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Interview of Milton Friedman

Conducted by Robert L. Hetzel

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Robert L. Hetzel: In the fall of 1946, you started teaching Jacob Viner's price theory course. And at that time, you were just Milton Friedman, so it was pretty extraordinary position to be starting out in after one year of teaching at the University of Wisconsin. What were the primary published works that you had done that caused the University of Chicago to hire you? What do you think was the major work...

Milton Friedman: I think you have to go back to something else, and I think you have to go back to the years of '32, '33 and '34 when we were all students there. And the faculty at the University of Chicago got to know that among the student body there, among the graduate student body, rather, that a sense in which, if I may say so, now, I would not have said so then, George Stigler, Allen Wallis and I stood out very much. I was a student in Viner's course, I took his course. And the faculty of the University of Chicago got to have an appreciation for those people before any published work came out. Now, you ask what publications. The most important publication I guess, up to that time, interestingly enough, I don't think anything was published in the field of price theory except in terms of income from independent professional practice. And I think that the book I wrote with Simon Kuznets was also served us for my doctoral dissertation at Columbia. I think probably the best thing to do from that point of view would be to look at my chronological bibliography.

Robert L. Hetzel: Sure.

Milton Friedman: And that right here, no, I'll get it. Thank Gloria for this one. I had co-authored with Carl Shouth and Ruth Mack in Brooklyn, that wasn't a very important book. It was the, in terms of books, the only one with income from independent professional practice, which came out in 1945. But in terms of articles, that's still in New York City. I had an article in the Quarterly QJE in 1935, about Professor [inaudible - 00:03:08] method of measuring elasticity of demand. And incidentally, let me go back again, I spent the year ’32-’33 as a graduate student in Chicago. But ’34-’35 as a research assistant to Henry Schultz and that probably provided more information to the faculty than did my publication. Because Henry Schultz’s book, “Theory and Measurement of Demand” he was working on. His masterpiece. A classic. I wrote considerable parts and that was known around the department. The department understood it. Schultz was a great man in a sense that I did not appreciate at the time, but have since, in the sense that he was open to criticism from a very junior assistant and had no -- so many people find it impossible to have as somebody as an inferior who may be their superior in intellectual ability.

[Comment deleted at interviewee’s request.]

Anyway, going on to your original question, if you look in here, there are 20 of his pieces before 1945. And some of those were pure and true statistics, like an article on Reichs, use of Reichs. But there was an article in the QJE and an article in the Journal of the Royal Statistical Society, Journal of the American Statistical Society, reviews in the American Economic Review, there was a published, but more important that the published material was a personal knowledge of the people at Chicago. Through, you must remember, that economics at that time even much more than now, was a fairly closed, self-contained profession, which everybody knew everybody else, who they were. And they knew them not only by virtue of the printed record, but by virtue of personal contact. Remember, during that period, we were all in Washington, it was the New Deal period and all the academic admissions was come to Washington, Viner, for example, was very close to the Treasury and was a major advisor to Morganthau. Still, as you know, they originally, that post was originally had been filled by George Stigler. He was the one that initially, and now take George, Ted Schultz had brought George Stigler to Iowa State when Schultz was Chairman of the department at Iowa State. Then when Schultz shifted to Chicago, became Chairman of the department of Chicago, it was natural for him. He knew all about George, he was aware of his [inaudible - 00:06:50]. So somehow, I don’t know. If you’re going to talk about how things might have been, the world might have been very different if George hadn’t spoken rather loosely and been misunderstood by Caldwell at that time. History would have been very different.

[00:07:23]

Robert L. Hetzel: The...

Milton Friedman: This shows a role that chance plays.

Robert L. Hetzel: Yeah. You have said a number of times that Viner was the one who gave you the sense of how the price system could allocate resources in a decentralized way, that he really made you see the power of the price system. And you may remember the last time I visited I was fortunate enough to meet Aaron Director and I asked him, when did you become a liberal and he said, well, it was with the course I took from Jacob Viner. Did, when you started teaching this course, did you have the sense that you were going to teach the course in the same way, that you had that same standard to major up to, were you aware of that sense...

