«History of Economic Ideas», xviii / 2010 / 3

THE CHICAGO SCHOOL. A CONVERSATION WITH PAUL SAMUELSON

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I held this interview with Paul Samuelson in his office in October 1997. He was gracious enough to spend approximately one hour and twenty minutes talking to me about George Stigler who he had known throughout his adult life. The reason for the interview was simple. As part of a long term research project on George Stigler and his response to critics of neoclassical theory, I spent two months travelling throughout the us, interviewing many of George Stigler's colleagues and close friends. I included within my round of conversations both Paul Samuelson and Robert Solow as two people who enjoyed a long acquaintance and/or friendship with George Stigler, though perhaps not his economic or political standpoint.

This interview with Paul Samuelson differs markedly from many of the other ones I conducted. Those tend to be more directed. In other words, it is not difficult to discern the direction and purpose of the interview by the questions I pose. Interviewing Paul Samuelson however proved to be far easier than any other task I set for myself. Literally, close to an hour went by between my first innocent background question and my next fully articulated one. I readily admit to coming across as a pale imitation of Socrates' slave boy in the Platonic dialogue *Meno*. My lines are reduced to the equivalent of «Yes, O Socrates». I leave only a few of these innocuous interjections in not intentionally to self-characterise myself as clueless, but to provide a couple of natural breaks which make reading such an interview a more comprehensible task. Critical readers might wonder why I didn't do more to take control of the interview. I can only respond that it was a rapid and almost unconscious choice on my part. Given Paul Samuelson's flow of thought and its usefulness for my own purposes, to interrupt would not only display needlessly bad manners but be deliberately counter-productive as well. What follows is a condensed version of the full session.

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The Interview

Let's start with some background information. I know you met in the '30s in Chicago.

I probably met George Stigler some time in the calendar year 1932. But I'm not certain. But it would have been at the same time that I met Milton Friedman and Allen Wallis. Those three I group together. They were the elite guard in the Chicago Graduate School in the 30s. I came to University of Chicago on January 2nd, 1932 and Aaron Director was my first teacher in economics. Aaron later played an important role in George's life. Aaron was the brother of Rose Director who later became Rose Friedman. But I probably saw George the most in my final senior year, which was the academic year 1934-1935. George and Allen Wallis had taken squatter rights possession of a storeroom in the basement of the social science research building where the economics department was. They had their little office there. And they were then, I suppose, second or third year graduate students. Was George born in 1911?

1911.

And did he come from the University of Washington?

Yeah, he had done an MBA at Northwestern and then did a year out in Washington before he came to Chicago.[1]

The reason that I saw them, and a good deal of them, was that to make some money during the Great Depression I had what was called an NYA (National Youth Administration), scholarship supplement. I suppose the Department secured that for me. I don't know how much an hour I received, maybe twenty-five cents an hour, but they had to find some perfunctory work for me to do. And just as in *Pinafore*, I polished up the brass on the door. I was given the job of dusting off in that Department Records Room the pictures of the great economists, Böhm-Bawerk, John Stuart Mill, David Ricardo, Adam Smith, and maybe Knut Wicksell. So I would be in that storage room doing my little 'make work job' and talking to George and Allen who were, of course, exalted graduate students. Well, you must have an impression of what George was like. I thought of the two of them then as being closer together to one another than either was to Milton Friedman.

[1] George Stigler's career prior to coming to Chicago in 1933 consisted of an undergraduate degree in business administration from the University of Washington in 1931. He then received a master's degree from Northwestern after a year of study. Stigler returned to the University of Washington for a year before heading to Chicago.

At that time.

At that time. But it was a mutual admiration society. And George was very amusing. Allen was very self-confident in any view that he had. One of the notable characteristics of George Stigler was his humour. There was an element of cruelty in that humour, and maybe that grew over the years. There are people who are still alive who carry the scars, and will to their grave. George had lunch, day after day, being very witty at their expense. I presume if you've spent some time examining his career you've encountered this. That was his reputation. George was a pretty self-confident person, also. At that stage he was, as many people were at the University of Chicago, quite besotted with Frank Knight. George's thesis topic was Carl Menger,[1] the father of the mathematician Karl Menger with a 'K'. I remember a sentence he said. «Carl Menger is very good, but everything good that is in him is already (I can't say already) in Frank Knight». Frank Knight's influence on the student body was profound and not, I say in retrospect, a hundred per cent positively constructive. Does Knight figure in your studies to any great degree? Knight is a most interesting character. You've seen, I'm sure, George's Palgrave article on Knight, his biographical piece. Knight had a very strong influence on George Stigler and all the graduate students. But Knight was in a kind of manic mood then about capital theory. He must have written some eight or ten articles in the 1930s. They were a mixture. Part was a proper correction of a view that some simple Böhm-Bawerkian period of production could serve as a surrogate for capital. But they were also full of Humpty Dumptyisms, such as the period of production is either zero or infinite by some particular Knightian definition of how it would be measured. I don't know that this aspect of Knight profoundly affected Stigler, yet it did have an effect on Albert Hart, who had trouble getting his thesis passed. In fact, Knight's major influence at that time resulted in the local view that Knight had done everything and there was nothing left to do. So, he was the cause that led to a pretty important generation of Chicago economists never getting their Ph.D. degrees.

I've heard that Allen Wallis never got his degree. Allen Wallis was of course, a bit more mathematical than George. So there is no reason why, whatever it was that Knight did in economics, that this should have precluded Wallis from taking a degree. Aaron Director never got a doctorate as well. But George must have acquired a decent knowledge of

[1] Carl Menger formed a chapter in his dissertation published in 1941. However, his first publication, prior to submitting his dissertation appeared in 1937, «The Economics of Carl Menger», *Journal of Political Economy*, 45, 2, 229-250.

German to be able to write his thesis. I never read his Ph.D. dissertation. I, of course, read his book on the History of Production[1] and there is a chapter devoted to Menger. I don't recall any particular profundity in his interpretation of Menger that I ever heard from George's lips or remember reading in any of his writings. But then I didn't read the dissertation. Now, George was a tall guy. Have you got that wonderful picture of Milton and George from the rear.[2] It's something those in the economics profession would instantly recognise. I think he had some kind of a minor limp. George though played tennis and he became a pretty avid golfer especially, I think, when he was a widower. So it wasn't a debilitating limp. Milton Friedman played tennis, and George and he must have been a sight on the tennis court, if they were doubles partners. Milton played to some degree up to a late stage. You know he has had a heart bypass. Two, at intervals of more than a decade I think. And I think that tennis and golf became a fairly important part of George's life maybe from the age of 50 onwards. Can you remind me what George's age was when his wife died?

Well, she died about '71, '72.

As late as that?

She died just as that book he did with Kindahl came out on 'Prices'.[3] That was about '71. He would have been about sixty.

Yes, he would have been. Well, that's interesting. I was always surprised, no reason why I should have been, that George never remarried. ...Are you talking to Solow?

