the development of Austrian economics since the South Royalton conference of 1974? Do you feel that we've been accomplishing something since then?

Hazlitt: I am and I do. The Austrian school has been growing numerically. It is making theoretical advances. There is an extremely promising young group of writers in it.

AEN: Austrians seem to have aligned themselves either in the Misesian praxeological camp or in the Hayekian evolutionary-institutions camp. What are your thoughts on that?

Hazlitt: Hayek is a great admirer of Mises, but he likes to compare Mises to Jeremy Bentham. Hayek thinks his own

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Books

Mark Blaug, *The Methodology of Economics, Or How Economists Explain*
(Cambridge: Cambridge University Press, 1980) pp. xiv, 296.

Reviewed by Israel M. Kirzner

Professor Mark Blaug is a highly respected economist who has established an eminent reputation in a number of branches of economics. Particularly in the area of the history of economic doctrines Blaug is well-known for the lucidity of his expositions and for his sensitivity to the subtleties of the wide varieties of theories that have been presented by countless economic thinkers during the past two hundred years. So that it is with considerable anticipation that one begins to read Blaug's comprehensive account of "how economists explain" — and with a corresponding degree of puzzlement, not to say disappointment, that one completes one's study of the book. Because this review is directed toward a specialized audience, it will (not without some danger of distortion) concentrate on the aspects of the work that relate particularly to the methodological concerns of Austrian Economics. Although Blaug refers only infrequently, and relatively briefly to Austrian Economics, it should be pointed out that the book contains a very great deal to provoke the interest, and the puzzled concern, of Austrians.

Blaug's rare ability to present complicated and subtle ideas with clarity, is fully apparent in the book. Its survey of developments in the philosophy of science, much of his detailed accounts of the methodological underpinnings of modern economics, and his comprehensive bibliography, combine to make this a lucid expository work, and a valuable source book in its field. Yet, as a statement of Blaug's own methodological credo the work must be deeply disappointing, especially to Austrians. Austrians must be disappointed not only, or even primarily, because of his cavalierly ill-tempered and contemptuous — and, let it be stated, less than fully informed — treatment of their position, but because Blaug's own views can hardly appear to Austrians to be anything but narrowly conceived and dogmatically asserted. Let us consider the twin pillars upon which Blaug's position rests, *methodological monism* and *falsificationism*.

"The methodology of a science is its rationale for accepting or rejecting its theories or hypotheses. Thus, to hold that the social sciences should employ a methodology distinct from the natural sciences is to advocate the startling view that theories or hypotheses about social questions should be validated in ways that are radically different from those used to validate theories or hypotheses about natural phenomena. The categorical denial of such methodological dualism constitutes what we call methodological monism." (p. 47). Falsificationism is, of course, the Popperian "methodological standpoint that regards theories and hypotheses as scientific if and only if their predictions are, at least in principle, empirically falsifiable." (p. 266). With views resting squarely on these pillars it is not surprising that Blaug declares Austrians to be persona non grata, and is with dismay compelled to find it "curious that Knight, who in the early 1930s had become one of the principal opponents of the Austrian theory of capital, should have continued throughout his life to take his methodological views straight from Mises and company." (p. 98). But, when one searches for any *reasons* for Blaug's insistence on methodological monism, or, to put it differently, for excluding a priori (!) even the *possibility* of scientific knowledge that does not meet the falsificationist demarcation criterion, one searches in vain! It is as if the case for methodological monism were to be established merely by the device (as in the above-cited passage) of characterizing the methodologically dualistic view as "startling". In fact Blaug's views seem to be established by assertion, and little more. The almost amusing flavor of Blaug's discussion is well-expressed in a passage in which he rejects a doctrine of "methodological individualism strictly interpreted." Such a doctrine, Blaug perceptively points out, would in effect "rule out all macroeconomic propositions that cannot be reduced to microeconomic ones, and since few have yet been so reduced, this amounts in turn to saying goodbye to almost the whole of received macroeconomics. There must be something wrong with a methodological principle that has such devastating implications" (p. 51). Surely an Austrian might be forgiven for responding to such an overwhelming line of reasoning, that there must then, presumably, be "something wrong" with a methodological principle that in effect asks the economic profession to say goodbye to the work of Mises and Knight. Indeed, since Blaug again and again repeats his charge that the "central weakness of modern economics" has been its "reluctance to produce the theories that yield unambiguously refutable implications" and "a general unwillingness to confront those implications with the facts" (p. 254), is one therefore to conclude that "there must be something wrong" with the methodology of falsificationism? (On this latter point see the excellent discussion in Professor Bruce Caldwell's stimulating new book, *Beyond Positivism, Economic Methodology in the Twentieth Century* (London: George Allen and Unwin, 1982), pp. 125-126.)

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approach is less rigid and dogmatic than that of Mises. But I'm not at all sure that the attempt to build a clean-cut unified system without fuzzy edges is necessarily to be condemned.

AEN: As a last question we'd like to know what else you may be working on aside from your forthcoming book on Bretton Woods?