Milton Friedman: Oh there's no question. The course I took was very largely Viner's course modified. I had my own notes from Viner's course, in fact I still have them somewhere, I'm not sure where they are now, they're probably in the archive, but there's no question. First of all, I should say, I had taught a course part time at Columbia and I had taught a course in Wisconsin when I was there for a year. And I don't think I taught theory, I didn't teach theory in Wisconsin, but I did.

Robert L. Hetzel: You did, of course, on the business cycle...

Milton Friedman: I taught a course on the business cycle and one on statistics. If I remember rightly. But when I came to teach the course on 301, the theory course, there's no question that I based it, it was an expansion of Viner's notes. Now over the course of the years it just developed of course, it changed from year to year and quarter to quarter, but it maintained the basic structure of, which I think underlies, was involved in Viner's structure. Teaching economics as a machinery for analysis.

Robert L. Hetzel: I want to recount my personal experience at Chicago and then relate that to your course. I didn't realize this at the time, but every course I took, when I went there, was basically good price theory. And I was always carrying around questions to think about; problems and I don't know, choose your terminology, other would people would say brainwash, but when I left Chicago, price theory was a way of thinking about the world and the reason it was so, was not because of the formal elegance of it, it was just something I had used so often, through experience, I knew it was a practical, useful sort of tool. So I assumed from the beginning that you taught your course, you were challenging your students to think about, to apply their analysis in ways that would show them that, hey, I can use this to sort of figure out things about the world. And they then see that yeah, this is a useful, this was a useful tool that requires them to sort of apply the tools that you were teaching.

Milton Friedman: But I'm pretty sure that that was not a conscious decision. Because I never sat down and said that's what I'm going to do, that it just came as a result of, that was the way Viner taught it. Knight when he taught 301, it was a very different kind of a course. Knight was Knight. But, I think it was part of the atmosphere of Chicago. When I was a student and also when I was on the faculty. To take economics seriously, I think that at that time, if you think of what was going on at a place like Harvard, they were in a sense not taking economics seriously. Economics was a game. And that's what's happened now with all the models. Economics is not a serious way of understanding the world. Economics is a branch of abstract mathematics. And the models are valuable in and of their own right. Even if they don't enable you to understand the real world. Certainly, real business cycle has that approach, has that cast to it. It's a game. It's an intellectual discourse. It's life through impure mathematics.

[00:12:47]

Milton Friedman: And that was never the case in Chicago. It always was, and that, I think, really traces back to Marshall. The engine of analysis phrase comes from Marshall. It's in the principals.

Robert L. Hetzel: But there's another aspect of it in this period. When you think about ISLM, I mean, on the one hand, macroeconomics could spend 20 years in the desert in the sense of having models that were not grounded in price theory; that had nothing to do with price theory.

Milton Friedman: But those models didn't really start, you're now thinking of the Keynesian revolution.

Robert L. Hetzel: Right.

Milton Friedman: That started in '36, of course, or earlier in Cambridge, but the world at large, '36.

Robert L. Hetzel: The purpose, there were two things. One is that the models did not come out of price theory, and the second thing was they were chosen to tell policy makers what to do rather than being chosen to generate testable implications which could be falsified and then, you know, forced to reject the model on a [unintelligible - 00:14:07]

Milton Friedman: This is mostly the interpreter of Keynes rather than Keynes himself. Keynes really was a Marshallian in method. And I think you can take Keynes and pretty much treat it as a straight application of price theory to the aggregate economy. With the big difference that whereas in price theory you regard the quantities as given and the prices as variable, he regarded prices as given and quantities as variable.

Robert L. Hetzel: That was the intellectual environment of the Depression where people though the price system did not work.

Milton Friedman: Exactly. Absolutely.

Robert L. Hetzel: But your point is that he retained the Marshallian frame.

