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THE ET INTERVIEW: ARTHUR S. GOLDBERGER

Interviewed by Nicholas M. Kiefer

Econometrics as practiced by Arthur (Art) Goldberger demonstrates extraordinary sensitivity to issues of measurement and model specification, and unusual care and caution in interpretation of results, as well as a thorough and comprehensive mastery of econometric theory. His landmark 1964 book, *Econometric Theory*, set a new standard of rigor in econometrics, and at the same time treated the important problems posed by limited and qualitative dependent variables years before any other text. Art Goldberger's work ranges from early contributions to macro modeling through demand analysis, multivariate modeling with latent variables, and models for sample selectivity, to his highly regarded work on important social issues of heritability of IQ, effectiveness of public versus private schools, and measurement of salary discrimination. Goldberger's influential work, especially on modeling latent or unobservable variables, is widely known and applied in sociology and psychology as well as in economics. Art has been at Wisconsin for many years, and this association is an important reason for Wisconsin's continuing reputation as a leading center for quantitative social sciences.

The quality and influence of Art Goldberger's work has earned him many professional honors. He is a Fellow of the Econometric Society, the American Statistical Association, the American Academy of Arts and Sciences, and the American Association for the Advancement of Science, and has

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twice been a Guggenheim Fellow. He is a Distinguished Fellow of the American Economic Association and a member of the National Academy of Sciences. Art gave the Woytinsky Lecture at the University of Michigan in 1985.

This interview took place on May 5 in Art Goldberger's office overlooking Lake Mendota. Art's remarks cover a wide range of topics, and I hope they are of interest to social scientists generally as well as to econometricians.

Would you tell us about your schooling at NYU as an undergraduate and then at Michigan as a graduate student?

Let me back up a little before then. I was born and raised in Brooklyn, in an Orthodox Jewish family, so that I studied at Hebrew school and Yeshiva through age 16 or 17 and got a rigorous education to that point. Then, when I went to NYU, to the School of Commerce, my idea was to major in accounting because accounting was a profession in which you could observe the Sabbath. This was a major consideration. I didn't want to be a doctor or a lawyer, but I was looking for a profession in which I could follow religious practice. As it turned out, the accounting course was a disaster, and after two years of that I realized that I didn't want to spend my life as an accountant. I had taken a couple of economics courses during the first two years and that seemed to be the only major in the School of Commerce that would make any intellectual sense. So I backed into economics that way, with some intention to go to law school after getting an economics degree. The economics program at NYU—this was 1949—was in a sense pre-Keynesian. I took a year-long course in money and banking in which Keynes's name came up only once. And it came up only because one of the students in the class asked who is this guy "Kuh-neeze" that we read so much about in the paper. The professor said well, you know, it's pronounced "Keynes" and he's been compared to William Jennings Bryan: they both believed in cheap money. Period. I did have one very good teacher in economics, Sol Fabricant, of the National Bureau of Economic Research, who taught economic theory then. I didn't take any math in college until my junior and senior years. I was very influenced by Sidney Hook, the distinguished philosopher who is now at the Hoover Institution. He was my teacher in 1950, a very remarkable man. I graduated with a B.S. in economics and at that point decided that economics was more attractive than law. I applied to half a dozen schools and was accepted—actually with TA-ships—at Stanford and Johns Hopkins. I knew I didn't want to go to Hopkins because the story was that you had to wear a jacket and tie. I don't know if that's still true. Is it?

I don't know.

It was at the time. Stanford seemed too far away. Finally, late in the spring, I got an offer of a research assistantship at Michigan and ended up going to Ann Arbor. Actually, I'd been turned down for financial aid at Ann Arbor that year. However, late in the season Larry Klein succeeded in getting a grant from the Ford Foundation to start off his research seminar. As he describes it, he went to the back files of the graduate school, culled through the rejects, and pulled me out and also Steve Vallavanis. Steve's is a name you may not know. He became a fellow graduate student. A brilliant guy, who went on to teach at Harvard and then was killed accidentally in Greece before he was 30. That was a great loss to the profession.

So you first met Lawrence Klein while you were a graduate student at Michigan?

Yes, he was on the faculty, and I was hired as his research assistant for my first year of graduate school and continued with that.

Could you describe the development of the macro modeling project?

