"An Interview with Milton Friedman on Methodology"
by J. Daniel Hammond
Hoover Institution, Stanford University, May 24, 1988

J.D.H. The first paper that I wrote about your work ['Monetarist and Anti-monetarist Causality', *Research in the History of Economic Thought and Methodology,* 1986] was an interpretation of the causal structure of your version of the quantity theory of money. In your letter commenting on my paper you objected strongly to my fundamental premise, that you think of the money supply (or changes in it) as a *cause* of the price level and nominal income (or changes in their values). You wrote: 'I have always regarded "cause" as a very tricky concept. In my technical scientific writings I have to the best of my ability tried to avoid using the word.' [Letter dated 13 June 1985, reproduced in the appendix to this chapter.] In the 1974 Institute for Economic Affairs publication, *Inflation: Causes, Consequences, and Cures* you wrote essentially the same thing: 'I myself try to avoid use of the word "cause," … it is a tricky and unsatisfactory word' (p. 101). What specifically is the problem you see in framing a discussion of macroeconomics such as the debate between monetarists and Keynesians as a debate over the causes of price level and income changes?

M.F. The problem that bothers me about cause is that it almost invariably leads into a problem of infinite regress. There is no such thing as *the* cause of anything. In a way, it's the same problem as the problem that bothers me about people who talk about theory versus facts. As you probably know I've always thought the best definition I could give of the difference between a theory and a fact is that a fact is a theory that we take for granted for present purposes. But there's no such thing as *a fact.* There's a way of looking at some facts and that's really a theory. But to go back, once you start talking about cause—money is the cause of inflation. Well then, what caused the money? And there's no stopping. The stopping point has to be based on the purposes of your investigation, on convenience and so on. People are always trying to get to ultimate causes. I think it's a foolish search. And that's fundamentally why I've tried to avoid using the word. That's why when I have used it I've tended to use it as 'proximate cause' or something like that. A statement that A is necessary and sufficient for B, might equally be that B is necessary and sufficient for A. It doesn't tell you that one is the cause and one is the effect. And indeed in most such relationships, it can run either way. If you have a necessary and sufficient condition so that A and B always go along with one another, then B is as much a cause of A as A is a cause of B. In order to distinguish between the two by saying which one is exogenous and which one is endogenous you have to bring in some outside information that is not contained in them.

J.D.H. What kind of information is that, the outside information? Isn't that information about causality in some sense?

M.F. Yes it is in some sense, but that's the difficulty. You can't say causality without saying, in some sense. That's why I prefer, if I can, to use words that indicate in what sense I'm using causality. So I would say, well I observe that increases in the money supply have always gone along with increases in income. Which came first? I look around and try to see. I want to step one step back in this causal chain and go to the next level. What is it that produced the money supply increase? What is it that produced the change in income, etc.? Let me give you a very simple example. The case is a beautiful case—it's the case of the 1880s and '90s and the effect of silver agitation on the price level. We had deflation. Did we have deflation because the money supply was going down relative to output? It's not at all clear. We had deflation because the agitation for silver made people think we were going to go off the gold standard.

If you're going to talk about causes, independent causes, it would be a more accurate description to say that the cause of that deflation was the agitation for inflation, that is, money was a medium through which that acted. In most cases when money produces inflation the reason money increases is because some sovereign is trying to get some more money or because somebody's discovered gold. But this is an interesting case because the reason you had the rate of change in the money supply that you did was that the money supply had to increase less than it otherwise would have in order to produce the deflation that you had to have to enable a capital outflow, which was produced by the fear of people that we were going off the gold standard. We discussed that case at length in *Monetary History* as you probably know.

It's always seemed to me one of the most fascinating cases because it illustrates so many different principles. It also shows what's wrong in my opinion with some of the more extreme versions of rational expectations hypotheses. Here you had a situation in which interest rates were high because everybody expected inflation. What you actually had was deflation. Were those expectations irrational? Not at all. There was a very high probability you would have inflation given them. So that that's the difficulty I think. At any rate, that's getting off the topic.

J.D.H. So on the problem of infinite regress—does that have to be an overwhelming problem, that you stay away from causality because there's no obvious stopping point?

M.F. Of course not.

J.D.H. But if you analyze affairs without using causal language, you still have the same problem, don't you …

M.F. Yes.

J.D.H. … .in deciding how far back you go?

M.F. Oh, sure, you have the same problem, but it seems to me—and maybe I'm wrong in this—maybe it would have been just as simple to use the word cause and to try to make clear what I meant by it. As I mentioned to you I think in one of my letters, I don't find Mackie's definition of cause very final. [J.L. Mackie, *Cement of the Universe*, 1974. See the letter reproduced in the appendix.] What caused the short circuit he refers to? You're back in the infinite regress problem. You haven't really solved what I think is the basic problem.

J.D.H. No, you haven't, but you have a framework with a number of different factors, each of which contributes to the result.

M.F. I understand. It's a very intelligent and thoughtful approach. I found it interesting and different from anything I'd seen, but I don't really think it solves my problem with the word cause.

J.D.H. In your paper, 'The Monetary Studies of the NBER,' [Annual Report of the National Bureau, 1964] you summarize the results of the studies this way, which sounds to me like you're talking about causes and effects and avoiding the word. You say, 'changes in the quantity of money have important and broadly predictable economic effects. Long period changes in the quantity of money relative to output determine the secular behaviour of prices. Substantial expansions in the quantity of money over short periods have been a major proximate source of the accompanying inflation in prices.' Would you have any objection to identifying that as a discussion about causes and effects?

M.F. If I take your phraseology and say cause in some sense, no. But leaving out the 'in some sense,' yes, because I still have to ask, why? That is only a statement about proximate causation.

J.D.H. Proximate in the sense of being nearest.

M.F. Let me put it differently. It seems to me, it indicates how you go about trying to understand the phenomenon—what you look for at the next step. It's not a stopping point. The problem with the use of the word cause is that I think there's a great tendency for people who use the word cause to think that once they can say that A is the cause of B, they can stop their analysis. Whereas from my point of view, to say that the changes in the quantity of money relative to output determine the long-run price level, that's a correct statement. But it doesn't tell me why the quantity of money went up as it did. And it will be different in different cases. The cause in one case is the discovery of gold in California and Australia; the cause in another case is the need of a sovereign to get more money, etc. I'm trying to look for a way in which I can unravel a problem and break it up into its relevant parts, and this tells me where to look for the next stage.

D.H. But it's only the beginning?

M.F. It's only the beginning. And cause sounds like you're going to close it off.

D.H. Well, I've picked up from reading Wesley Mitchell and from reading J.N. Keynes and some others the idea that at the turn of the century there was an association between the term because' and the term 'final cause'—that when people spoke of the cause of, say, the business cycle, they were thinking in terms of a final cause—you've gotten to the end of that chain.

M.F. That's right.