After you.

Good. Bob Solow and George Stigler, were an odd couple. They became very close friends, fine friends after they both spent a year at the Stanford Behavioural Centre. I'm a little shaky on that but I think it was about 1958. That same year Milton Friedman was there. And Karl Popper actually was there that year but only came in at midnight, when no smokers were allowed. But it was a pretty stellar year and that's when they met. And, of course, they both were very witty. They had that in

[1] George Stigler's dissertation became the basis for his first published book in 1941, *Production and Distribution Theories*, New York, Macmillan.

[2] This picture is reproduced in George Stigler's autobiography, *Memoirs of an Unregulated Economist*, New York, Basic Books, 1988. The picture is well worth a look being reminiscent of Don Quixote and Sancho Panza.

[3] The correct date is 1970. The precise title (written with James Kindahl) is *The Behavior of Industrial Prices*, National Bureau of Economic Research, New York, Columbia University Press.

common. But the reason why it was such a surprising friendship was that George was known to be a very conservative fellow and many of the things about Bob Solow were not at all. ...Well now. I think there were some changes in George's thought over the years. Did George go first to the University of Minnesota? Was he ever at Iowa State?[1]

That was his first job, for about a year.

He went to Iowa State?

Under Theodore Schultz who was there.

Yes, Theodore Schultz was building up a powerful department at Ames.

Yes, that's right.

I had never heard of Theodore Schultz at that time when he was beginning to build up his department. And there were some other very good people who went to Iowa State like Gerhart Tintner. He was not George's type particularly. He stayed there a long time. Oz Brownlee, I don't remember whether he had been at Chicago or not as a graduate in any sense.[2] But he was the one who broke up the Iowa Department because he wrote a pamphlet in the early war years entitled something like, *Margarine Not Butter*,[3] the margarine or butter pamphlet. He argued that in a time of scarcity, such as the war, the fat from the ox was a better social bargain than the fat from the cow. And then the fat was in the fire. The farmers really were hopping mad. They said, «We don't want to pay to have a Harvard down here in Iowa». And, what may have been momentarily, the fourth or fifth best economics department in the country simply disintegrated. I don't know whether George went to Minnesota before that happened.

I think that he might have. He was only at Iowa for maybe about a year.

About that. There were damn few jobs in those years. And certainly damn few jobs at the University of Chicago. The Great Depression did

[1] George Stigler started his professional career at Iowa State College in 1936. According to his own recollections (1988, 38) «It was one of only two available academic posts (the other was Ohio State) known to my professors at Chicago in that year, and it would not have been available if Homer Jones had not turned it down». Stigler then moved on to the University of Minnesota in 1938 and remained there until 1946. However during the war years he was on leave, first at the National Bureau of Economic Research and then at the Statistical Research Group at Columbia University.

[2] Brownlee worked as a research associate at Iowa State, coming there in 1939 after completing a Master's degree from the University of Wisconsin. He received a Ph.D. from Iowa State in 1945. Most of his career was spent at the University of Minnesota (1950-1985). Before Minnesota he had taught at Carnegie Institute of Technology and the University of Chicago.

[3] O. H. Brownlee, «Putting Dairying on a War Footing», 1943; original [retracted] version of Pamphlet no. 5 *Wartime Farm and Food Policy Series*, Ames (10), Iowa State College Press.

hit Hutchins' Chicago pretty hard in the investments it had made.[1] And the number of full professors suffered attrition. So there were very few appointments. Lloyd Mints, he was my teacher in 1932, was probably already 45 in 1932. I haven't checked the date. He lived to be over 100.[2] But he was only an assistant professor. Henry Simons might have been an Associate Professor, but I don't think there were very many promotions. So, there is no need to explain why none of the people I am talking about were asked to stay on at University. But I'll give you an example of the characters of these people. I may say that Allen Wallis and George were very good to me. They formed an opinion that I was a person of some ability and they spent a lot of time talking to me. But I'll tell you a Stigler story. This concerns Allen Wallis too. Neither one of them liked nor thought very well of Henry Schultz. They thought that he was not a bright mind, not a profound scholar and something of a bluffer in mathematical economics. I think they were too hard on Henry Schultz. Henry Schultz was a very serious, very hard working guy who had been a student of H. L. Moore at Columbia, really his prize student. H. L. Moore was a strange man, who had some psychiatric problems later in his life. Henry Schultz got a call to the University of Chicago in late 1920s. Chicago was not free of anti-Semitism, but it was relatively free in those days. And as a result, I think that explains in part, its greater pre-eminence in those years, the early Hutchins years; the years just before Hutchins and up until, say, the end of the 1930s. It kind of had a monopoly on talent because Harvard, Yale and Princeton and a lot of the prevailing academic life was discriminatory against the Jews. But in any case this story about George and Allen I probably had from Allen but it might be confirmed by Jacob Mosak. Does that name mean anything to you?

Yes.

Jacob Mosak didn't have a lot to do I think with George, or Allen, because he became an assistant to and a disciple of Henry Schultz.[3] Milton Friedman, actually must have worked a brief time for Henry Schultz. Because if you look in Schultz's *Theory of the Measurement of*

[1] Robert Hutchins was Chancellor of the University of Chicago, 1946-1951. Prior to that he served as President of the University of Chicago from 1929-1946.

[2] Lloyd W. Mints (1888-1989) was a key figure of the University of Chicago Economics Department in the 1930s and 1940s. He was known for his contribution to monetary theory, particularly his reformulation of the quantity theory of money. He came to Chicago in 1928 and remained there until his retirement in 1953. At that time he turned his back on economics, starting a second career as a cabinet maker while living with his sister outside of Ft. Collins, Colorado.

[3] Jacob Mosak also was on the Cowles Commission research staff in the early forties and was associated with the Walrasians then at Chicago rather than the self-proclaimed Marshallians.

Demand in 1938, published just scarcely a year before he died with all his family in an auto accident, there is an acknowledgment to Milton and a reproduced Friedman section.