Milton Friedman: We analyze it entirely within the Marshallian framework. It was really when Hicks came along. Hicks and who was the other one who produced the ISLM verbiage at the same time. Har-- no, Harry didn't.

Robert L. Hetzel: Hicks is the one who...

Milton Friedman: But Hicks is the one who mostly got the credit for it.

Robert L. Hetzel: Modigliani.

Milton Friedman: But, so far as Chicago is concerned, money was being taught by Lloyd Mince and that was still classical. He was there, when did you come? Which year did you come?

Robert L. Hetzel: Well, I was an undergraduate there, so I started in '63. But I had to leave.

Milton Friedman: Mince had [unintelligible - 00:15:57] by then.

Robert L. Hetzel: I had to leave because of the War and so I left then.

Milton Friedman: You started in '63.

Robert L. Hetzel: Yes.

Milton Friedman: Then he left because of the Vietnam War.

Robert L. Hetzel: Right.

Milton Friedman: And then you came back when? Mince had...

Robert L. Hetzel: Seventy, I think.

Milton Friedman: Yeah, Mince had gone long before you came on the scene. He was still there when I first came on the scene. When I first came on the scene, Mince was teaching money.

Robert L. Hetzel: And, but, then the, I mean, there's two things in terms of thinking about models. I mean, one is whether the price system works and the other is how you think about the price level. And at that time, profession was thinking of the price level and institutional data controlled by a large monopoly power, by large corporations, and Mincer still had the idea that it was something that the Central Bank had to tie down. And that very, forces you to think in a very different way. That combination of that the price system works, and if you put the ideas together that the price system works and the price level is part of the price system that it's a equilibrating variable, but the changes in prices that account, that count, are relative not absolute prices. Then you put those two ideas together and you have the quantity theory. You're forced to think in those...

[00:17:27]

Milton Friedman: But I think relative prices count for allocation of resources, but the absolute price counts for the money market. That is, for the, which is the quantity theory comes in. The quantity theory is a pure price theory concept. Absolutely.

Robert L. Hetzel: But it is, it is a variable that, it is an equilibrating variable. In the case of a closed economy, it equilibrates the...

Milton Friedman: Oh sure, it equilibrates the demand and supply for money.

Robert L. Hetzel: Right. And in the case of fixed exchange rates, it equilibrates the relative price levels between countries to give you the real terms of trade. So, I think that what, in terms of thinking about why you were willing to use the quantity theory, it had to be in part because you came through this Viner tradition of thinking that the price system works and you were willing to think about the price level as an equilibrating variable when the rest of the profession thought that was crazy because it was just, the price level was going to be set by large corporations and by government controls and to think of it as a variable that could equilibrate...

Milton Friedman: That also was the classical tradition. It was a tradition that I was taught as a graduate student, Mince taught, in 1932, '33, '34.

Robert L. Hetzel: But what was...

Milton Friedman: And it was a coherent framework because what Viner taught, what Mince taught, what Aaron Director would have taught, fit together and were consistent with one another. They all believed that the price theory worked, the system worked and they all believed that economics was the instrument of analysis and not a separate discipline, a game not a problem of mathematics. See, Paul Douglas was also an important part of that intellectual atmosphere in Chicago. Even though his main field was later, he was reviewed as a labor economist. He was very much of a quantity theorist.

Robert L. Hetzel: So, when you went to France to serve as a consultant for the Marshall Plan, let's talk about how European countries could make their European payments work and begin to trade unilaterally rather than bilaterally. I think you had the Keynes' track of monetary reform as an analytical apparatus and so did everybody else, but other people weren't willing to use it because they weren't willing to make this leap of faith that the price level could vary to equilibrate the balance of payments. And you were willing to say, yeah, that's the way the world works.

Milton Friedman: But I'm not sure...

Robert L. Hetzel: It was the faith in the price system that other people didn't have at the time. It wasn't that the analysis was suddenly just invented, it was there, but there had to be some reason you were willing to use it and make predictions with it when the rest of the world...