J.D.H. Did that make an impression on you?

M.F. Not that I know of.

J.D.H. Do you know how you came to …

M.F. No, I haven't the slightest idea. I have to confess, that in reading what all you people write, I get tremendous insight into what I thought. [Laughter] It's wonderful how much better hindsight is than foresight. I may say that, personally, I have never been very introspective in that kind of a way or personally psychoanalytic and I haven't the slightest idea how I came to that view.

J.D.H. Could you tell me a bit about the effect, to the extent that …

M.F. Are you talking about the second question?

J.D.H. No, we're still on one here—on a follow-up. I'm interested in your response—any effect that your exchanges between John Culbertson in 1960 and '61 and with Tobin in 1970 perhaps had on your thought about causality in economics because both of those were …

M.F. I don't think it had any effect on causality at all.

J.D.H. They were both about causality.

M.F. I know. They both had to do with the question of precedence in time. My feeling is that they pointed out a defect in my exposition, but not in my thought, and that really I had never supposed that precedence in time per se necessarily indicated causation. That is, I was fully aware of the earlier literature of causation versus correlation and spurious trends, etc. So it would never have occurred to me to say that because A precedes B, that means that A is the cause of B in the sense in which they were trying to use it. But I could see that they had a valid case for complaint that my language suggested that. So I think it did have an effect on the language I used subsequently. I think the reason I stressed the lags in the case of money, was not as a means of determining cause, but as a means of pointing out why it would be difficult to use knowledge about the relation between money and income as a control device. If they operated simultaneously you could lean against today's wind, but if there is a lag and especially if the lag is variable—the length of it doesn't matter from this point of view so much it's the variability of it that's crucial—then it would be very difficult to use it. That was why I was so much interested in the lags in the money case. By the time I came to that point, I didn't have any question about whether money had an independent influence. As I say, this may all be retrospective nonsense because it's very hard to go back and say exactly what you though at the time.

J.D.H. That's the impression I've gotten from reading in your work before that. One other question before we move on to two. One of the criticisms that Culbertson made that echoed a criticism by C.R. Noyes of your dissertation was that you're isolating one factor and ignoring other factors which are important causes, and this makes your results suspect. Did those criticisms have much of an impact?

M.F. I don't believe so. I cannot recall that they had any. I can recall definitely that the issue of timing did have an impact, but this I don't recall having any impact. Remember, what they are doing is echoing a standard complaint, and if it had any influence on me it would have come from Mitchell, not from them—much earlier. Mitchell was always talking about the pound of *ceteris paribus;* once you let these dogs out, who knows what happens, etc. So any influence it would have had would have been much earlier.

J.D.H. What was your response to Mitchell at the time?

M.F. Of course he's right. But you don't draw the conclusion he drew. You draw the conclusion rather that you've got to find a way in which you can determine which of the variables are important, and see what in fact happens to them and what their influence is. See, he tended to regard that as a stopping point. Whereas again, it seemed to me it wasn't a stopping point. It pointed out a very real problem, of which I may say, nobody was more aware than Alfred Marshall. So there wasn't really anything new about that.

J.D.H. You see problems in framing economic issues such as the Keynesian–monetarist debate as disputes about the causal role of money. For the most part you have not used the word 'cause' in your writings. Yet you have used it on occasion in your 'popular' writings. For example your 1963 lectures for the [Indian] Council for Economic Education are entitled *Inflation: Causes and Consequences,* and chapter 9 in *Free to Choose* includes a section entitled 'The Proximate Cause of Inflation'. Does this indicate an essential rift between your 'scientific' economics and your 'popular' economics?

M.F. It isn't a rift between my scientific economics and my popular economics. It's the fact that I'm addressing a different audience. The circumlocutions that may be appropriate for a scientific audience will lose you your popular audience. And in general, you have a different aim in view when you're writing for a scientific than for a popular audience. For a scientific audience, you are really part of an ongoing process of cumulative knowledge, you hope, in which the other side has built on your work, or will add to it, or will subtract from it, or will test it, and so on. In respect to popular writing, let's say that one is a wholesale activity and the other is a retail activity, and in popular writing you're trying to convey certain ideas to people, and you don't want excessive qualifications to get in the way. I think there are two equal problems in popular writing. One is oversimplification and the other is overcomplexity. You've got to somehow steer between those. I think people who say things that are wrong on the grounds that they can only get a simple idea across, and while this simple idea is wrong fundamentally, it's the right one for this purpose, are doing a great injustice to themselves and to their audience. People can and will understand a fairly sophisticated argument if it's presented simply, in clear language. And I think that the whole trick in popular writing is to try to find the proper balance between them. And I'm sure that explains why I use the word cause because it's a much simpler term than to talk about necessary and sufficient, or about determined by, or dependent on, or approximately dependent on. Now proximate cause is obviously an attempt to have my cake and eat it too.

J.D.H. If avoiding the word 'cause' in your scientific writings indicates that the analysis is not causal in its structure, what then takes the place of causality in the structure of the theory? Does this not leave you open to the criticism that monetarism or the quantity theory is based on nothing more than correlation between money and the price level or nominal income?

M.F. No, I don't think that—I don't know what it means to say that the analysis is not causal in its structure. I just don't understand the language. The whole purpose of an analysis is to try to understand real phenomena in such a way that you can predict what's going to happen. If you want to call it causal—you know it seems to me that's a purely semantic discussion, and I really find it very hard to get involved in the semantic discussions.

J.D.H. So your avoiding the word cause is really a semantic choice? …

M.F. Absolutely, absolutely.

J.D.H. … and indicates nothing beyond that.

M.F. Nothing.

J.D.H. Do you recall either Jacob Viner or Frank Knight talking specifically in their theory courses about the meaning of causality or problems with causality in economics?

M.F. No. The answer to that is no. I don't recall it.

J.D.H. How about …

M.F. Maybe they did—I don't know. I just don't recall it.

J.D.H. Do you recall Wesley Mitchell discussing any kind of philosophical or methodological problems with causality in either his history of thought course or his business cycles course?

M.F. I guess I took his business cycles course as well as his history course. I took whatever Wesley Mitchell offered that year. And he surely talked about these issues, but I don't recall anything very definite at this distance in time.

J.D.H. He was concerned with the final cause—with people tracing one …

M.F. The problem with causality in respect of Mitchell is that it is all mixed up with the discussion of the role of theory and analysis. And Mitchell was fundamentally anti-theoretical. He would never say that. Mitchell was one of the broadest of human beings. He had an extraordinary amount of tolerance for anything. He wasn't going to oppose theory because it was theory. He had a good deal of respect for purely theoretical writing. But fundamentally he was anti-theoretical. And somehow or other the discussion of causes gets all mixed up with that. I've been much more interested in the interrelations between theory and empirical work than I have with whether it's causal or not. I must say frankly, that while I've been skeptical of the word cause, and I've always avoided it, I really haven't given a great deal of thought to the philosophical issues involved in defining cause.

