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The Experiment in Applied Econometrics

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Christmas is upon us. For our three-and-a-half year old grandson, my wife found a wonderful video tape. It's a Disney-type adventure in which the boy himself is the central character. You first send the seller of the video his picture, and somehow the child becomes the character in the film. As he looks at it, he will see himself in the drama.

I felt like that at this conference. It is a rare and odd experience.

I have also recently encountered a fashionable idea in economic theory. (Theory is full of fashions that come and go.) This idea starts from the current emphasis in modern economic theory on intertemporal choices, for example life-cycle consumption decisions. Some young theorists think they detect, if only by common introspection and observation, dynamic inconsistency in individual time preferences. People seem to have a decisive preference for doing onerous things tomorrow rather than today. Yet looking into the distant future, they don't discount, say, 30 years from now significantly more than 29 years. But when 29 and 30 become zero and one, then they will discount 30 to 29 as they now discount 1 to zero.

Theorists who worry about this fancy the notion that different individuals with different utility functions are involved. Here I am in this video tape where all of a sudden the participant is not I, a 78-year old guy, but another James Tobin, only 29-30 years old. As I re-read this article I thought "that earlier J.T. wrote a pretty good paper". Well, anyway, it's a very interesting experience, for which I am as grateful as I expect my grandson to be for his Christmas present.

I shall tell you about how I happened to write this paper in the first place. I had been a graduate student at Harvard from 1939 to 1941, just before the US got into the Second World War. Then I was away from economics for almost five years, mostly in the U.S. Navy on a destroyer. In January 1946 I went back to graduate school at Harvard, wrote my thesis, and earned my Ph.D in another year and a half. The thesis was on the consumption function. In it, I pragmatically combined information from the 1935-36 U.S. national budget study with national income accounts time series. (Thanks to work relief in the United States in the depression, the country was able to carry out a large and detailed budget study.) This was a prelude to my 1950 paper on food demand.

Let me digress to tell you about statistics and econometrics at Harvard in those days. Mathematical statistics at Harvard was very good. I took a course from an eminent scholar and wonderful teacher, E.V. Huntington. But there was not much econometrics around at Harvard. In what was called "Business

Cycles" Professor Edwin Frickey was decomposing time series into seasonal, cyclical and trend components. I never did understand how he was doing it, nor could he show us how to do it ourselves. The senior economic statistics teacher, Professor William Leonard Crum, thought that his duty was to warn us about all the "booby traps", all the things that could go wrong. Those of us interested in econometrics had to study it on our own, reading the outputs of the Cowles Commission, then in Chicago. We did have one visitor, the Swiss econometrician Hans Staehle, who was interested in empirical demand analysis. That was very good, but on the whole there was little econometrics at Harvard in those days.

In America, beginning early in the century, statistical analysis developed in conjunction with agricultural economics, in the federal Department of Agriculture and in the state agricultural schools and experiment stations subsidized by the federal government at "land-grant" universities, especially in the Midwest. These statisticians pioneered demand and supply analysis. Even before the First World War, they were quite conscious of identification problems. How could you tell whether you were getting a supply curve or a demand curve or some uninformative mixture of the two?

At Harvard there fortunately was a Professor of Agricultural Economics in the economics department, John D. Black. He had the only electrical-mechanical calculators around. I was not studying agricultural economics, but I did persuade Professor

Black to let me use his facilities, and that's where my calculations for the thesis and for the food demand paper were done.

As I recall, it took two or three days to do a regression with three regressors. That was an automatic incentive to think carefully in advance about your specification. You were not able to press a button and compute another specification a second later. Maybe nowadays we should impose a tax or a quota to play the same role! Anyway there is quite a difference between the scarcity price of calculations reported in an article written in 1949-50 and in the papers written for this conference.

After I got my degree I was fortunate enough to get a post-doctoral fellowship, three years to do whatever I wanted. It helped me make up for the time I had missed during the war. One of the things I wanted to do was to try to write as good a paper as I could using the cross-section/time-series coalition. The reason for doing it on food was to avoid the complication of the durable goods included in aggregate consumption. I was not abandoning my interest in the consumption function, but the food paper was supposed to be a sort of baptism in econometric method.