But anyway, Wallis and Stigler took Schultz's course, which was a very serious assignment because he assigned a lot of work and you had to sign up for a double quarterly credit if you took the course at all. And under the quarter system each subject had a lot of hours attached. You spent a lot of hours on a subject for a third of the year, instead of the way things are divided under the more common two-semester system. Well, because they were contemptuous of Schultz, and kind of mean, they played mean games with him. And in one case they went to Henry Schultz and said, «Professor, we have an argument between us which we can't settle. We know the formula for the area of a unit square, and we know the formula for the area of a unit cube. But we can't agree on what the general formula is for a four dimensional regular solid. Would you please decide between us?». They knew that Henry Schultz thought that Allen Wallis was a better mathematician than George Stigler, so they gave George Stigler the correct answer and they gave Allen Wallis the incorrect answer. And, as it was basically described to me, Henry Schultz was proven to be a four-flusher. I don't know if that word means anything to you. It means a bluffer. As they saw it, the insecure professor fell into their trap and came out in favour of the Wallis formula. Well, that's a story on three people and not on Schultz alone.[1]

George actually had, although he wasn't very deeply trained in mathematics, obviously he had an original mind. For example, I can think of two things that he did, that a person who was illiterate in mathematics would not have been expected to do. His «Least-Cost Diet» article, I should think came out about 1939, or some date like that, it came out in the *Journal of Agricultural Economics*, done when he was at Minnesota.[2] It is a clear formulation of a particular version of a linear programming problem. George did not know there was a prior history of this kind of problem and he did not work out the Dantzig simplex method for solving it. But he was able to get lower bounds and upper bounds. He understood that the cost could be brought down to this level and that it would involve only a certain selection of the foods on the menu. He correctly understood that the data of the problem would be what the nutrient requirements are. This would include calories, vitamin A, vitamin B, all of which were prescribed by the National Research Council for

[1] Compare George Stigler's own recollection of this incident in his autobiography *Memoirs of an Unregulated Economist*, 25-26.

[2] The article came out in the May edition of the *Journal of Farm Economics*. The title of the article is «The Cost of Subsistence»: G. J. Stigler, «The Cost of Subsistence», *Journal of Farm Economics*, 27, 1, 1945, 303-314.

Good Health. The prices of a large number of goods, and the nutrient loadings for each one of these goods were based on what their calorie content was. What their vitamin A, vitamin B, vitamin C, vitamin D and so forth were. It's an early example of linear programming and got a good deal of attention. I think when he specified that one should eat just beans and I can't remember what else…

Cabbages I think

Something like that, anyway, I think his article ended with something like 'but don't invite me to dinner'. Now that was one of the things he did. The other mathematical piece is of purely intellectual interest. Suppose you could go into any geographic almanac and you made an array of all the rivers shown by length. Out of the first ten digits: zero, one, two, three, four, five, six, seven, eight, nine, you will find a preponderance of zero, one, two, three, four digits over the five, six, seven, eight, nine digits in the lengths. That's a brute fact. The same is true in a table of logarithms. Let's say it's a seven-place table of logarithms that has these same digits. You can work out how often zeros occur, one and so forth. This is exemplified by the fact that the earliest pages in a book of logarithm tables are the dirtiest. And I think that this brute fact has been commented on in the literature. But George got interested in this when he was at the Applied Math Panel of which Allen Wallis was the head. It had been established under Mina Rees who in the war effort was the Chief of mathematical projects. Now George must have a published or unpublished *memorandum* on this subject. There are many different publications on it. This is just the kind of thing his son, Steve,[1] would know. I think he would know it backwards and forwards.

Not because, so much, that his father was involved, but it's the kind of thing that would much interest him. I know Wendell Furry, the physicist, and later one of the victims in the McCarthy era, from Harvard has written on the subject. I don't remember if I ever knew the degree to which its result is a purely logical one. It has nothing to do with anything factual about a river, but has something to do with a way of describing numbers in a decimal system, or in a system of zero and ones, a dyadic system. I suspect that, whatever it is, whether it has something to do with an empirical fact, or whether it is a pure artefact of logic, it would re-occur in all number systems. If the Mayans used twelves, or sixes, that wouldn't matter. Now, that's the kind of thing you would do in wartime, when you are working very hard and you just want a diversion. Of course that was somewhat strange place for George to be,

[1] His son, Steven Stigler, is Professor of Statistics at the University of Chicago and has also written on the History of Statistical Thought.

I would think, but I think it was purely that he had no interest in carrying a gun and exposing his chest to random bullets. It isn't always what you know, but whom you know. And he certainly had literary skills.

And they were producing a lot of publications. If you needed to know more about that, I would think that if Milton Friedman's memory is good, he would know exactly what George was doing there. Milton was a more creative mathematical type in sequential analysis. I knew a lot about that Applied Math Panel because my colleague, Harold Freeman, who just died days ago at the age of eighty-eight, was there. He was quite a character in his own right. He would regale me with stories about that very group. His stories involved, on the one hand, Allen Wallis, George and Milton Friedman. The order in which I put them means nothing. Actually George would have been the least important. And on the other hand, the great statistician Abraham Wald and his pal and friend, I think it was Jacob Wolfowitz.[1] And Wolfowitz in particular was incensed against the Friedman/Wallis group because he thought they were stealing Wald's stuff. And Allen Wallis, because this is the kind of a guy he is, got everybody very conscious about priorities. So people were keeping notes, «I was sitting on the toilet and it was ten-o-three when I happened to look at my watch, which is when I got this idea. This was earlier than somebody else who was sitting in a bathtub and said 'Eureka!'» That kind of thing is like AIDS or herpes. It spreads. You just need one rotten apple and then everybody is doing it to a characteristic degree. Now I was saying that George was globally very witty and that there was an element of cruelty in it.

Allen Wallis wasn't that witty although he could be whimsical. But there was an element in him of ruthlessness. I remember a story, I don't remember the particular character, it may have been a guy named Schwartz, who was involved with one of the books which the Applied Math Panel put out. On it was a list of names, it's not important I get this right, let's say Wallis, Friedman, my colleague Harold Freeman and maybe a fourth. But Schwartz's name wasn't on it. So he went to Wallis and complained. He said «I want you to know that I think I did the work and I deserve it. And it's important for my career that I receive proper credit for this». Historians of the sociology of science, like Robert Merton, would understand this. It isn't just dollars that people work for directly. And Allen Wallis wrote this sort of letter back to him. He said «I take your point. I understand your point of view. But I've decided that your name will not be on the book. And I have to tell you the reason for it. Because your name doesn't deserve to be on the book».

[1] His son, Paul Wolfowitz would become one of the more influential neoconservative voices in the George W. Bush administration.

End of letter. That would be quite difficult to deal with. I recall Al Bowker – has his name figured at all in what you're doing? If you were writing a biography of Allen Wallis, it would. Bowker was an undergraduate student here at MIT. He had a lot to do in his life with scientific and academic administration and also with Allen Wallis. I once said to him, «Will Allen Wallis ever get to be President of Stanford University?». And he said «I don't think so. But it's not because he wouldn't like to be. And it's not because he necessarily makes the wrong decisions. But he insists upon giving you the reasons for his decisions and they chill your blood».

Well, this was just what that congenial group was like. But let's step forward. George's early work was primarily in the field of history of thought, of course, and in industrial organisation. I think in the course of his life he had a change in his viewpoint. And I believe this is evident in that short breezy autobiography that he was persuaded to write by the Sloan Foundation Committee on Scientific Autobiography.[1] He was reluctant of course to do it, but he then did it in a remarkably short time once he made up his mind. The key character in this change was probably Aaron Director. Which is surprising as Aaron Director was a scratch tenure appointment at the University of Chicago. He published almost nothing and never took his Ph.D. degree, but he was and I guess I should say «is» because is still alive and in his nineties.[2] Are you seeing him?