J.D.H. Henry Schultz studied, I believe, under Karl Pearson …

M.F. Yes he did.

J.D.H. … the statistician and philosopher, in London. Did you pick up any philosophy from him?

M.F. No. Henry Schultz was a good mechanic, but he wasn't really very smart. I remember his talking about Percy Bridgman and operationalism, and so on, and I undoubtedly picked up the words. Henry Schultz had a great deal of influence on me, primarily by recommending me to Hotelling at Columbia. [Laughter] That was unquestionably the major source of influence. Henry Schultz was a fine person. And he is an example of the extent to which perspiration can take the place of inspiration. His book on the *Theory and Measurement of Demand* is a great book—even the parts I didn't write. [Laughter] I did write parts of it, but not the statistical part. And he had earlier done the statistical work. He took a small problem, statistical demand curves, and he concentrated all his effort on that for years. And the result was a body of knowledge that stands up to this day. It's had a great deal of influence, and deservedly so. As I've gotten older, my opinion of Schultz has gone from one extreme to the other. At the time I was a graduate student, or at the time I was a research assistant, I must say I had close to contempt for him, because he just wasn't very smart. But the more I've observed economics as a discipline, and what people have done, the greater the respect I've had for Schultz. Because he did pick a significant problem. He was willing to learn. I was a brash, abrasive youngster, and when you stop and think of it, it's kind of remarkable that an established professor at the University of Chicago, Henry Schultz, would have let a brash twenty-one-year old youngster tell him, 'Well, you know that's just plain wrong-the right way to do it is this'. I'm not exaggerating. I'm sure I spoke in those terms and in that way. So that he was open to persuasion and open to charges of error. He had no arrogance. None whatsoever. I didn't realize at the time of course how rare that is. One of the major flaws of great people, or people who are potentially great, is their unwillingness to have as assistants or associates people who are as good as they are. Henry Schultz did not have that defect at all. He was looking for help wherever he could get it, and he judged help entirely in terms of the contribution it could make to his scientific project. So as I say, as I've gotten older my opinion of Henry Schultz has gone up. Now of course you know, his life ended tragically. He was on a vacation in California, driving a car, having just learned to drive six months earlier and went off the edge of a cliff. And he killed himself and his whole family. That was in about 1937 or '38 or something like that. To go back to where we started, so far as Schultz was concerned what I learned from him, as I say, and you will see it if you look at my Economics 311 and 312 notes, was technique. I was directed to readings, and above all, he recommended to Harold Hotelling that I get a fellowship to Columbia, which Harold Hotelling arranged for me to get, and that was his real contribution. The year after that I came back to Chicago and worked as his research associate, in the course of which, as I say, I worked on his *Theory and Measurement of Demand* and wrote some of the sections of it, as he acknowledges in the preface. I'm not saying anything out of school. I'm not saying anything he wouldn't have said himself.

J.D.H. Did you read much philosophy?

M.F. None.

J.D.H. Any philosophy when you were a graduate student?

M.F. None that I recall. Not only that, I don't recall ever having read much philosophy. Certainly about the only methodology philosophy I've read is Popper. I have read his *Conjectures and Refutations* as well as, of course, which is not methodology, *The Open Society and Its Enemies.* I think those are the two main things of Popper's that I've read. Outside of that, I'm sure I've read bits and pieces of philosophy here and there, but I've never systematically read philosophy.

J.D.H. I noticed that in the *New Palgrave* Alan Walters says that in your 1953 methodology essay you introduced Popper's philosophy of science to economics. Would that be an overstatement, then?

M.F. No. My introduction to Popper did not come from writings. I met him in person in 1947 at the first meeting of the Mont Pelerin Society, when Hayek brought a bunch of people together, and I met him at Mont Pelerin. I was very much impressed with him, and I spent a long time talking to him there. I knew about *Logik der Forschung* but it was in German; there was no English translation at that time. I can read a little bit of German, but it's beyond me really, so I never read anything in original German. I didn't read his *Logik der Forschung,* but I knew the basic ideas from my contact with him, and I have no doubt that that contact with him did have a good deal of influence on me.

J.D.H. In that light it's rather strange that your methodology has been labeled instrumentalism, which is a view that Popper was very critical of.

M.F. Much later. Popper has changed as a human being, as well—I don't know about his methodology. I haven't kept up with his methodology. I only know about this attack on instrumentalism from people like you and so on telling me about it. His book on *Conjectures and Refutations* doesn't contain—maybe I'm wrong—but my recollection is it doesn't contain … Does it contain any … ?

J.D.H. I'm not sure.

M.F. I'm not sure either. At any rate, I was very much impressed over the course of the years at how much Popper had changed as a human being, and particularly in the direction of authoritarianism—a certain element of intolerance, which was not present in his earlier years.

J.D.H. Did you meet Hayek at the same time?

M.F. No. I had met Hayek earlier than that. Let me see—Hayek wrote *The Road to Serfdom* in 1944. He came to the United States—it was published in the United States in '45?

J.D.H. I don't recall.

M.F. It was published in Britain I think in 1944 and the United States in 1945. Aaron Director, who has an office right down here and is my brother-in-law, was responsible for getting the University of Chicago Press to publish *The Road to Serfdom.* He had known Hayek in England, before World War II, when he spent a year in London, mostly with Lionel Robbins and Friedrich

Hayek at the London School. So when Hayek came over here, in 1945 or '46 whenever it was, I met him somehow; I don't really know how. When he called together this meeting at Mont Pelerin, and I was asked to go and did go, that was the first occasion in which I had more than casual contact with him, but I had already read *The Road to Serfdom,* and had already met him.

Hayek is an interesting case from a methodological point of view. Let me go back to something we were saying at lunch, which this brings to mind. I was talking about the fundamental issue of why people hold the views they do. I'm particularly interested from that point of view about the so-called Austrians, or von Misesians. Because their philosophy which admits no role whatsoever for empirical evidence—it's entirely introspective—leads to an attitude of human intolerance. I think anybody who holds that methodological view either is to begin with, or ultimately becomes, an intolerant human being. And the reason is very simple. If you and I disagree about a proposition, the question is how do we resolve our difference? If we adopt a Misesian methodological point of view, the only way we can resolve our difference is by arguing with one another. I know it from what's inside me, you know it from what's inside you, and so you have to persuade me that I'm wrong, or I have to persuade you that I'm right. There is no other appeal. And so ultimately we have to get to fighting. I'm right, you're wrong; I'm right, you're wrong; I'm right, you're wrong. The virtue of what I take to be the original Popperian methodological view, or of the point of view that I really adopt, which is neither Popperian or von Misesian. What it really is is more Savage–de Finetti. You asked if I read methodology and philosophy, I've read a great deal in the field of statistical methodology and statistical philosophy, and Jimmie Savage was one of my closest personal friends. And as you know we collaborated extensively. He was one of the few people I knew whom I would unhesitantly classify as a genius. And Jimmie said, and this is a crucial point, 'the role of statistics is not to discover truth. The role of statistics is to resolve disagreements among people. It's to bring people closer together.'