Well, I can describe it from my own perspective, which was as a research assistant, primarily a data looker-upper and calculator person. Of course, this part of Klein's work at Michigan was a continuation of the work he'd done at Cowles, the project on economic fluctuations in the U.S. that led to his 1950 book. What's called the Klein-Goldberger model is really Klein Model IV. I was a clerk on the model, but he was generous enough to put my name on it. . . . I could tell you about computing in those days.

That would be interesting.

This was 1951, and computational facilities then were desk calculators. Have you ever seen one?

Mechanical?

Electromechanical. It ran by electricity, but it was about the size of an office typewriter, and it had a keyboard with 10 columns of keys. In each column, the keys ran from 0 up to 9. When you entered a number in you hit one key in each column, and then hit the "enter" key. The readout was like an odometer in a car. (There was still a hand-powered calculator around in those days, which one of the people at Michigan insisted on using. That one you ground like a coffee grinder to do addition—you know, to get the gears moving.) We learned some tricks on using an electromechanical calculator to compute moments, and one trick is this. Suppose you are working with two variables and the data for each have only two digits. Now if you can imagine entering the X variable way over on the left of this keyboard, and the Y variable way over on the right, and some zeros in between, then when you hit the "square" key, of course what you get is X-squared and Y-squared, and sitting there in the middle is twice XY. As long as you didn't have too many digits in each of the columns they wouldn't overlap at all. The calculator would accumulate for you, so you would end up with the sum of X-squared, a few zeros, twice the sum of XY, more zeros, and the sum of Y-squared. That way you could get those moments just by entering each observation only once. A great innovation. Another major innovation at the time was a little counter that attached to this calculator. The counter would literally count each time you made an entry, so if you were entering, say, 30 observations and accumulating sums of squares and cross products, this counter would go 1, 2, 3, 4, 5, and so on. So if you lost your place while punching numbers into the keyboard, you would be able to recover. That was a major technological innovation.

So you were involved with data and with applications from the very outset at Michigan?

That's right, part of my job was going to the national income accounts, pulling out the data, and constructing variables for the macro model. So my accounting training came in handy after all. The computation, of course, was a major, a major consideration. To do a regression with 20 observations and 4 or 5 variables was a matter of several days. See what you're missing?

Were there other economists at Michigan or elsewhere who particularly influenced you while you were in graduate school?

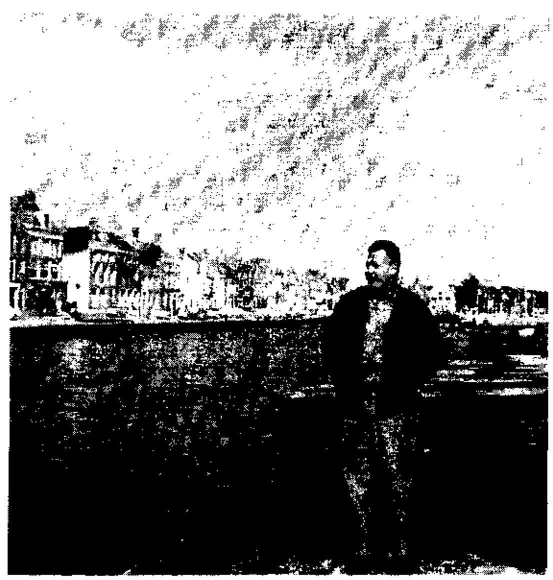
Klein, of course, was a strong influence on me. I learned research methods and what econometrics was about from him. As far as what economics was about, Richard Musgrave, who was on the faculty there, was excellent. What little economics I know I should attribute to him. Dan Suits became my thesis adviser after Klein left Ann Arbor. Suits taught me to be skeptical about fancy statistical methods; that was a very useful lesson. I should say that I've never had a course in mathematical statistics. It shows. In college, I had the cookbook economic statistics course, with really no statistical inference. And then at Michigan I jumped right into Klein's econometrics course, which started out, as I recall, with estimating a dynamic equation with autocorrelated disturbances, and I was not at all sure what probability meant. I did sit in on a linear algebra course at Michigan.

Was the program organized as many modern ones are with micro, macro, and econometrics in the first year?

Well no, it wasn't like that. I had money and banking the first year and no pure microeconomics course. This was characteristic of Michigan, I think, where the emphasis was certainly macro and money.

You visited at the Netherlands Central Planning Bureau in 1955 and the Econometrics Institute in 1959. This was an important time and these were important institutions. Could you describe the research atmosphere and tell us what projects you worked on during those visits?