I actually already have had an interview with him, and with Milton Friedman and Rose Friedman as well.

Right, well those are the most important interviews you could have, especially if you can add Allen Wallis.[3] But, Aaron Director was extremely conservative. Why, I don't know. By the time I knew him he was already like that. And he was an iconoclast. But he didn't develop new data with respect to industrial organisation. He didn't develop and articulate new theories. He just said that the conventional belief wasn't so.

Now, the typical thing would be that George, I think in his early stage, believed it was all important to study the technology in the steel industry. And if it were the case that the market was large enough, you could

[1] The book is a well written, enjoyable read of an academic life. The author reveals very little of his private life.

[2] Aaron Director born 1901, died on September 13, 2004 at the age of 102. At the time of the interview he would have been 95. His sister Rose married Milton Friedman, both having been graduate students at Chicago in the thirties with Aaron then part of the staff as an instructor.

[3] I unfortunately failed to do so. Wallis died in 1998, the year following this interview.

replicate, in each market three, four, five, six, seven or eight equally shaped, u-shaped cost curves at about the same time. You get lumpiness, but that lumpiness when you have eight firms diminishes the monopoly power of any in such a way that you could have effective competition. Not perfect competition, but effective competition. And the government wouldn't have to necessarily do anything about it. But in the beginning, he was of the belief, that in some of these industries, there were unexhausted economies of scale. I think he must have written an article at one time, maybe in the JPE saying that, almost in the title.[1] Smith was right that unexhausted economies of scale will render competition imperfect.

By the last part of his life, whether in the last half or the last third, it was my impression that George was of the opinion that *laissez-faire* itself pretty much approximated to tolerably effective competition. And I think Aaron Director was the prime source of this view. George also gave signs, I don't know whether it's in his biography of Frank Knight in *Palgrave*,[2] of real disaffection with Frank Knight. The besottedness faded away. And I think that may have also been Aaron Director, although Aaron Director and Frank Knight were close and intimate. It was only Frank Knight who got Aaron Director his professorship. Of course, Aaron Director became prominent as a university teacher, and really had an influence, a profound influence, upon American IO policy, when he became a lecturer at the University of Chicago Law School. I think he was just replacing Henry Simons.[3] Simons had been doing that and had committed suicide in one of the early post-World War II years. Frank Knight was conservative. His prime characteristic though was that he was a flaming atheist and he just couldn't leave the subject alone. He was an iconoclast, but he was also very critical of simple conservatism. His views were complicated. I always think of the first Chicago School as Knight and Viner, and to a degree Schultz. You'd also have to include Douglas – although they all ganged up on Douglas, they were all very critical of him – and of course Henry Simons. There was also Harry Gideonse, probably a guy whose name you don't know, who was brought in to organise and teach a big undergraduate survey course in the social sciences. And that first Chicago group was conservative by standards of the time and less so by standards of today's time. But still, as a group, they were on the conservative side. It was no way as con-

[1] This reference would seem to be to his «The Economies of Scale» article published October 1958 in *The Journal of Law and Economics*: G. J. STIGLER, «The Economies of Scale», *The Journal of Law and Economics*, 1, 1, 1958, 54-71.

[2] The reference is to *The New Palgrave: A Dictionary of Economics*, ed. by J. Eatwell, M. Milgate and P. Newman, 4 vols, London, Macmillan, 1987.

[3] This occurred in 1946.

servative as the second Chicago School of Director, Friedman, if you had to skip all names but one; I think Friedman's name should be the one you should keep, and Gary Becker. I'm sure I'm leaving out some people.

But Stigler and Friedman jumped on to Ronald Coase and felt that the Coase doctrines about transaction costs and property rights – just get the property rights right then *laissez-faire* could be relied upon – was the lifeline that they sought. Now, all that I know about this part of the story is what's called the Coase Theorem. And that's a coinage of Stigler's. I don't think Coase knew what his theorem was. There's great argumentation as to whether there is a theorem. And in George's writings on that, but also in his little autobiography, he discusses, in detail, the evening dinner at which Coase started out with everyone as his opponent but then all of them got converted.[1]

Now, George was very learned, he was hard-working. ...I'm now going into his History of Thought work. He was perhaps not as learned as Jacob Viner or Edwin Cannan or Piero Sraffa, as far as the documents, but he had a lot of depth and breadth. George wrote short and dogmatic articles. I heard it said that that little book of the five lectures at the LSE, that he went there and I don't know whether his first lecture was half an hour or something like that and then he left. He never answered criticism. I never understood that. That's just alien to my nature. If George made a mistake, and I believe he did when he specified what Wicksell thought and what Nicky Kaldor thought. He thought that Ricardo, in his last edition chapter on machinery, where Ricardo recanted his earlier view, and came to believe that machinery could hurt the demand for labour, that Ricardo simply made a mistake. Ricardo must have had a hardening of the arteries because Ricardo seemed to these people to be saying that what happens under competition is not *Pareto optimal*. (*Pareto optimality* is not an expression that would have meant anything to Ricardo.) I think that's a misreading of Ricardo. If Ricardo had been saying that, given the conditions of the problem, then he would have been making a mistake. But all Ricardo was saying was that there could be a change in technology. Technology could shift the incidence of distribution between labour and other produced inputs and that could destroy the total amount of national income. Because Ricardo believed that when the wage fell, in the short run, population would decline.

Now it doesn't mean that the output with that population – whatever it is, before, after and during, isn't being produced at a *Pareto Optimal* level of efficiency. There's just less inputs available. This describes labour as an endogenous variable in the classical Ricardian system.

[1] Aaron Director hosted that dinner at his house.

Well, I wrote a couple of articles pointing it out and reiterating that there really wasn't anything, in my opinion, in Ricardo's language which relied upon sticky wage rates or on involuntary unemployment. What he was describing was perfectly consistent with what would happen in a classical scenario of his kind. And *Pareto Optimality* would be maintained at all times. I published this in two places. I don't remember what the order was. There was some difference in the time frame. But one was the Journal of Scandinavian Economics that Wicksell had once edited, and the other was the JPE.[1] Now the JPE might even have been the first publication. George Stigler *de facto* was an editor of the JPE at that time. I don't know whether in that year he was a *de jure* editor because that was a shifting thing. And he accepted the article. The article was accepted and he would surely have been asked since I directly referred it to him as an editor. I never got a note from George saying «Well, this time around I've got to admit I was wrong and your reading was right». Not at any time. And a lot of people would tell me that if they wrote to him complaining about something, he would answer something like «Well, if you're the kind of person who believes that, then you're just the kind of person who believes that».