Go back and look at it in those terms. Suppose you adopt that point of view—the methodological point of view I adopt—and suppose you and I differ, and we come to the point where after arguing with one another we're at an impasse. Well then we have a recourse. I could say to you, "Tell me, what evidence would I have to get that would persuade you you were wrong?" And you say to me, "What evidence would I have to get to persuade you?" And then we can go out and look for the evidence. The way in which Jimmie Savage would describe that would be to say, you have a set of personal probabilities about events of the world, and this particular proposition we're arguing is one of them. I have a set of personal probabilities. Those personal probabilities differ. That's why we argue. The role of statistical analysis is to lead us to reconsider our personal probabilities in the hope that our personal probabilities will come closer and closer together. So you start out thinking in the simplest case that this coin is a fair coin and I think it's unduly loaded heads, and so we discuss the experiment which will lead us to discriminate and then we go and toss the coin. And either I say 'You're absolutely right', or vice versa. What we've done is to revise our personal probabilities. Your personal probability was 50 per cent—we haven't demonstrated that there's any such thing as 'the' probability which is 50 per cent. That's what Jimmie means when he says you're not searching for truth. Because if there be a truth, there's no way of knowing when you get it.

The trouble I always have with those people who say, you're not looking for truth. Of course I'm looking for truth. But the question is, how do I know when I get it? And the answer is, I never will. And therefore no matter how much evidence I get, I never can have one hundred per cent confidence that I have the truth. Going back to the von Misesians, that's why I think that their praxeological philosophy leads to intolerance. You'll notice that Mises himself was a highly intolerant person. Ayn Rand was a highly intolerant person. As he's become older, Popper has become an intolerant person. Hayek is a very interesting case, because I think Chicago in particular had a sufficient influence on him so as to move him away and he is not nearly as intolerant as the other von Misesians. Same thing was true of Fritz Machlup, who was another disciple of von Mises, but neither of them were anything like as intolerant as von Mises himself. But this crew of people down at the Mises Institute which is near you …

J.D.H. At Auburn.

M.F. They're just as intolerant a bunch as you can find. Now, how does that get us into some of the issues we're talking about here? Very directly. One of the most beautiful little examples of evidence that contradicts a theory is the evidence that I presented in one of my articles on the National Bureau's work on what was wrong with von Mises's cycle theory. I don't know if you remember; it hasn't gotten much attention. But I think it's really a beautiful little methodological example. The von Mises theory of the cycle is that the 'cause'—they would use the word cause—the cause of the depression is the prior expansion. That means, it would seem to me, that if you have a big expansion, you are going to have a big depression. You have a little expansion, you'll have a little recession. So I went and I looked at the relationship between the amplitude of expansions and the amplitude of the succeeding recessions. There's zero correlation. On the other hand, there's a very high correlation between the amplitude of a recession and the amplitude of the succeeding expansion. That's utterly inconsistent with the von Mises theory. It must be that what happened during the recession influences the succeeding expansion. It seems to me that that one little bit of evidence is a decisive refutation of von Mises. Have the von Misesians in any way stopped saying exactly what they were saying for fifty years? Not a word of it. They keep on repeating the same nonsense. What they call scientific work isn't scientific work at all. Because they regard facts as ways of illustrating theory, not as ways of testing theory. So their scientific work is from my point of view useless I don't know about their philosophical work.

J.D.H. Going back to something you said at lunch along those same lines—you said that today you believe that ideology is more important in influencing people's views of the facts than you once thought. At this point would you look back on the origins of the NBER—of what they expected to do—to accomplish—as being a little naive in terms of applying the tools of science to settle issues once and for all?

M.F. I said to you at lunch two things. I said I had come to question it, but I still took it as an operating hypothesis. Let's see what I mean by those two statements because they bear on exactly what you're saying. Insofar as it is true that the views of facts are determined by ideology there isn't anything you can do about it. On the other hand, insofar as there is some truth in the opposite relationship or in the fact that people are willing to be persuaded by evidence, then there is something you can do about it. And that is what Mitchell set up the National Bureau to do. It was a worthwhile purpose and it remains a worthwhile purpose. The only difference is, I'm not as optimistic about how far reaching its effects can be. But none the less it's the only possible hypothesis, it seems to me, which I can proceed on as a working matter.

J.D.H. In 'The Marshallian Demand Curve' [*JPE,* 1949] you echo Marshall's description of economic theory as an 'engine for the discovery of concrete truth'. You compare the Marshallian conception of economic theory with the Walrasian conception of what theory should be and should do. My understanding of your distinction is that Marshallian theory is problem oriented in the following sense: 1) that it is focused on actual problems from the world of experience; 2) that one begins analysis of a problem well-armed with observed and related facts; 3) that the structure of analysis is dictated by the specific problem one is dealing with; 4) that real world institutions are accounted for and dealt with; 5) that definitions of terms are problem specific; and 6) that mathematical considerations do not take a dominant place in the analysis. The Walrasian approach is to be more concerned with generality; to make theory more abstract, and less connected with problems of policy or experience and institutions; to check the theory or otherwise resort to empirical evidence only after the theory has been worked out; and to emphasize logical consistency and mathematical elegance. Do you have a sense of how you first came to make this distinction and how or why you saw it as important?

M.F. I don't really have the sense of how I first came to make the distinction or why I said it was important. I haven't thought about the question, and offhand in thinking about it, I really don't know. It's a distinction I made from fairly early on.

J.D.H. You made it early, and I've come to think that it may be one of the keys to your methodology and perhaps …

M.F. I suspect that came from Burns. That's my guess, but I really couldn't document it—because he was so imbued with Marshall. You see, he was very much a disciple of Marshall on the one hand, and Wesley Mitchell on the other. And Wesley Mitchell would have impelled in him aversion to the pure abstract Walrasian, while Marshall would have impelled in him his problem-seeking approach. I suspect that that's where it comes from, but I really can't say. That's just pure rationalization.

J.D.H. Your criticism of Lange's *Price Flexibility and Employment* as 'taxonomic theorizing' in your 1946 review [reprinted in *Essays in Positive Economics*] bears a certain resemblance to Viner's first publication, 'Some Problems in the Logical Method in Political Economy' [*JPE,* 1917]. There he argued that there are three methods of obtaining generalizations for deductive analysis: 1) by complete enumeration of individual propositions; 2) by assumption; and 3) by inductive inference. He argued that the first, which appears similar to what you call taxonomic theorizing, is not a practical manner of reasoning. The second, Viner contended, becomes induction when brought in contact with reality. So, he argued, all reasoning requires induction unless it is 'wholly abstract or hypothetical'. Do you recall this or other particular methodological writings of Viner's as having an important influence on your own views about methodology?