Milton Friedman: But I'm not sure that I want to make any statements about what other people thought. Because, certainly by that time, you're talking about 1950, and Keynesianism was riding high, there's no question about that. And the Keynesian view was entirely, was that, as you say, that price level was more or less, in principal, it was related to the quantity among everything else. But in practice, it was largely institutional data. And that the money supply would adapt to it rather than the other way. Certainly this was a way people like Caldor would have approached it in thought. So, I don't think, but, why they took that position, you know, the psychology of it, I'm not going to get into that or get involved with it. I have a hard enough time knowing what I thought and why let alone what anybody else thought and why. And, so, that's your interpretation, not mine.

[00:22:48]

Milton Friedman: But I don't doubt the general economic atmosphere that was produced by Chicago had a great deal to do with it. See, other people who came out of that; take the case of Herb Stein. You've mentioned that rou've gone through his letters. Now, Herb was a product of the later '30s. He was there four or five years, I never knew him in Chicago. But he came out of it, he would also have been a quantity theorist. And I suspect if Herb had gone to Paris, he might very well have come to the same conclusion as I did.

Robert L. Hetzel: Let's jump up to December 1967 and your presidential address and the natural rate hypothesis which you first applied to the interest rate. The interest rate, like the exchange rate, is a nominal variable with a real kernel. And that real kernel is a price that serves as an equilibrating mechanism. Now, with flexible exchange rates, you took

Keynes' analysis in the tract, and you gave it life. And you did the same thing with Irving Fisher's analysis much later. And the question well why didn't somebody else, the formal framework was there, and it's got to be because you were willing to see the real rate of interest, the kernel of the nominal rate of interest as a price that worked as an equilibrating variable. So it had to have a unique value at a time when other people thought it was just a lever for pulling on and for controlling the economy because the price system didn't work, so...

Milton Friedman: I'm not sure that's right at all. No. I don't think so. I used, in the talk itself, and much earlier, I used the [unintelligible - 00:25:11] concept of the natural [inaudible - 00:25:19] as a terminological rather than an analytical device. In that paper, the real rate of interest doesn't measure into that whole discussion. It's a real rate of unemployment and [unintelligible – 00:25:33]. What enters in, is the notion of--what's really more important, is the notion of expectations. And what you're talking about is the difference between market prices and expected, what, expectations, and that expectations are slow to adjust and that's why we get the lag in business. But unless I am mistaken, and you can check that very easily, and unless I'm mistaken, the real rate of interest is not mentioned in that paper. I agree with you that I would have regarded the real rate of interest as serving the equilibrative function, I'm just saying that that paper is not-- I must have a copy of it somewhere around here. Now where will I have a copy of that?

Robert L. Hetzel: But, the real rate of interest has a unique equilibrium value because of the role it serves in the price system and, you know, that's why the Fed can't set the nominal rate of interest arbitrarily. But, at the time, in the ISLM framework, you don't, the real rate of interest just is kind of whatever, you know, the monetary and fiscal authorities [unintelligible - 00:27:01]

Milton Friedman: [unintelligible - 00:27:01].

Robert L. Hetzel: Yeah.

Milton Friedman: Right. And it's clear that I was not assuming the price level to be held constant, but the, the 1967 paper was about the equilibrium, if you will, about the equilibrium price level and the equilibrium wage rate and not about -- the same analysis can carry over to the real interest rate, I'm just saying that as a historical fact, I don't believe, unless I am mistaken...

[00:27:36]

Milton Friedman: Essentially that same thing is in this, in the chapter in my price theory.

Robert L. Hetzel: Right.

Milton Friedman: Yes, wage determination and unemployment, that's what I called the chapter that deals with that, I'm just curious for a moment, where do I discuss the real rate of interest in here?

Robert L. Hetzel: Well, it's at the beginning of it. You know, I should have checked that before I...

Milton Friedman: Thereto, in respect to the real change rate you have the same phenomenon at that time, again it was taken for granted that [unintelligible - 00:28:57] could set nominal exchange rates. And that was part of the [unintelligible - 00:29:02]. If prices are, price level is different, then the nominal exchange rate is the real exchange rate. Then you can set the nominal exchange rate and you can set the real exchange rate.