I went as a Fulbright grantee to the Central Planning Bureau in 1955. This was while I was still a graduate student at Michigan, and my idea was to work with Tinbergen; I don't think I'd heard of Theil at that point. It turned out that Tinbergen was retiring from the CPB that year, and that Theil was spending the year in Chicago, so I didn't meet Theil. I worked with the group at the CPB that was doing long- and short-term planning, and did a little work on what was later to be my thesis, on impact multipliers and dynamic properties. So in fact, I learned that technique from people at the CPB office—how you run out predictions of delayed effects of policy changes. When I went back to Holland in 1959, I was at the Econometric Institute, which was Hans Theil's research group. That was a very important year for me because, by then, I was finished with the thesis. Theil operated according to a system. He had a group of graduate students and they would be assigned topics to work on. Every week, at the minimum, they would come in with what they'd done that week and he would guide them. Naturally, he dealt with me the same way, that is to say "Here is a project to work on, I've already thought about it a bit, come in and check every week." And the great thing I learned from that—I realize that kind of experience may not sound appealing now—was what a researchable topic was. A researchable topic is something you can get an answer to, and that is something I had not learned at Michigan where I was working on a large-scale project and not



Holland 1962

something manageable on my own. As it was, the experience with Theil was crucial for me.

Was your own work influenced by the work at the Cowles Commission?

No, not directly. I was never associated with it. Obviously Klein was coming from Cowles.

You seem to have left macro modeling as your research area after the 1959 book. Could you tell us what drew you away from that line of research?

I realized that I didn't know enough about macroeconomics. That book was my thesis, as you may know, and I just didn't know where to go myself from there with macro model building. I was at Stanford for three years in this period. My first teaching job was there, and I was hired by telegram, sight unseen. What happened was that Henk Houthakker and Ken Arrow (Ken taught econometrics then) were both going to be on leave the following year, so Stanford was kind of desperate to get somebody to teach econometrics. That was the first year I was spending in Europe, and I remember getting a cable at American Express in Genoa, saying "can you teach next year for us?" and I wrote back saying "sounds very good, do you know I don't have my doctor's degree yet," and they wrote back saying "that's okay." So, I taught. I was an Acting Assistant Professor which was a one-year appointment. I was renewed for one year and then for another, and then terminated. In those days that was fine. I mean, the fact that I had only a couple of month's notice at the time didn't bother me. Of course, the academic marketplace was different then.

You've been associated with the University of Wisconsin since 1960. By American standards this is a long time for a distinguished researcher to stay at one institution. What has the research environment been like at Wisconsin during this long period?

Well, if I go back to 1960, which is when I came here after Stanford, Wisconsin was a very exciting place. Guy Orcutt had arrived two years earlier, with very ambitious plans for modeling a socioeconomic system. There was a great focus on the use of micro data, which was a new kind of focus at that time, and a great focus on team research. Orcutt's view was that the economy and the society are made up of various components and that the way to go about modeling these was to get a team of researchers and have a couple of members of the team handle each of the components or sectors. That project was the core of what was called the Social Systems Research Institute, SSRI. And so the common interest in model building and recruitment of a lot of new faculty members made Wisconsin an exciting place to be. Over the years since then we've been blessed by having a lot of good econometricians – Arnold Zellner, Harold Watts, Dennis Aigner, Lau Christensen, John Geweke, Gary Chamberlain, and now Chuck Manski and Jim Powell. So we have had a great group – not all at once, unfortunately. There's also always been a very serious interest among a lot of people in the department in quantitative empirical research. That's been an important stimulus for me throughout. I've been influenced by people who work in labor economics here, Glen Cain in particular. The research environment has always been congenial. The focus that Orcutt brought, which was interdisciplinary, was also important for me. It was natural to talk to sociologists as well as to economists. The Poverty Institute, which came along a few years later, has been another center for collective work in empirical economics that raised econometric questions.

What makes a research environment good?

Well, respect for colleagues is one element. What you need are colleagues who, collectively, span a range of interests and have a variety of skills. You want people who'll bring you problems, and other people who'll help you to solve them.

Your 1964 book, *Econometric Theory*, is one of the first to alert readers to the important limitations of the linear regression model when the dependent variable is categorical, or when it has limited range. Yet it took applied econometricians a very long time to use the more suitable methods like probit and Tobit analysis. Why?