M.F. To the best of my knowledge, I don't believe I ever read that essay of Viner. I may have, but I don't remember ever reading Viner's essay. I don't think he assigned it in class—I'm pretty sure he didn't. I have no recollection of ever having read it, which doesn't mean I didn't.

J.D.H. Would he have made the Marshallian–Walrasian distinction?

M.F. He very well may have, yes. He was very much of a Marshallian, of course. And he very well might have.

J.D.H. You said in the dialogue on the history of law and economics at Chicago [Kitch, *JLE,* 1983] that Jacob Viner began the real tradition of Marshallian (as opposed to Walrasian) analysis at Chicago. Did he use these labels in his economic theory class or did you or someone else coin the label in an interpretation of what Viner taught?

M.F. I don't recall his using those labels. So far as the use of the actual labels was concerned, I probably am responsible for coining them in connection with the review that I wrote of Jaffe's translation of Walras's *Elements.* But I wouldn't swear on a stack of Bibles that he didn't use the labels.

J.D.H. Would you say that his 301 course had much of a methodological content?

M.F. That depends on what you mean by methodological. It had no explicit methodological content whatsoever. But there was a very strong implicit methodological content, since you came away very clearly with the feeling that you were talking about real problems. Part of the distinction is viewing economics as a branch of mathematics—as a game—as an intellectual game and exercise—as Debreu, Arrow and so on—and it's a fine thing to do. There's nothing wrong with that. After all, mathematics is a perfectly respectable intellectual activity, and so is mathematization of economics or anything else. The other part of it is viewing it (using Marshall's phrase) as an engine of analysis. And there was no doubt that Viner viewed it as an engine of analysis, and no doubt when you were in his course that you came away with the feeling that economics really had something to say about real problems and real things. In that sense it had methodological content.

J.D.H. Would you say …

M.F. Let me give you a little example, which comes closer in some ways to your interest. Viner's review of a book on the cement industry [*JPE,* 1925]—I've forgotten whose book it was—in which he made fun of the author who pointed out that all of the cement producers were charging the same price and concluded 'After all, it's an essential feature of a purely competitive equilibrium that all the prices of the same product are the same everywhere.' And Viner literally made fun of him, arguing that that was sure evidence that you were not in a purely competitive situation. Well, you can see that that really has a very strong methodological element in it, and really reflects the distinction between these two approaches.

J.D.H. And I suppose Knight's courses would have? Or …

M.F. No.

J.D.H. Maybe not.

M.F. No. Knight's would not have had any methodological component at all. Knight never was influenced by any facts that he didn't observe casually himself. Knight was very funny that way. He was absolutely persuaded that inequality tended to increase. At least a half a dozen times we talked him out of it. And a half a dozen times he would come back. He'd be perfectly willing to be talked out of it, and he'd be persuaded that there was a logic both ways, and it didn't have to happen. Next time the subject came up he'd be right back.

J.D.H. You indicated to me in correspondence [28 March 1986] that your interest in methodology was not sufficient to get involved in the debate that grew out of your 1953 essay. Do you see your work as becoming less methodologically oriented after 1953?

M.F. I'm a little uncertain how to answer your question. I don't know what it means to say that the work is more or less methodologically oriented. I have since 1953 not written anything that was explicitly devoted to methodology. My remarks about methodology have always been comments in the course of some other discussion. They've been by-products of something else. On the other hand, so far as my positive work is concerned, it's obviously been affected by my methodological views. In that sense it's been methodologically oriented. But if you mean, have I deliberately undertaken work in order to illustrate methodological principles, the answer is no. I've undertaken work to try to find out something, and I believe that there's a certain way of finding out something that is more effective than some other way, and that's the way I tend to go about things. But it hasn't been methodologically oriented in any other sense.

J.D.H. Before 1953 …

M.F. It wasn't before 1953 either. I don't really remember why I wrote that article to tell you the truth.

J.D.H. Well that's what I was going to ask. I find a lot of methodology in your work before 1953 but it's almost always within the context of some economic problem, and I was going to ask, why that introduction to a collection of essays?

M.F. I really don't know. I really can't answer you. What I don't know is whether the idea of the collection of essays came first, or the essay came first. What happened was that a friend of mine who was running the social science branch of the University of Chicago Press came to me with the idea that I should produce a collection of essays. I was amenable to that, and I think we put together this collection. What I don't know is whether then I said 'well gee, there ought to be an introduction to this', and so I wrote this as an introduction, or whether I had written the essay earlier, and said 'this is a good place to put it'.

J.D.H. You made a comment on Ruggles for the AEA collection [B.F. Haley, ed., *A Survey of Contemporary Economics,* 1952] which was very short, but it showed up …

M.F. That was some kind of a meeting or something, a conference or something in which I was invited to do that, to comment on Ruggles.

J.D.H. I don't remember what the occasion was …

M.F. And of course, I should say, that no doubt one of the things that sharpened my interest in this goes back much earlier, to the period when I was at the National Bureau, and particularly when I was serving as the Secretary or something of the Conference on Income and Wealth. I edited the first three volumes of their series. And I believe that had quite a bit to do with forcing me to clarify my ideas about how one should go about empirical work. Because so much of the national income stuff was pure empiricism of the rawest kind. And I kept revolting at the kind of considerations that the measurers were bringing in, trying to decide various issues, like what value should be attached to government services and things like that. I think you will find that most of my comments on papers in those first three volumes reflected my approaching them from the theoretical point of view and being dissatisfied with the purely empirical approach. So I suspect that helped, but then I got diverted, because I first went to Washington during the War, for two years in the Treasury, and then to New York for two years in the Statistical Research Group as a mathematical statistician. So why in the early 1950s I was led to write this essay, I have no idea.

J.D.H. Would it be an apt characterization of your interest or perspective on methodology to say that you are a Marshallian on methodology in the following sense: you think methodology can be important within the context of particular questions about economic theory, but is less useful as an area of inquiry unto itself?

M.F. I don't want to make any judgement about what's useful as an area of inquiry. I'll make a judgement about what's useful to me. I decided that I was more interested in doing economics than in writing about how economics should be done. This was partly determined by the fact that I came to the conclusion from the empirical point of view that there was very little correlation between what people said about methodology and the way in which they did economics. And therefore it must be that straightening anybody out on methodology had very little influence on what he did. So I decided I'd better do methodology, as it were, instead of make methodology, if you want to put it in those terms. But I'm not going to make any judgement about what's useful for other people. The proof of that pudding is in the eating. If somebody finds it useful, it's useful.

J.D.H. But for yourself, this might be an apt characterization of your perspective?