Robert L. Hetzel: Okay. So then, this won't take much longer, I just have a few more things I want to ask you about. So, you went from the case for flexible exchange rates is your first sort of obviously quantity theoretic analysis.

Milton Friedman: No.

Robert L. Hetzel: We've argued about that before, it's what...

Milton Friedman: I would argue that the paper I wrote, had in the AER on the table of stability...

Robert L. Hetzel: The framework for monetary...

Milton Friedman: The framework for monetary stability is also a quantity theory analysis.

Robert L. Hetzel: It runs more off the real balance of that and there's no discussion price level that, there's no discussion of price level stability, it's in the...

Milton Friedman: No, it's not in here, it's in [unintelligible - 00:30:36] of economics.

Robert L. Hetzel: It's the, yeah.

Milton Friedman: I've got that here, too, but I won't pull it out.

Robert L. Hetzel: Okay.

Milton Friedman: And anyway, it doesn't matter.

Robert L. Hetzel: Sure, sure, no.

Milton Friedman: You know, I don't think it's, I don't personally regard it as very important, whether it was in 19X or 19Y.

Robert L. Hetzel: So, after the case for flexible, after you do the analysis in the case for flexible exchange rates in 1950, then you do the study of inflation in three wars. And you see the money stock as the single variable that can provide an explanation for inflation over those three very different periods and you see the...

Milton Friedman: Have you seen this?

Robert L. Hetzel: No.

Milton Friedman: It's a beautiful little thing. It's again about the Civil War. And as soon as they, the Confederacy kept a spy on money by a third, only because of the Mississippi had been lost, they couldn't control it in the West. So they had the nice comparison between the two Richmonders. The question is...

Robert L. Hetzel: See, and you know, I would like really, you know...

Milton Friedman: The question is whether it's war news or quantity in money.

Robert L. Hetzel: Right. Yeah.

Milton Friedman: And this, your mention of the three wars, I had just thrown that away, I had seen a much earlier version of it and commented on it.

Robert L. Hetzel: Mitchell v. Newcomb.

Milton Friedman: Yeah. Exactly.

Robert L. Hetzel: So, out of that analysis, you made the hypothesis that money demand is stable, and that gave you the way of making empirical predictions that you could then challenge the ISLM framework. That is, ISLM had been used as an expositional device for policy makers, but it had never been used to generate testable implications.

Milton Friedman: [unintelligible - 00:32:51].

Robert Hetzel: So, you were the one that sort of laid down the gauntlet and said, okay, let's take this model seriously, the ISLM, and what are its predictions and you know, against quantity theory predictions. So, you then had a way of, with the case for flexible exchange rates, the empirical predictions were of the sort that if a country tries to maintain an overvalued exchange rate, it's going to have to disinflate and vice versa and of course, over the years, those predictions have done very well indeed. And I think people just, so those are part of how economist think now and without realizing that, you know, they have to sign on to the quantity theory if they're going to commune to that analysis. This gave a different set of implications. When you were thinking about predicting income from autonomous expenditures, you realized that autonomous expenditures were part of incomes so it became natural to think in terms of predicting consumption and when you began to think in terms of predicting consumption, that's what pushed you into thinking about the problems that led to a permanent income hypothesis and that through this...

Milton Friedman: No. No. No.

Robert L. Hetzel: That was not that...

Milton Friedman: With the permanent income hypothesis really, rose out of the work I had done during the '30s and in the national [unintelligible - 00:34:32] commission. The study of consumer purchases.

Robert L. Hetzel: Right, but I had assumed that when you began to think about predicting consumption, then given that problem, then you went back and drew on the earlier work that you had done. That's what renewed this interest in predicting consumption. It was this Freidman-Meiselman way...