Or to take a different case, and probably a case for which Stigler will remain well known, to the degree that anybody remains well known over time. When he reviewed, I think it was Sraffa's edition of Ricardo, about the time that it came out – it may not have been in this same issue as the review – he also had an article with the title, «The Ninety Three Percent Labour Theory of Value of Ricardo».[2] And his argument was very simple. Certainly an argument like that can be found in Ricardo. I don't strictly believe that every industry has the same relative mix of interest cost, time cost, and labour cost. But, suppose you take a realistic example, and that's what Ricardo's typical examples – numerical examples – are. And you let the profit rate go through a great variation. Now calculate how much that affects the relative prices of costs for corn, let's say it's corn. It'll be only a five percent change, a three percent change or, to be liberal, a seven per cent change. So, it's ninety-three percent accurate.

Well, first, it depends on which numerical example you use. And particularly if you use those which Ricardo didn't much use. It is really more congenial to the Marxian literature and von Bortkiewicz to go into Sraffa-like input/output. Let the profit rate become very near the

[1] The two articles are: P. A. SAMUELSON, «Mathematical Vindication of Ricardo on Machinery», *Journal of Political Economy*, 96, 2, 1988, 274-282; IDEM, «Ricardo was Right», *Scandinavian Journal of Economics*, 91, 1, 1989, 47-63.

[2] The exact reference is, G. J. STIGLER, «Ricardo and the 93% Labor Theory of Value», *The American Economic Review*, 48, 3, 1958, 357-367.

maximum it can become in such a market. You can have extreme sensitivity. It can change the price ratio by – and indeed a good example to use – as much as ninety-nine per cent. That's a far cry from George's 3%. I wrote about this and some Australians wrote about this, but I never saw any commentary from Stigler. It was almost a Schumpeter-like attitude. Schumpeter in 1911 wrote that the interest rate will become zero in a stationary state.[1] After there had been no exogenous development and no technological change, the violin strings plucked by innovation will dampen down. And when it dampens down, until the next perturbation, the interest rate will dampen down to zero. Well this particular view is based on a number of what I believe to be intuitions in Schumpeter's mind, but he never really thought through the problem. Frank Knight made similar mistakes, but on the opposite side. For him, the interest rate can't ever go to zero. Schumpeter never lectured on this subject voluntarily, in all the Harvard days I knew him. But Paul Sweezy once persuaded him, and got him to do so.[2] But I think that he believed you go down in history books for what your ideas are. You don't admit a mistake, 'let's go down with all flags flying'. And there was something of that, I think, in George.

People in our profession have always been kind of scared of Milton Friedman as a polemicist. So, he gets away with a certain amount of murder. And when he's safely dead and when they've salted his grave against any revival, the daggers will come out. I'm flogging the point. That doesn't change his overall status. The same is true in some degree of George as a witty polemicist. People tended to be scared of him including those who deal with History of Economics doctrine. George had lots of dicta that can be quoted. I don't know that I'm getting any of these things just right, but one is that you should judge a person by his central message. What his impact was at the time and not what you can go back and read into it, but which nobody in those times would have done. Well, the answer is 'yes' you should. And yes, you shouldn't. You should be pointing out this – I'm telling you what some of the other dicta are that usually sweep the field – but really the jury has to be permanently out on that. It's the kind of sweeping assertion whose

[1] The reference is to SCHUMPETER's, *Theorie der wirtschaftlichen Entwicklung*, Leipzig, Duncker & Humblot, 1911 (Eng. transl. *The Theory of Economic Development: An inquiry into profits, capital, credit, interest and the business cycle*, London and New York, Oxford University Press, 1934).

[2] Surprisingly enough, Paul Sweezy was Schumpeter's graduate assistant at this time.

I was there [Harvard] for eight years, I think from '34 until I went into the army in 1942. That was the period when Schumpeter was at his peak. He was a magnet for people from all over the world. They wanted to study with, or have an opportunity to study with, him at a higher level. For a year, or maybe for two years, I forget now, I was working with Schumpeter. I was his graduate assistant (Conversation with Paul Sweezy, November 1997).

negation also has something to it. I'm not talking about the truth content, because you can't have a proposition which is both true and untrue, but I'm talking about what is a truthful approach. Now let's see. Another strong characteristic is the unity of this group who properly felt that they were lone voices crying in the wilderness. And that most of the profession was against them.

They defended each other. Now, Aaron Director, for example, would never have written a good letter of recommendation for somebody who wasn't a staunch conservative but neither would Milton. And I remember for years after I left the University of Chicago, when they were contemplating influential appointments they would ask me about the person, «Is he really sound?». In fact, Milton once showed his naïveté to me, but it wasn't about appointments. He said, «Tell me the truth, is Galbraith a Commie?». You know the amount of naïveté that's in that. I've done a lot of thinking about my old... I can't say my old religion, although I was trained by the Jesuits, so I know it. But I once did a little informal investigation of whether people who were economic libertarians and tended to favour low taxation and low regulation and *laissez-faire,* were also people interested in civil liberties and freedom of expression, and that sort of thing. So I would ask innocent questions. «Now what do you think of this group?». Of course, I had a placebo question control group. «What do you think of the fact that this professor at the University of New Hampshire, the one who invited Paul Sweezy in the McCarthy era to give a lecture, is losing his job because neither he nor Paul Sweezy will testify as to what was the content of the lecture?» And Milton said, «Gee, it's a simple case. It's a free speech society. If a man will not do what he should, this professor should be fired. Society has a right to know». I said «You don't understand. They've got the notes on the lecture, *verbatim*. It's not a question of information».

I mentioned the name of Wendell Furry earlier on, when we spoke about this problem of digits. Wendell Furry, who was a son of a minister, had been a member of the Communist Party. That was not a crime at that time. I don't know whether it ever became a crime to be a member of the Communist Party. Under legislation, it became a reason not to be admitted to the country, things like that. And many people like him in universities and in this community were called before the House Un-American Activities Committee and required to confess that they had been a member of a communist cell, required to name names of those who were in the cell with them. And Wendell Furry was no exception. But he said «I freely admit I was a member of the Communist Party. I was a member from this date to that date, but I will not name any names». Well his job at Harvard was in peril, he was in contempt of

Congress, and all the rest. The Harvard lawyers said to him «But don't you understand, they know those names already?». And he said «Of course I know that, but this is something that a person of character doesn't do». And Wendell Furry was actually worthless at anything but teaching advanced physics and writing about it. He couldn't even run a shoe store or anything. But with him it was just never a question. And as it happened, the virus ran its course so he actually died a member of the Harvard faculty. The case against him, the Federal case, was dropped after a mistrial. It was never re-started.[1]

Well, the point of my story was, now, what would Milton Friedman think about such a case. I didn't actually ask Aaron Director this or receive an answer. I don't remember doing that. We can't simply use this story as evidence, but this group, they really had no interest in such things. I was only able to develop one case, and that was Fritz Machlup who came to this country as an immigrant from Austria. He was part of the very conservative Austrian tradition, but he became prominent in the American Association of Professors for Academic Freedom, and so forth. And when I said that to one of his students he replied «You don't understand, it's just because Fritz liked professors». I said «I don't care. You've got to put him down for where he stood on this issue. He actually had a thing for market freedoms and also had a thing for freedom of expression». But I was sorry I wasn't able to get more, to ask more people and to arrive at a happier finding.