M.F. Yes, unquestionably, it was an apt characterization for me. This wasn't the way in which I thought either my interest or my abilities were best directed. On a broader level—if I may bring Alfred Marshall back into it from a wholly different point of view—Alfred Marshall, to begin with, was extraordinarily sensitive to criticism. When somebody—in a footnote in an article in the *QJE*—made a negative criticism of, not his *Principles,* this was before that, but *The Economics of Industry,* which he wrote with his wife, or which his wife wrote and he signed—I'm not sure which [Marshall and Marshall, *Economics of Industry*]—he immediately came back with a three-page answer. And he did this three or four times, and then he suddenly decided that he wasn't going to waste his time answering criticisms, and for the rest of his life he never answered a criticism. On the whole, I think that's the right approach. And I decided early on that so far as possible, I was going to resist the temptation to respond to criticisms unless it advanced the subject in question. I haven't succeeded in following that perfectly, but I've tried to follow that practice. And since I wasn't doing any more work in the field of methodology, it didn't seem to me I ought to spend any time answering the various comments that came along.

J.D.H. A distinction between theoretic and atheoretic analysis was very much the point in Jacob Viner's 1929 [*QJE*] review of F.C. Mills' *The Behavior of Prices* and in the 1939 appraisal of the book sponsored by the Social Science Research Council [R.T. Bye, *Critiques of Research in the Social Sciences: II, An Appraisal of Frederick C. Mills' The Behavior of Prices*]. What do you see (or did you see during the 1930s) as the relationship between the early NBER business cycle work of Mills and Mitchell and neoclassical price theory?

M.F. There's no doubt in my mind that I regarded Mills' *Behavior of Prices* as a horrible example of the way you should do empirical work, and that I thought it was some of the worst of the NBER stuff. The NBER cycle work of Mitchell was not the same as that. That had more meaning and more substance. That was simply because Mitchell was a smarter person than Mills. Mills, if you'll pardon my crude characterization, was not very smart. He was like Schultz. But he picked out the wrong problem, while Schultz was wiser in picking out the problem he picked out.

J.D.H. You said earlier that Mitchell was not a theorist.

M.F. He was not.

J.D.H. But in your commemorative paper, you entitled it 'Wesley Clair Mitchell as an Economic Theorist' [*JPE,* 1950] and you didn't make that argument there.

M.F. I didn't make that argument there. I really haven't given you the right impression. Mitchell was not a natural theorist. That is, the difference between Mitchell and me is not at all in our abstract ideas of what theory ought to do or what its role is. In that sense, Mitchell was as much of a theorist as I am. The difference between us is that my natural instincts are theoretical and his natural instincts are not. That's what I meant by the statement that Mitchell was not a theorist. I don't mean that he didn't have respect for theory or that he wasn't concerned with theory, but he wasn't a natural theorist. He couldn't make theory. He could do theory, but he couldn't make it. And in that sense—his work was on a different level than Mills' altogether.

J.D.H. NBER-style business cycle analysis has come to be associated largely with your work (and Mrs Schwartz's). But before that the association was with Mitchell, Burns, and Mills. T.C. Koopmans treated Burns and Mitchell's *Measuring Business Cycles* harshly in his review [*RE Stat.*, 1947]. For largely the same reasons, Viner had earlier been very critical of Mills' *The Behavior of Prices* [*QJE,* 1929]. Having had close ties yourself to Burns, Mitchell and Viner, and having been in close proximity to the Cowles Commission at Chicago, where did your sympathies lie in this debate on the nature and role of theory in business cycle analysis?

M.F. Koopmans was just foolish. There is no doubt whatsoever that in a debate between Koopmans, my sympathies were entirely on the Burns–Mitchell side. I thought that Koopmans' was a very sophomoric attack and had no effective positive content—he didn't tell you where you went from here. And of course you realize that I had been involved in very long arguments with the Cowles Commission people when they were in Chicago. And that I was anything but an admirer—anything but a devotee, or a disciple of their belief that the way to understand the working of the world was to construct big econometric models. In fact, I was a major critic of the kind of thing they were doing in Chicago. I introduced the idea of testing their work against naive models, naive hypotheses and so on. So that I was very unsympathetic to Koopmans from the beginning—before he wrote the article. And certainly I didn't get any more sympathetic as a result of that article.

J.D.H. Would you have said that they were working on either the Walras problem or the Cournot problem? You made that distinction …

M.F. They were working on the Walras problem—unquestionably.

J.D.H. In a long footnote in 'The Methodology of Positive Economics' you discussed the 1940s Cowles Commission work on making theories testable, out of which grew the idea of the 'identification' problem. I read the tone of the footnote to be mildly critical of the Cowles Commission programme. Did you think they were on the wrong track?

M.F. Oh, sure I thought they were on the wrong track—no question.

J.D.H. There's a body of their work explicitly on causality—Koopmans, and Orcutt, and Simon.

M.F. I never really got involved in that. I don't even know whether I read most of it. When I said I thought they were on the wrong track, it had nothing to do with that. It had to do with their belief that you set up these big models—I think the comment I wrote at one point (or said—I don't know) in connection with Christ's evaluation of the models really expressed my views at that time.

J.D.H. One concern that was expressed about Wesley Mitchell and the NBER during the 1920s and '30s was that they were setting out to make economic *theory* anew. Along this line, what was your impression from your association with Arthur Burns at Rutgers and Mitchell at Columbia?

M.F. I really don't know how to answer your question, because that was never a concern I had or thought that they were trying to do. As I say, coming to it with my own background from Chicago, I was very much more of a theorist in a certain sense—I started from theory to a greater extent than they did. But I don't think they were setting out to make economic theory anew. They may have envisioned a different type of theory. I really have no very sensible things to say in answer to that.

J.D.H. It is part of the received history of Chicago economics that Knight's and Viner's concern and ability to make price theory the focal point of the graduate curriculum is a big part of that which set Chicago apart from other schools. In going from Chicago to Columbia as a graduate student in 1934, back to Chicago in 1935, and to Columbia again in 1938, did you find that Chicago during that period really was distinctive (compared to Columbia)?

M.F. There's no doubt that Chicago was distinctive, and has been ever since. The real distinction was not making price theory the focal point of the graduate curriculum. That isn't the real distinction at all. The fundamental distinction is treating economics as a serious subject versus treating it as a branch of mathematics, and treating it as a scientific subject as opposed to an aesthetic subject if I might put it that way. The fundamental difference between Chicago and for example, Harvard, and Columbia to a lesser extent—Columbia at that time was something of an amalgam of Chicago and Harvard. The fundamental difference between Chicago at that time and let's say Harvard, was that at Chicago economics was a serious subject to be used in discussing real problems, and you could get some knowledge and some answers from it. For Harvard, economics was an intellectual discipline on a par with mathematics, which was fascinating to explore, but you mustn't draw any conclusions from it. It wasn't going to enable you to solve any problems, and I think that's always been a fundamental difference between Chicago and other places. MIT more recently has been a better exemplar than Harvard. And of course there are no such things as one hundred per cent pure cases either at Chicago, or elsewhere.