It was primarily a computational matter. Nonlinear optimization was not a trivial matter in those days, the early 1960s. The reason why there is material on that in my book is directly because of the Orcutt project in which one of the major problems was to model at the micro level things like durable expenditures, and debts, and marriage, all censored or limited dependent variables. So it was just the natural thing to be looking at. Your question was why it took econometricians a long time. Now I give Tobit estimation as a homework assignment in our first year econometrics course, but then even probit was thought of as a complex calculation.

Is it true that you coined the name Tobit?

I don't know. Maddala says so – Maddala's book gives me credit for that. It's quite possible. Where the name came from is obvious. The Tobit model was thought of as an extension of probit rather than as a case of censored regression, so Tobin's variation on probit couldn't be named anything else except Tobit. There is a story – I don't know whether you know it – that Jim Tobin is unhappy with the name.

I didn't know that.

The reason for that is—do you know the book *The Caine Mutiny*?

Yes.

Now there is a character named Tobit in that book who appears briefly early on, a fellow student of the hero at the officer's training school. He's a character who is described as brilliant but somewhat unpleasant otherwise. That character, who's called Tobit in the book, *is* Jim Tobin. He was a fellow student of Herman Wouk—he was in the Navy with Wouk. I honestly didn't know about it at the time.

Now there are other names I've invented that you may not know about. There was Orbit, which was my name for a procedure of Orcutt's which would be doing the following: when you had a censored dependent variable—say expenditures on durables—you would have one equation that tells you yes or no, and that would be a linear probability function. Then there would be another equation which would be just the linear regression for those who did purchase. That pair of equations together—I think it's described in my textbook [(1964) p. 252—NMK]—was called the twin linear probability model, but the proper name was Orbit. Now there are other names which I've invented. Let's see, I made a list. One that is not well known, fortunately, is *homoregressivity*. You know what homoskedasticity is, right? Where the variance is constant, that's to say the skedastic function is constant? So homoregressivity obviously has to mean that the regression function is constant or horizontal. Actually, the concept—or rather a better name for the concept—I don't think I invented the concept, but I certainly did popularize it, was *mean independence*. I don't think you can find that phrase in any place until my 1968 *Topics in Regression Analysis* book. If you go to econometrics texts, and to statistics texts also, there is stochastic independence and uncorrelatedness, and nothing in between. That notion of the conditional mean being constant—homoregressivity, or mean independence—seems to be a very useful idea. I have found it explicitly only in one psychometrics textbook under another name.

What name, do you recall?

It's called linear experimental independence in Lord and Novick's 1968 book, *Statistical Theories of Mental Test Scores*.

There is one more expression which you haven't asked me about because you haven't heard of it, and that's micronumerosity.

Micronumerosity?

Take a look at any econometrics text; there's always a chapter on multicollinearity in which the author broods about all the terrible things that happen when you have multicollinearity. It turns out that the terrible thing that happens is you get large standard errors.

Right.

Right. And that's what you get. It's very analogous to trying to estimate a population mean when you have only a small sample. You get a large standard error. Right? Nevertheless, you don't find any chapters in the textbooks about the terrible things that happen when you're trying to estimate a population mean and have a small sample. Why not? Because there's no fancy name for that situation. You can't just say "estimating a population mean when you have a small sample." So I've constructed a fancy name, and now there'll have to be a chapter, and that name is "micronumerosity."

Actually, I've written a parody of the paragraphs that appear in multicollinearity chapters.

[The reader is invited to compare textbook discussions of multicollinearity with the following—NMK]

> The extreme case, perfect micronumerosity, arises when $n = 0$, in which case the sample estimate of μ is not unique. (Technically, there is a violation of the rank condition $n > 0$: The matrix (0) is singular. See Hadley, *Linear Algebra*.) That extreme case is easy enough to recognize, but "near micronumerosity" is more subtle, and yet very serious. It arises when the rank condition $n > 0$ is barely satisfied. Near micronumerosity is very prevalent in economic research. Later we will discuss tests for the presence of micronumerosity and see what can be done when those tests suggest that micronumerosity is lurking in n. But first, its tragic consequences.
>
> Consequences of micronumerosity:
>
> (1) Precision of estimation is reduced. There are two aspects of this reduction: estimates of μ may have large errors, and not only that but $V(\bar{Y})$ will be large.
> (2) Investigators will sometimes be led to accept the hypothesis $\mu = 0$ because $\bar{Y}/\hat{\sigma}_{\bar{Y}}$ is small, even though the true situation may be not that $\mu = 0$, but simply that the sample data has not enabled us to pick it up.
> (3) The estimate of μ will be very sensitive to sample data, and the addition of a few more observations can sometimes produce dramatic shifts in the sample mean.
> (4) The true μ may be sufficiently large for the null hypothesis $\mu = 0$ to be rejected, even though $V(\bar{Y}) = \sigma^2/n$ is large because of micronumerosity. But if the true μ is small (but nonzero) the hypothesis $\mu = 0$ may be accepted.
>
> Testing for micronumerosity:
>
> (1) Tests for the presence of micronumerosity require the judicious use of various fingers. (Some researchers prefer a single finger, others use their toes.) A generally reliable guide may be obtained by counting the number of observations. Most of the time in econometric analysis, when n is close to zero, it is also far from infinity.
> (2) Several test procedures develop critical values n^*, such that if $n > n^*$ then micronumerosity is no problem, but if $n < n^*$, then micronumerosity is a problem . . .