Now let's see. I think I've pretty much shot my bolt. I don't know a lot about George and the work he did on the importance of information in economics. I think this would be one of the things that his supporters would write down. And I don't know but whether there's a direct and strong link, in that later position of his, with his early study on Carl Menger.

But there is in fact an element of that in the earlier Austrian writing, all the way through actually up to Hayek. Now, I don't remember any particular adulation by George of Hayek. George, I think, was a member of the Mt. Pelerin Society, wasn't he?

Yeah, he was ...

And Milton, and so was Hayek who also went to the meetings, and von Mises. I never heard George make jokes about Ludwig von Mises' extremism, although there were a lot of jokes made at the time, but George was generally on the rightish side of most issues. Now George wasn't especially lucky in his academic career. He went from Minneso-

[1] Wendell H. Furry (1907-1984) was defended by newly appointed Harvard President Nathan Pusey who in 1954 refused to cave in to McCarthy's demands to fire him.

ta to the war effort I guess. Then, did he go to Brown for a very short period?

One year.

And then he went to Columbia?

Yeah, because he had that problem with the Chicago job. He claimed that Stigler was far too empirical. And they gave the job to Milton Friedman instead.

I see. I didn't know that. That would have been around 1947.[1]

Yes, '47, '48.

But what is a little bit surprising in the first place, Milton only got an offer of an associate professorship. And he accepted it. Which I think was too little and rather late. Now Milton had certain troubles, because of two things. Anti-Semitism, but also people were afraid of him, his corrosiveness and so forth. Gottfried Haberler wanted Milton Friedman to be appointed to Harvard and somebody like Ed Chamberlin, who was a very conservative person was the department member most violently opposed, because the Chicago School hated both the theories of Imperfect and of Monopolistic Competition.

Did you ever figure out exactly what was behind that?

It started with Knight. Knight was actually a teacher of Ed Chamberlin at the State University of Iowa. That's not at Ames. That's at Iowa City. And that isn't what its name is today. Its name to-day is the University of Iowa. Knight always said, all that's good in Chamberlin he got from me and there isn't anything good in him. You know, something like that. And, there's no reason why this should have been of any importance, but it really riled Knight that Chamberlin was a Catholic convert. «The man believes in the Immaculate Conception. What can you do with him?». Knight would say. So from the start, of course, they didn't like the notion that if you were analysing imperfect competition, you were analysing cases of market failure. They always played this down. Now, the early Stigler wasn't as strong on this as he was later on. But Fried-

[1] I am also not quite accurate here. The actual year was 1946. George Stigler took up a position at Brown instead. As Stigler relates the incident in his *Memoirs of an Unregulated Economist*:

In the spring of 1946 I received the offer of a professorship from the University of Chicago, and of course was delighted at the prospect. The offer was contingent upon approval by the central administration after a personal interview. I went to Chicago, met with the President Ernest Colwell, because Chancellor Robert Hutchins was ill that day, and I was vetoed! I was too empirical, Colwell said, and no doubt that day I was. So the professorship was offered to Milton Friedman, and President Colwell and I had launched the new Chicago School. We both deserve credit for that appointment, although for a long time I was not inclined to share it with Colwell (STIGLER 1988, 40).

man was from early on. And I think that part of the reason for it, was this development of his, was it 1953, his version of positivism?[1]

Yes

It's partly a licence for self-indulgence. You don't have to have a correspondence between a theory and the facts, or a close correspondence. In fact, the theory is all the better if it doesn't fit the facts, closely. And I think that there are some profound errors in that form of positivism, but it is there for a purpose. Do you think the cigarette industry with only four big producers in it is not competitive? Well, if one raises its price, another one will and so forth. That's the same paradigm of comparative statics that would happen under competition. So under the doctrine of 'as if', we can use the competitive theory. And as I said, the early Stigler didn't quite believe that, but the late, greater Stigler sort of believed that the facts had changed or had only now been properly interpreted. You could see this in the role that information played for Stigler. It also extenuates what had seemed like market failure because it is all very well to have one price but that's under the naive assumption that you could have ideal information.

And, actually they're working out their own version of a theory of imperfect competition. It just isn't the Joan Robinson or the Ed Chamberlin version. But, just to go back to this 'civil liberties' versus 'economic freedom' discussion and this is tangential. When I first came to Harvard in 1935, the University got a grant from Thomas Lamont of the First National Bank, now the Morgan Guarantee Bank, to establish an Institute Professor. I don't know whether it was the first University Professorship at Harvard. I think it wasn't. I think the Dean of the Harvard Law School, Roscoe Pound, might have gotten the first one. But it was at least the second one. And they had a wide choice of applicants. It was a very cushy job, no teaching duties, a lot of surplus hours, and a salary for life essentially. Well, they scoured the world. They didn't give it to Schumpeter. They gave it to Sumner Slichter. You ever heard of him?

Yes. Labour.

A leader in labour economics, but he was also a pretty good rough and ready macro forecaster. He was probably the most popular and the highest paid lecturer to the business community, but a complete loner. He never showed any manuscript of his to a colleague. He split his time between the Business School and the Economics Department. And he had a little bit of the institutionalist colouring of the University of Wisconsin because he came from the University of Wisconsin where his fa-

[1] M. Friedman, *Essays in Positive Economics*, Chicago, University of Chicago Press, 1953.

ther had been a famous dean of mathematics and engineering. Now, Aaron Director came to visit me for a weekend at Harvard and he said «Why didn't you give it to Frank Knight? He would have liked to have had the job». And I said «Well, would he have accepted it?». Because I had heard the story that when Allyn Young went to England and they had to replace him, Harvard extended a call to Frank Knight who had earlier been Allyn Young's thesis student at Cornell University. And in fact, it was always rumoured that what was good in *Risk Under Uncertainty and Profit* came from Allyn Young who never published much. The rumour about Chamberlin's economics was the same, the *Theory of Monopolistic Competition*. I think not true in either case, in my observation. And he said «Well, in 1927 Frank Knight refused the call and the reason was that he didn't approve of President A. L. Lowell's treatment of the Sacco and Vanzetti case, because in that case... I don't know if you know that case. Lowell, the head honcho in the review committee, he said that there had been no miscarriage of justice and it was a *cause celebre* all over the world. That's the good part of the story. But then, according to Aaron Director, who shared a cabin in the sand dunes outside of Chicago with Knight, Knight now (1935) said, «What a fool I was». I found that sad.