J.D.H. You stated in your Trinity University Nobel Economists lecture [in W. Breit and R.W. Spencer, *Lives of the Laureates*, 1986] that you thought the combination of theory at Chicago and institutional detail and empirical work that you found at Columbia was ideal for a budding economist in the mid-1930s. Was the *friction* between theorists such as Viner and institutionalists such as Mitchell an important part of this fertile experience? Did you get from either of them the impression that you (or anyone) would have to choose one way or the other?

M.F. No, I never got the impression I'd have to choose one or the other. I thought I got a great deal out of Viner, and I thought I got a great deal out of Mitchell. I got different things. But, I never felt that there was a …

J.D.H. And Clark?

M.F. And Clark. I didn't get a great deal out of Clark as I said, because he was more or less an amalgam, a mixture of the two. Mo Abramovitz was one of my closest personal friends at that time and since, and I always found it puzzling what Mo got out of Clark, to tell you the truth. But he did—he got a great deal out of him and thought he was a remarkable person. I liked Clark—I'm not in any way running him down. He was an extraordinarily able man. His *Economics of Overhead Cost* is a first-rate book. What was his thing he wrote in the 1930s having to do with planning … ? 'Planning Public Works', or something like that [*Economics of Planning Public Works,* 1935]. I didn't find that at all helpful, so that I never really got a great deal out of Clark from my point of view.

J.D.H. Harold Hotelling, Mitchell and Henry Schultz were all three heavily involved in statistical work during the 1930s. Are there distinctions to be made in how you would attribute influence on your own development as an economic statistician among these three of your teachers?

M.F. There's a very, very big distinction among them. Henry Schultz is distinctive from the other two in the sense that he chose a narrow subject, and he dug very deep in it. And the range of tools he deployed were fairly narrowly limited to multiple regression. He had some interest, I may say, in spectral analysis or periodogram analysis, Fourier series, partly because he had acquired a beautiful machine, a mechanical machine—it was lovely to look at and he displayed it to all visitors but he never used it—which had nice spherical glass balls and so on that rolled across the surface, and if you rolled it over a graph series it would generate the Fourier series that produced it. So Henry Schultz was very clear and very specialized.

Harold Hotelling and Mitchell were wholly different. They were poles apart. There was no relation between them at all. Harold Hotelling was a mathematician. Mitchell was a statistician. It was mathematical statistics that interested Harold Hotelling. But Harold Hotelling had an amazing character of analyzing what looks like a purely abstract unimportant problem, that turned out to be extraordinarily relevant and of great practical importance, although not by using statistical data or anything. Let me give you some examples. His article in the 1930s on 'The Economics of Exhaustible Resources' [*JPE*, 1931] is undoubtedly the most important bit of writing that has been done in that area. It has underlain all of the postwar discussion of the oil problem—of the effect of OPEC and so on. If you look at that material, you'll find that the reference most often cited is Harold Hotelling. Why did he undertake it? It was a purely abstract idea, but he somehow had the insight that it was going to be important. He wrote an article on canonical correlation which ultimately turned out to be the foundation of factor analysis. During World War II when he worked with us at the Statistical Research Group we had a problem of how you were going to test a Norden bombsight. The Norden bombsight was a famous bombsight that was used for sighting from an airplane where you would drop a bomb. They were very complicated mechanisms. We, the Statistical Research Group, were very heavily involved in quality control, in judging production lines. That's where sequential analysis was developed. And Hotelling was given the problem of trying to work out a method of quality control for the Norden bombsight. When he finally delivered his report, the last Norden bombsight had been manufactured. It was utterly useless for practical purposes, but it turned out to be the founding document for multivariate analysis later on.

So, Hotelling had a really enormous insight in economics and in statistics—his Edgeworth taxation paradox article, his exhaustible resources piece in economics. His canonical correlation, and his Norden bombsight thing, and a couple of other things he did had the same kind of an impact on the purely statistical side. But, you can see that's a different universe from Mitchell. Mitchell could no more have done any of that than he could have flown to the moon. What Mitchell had was an extraordinary capacity for bringing together a very large mass of material, extracting common elements, and describing what he found in language that was clear, unambiguous and understandable. Mitchell had a great capacity for clear writing—and speaking—you could listen to an extemporaneous lecture or talk of Mitchell's, and take it down word for word, and you would find that it was perfect. It could be published word for word without a grammatical error, or any infelicity or anything. He was absolutely remarkable in that respect.

J.D.H. Do you think that Knight and Viner had any direct influence on your way of doing empirical work statistically?

M.F. No. None whatsoever. Hotelling and Schultz had much more influence on it.

J.D.H. How about Allen Wallis? Was there a difference between the …

M.F. No. Allen and I would have agreed almost entirely on everything. Allen—again, he was one of my closest friends at Chicago, and has remained so the rest of my life. I tried to persuade Allen not to become Dean of the School of Business. I still think that if he hadn't gone off into administrative work he would have made very important contributions to economics and statistics. But, it's clear that his comparative advantage was in administrative work and he's had a remarkably successful career in this field. But from the very beginning I think Allen and I saw eye to eye. I don't believe there was ever any real difference of opinion.

J.D.H. Would you classify yourself as a Bayesian?

M.F. Yes, and no. I don't like to use the word Bayesian. I would rather refer to de Finetti and Savage, and they are regarded as Bayesians. But the crucial thing is not inverse probability. What's crucial is the notion of personal probability. It's really objective probabilities versus personal probabilities as the key element. I would classify myself as personal probability, and I would say that from a methodological point of view, Jimmie Savage has exerted as much of an influence on me as Popper did. And if Jimmie Savage was alive, he would have said the same thing about me I know. We were very good friends, but also we had a great deal of influence one on the other. And the articles we wrote jointly were really joint, though there are some parts of them that were clearly Jimmie's. Surely the purely axiomatic logic, the underpinnings of measuring utility are all Jimmie's. But I really can't say that there's any part of it that he didn't contribute to or that I didn't contribute to.

J.D.H. You suggest in the 1981 discussion on the history of Law and Economics at Chicago [Kitch, *JLE*, 1983] that Aaron Director was an important influence on your research. George Stigler mentioned in the same discussion that Director insisted on profit maximization explanations of behavior, as opposed to detailed accounts. If one takes this type of analysis as a hallmark of Chicago economics and of your own, how would you compare Director's influence to that of Knight and Viner?