Milton Friedman: Friedman-Meiselman, well, yeah, but so far as the consumption book itself, *The Theory of Consumption* goes, I don't think it developed out of that at all. It developed entirely not only out of the area work, but much more. I look back and we were in contact with people who were working on this problem and this was a time when because of Keynes, the consumption function had taken a, had become of great interest. And so you were having all sorts of theories, relevant income hypothesis, theories and so on. And it was contact with Margaret Reed and Dorothy Brady and much more than the monetary, I don't think, I think, in my own recollection, I would say that the monetary work and the theory of consumption function was completely separate from one another. But the Meiselman paper, the paper, I did with Meiselman, that certainly grew out what you're saying. Let's take an empirical set to see what are the implications, and once you write it down, you see that it you were going to correlate the income with investment, you're correlating part of income with itself. So you've got a serious correlation. And then you do have to predict consumption. And then, no doubt at that point, a connection with a firm income hypothesis is staring you in the face. But, I don't think that, in actual research terms, they had any relation to one another.

Robert L. Hetzel: Okay. I thought the one sort of -- I'm trying to go back and think about the other...

Milton Friedman: You're thinking about it retrospectively.

Robert L. Hetzel: Sure.

Milton Friedman: You're making life much more logical and coherent than it really is. What interests me much more in this whole process, is I really have no way, no, I really don't know where the interest in methodology came from. What got me started along that line. Because I'm sure that my, that these methodology [unintelligible - 00:37:15] had more to do, had a great deal to do, with my going after things like the pre-Civil War, pre-war comparison. It was an attempt to get well, certainly that piece is intimately related to the theory of the consumption function. And that the whole structure of the theory of the consumption function was trying to carry out the idea that we might [unintelligible - 00:37:47].

Milton Friedman: To start the hypothesis, see what its implications are, test those implications. And see if they are contradictory.

Robert L. Hetzel: Right. The way I think of it, is that this was a time when the economics profession was trying to decide whether it was going to become neoclassical economics or institutional economics. And to become neoclassical economics was to make it into a science. And there were two things going on at the time. One, was taking the marginal insights of the earlier generation of people like [inaudible - 00:38:47] and Marshall and putting them into a coherent mathematical framework. And one group went off and did that in pure theory. And at the same time, in the 1930s, you had the development of statistical inference testing with Fisher, Sir Ronald Fisher in England. So, in terms of putting, making neoclassical economics into a science, the issue was how to fuse those two branches and some people had a mind for pure theory, like Arrow and Debreu, and some people had a mind for sort of pure measurement like Kuznets and Abromovitz, but you had a mind that on the one hand, you liked the simplification that came through intellectual abstraction. In that sense, you were a pure intellectual. On the other hand, you had a pragmatic sort of American temperament of how does the world work. So...

Milton Friedman: This is really Harold Hotelling.

Robert L. Hetzel: Yes. So you were, you were young at a critical time and you had an unusual temperament to your mind. On the one hand, you liked the simplification that came through intellectual abstractions, but at the same time, your curiosity about the world wouldn't leave you alone. So you were able to put those two things together in a unique...

Milton Friedman: I was very fortunate because I had the Chicago theory and the National Bureau empiracy. I think National Bureau, my experience with the Bureau but I really still don't know what got me started writing along that line. Now, partly, in retrospect, I think you can construct various explanations. It was a dispute at that time about whether you should go around and ask people questions about whether they use marginal economic analysis in running businesses. A hitch stump. And that, generally, had been a discussion with [unintelligible - 00:41:23] involved and various other people on marginal analysis. But I still don't know, as I said.

Robert L. Hetzel: Some of my hypotheses haven't worked out very well. One was that at the University of Wisconsin there was debate over whether the department should look, continue to look the way it had under Commons, or whether it should become a neoclassical department, but that didn't seem to be very, and one was that it was your debates with the Cowles commission people over how to do the statistical inference that was associated with their view that you should describe the world as a set of stochastic difference equations, but your methodological views, I mean, you developed your views before you began to debate.

[00:42:28]

Milton Friedman: That's right.