I think it may surprise you how conscious I and my friends were about the paucity of information in economy-wide time-series data. Perhaps we were more conscious of it than practitioners are now. Why was that? One reason was some work of Richard Stone's, a principal components analysis of U.S. macroeconomic time series. Remember that the only time series national accounts available in the late 1940's were annual data between the two wars. In Stone's principal components analysis, the first component is just the pervasive business cycle dominating 1920-1940 macro fluctuations. It is quite pronounced, shows prosperity in the 20's, then a deep depression, and then a recovery from the depression, ending in a 1939-41 military boom. More than 90% of variance in these series are "explained" by this first component and there are really only one or two other identifiable components. The reason everything looked so good - the Keynesian consumption function looked great - is that consumption and income, and almost everything else move with the pervasive business cycle. Back then, we were very conscious of this problem with time series, and that's one reason why we thought cross-sectional data would be a good idea, not to be used in place of, but in addition to, time-series data. Maybe things aren't so collinear now. We have longer series and more "natural" experiments.

In the third year of my fellowship, I went to Dick Stone's institute, the Department of Applied Economics, Cambridge, England. The food paper had been virtually completed beforehand. I had been in England for only three months when I gave it as a paper at the Royal Statistical Society in London, quite an awesome scene, quite formal, very non-

American. The DAE was a great help in calculations and charts; I got good advice from Durbin and Watson and Stone himself. The whole year was excellent experience. There was a great group of people: not only Durbin and Watson, but also Michael Farrell who alas died young, and Henrik Houthakker. Orcutt and Cochrane had been there the year before.

My purpose in using cross-section data was to dodge the collinearity of aggregate time series. The criticism here in these meetings was that I overstressed that difficulty. Unfortunately, the issue is clouded by discrepancy in the definitions of consumption between my cross-section and time-series data, to which Dick Stone called attention. (Most of you read the discussion at the back of my 1950 paper.) Evidently, if the definitional discrepancy were rectified, then the difference in estimates of income elasticity, between the time series and the 1941 and subsequent budget studies, probably would be diminished. It is unfortunate that nobody has done that: it seems that it is not hard to do now.

At the same time, I don't agree in principle that if time series regression gives a different number from the budget study estimate the time-series is right and the budget study wrong.

No. What worried me was that whatever number you assumed for the income elasticity, a regression estimate of the price elasticity would be that same number with a negative sign. I showed this collinearity in my paper by Frisch's confluence analysis, now an archaic technique no one would use. It was high-tech in the 1940's.

I realise that there are plenty of problems with cross-section data. For example, consider savings data. To understand them, you need a stock-flow mechanism, some relationship between the stock of wealth that an individual or family already has and the amount they save. The natural presumption is that the more wealth the consumer unit already has, the less saving it will do. On the other hand, a cross-section reflects persistent personality differences among people. Some are thrifty and some are not. Those who report a lot of wealth in the cross-section are the thrifty - that's why they have a lot of wealth. The chances are that they are going to continue to be thrifty. You are going to see saving apparently positively related to wealth. But that is not a cause-and-effect relationship. The economic theory says that the more wealth you have, the <u>less</u> you are going to add to it.

The only escape is re-interviews or panels of identical respondents. The latent variable, the personality, remains the same, so that changes in individual saving over time can be attributed to wealth, income, and other determinants.

In the 1950s I became quite interested in analysis of cross-section data. After I returned to the United States, I spent some time at the Survey Research Center at the University of

Michigan working with George Katona, James N. Morgan and Laurence Klein. They were re-interviewing their respondents, and eventually collected panel data. Now we have samples with plenty of observations in the Research Center's Panel Study of Income Dynamics. Unfortunately they do not obtain saving and wealth observations, but we can hope. Problems are better resolved by new data rather than by arguing about how to treat existing data.

One problem in my 1950 paper was to distinguish between changes in average income and in its distribution among households. I wanted to interpret aggregate time series as changes in average income with the distribution constant, defining "constant distribution" as suggested by Jacob Marschak.

Despite the impression of some conflicts, disagreements, and chaos in the eight papers that I heard about over the last couple of days, I think there was a fair amount of agreement on the general specification of the demand function, and on what the explanatory variables are. And there was apparently little disagreement on the shapes of the functions. It was not a contentious couple of days. I didn't come away thinking that econometrics was in a crisis. I must admit that I am not adept at many of the new techniques and diagnostics commonplace in modern econometrics. Maybe I start out with a little more skepticism than most of the practitioners do, but on the whole I think the project was successful.

I congratulate Magnus and Morgan for the initiative in doing this experiment. Many talk about retrospective tests of econometric tools, without doing them. Here it has been done under your leadership.

Personally I enjoyed the attention. I'm proud and grateful, that my work of long ago was selected as a target and impetus. Thank you for bringing me here. I wish you success in your future studies of applied econometrics.