Hazlitt: I have in mind a book of which I have so far written about a half-dozen chapters. It will be called *Is Politics Insoluable?* This is on the question of what should be the actual agenda of the state. It also asks whether or not it is possible to even draw up a workable set of rules for an ideal state. Is there any point in drawing up plans for an ideal state when you know that the yen for power on the part of individuals will forever make all such ideal states utopian? Is it possible to draw up rules for an ideal state, or is that just a way of day-dreaming?

AEN: Earlier you expressed some reservations about radically free-market solutions; now you are expressing pessimism concerning political arrangements. At any rate, you seem to be more skeptical than you were when you wrote *A New Constitution Now.* Are you looking back at your old work and asking the questions over again?

Hazlitt: Yes, I am. When I wrote *A New Constitution Now* I was sure, as I still am, that parliamentary government tends to be a better form of government than our own presidential system. We have a method of selecting presidents that has been lowering and lowering their quality right along. A person selected as a prime minister will almost necessarily be a better person than one elected president under our system, for under our system we're governed by the calendar and the clock. But now my doubts run deeper as to whether even the parliamentary system brings the most desirable persons into power. Nonetheless, I do think that we must live with the dangers of democracy rather than turn to another form of government which will almost systematically be worse. Anyway, I find as I grow older that I'm less sure of all my answers than I used to be.

AEN: Well, if a man with your knowledge and understanding of market phenomena doesn't have firm answers to these questions, that certainly serves to indicate that Austrians have a great deal of work to do. Thank you, Mr. Hazlitt.

(Interview conducted August 31, 1983, by George Selgin, Don Boudreaux, and Sanford Ikeda.)

Books: Mark Blaug (Continued from page 5)

Let us have done, however, with the fun of amassing such embarrassingly tempting debating points, and consider more seriously the methodological status of one simple, important praxeological theorem. Surely no more central economic theorem can be stated than that which asserts a tendency, within a given market to which entry is free, for the prices at which a given commodity is sold, to converge towards a single price. Few economists would question the validity or the scientific character of the theorem. (It was of course the basis for Jevons' famous Law of Indifference.) Now, the theorem certainly fails to meet the falsificationist criterion. For all kinds of good reasons (having to do for example with the difficulty of pinning down precisely what is to be taken, in a constantly changing world, as the empirical expression of "a tendency", of "a commodity", of "a market", and so on) we would be indulging in utter self-delusion were we to claim that the theorem offers predictions capable, even in principle, of empirical refutation. Yet surely the theorem affords us insights into the workings of markets which, once having glimpsed, no economist would be prepared to surrender. And this last assertion surely applies with equal validity to economists who may believe that in fact entry into markets is not free, that kaleidic changes in preferences and in supply conditions continually frustrate the tendency asserted in the theorem. One can readily appreciate the intellectual satisfaction obtained from witnessing the successful withstanding, by predictions made on the basis of scientific theory, of repeated empirical tests. And one can readily appreciate that a falsificationist approach affords a scientific community a useful means by which to weed out, and to protect itself from, fraudulent assertions made in the name of science. But surely all this does not provide grounds for rejecting out of hand important conclusions, based on careful, rigorous reasoning capable of being checked by one's professional peers, — simply because the nature of the subject matter happens to preclude such satisfactions and such useful protections.

The truth surely is that the postulation of a tendency-towards-a-single-price simply expresses our understanding of the systematic ways in which purposeful human beings revise their plans upon the discovery that they have hitherto overlooked attractive available opportunities. The market regularities that may be based upon such systematic plan revisions are *manifestations* of more abstract regularities that are able to be grasped *quite apart from their particular empirical manifestations.* To refuse to deal with such non-empirical regularities on the grounds of falsificationist methodology is not merely to be applying an arbitrary, self-

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Books: Mark Blaug *(Continued from page 6)*

denying ordinance — *it is to don blinders that prevent one from seeing that which surely is there to be seen.*

The Austrian approach to economics finds systematic logical relationships generated purely by the purposefulness of acting, reasoning human beings. Now *if* these relationships do in fact obtain, and if these relationships are, by their very nature, *not* capable of generating empirically refutable predictions, then any methodologically monistic falsificationism must have the consequence of barring scientific attention to a realm of reality that is capable of being seen and understood. To be sure, if one is convinced a priori that no abstract relationships exist, then this latter danger must appear to be wholly flimsy. It is now fifty years since Hayek observed that it is not surprising "that people who have never really studied economic theory will necessarily be doubtful of the legitimacy of its existence." Those who have never glimpsed the kinds of non-empirical regularities upon which applied economic theory rests, may indeed be excused for doubting the scientific legitimacy of anything but empirically falsifiable propositions. But to those who have grasped the non-empirical regularities yielded by praxeological reasoning, it must seem nothing short of obscurantism to proscribe such systematic, fruitful analysis on altogether arbitrary grounds. What is ultimately disappointing about Professor Blaug's book is that so outstanding an economist, commanding so wide an acquaintance with economic doctrines, has arbitrarily denied the very legitimacy of praxeological theorizing, when in fact such theorizing constitutes the inner core of economic reasoning employed, whether they recognize it or not, by virtually all the economic theorists (Austrians or others) cited by Blaug himself in the course of his gifted, but deeply flawed, book.

Israel M. Kirzer, who received his PhD under Ludwig von Mises, is professor of economics at New York University.

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Ludwig von Mises *(Continued from page 7)*

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