Knight gave a famous lecture in 1932, I think it was, that the world was coming to an end, and that there was only a choice between fascism and communism.[1] He said that, «As for me, I would choose communism». He later tried to get all the copies back. So, Knight who had been divorced about the time of the Great Depression had money worries. I don't know whether they were out of proportion to his actual alimony but he had worrying problems. But like Irving Fisher who also had financial reverses, they not only affected in Fisher's case his personality but these matters actually affected his policy formulations and recommendations. I think I've shot my bolt. But sure, ask me any questions.

Oh, sure. George Stigler seems to have had, all through his life, a certain concentration on income distribution issues.

You mean imputation.

[1] The lecture has been published as F. Knight, «The Case for Communism: From the Standpoint of an Ex-liberal», in W. J. Samuels (ed.), *Research in the History of Economic Thought and Methodology*, Greenwich (CT), 1991 (*Archival supplement*, 2), 57-58. The lecture was privately published by Knight in 1933, along with two other speeches, in an edited volume: F. Knight, *The Dilemma of Liberalism*, Ann Arbor (MI), 1933. The 1932 lecture in which Knight urged his audience to vote communist in the coming election was given under the auspices of the Communist Club and the National Student League at the University of Chicago.

Right. And at least in the later years, say in the '80s Tanner Lectures he gave, later published as The Economist as a Preacher, *he seemed to want to push that not only was it efficient, but it was also somehow ethical as well.*

You mean, what is, is right. Okay, I imagine that he got this from Milton Friedman. This happened around 1952, at the Paris Colloquium or Conference on Risk, put on by The Econometric Society. Milton Friedman gave a paper which said in effect, life is a constant procession of events that impinge on us with a considerable amount of uncertainty out there. (This is my broad gloss on what he said.) At every stage on the road there are forks in the road, and we are making choices. And, in effect, we end up in the beds that we have made for ourselves. This would have grown, in Milton's mind, out of the Friedman / Savage article of 1948 on gambling.[1] You postulate an epicycle in the form of a convex stretch, a non-concave stretch of the utility function so that the people falling in that become inveterate gamblers. And so the inequality is the result of their own *ex ante* decisions. Now, it's undoubtedly true that if everybody started out exactly alike in genetic composition and environment – for this they would have to be clones, identical clones – and if for some reason, even though they are clones, they differ in their risk aversion, then, what you will find is that those with the greatest risk tolerance will end up bi-polar at the extremes more than the people with less risk tolerance. And so what is, is right. Now, that's in a *JPE* article. Probably in 1953, I don't know.[2] I would speculate that this would have been an important source. Because George Stigler, who was very critical of people, was almost worshipful of Milton Friedman. And I remember that one of his dicta was that a Milton Friedman theorem was more credible than any other theorem, because everybody picks on Milton. It's an unfair world and so forth, which means that he gets a more rigorous testing than anyone else.[3] Doesn't he have genuinely adulatory remarks in his autobiography about Milton?

[1] The article is M. Friedman, L. Savage, «Utility Analysis of Choices Involving Risk», *Journal of Political Economy*, 56, 4, 1948, 279-304.

[2] The reference seems to be to M. Friedman, «Choice, Chance and the Personal Distribution of Income», *Journal of Political Economy*, 61, 4, 1953, 277-290.

[3] This particular *dictum* found its way into print at least once. Talking about the need to generate quite deliberately controversy, Stigler contrasts early Walrasian economics with the theories of his close friend Milton Friedman. In his evaluation of studying the History of Economic Thought («Does Economics Have a Useful Past?», in *The Economist as Preacher*, Chicago, University of Chicago Press, 1982, 107-118) he claims: «The sterility of the early Walrasian system arose because it was ignored by most economists and adopted by a few but criticized by almost none. Milton Friedman's work is bound to be spread rapidly in the science and to achieve a wide scope and high rigor because of his wondrous gift of eliciting the probing attention of eminent contemporaries» (111).

Yes.

Now, what you have to understand with somebody like Allen Wallis, and so to a degree those people who were in his circle, is that Allen Wallis had the sharpest priors – I'm using the language of Bayesian probability – of anybody I ever knew. Almost no new data could change his view for this reason. On the other hand, if he thought of somebody as a dangerous, or an incompetent thinker, but Jimmy Savage assured him that the man was very smart and had good judgement that carried more weight with Allen Wallis than a two-year study of the person's vitae and an audit of his writings. There's an in-group of the good guys and the much larger out-group. This showed itself in things that aren't even political. Just as an amusement I used to do a little Diogenes-like anthropological study of statistician friends of mine on what their attitude was on cigarette smoking. This was in the years when it had been nominated as an important cause of mortality, excess mortality. Let's say for my money that already the evidence was overwhelming. But this was denied. And when I went to Allen Wallis he said «There's nothing to it. Next thing you know they'll be saying coffee causes cancer». Or something like that. And this was right on my prediction. Before I went to talk to him, I predicted what his response would be. And I remember saying to him «Now, what about Milton?». And he said «Well, Milton agrees with me. However, he has quit smoking». But you know what my skeleton key, my variable was, in making these judgements? I went to Howard Raiffa[1] and he said «Oh, I wouldn't touch them on the basis of what we have». Now, how much of an admirer of R. A. Fisher are you? R. A. Fisher was a genius. He was the genius of first half of the 20th century in statistical theory. But he was an extremely opinionated man, a man of strong opinions, including strong eugenic and race kinds of things, and a very disagreeable man in many regards. There is a very good biography of him by his daughter, who of course is not a critic, but you just have to read the facts. Well, R. A. Fisher refused to believe there was a link between smoking and cancer. That was all poppycock. In fact in one of his articles, he purports to have a sample in which the inhalers have less lung cancer than the others. One of my younger colleagues here, who is both an MD and an economist, once went through the Fisher literature to see whether there was any saving grace, and there really isn't. But, Fisher was the enemy of Neyman-Pearson[2] be-

[1] Howard Raiffa (1924-) is a pioneer in the field of Decision Science and Game Theory. He is currently the Frank P. Ramsey Professor (Emeritus) of Managerial Economics, a joint chair held by the Business School and the Kennedy School of Government at Harvard University.

[2] Egon Pearson (1895-1980) was the son of Karl Pearson (1857-1936). The Neyman-Pearson lemma was the joint work of Jerzy Neyman and Egon Pearson.

cause Karl Pearson had been very mean to Fisher and Fisher in turn... well, abused children become abusers.

Now John Tukey, who was a very great American statistician, was also wrong. My maths teacher, Edwin Bidwell Wilson from Harvard, went on the payroll of the tobacco industry. In his case, he was Dean of the School of Vital Statistics and he was just so jaundiced by false understandings of probability. If six people in the same street get cancer, there would be a state legislative investigation. But it was interesting that in this particular group, this also was the case with Allen Wallis for example, they liked the complicated explanation better than the simple one. It was kind of like syncopation, always the after beat. So there was an awful lot of nonsense going on. I don't know if any important doctrinal differences ever developed between George Stigler and Allen Wallis and Milton Friedman. I'd say the only thing I can see at the very end is that Milton Friedman remained very much a policy person, pushing policy views.