Your famous 1967 monograph "Functional Form and Utility: A Review of Consumer Demand Theory" is an invaluable source for demand analysts and microeconomists. I understand the monograph is now published by Westview Press of Boulder, Colorado. Why the 20-year delay?

I wrote the monograph as a preliminary, or prologue, to the empirical work that we were about to do with national time series data. It was for my own guidance at the time, it wasn't at all original material, and it's a very long piece, so I never thought of sending it to a publisher. It did get a wide circulation . . .

I learned demand theory from that as I'm sure did many others.

It did get a wide circulation. Up to a couple of years ago I was still getting requests for copies. Now it's been published and presumably nobody will buy it. My guess is that it was very highly valued because it was scarce. I really was just trying to lay out material for myself, trying to mix the economic theory with some attention to what the empirical work was going to be.

Can you tell us why you stopped working on demand analysis after about 1970, or perhaps you didn't and I'm uninformed?

No, I pretty much did, although I had a student (Ted Gamaletsos) who finished a dissertation then, and another (Ching-Fan Chung) who recently completed his. I enjoyed focusing on one topic in economic theory, getting it down fairly tightly, and using it to set up statistical models and to learn a bit about multivariate analysis. There was the satisfaction of taking seriously the use of microeconomic theory to guide analysis of macroeconomic data. I guess I became more skeptical about that afterwards.

More skeptical about the usefulness of micro theory?

Of its relevance or applicability to macro data. I mean it was cute to say that you have the U.S. economy as a whole maximizing utility, but it didn't seem very plausible. The micro theory did give lovely restrictions on demand systems . . .

I find there are two schools of thought associated with demand systems. There's the work on aggregation by Mantel and Sonnenschein and others which proves that the aggregate excess demand function can be anything that satisfies Walras' law.

That's right.

But then there's the other point of view that you associate with, I think, applied people like Louis Phlips and perhaps goes back to Hicks that this model is so simple and naive and stylized that you couldn't hope for it to do anything but explain the aggregate or average. It certainly couldn't explain any individual.

It's not clear in what sense it helps you to explain the aggregate. My recollection is that if you look at, say, income elasticities in these aggregate demand systems they're pretty well estimable, no matter what functional form, and no matter what restrictions on the demand system that you incorporate. You get pretty robust estimates. If you look at price elasticities, they tend to be all over the map when you do unrestricted multivariate regression. You get numbers, and if you impose indirect addilog or some other utility function you get other numbers, and nobody has a really good feel as to what numbers are credible. Price effects are where the use of utility maximization makes a difference. So, it's kind of fun to work with aggregate data as numerical illustration of the theory, but I wonder if there are any policymakers in the world who care whether the cross price elasticity for some broad category of consumption expenditures is 0.1 or 0.5 or 3 or 4, or who would do anything differently if another demand study did come up with a sharp estimate. That's a problem with a lot of empirical research, the question "what is it good for?"

Would you describe your work on the IQ controversy? How did you become interested in the topic?