Robert L. Hetzel: One hypothesis was that it was interaction with people like Karl Popper, but you developed your views before so, that's why I, that's why I have this, this explanation I just gave you which is that there are these two currents in economics that have to be married to create what we now, except as neoclassical economics and it was the temperament of your mind.

Milton Friedman: Well, I think George Stigler and Alan Wallis played a role somehow. And George, you see, and the interesting thing, if you go back to George's textbook, *The Theory of Price,* I think it's the only textbook you'll find that starts out by saying we want to explain a real phenomenon. Unless I'm mistaken, at the very beginning -- this is a, well I have a later edition, but this is, I think, no this is a revised edition but it's not the third edition. Not a really revised edition.

[00:43:59]

[END TAPE 47, SIDE A]
[START TAPE 47, SIDE B]

[00:44:02]

Milton Friedman: ...George was trying to think of it in terms of an engine of analysis and not in terms of an electoral system. Not in terms of general equilibrium structure, but of a supply and demand, [unintelligible - 00:44:20] and I believe he used a concrete example of a market phenomenon out of his [unintelligible - 00:44:25]. Les Martin's [phonetic] phenomenon.

Robert L. Hetzel: So, it's how you keep Marshallian analysis and yet work out the mathematical foundations. How you keep the ability to prove and disprove hypotheses and yet have a formal and coherent...

Milton Friedman: Have a, formal, system...

Robert L. Hetzel: And it's putting those two together and it was in, it was what had to be done, but the sort of striking thing about it is how hard it is to do even today. And how few people, how few people really successfully combine those two [unintelligible - 00:45:27].

Milton Friedman: See, the Marshallian demand curve, my paper was from Marshallian demand curve, which is very early in this, probably '49, is really about the same problem.

Robert L. Hetzel: Sure. How you give substance to the incoming substitution effects in a way that it has some content for how you...

Milton Friedman: But it's consistent with general equilibrium analysis. You make partial equilibrium analysis. Which is what you've got to use for, if you're going to understand real economics. The general equilibrium analysis, which is used by to be consistent with, if you're going to have a coherent framework.

Robert L. Hetzel: So, you're right, it had to be, to some extent, having Alan Wallis and George Stigler to talk your ideas over with, but it had to have something to do with the badgeries of the time and there's going, working for so long as a statistician. Because then, when you came back to doing theory, you were a person trained to do statistical inference.

Milton Friedman: That's right.

Robert L. Hetzel: So, if you hadn't had the, this is to say, the serendipity of having to go to Washington and do consumption studies and then working with Kuznets on the national and covenant product accounts data and doing the statistical inference testing in the wartime studies, you wouldn't have had that apparatus and...

Milton Friedman: No, I think that's after, I think that's after [unintelligible - 00:47:07].

Robert L. Hetzel: I want to ask one more question and then I will, I think I've overstayed my welcome. In terms of understanding what was unique about monetarism, I know you don't like that word, it didn't come until '68, but the way I think of it is the way you gave empirical content to the quantity theory, how you made the quantity theory into a framework that would yield testable implications. You started with certain quantity theoretic abstractions, the price levels of monetary phenomenon and the price level varies to equilibrate into the money market or the balance of payments. But then, what was, but that was in, sort of, that was standard, you know, Keynesian monetary track analysis. But, you gave the quantity theory content by combining it with empirical hypotheses that were tested over a variety of monetary regimes, countries and times. Of course, the stability of the money demand function, but the temporal relationships between a change in money growth and a change in nominal income and a change in inflation. So, that created a, sort of, a, that created a combination of tested empirical hypotheses and theory that you could then use to make additional hypotheses, so it was this common and sort of, in terms of the methodology we just described, it's not a pure theory. People on one side or the other would say it's sort of a mongrel theory. But it worked as an engine of analysis again and again. You were able to talk about monetary theory and make predictions in a way that you wouldn't have been able to do if you hadn't been able to straddle both sides of economics, empirical and theoretical.