In line with that, George Stigler pushed his idea of 'what is, is best' to the degree where he would say «Suppose there is for instance, a sugar subsidy that's remained in place. Suppose we can actually calculate the social cost of maintaining it. However, if it has remained for 50 years and passed the test of time, it must be, as far as the public is concerned, an optimal way to redistribute income and therefore economists can't really attack it, because it is all motivated by self-interest».

This is a little bit like Frank Knight's position against Henry George and the single tax. Land is inelastically supplied. There is no dead weight loss from imposing taxes on it, but why would you do it at this stage, when people in good faith have bought land over the years. Besides, how can you separate the investment that has been made in the land from the Ricardian original inexhaustible value of it? But you know this argument that you're making is Gary Becker like. I sometimes think some of the Chicago people are hopeless. Well, I wouldn't include Milton as among the hopeless because he was smart enough to punch his way out of a paper bag sometimes. But in the end he didn't want to do so. I think that's the case with 100% money, which was just a crotchety part of the first Chicago School. Irving Fisher also embraced it.

The only thing it fits into is Milton's later monistic monetarism where, if you have a 100% reserve ratio by law, then you can't have a variable *de facto* reserve ratio and therefore you won't get an additional component in the variance of the money supply. And of course getting a variance in the ups and downs of the money supply is the worst thing possible. Gary Becker, I think, cured him of that. Probably he said «Look. You have barriers to money in the banking system and private banking under one disguise or another will inevitably arise. You will simply make the banking system ineffective with a kind of Gresham's Law arising». And I think Milton quietly changed, he just quietly dropped that. He doesn't particularly announce changes in positions, but instead, lets them just decay away. That idea actually became a prominent principle, that there's always a way out of bad regulations. If you do it by surprise... This gets into the Lucas critique. If you do it by surprise people can be cheated the first time. But give them time and they'll work the market around. So don't worry. In fact, Lucas's rational expectations really stole the show from Friedman's monetarism. There's almost nobody left, including I think Milton quite quietly, who believes that there's a tight relationship between one of the M's and effective demand.

Now when George Stigler got the Nobel Prize and he actually went to Washington, he was given the Science Medal of Honor by Reagan. He was asked what fiscal policy should be, or something like that. What was very uncharacteristic for an economist is that he said what Gerald Debreu said when he got the prize. «I don't do that sort of thing. And so I don't have any interesting opinion on that». I think George said, in effect, I'm a micro-economist and not a macro-economist. But I think like Armen Alchian,[1] who is more Catholic than the Pope, who never went to University of Chicago but is a real Chicagoan, he does end up doing some simple macro theory. I imagine when all's said and done – I don't remember George writing particularly on the real bills doctrine or the quantity theory – George would say «inflation is everywhere a monetary phenomenon».[2] This is like taking a personality loan from people whom he admires, who believe that kind of thing. More than

[1] Armen Alchian (1914-) spend his academic career at UCLA, starting there in 1946. The Economics Department became closely identified with the Chicago School. Alchian himself is best known for his pioneering work in the economics of property rights. A seminal work, «Uncertainty, Evolution and Economic Theory», appeared in the *Journal of Political Economy*, 58, 1950, 211-221.

[2] In an interview conducted by *The Region* (May 1989) the official publication of the Federal Bank of Minneapolis, http://www.minneapolisfed.org/pubs/region/89-05/int895.cfm, George Stigler clearly confirms Paul Samuelson's claim.

Well, I'm a monetarist in the sense of believing that the control over some money supply is important (which measure of money and over what periods, for example, are decisive questions in the control over the rate of growth of the price level). I think that the rate of growth of money is a critical variable in controlling inflation and that, for example, the massive troubles we are having with the savings and loan industry are, in part, the product of the fanatical inflation we had at the end of the '70s and the beginning of the '80s. Those alone are indications of the kind of costs that are imposed upon a society. It wouldn't be too bad maybe if you went completely crazy, like the South American countries, and let inflation go on and everybody indexes on some more stable currency, and so forth and so on. But we aren't going to do that. We're going to put all kinds of strange regulations in: we won't let this go up, and we'll let this price go up. They cause immense distortions in an age of inflation. That's one of the great problems plaguing the Israeli economy.

most, I think, George kept out of things that he felt he wasn't entitled to an opinion on. Most economists would say «How do you spell 'gold'? And then they'll tell you what we should be doing about gold or anything else that you can imagine».

Okay, I have one last question. This is related in a way to your last statement. In his writing, Stigler is very clear that he doesn't believe ideology has much to do with economics.

Well, this is a popular Friedman view too. And it's wrong. I say that flatly. But it's interesting that just recently – I have somewhere a National Bureau Yellow Jacket manuscript of a research study by Victor Fuchs from Stanford University, Jim Poterba from this University and Allan Krueger of the Woodrow Wilson School at Princeton.[1] They did an extensive sampling of economists in two areas of economics. One was labour economics and there was another, approximately equal size sample. I can't remember how large these samples were. Whether we're talking 50, 150 or 200 in each discipline, I'm not sure my memory's precise.[2] And what they did was they gave a whole set of questions on what each person's factual opinion was on that question. What do you think is the elasticity of supply of labour under this condition? And so he had all these factual differences in the group. But they also asked questions about their value judgements. They asked these in whole different areas. The third less important area were their political affiliations, which I presume these days would be Republican or Democratic, but they might have gone further. Then they tried to see how you explained the differences in policy recommendations. And their finding is the opposite of Milton Friedman's. There was very considerable degree of consensus on factual matters. There were some differences in the degree of confidence they had in their answers. The confidence intervals varied quite considerable. And in particular, the people whom in this sample had aberrant factual beliefs, if most people thought the elasticity of supply was a small plus, but you got somebody who had it a very large plus, or somebody who had it a very large minus, those people would have much wider confidence intervals as well because the authors got data on what they regarded as their confidence intervals. Now what they

[1] V. R. Fuchs, A. B. Krueger and J. M. Poterba, «Why Do Economists Disagree About Policy», NBER Working Papers W6151 August 1997, 71-49. This was later printed in the *Journal of Economic Literature*; Iidem, «Economists' views about Parameters, Values and Policies: Survey Results in Labor and Public Economics», *Journal of Economic Literature*, 1998, 1387-1425. Curiously enough, Paul Samuelson has a published article in the same volume.

[2] The three authors base their work on surveys sent out to «specialists in labor economics and public economics at the 40 leading research Universities in the United States.» (Fuchs, Krueger and Poterba 1997, 1).

found was the difference in their policy recommendations were – I'm using your language, not their language – ideologically premised values. They were not fact driven. Now there are a few cases like the minimum wage or Ricardian comparative advantage where you can almost get certain unanimity, free of ideology. But these are exceptions in my opinion.