M.F. Your question is really very difficult. Because if you look at what Director did—the fascinating thing about him is that he was one of the students of Knight who I think was in some ways rendered infertile by Knight, repressed by Knight. He came to Chicago as a student of Paul Douglas and the only real book he wrote was a book he wrote jointly with Paul Douglas before he came under the influence of Knight [*The Problem of Unemployment*, 1931]. Aaron, at the Law School, did not write much himself, but he had a tremendous influence on other people. The most important article John McGee ever wrote was an article on the Standard Oil–Rockefeller case [*JLE*, 1958]. The allegation was that Standard Oil got a monopoly by undercutting its competition with the idea of subsequently being able to raise its price and get it all back. It was unquestionably Aaron who pointed out that that made no logical economic sense, and McGee who went back to zero in on the case and found that it didn't make any empirical sense either. If you look at the introduction to Bob Bork's book on antitrust [*The Antitrust Paradox*] you will find there that he attributes to Aaron fundamental responsibility for his own change of ideas. If you take the work that's been done on tie-in pricing, it traces back to the influence that Aaron had on the people who did it.

So, where Aaron had his influence, he was different from either Knight or Viner. Knight was a philosopher, as I say, fundamentally. Viner was a marvelous teacher, an excellent technician of theory, and his basic interest was in the history of economic thought. Aaron had a great deal of interest in the history of economic thought also, but he also had a real interest in how to apply the theoretical ideas of the economics of the firm to the individual firms or the individual industries. And, in the course of teaching, particularly in the course of teaching the course at the law school with Ed Levi on antitrust, the subject matter that he was teaching sort of generated all these interesting examples. How do you explain the fact that IBM would only rent and not sell its machines; it required the people who rented the machine to buy their punchcards from it and not from anybody else. The antitrust cases automatically brought up these puzzles. How do you explain them? We say that these companies are trying to maximize profit. How does this fit in with maximization of profit? And that was the origin of much of Director's influence. If you look at my price theory book, there is in the back a collection of problems which I gave to students. A large fraction of those problems came out of Aaron Director, exactly in that way. And that's the sense in which he was different from either Knight or Viner. He had, as it were, Viner's command of economic theory, and belief in economic theory as a real thing, but he had something that I think neither Knight nor Viner did have, which was this interest in solving these concrete problems—in particular, problems in the area of Industrial Organization. That's what led him to found the whole field of Law and Economics.

J.D.H. Using your terminology, would you refer to him as a Marshallian?

M.F. Oh yes.

J.D.H. The same sense that you are?

M.F. The same sense, absolutely.

J.D.H. Reder [JEL, 1982] argues that the key to the dominance of the 'Chicago view' at Chicago was in the appointments of yourself, George Stigler and Allen Wallis to the faculties. He gives the label 'Tight Prior Equilibrium' to this 'Chicago view'. 'Tight Prior Equilibrium' is the use of Pareto optimality as a premise of applied work, in conjunction with other assumptions taken as 'first approximations': transaction prices are market clearing; information is acquired to the point at which its marginal cost equals its price; and neither monopoly nor government intervention prevents marginal products and resource prices from being equalized across uses. Do you see this identification of the 'Chicago view' as essentially the same as the 'Marshallian' approach? Are there important differences? Would it matter to you whether one or the other was used to describe your approach?

M.F. What Reder is describing is substance rather than methodology, and so I don't regard his identification of the Chicago view as essentially the same as the Marshallian approach at all. But neither do I regard it as in contradiction. I think you're talking about different things. About non-intersecting things as it were. The Marshallian approach has to do with what you're trying to achieve, what your purpose is, what your objective is, and how you go about it. What he calls the 'Chicago view' is a set of tentative hypotheses, substantive hypotheses, accepted as starting points for investigation, and I think that's a correct description. But the starting point for an investigation, and the methodological approach to an investigation, are two very different things, and I would say that those two views are complementary, rather than any way contradictory or competitive. And I think that covers that part.

J.D.H. Thank you so much. I certainly appreciate it.

M.F. That's alright.

Appendix

HOOVER INSTITUTION ON WAR, REVOLUTION AND PEACE
Stanford, California 94305-2323

13 June 1985

Mr. J. Daniel Hammond
Department of Economics
Wake Forest University
Winston-Salem, North Carolina 27109

Dear Mr. Hammond,

I appreciate your sending me a copy of your paper on ‘Monetarist and Antimonetarist Causality’. My feeling after reading it, if I may put it very bluntly, is that I have been stuffed with straw and attacked. I have little quarrel with your substantive conclusions; I have a considerable quarrel with the rhetoric.

I have always regarded ‘cause’ as a very tricky concept. In my technical scientific writings I have to the best of my ability tried to avoid using the word. In the quotation with which you start the paper I do not say at all that money stock is a cause. I believe that you will not be able to find a statement in the *Monetary History* or in other scientific writings of mine in which I make such an assertion. This is clear from your own summary of our comments about the theoretical and empirical elements in respect of inflation.

I must confess that I departed from my determination to avoid using the word ‘cause’ in *Free to Choose* which was intended for a relatively popular audience but even there, in addition to the quotes you give from it, it seems to me it is relevant to note that first I say: ‘The recognition that substantial inflation is always and everywhere a monetary phenomenon is only the beginning of an understanding of the cause and cure of inflation’; and then I go on to say which direction you have to go. Moreover, with respect to whether inflation is always and everywhere a monetary phenomenon, on the bottom of page 255 of *Free to Choose* I point out that this is essentially an empirical statement, that ‘to our knowledge there is no example in history of a substantial inflation that lasted for more than a brief time that was not accompanied by a roughly correspondingly rapid increase in the quantity of money; and no example of a rapid increase in the quantity of money that was not accompanied by a roughly correspondingly substantial inflation.’ I do not believe that those statements justify your statement on the bottom of page 14: ‘Milton Friedman’s identification of money growth as THE cause of inflation … ’. Even in the less rigorous *Free to Choose* statements I insert weasel words such as ‘substantial’, ‘rapid’, ‘roughly correspondingly’. Clearly, for anybody who is at all sophisticated about the economic relations those qualifications are inserted precisely because I believe that other factors do affect what happens to inflation because the theoretical analysis in terms of the quantity theory that you outlined is really what underlies my analysis.

I have no quarrel with your saying that Kaldor uses the same implicit concept of causation as I do. I believe he is simply factually wrong in his assertions about what happened, but that is a wholly different argument than the one you make here.

In short, you have attributed to me a definition of 'cause' as a general definition that to the best of my knowledge I have never stated or published anywhere. With respect to the Mackie definition of 'cause', it is a plausible one but I am by no means persuaded that it resolves the ambiguities. The problem is in your simple example, if you say the short caused the fire you have to ask what caused the short. This is a process of infinite regress. That is why I have tended to try in my technical writings to avoid using the word 'cause'. Indeed, even in *Free to Choose* on page 253 the heading of that section is 'The Proximate Cause of Inflation'.

One final comment. Antonio Martino's statement comes closer to fitting your straw man than any statement of mine you have quoted. But I doubt that you would want to have your paper rely on a quotation from him rather than from me. I personally share your criticism of his statement. It is a careless and superficial statement, and he would be the first to admit it if he were pressed on it.

Sincerely yours,
Milton Friedman
Senior Research Fellow

10/1/12