[00:49:29]

Milton Friedman: But surely, it's pure what a pure theory is. $S = 1FGT^{2.}$ That's an equation. It's a pure theory. It has obvious predictions. Some of which would be wrong if you try to apply it to a feather floating and then dropping the feather. Now. Is there a pure theory there? Yes. $S=1FGT^{2.}$ But then there are rules for applying it, its definition to which, how you're going to measure time, how you're going to measure distance, so it's a, if you take natural science, don't you have exactly the same combination. You have hypotheses like the hypothesis that a body falling in a vacuum will accelerate at a constant rate. And then you convert it into a predictive formula like $S=1FGT^{2,}$ then you try to measure J and you try to define under what conditions you expect it to work. And it seems to me that the, that what's going on is simply trying to make economics into a science of the same kind as the natural science. And so when you say it's a mongrel, I'm not saying you say it's a mongrel, nothing wrong with being a mongrel, a mongrel, that's a way of developing new species. And again, that's strictly Marshallian. Marshall does the same thing when he discusses long run versus short run.

Robert L. Hetzel: What variable adjusts first is...

Milton Friedman: Empirical matter.

Robert L. Hetzel: ...empirical judgment.

Milton Friedman: Absolutely. Right.

Robert L. Hetzel: I, of course, one thing that leads to sterility and economic analysis is the feeling that everything has to be developed from first principals.

Milton Friedman: I think that's absolute nonsense. I agree. It converts it into a mathematical game, not a, how do you develop $S=1FGT^2$ from first principals?

Robert L. Hetzel: It didn't, it came from watching falling bodies of different masses.

Milton Friedman: That's right.

Robert L. Hetzel: See, now you can see what you can abstract from.

Milton Friedman: [unintelligible - 00:52:55].

Robert L. Hetzel: [unintelligible - 00:52:56].

Milton Friedman: Well you form it and the hypothesis that a body falls, accelerates at a constant rate. And then you can mathemetize that. They, I spoke a little too strongly about [unintelligible - 00:53:14]. It isn't. That is to say, the notion that there are individual choices and decisions that underlie these magnitudes is a useful notion. It's suggesting what variables will be important. But the notion that you should not express a hypothesis about aggregates unless that hypothesis can be rigorously derived...

Robert L. Hetzel: From first principal.

Milton Friedman: ...from first principal, so that I believe is not a useful idea. That says you've got to have a full [unintelligible - 00:53:51] general equilibrium model for every proposition about any part of the economy.

[00:53:59]

Robert L. Hetzel: And that's what makes it so hard to do economics now, because if you want to do economics, you have to develop this abstract mind and then you go look to see if it fits the data.

Milton Friedman: No, no.

Robert L. Hetzel: And it's, the model's complicated. And it's going to fit along on some dimensions, in other dimensions it won't fit along so you're never able to go back and reject the original model.

Milton Friedman: Well, you get the model from the data as well, it seems to me it's a two way process. It's not a one way process. But it is...

Robert L. Hetzel: It is the way that so many people do economics now, they feel like they have to write the complete model out first, it has to be [inaudible - 00:54:46] and then when they've done that, then they go look to see if it fits the real world. And it will fit along some dimensions and other dimensions it won't fit along and they have no way of then going back and rejecting the model, so they, they make it more complicated. You know, they add more epicycles to it to try to get it to fit better.

Milton Friedman: That's right. And that again, is a correct analogy with a physical scientist. They [unintelligible - 00:55:18] or accept your theory did not fit the data any better than the epicyclical theory, but it was a simpler, with an outcome that's razor thin.

Robert L. Hetzel: Yeah, given the precision of astronomical measurements at the time, we couldn't differentiate between the Copernican [unintelligible - 00:55:45]...

Milton Friedman: That's my understanding.

Robert L. Hetzel: ...different theories. They had observational equipment. Just one with a lot simpler, sort of appeal.

Milton Friedman: That's my understanding of it. Okay, you got any more bad questions?

Robert L. Hetzel: Oh, I do, but I'm not going to, I'm not going to bother you.

[00:55:45]

[END OF RECORDING]