That started in 1972/73, when I was on the faculty here at Wisconsin but spending the year on leave at Stanford. It was my first year back at Stanford in many years. That was the year in which Arthur Jensen at Berkeley, an educational psychologist, and William Shockley, an engineer at Stanford, were making a big deal about racial differences in IQ. Some of it arose in the context of the question of why educational programs like Headstart were failing, allegedly. Jensen's explanation in part was that the teachers were not doing a bad job, but the kids were dumb, dumb genetically, not just temporarily. And it was a very hot topic at Stanford and in the Bay Area that year. So I started to take a look at the data that were being used to support this notion that the black/white differences in IQ were primarily genetic. It turned out that most of the evidence that was being used had nothing to do with blacks—you probably know this—it was data on twins, on white twins, and particularly data on the similarity of the IQs of identical twins who had been separated since birth, allegedly. At the time there were only 4 studies on separated identical twins. There was an American sample of 19 pairs of twins, and there was a Danish sample of 12. Then there was one British sample of about 40, and then a larger sample reported by Cyril Burt, a British psychologist. The first three I mentioned had books written about them, case studies, quite detailed descriptions. Burt had no book and no descriptions. (Since then we've learned that he had no twins, either.) When I read the three books I learned that—well, the twins had not all really been separated completely since birth. I mean there was no true experiment that had taken place. That raised the possibility that the similarities in IQs were partly due to similar environments. That's how I got started on the topic.

Now Jensen had been describing the fact that you can take data on pairs of identical twins and of fraternal twins, and on pairs of brothers, and parent/child, and others kinds of kin, and fit models to estimate the proportion of the variance in IQ that is attributable to genes. At first I had thought, naively, that that analysis was genetics in the sense of microscopic examination. Then I realized that the data consisted of correlation coefficients. Essentially you fit a structural model to the correlation coefficients, and one parameter in the structural model is called "heritability." That was easier for me. I didn't have to learn any biology in order to go on.

After all, one thing I should have been able to do by that stage was estimate a structural model. I started finding exaggerated claims about the accuracy of the fit. I found in some of the models for which estimates had been reported that the model was actually not identified. They had misprogrammed—they being some biometrical geneticists—one of the equations in their model and had accidentally gotten a model that *was* formally identified. But that wasn't the model they intended to estimate, and it wasn't a coherent model.

I got interested in the IQ controversy, no doubt, because of the social context, and then what kept it interesting was the fact that I thought I had something to say as a result of using what I knew about econometrics and statistics to critically evaluate the work that had been done in that field.

Well, the geneticists must have loved that.

Some did. I was invited to a conference on genetic epidemiology. I have an article published in the *American Journal of Human Genetics* and a chapter in the conference volume on genetic epidemiology. Genetic epidemiology turns out to be in part statistical structural model estimation.

Did any lessons from your work on the IQ controversy carry over into your work in econometrics?

It probably made me more skeptical about journal literature in any field, that is, in economics also. My recollection is that somebody, a noneconomist, asked me at the time "You pick out all these flaws in Jensen's work and the biometricians' work, but if you would look at economists' work that closely, would it turn out any better?" I wasn't sure about the answer, so now I'm more skeptical of things I read. If I'm interested in an empirical paper, I'm inclined to go back and search for mistakes.

What's the record been for economic articles?

It's a lot better than the IQ literature was, if only because we have so many more sophisticated critics around. We're such a large discipline that there are people who keep other people honest.

The other lesson from the IQ controversy, which I already knew about a bit, was that you can do a lot of modeling with unobserved variables. These models that the geneticists were using were in fact nonlinear latent variable models.

You have served on the panel on pay equity research of the National Research Council. Would you comment on the methodological issues involved in measuring salary discrimination? Would you care to comment on the intellectual level of the debate?

Yes, this is not going to be interesting. Our panel's job was only to hold a contest for research grants on pay equity research. We solicited applications for very small research projects, chose among those, and then funded a dozen. There was a conference based on those projects, and a volume will be published. The panel in this case was not at all charged with evaluating the evidence on comparable worth. As to the intellectual level of the debate, at least to judge from the research papers we saw, it was every bit as respectable as anything in mainstream labor economics. I realize there's been a heavy ideological component to some work on pay equity on both sides or on all sides of the issue.

Would you comment on your work on reverse regression?

Now that, of course, is independent of the panel. Reverse regression was another example of my being stimulated to work on a topic by being bothered by evidence that people had presented. It was pretty clear to everybody that any reported finding of discrimination based on conventional regression of salary on measured characteristics of the individual and gender, say—any analysis like that—would be subject to the objection that you'd left out some variables. That's exactly what was going on in the courts at the time. The employer's defense to a *prima facie* finding of discrimination, that is to say a gender effect after controlling for some variables, was that some other relevant variables had been left out. That was perfectly respectable.

But then this idea came along that the right way to estimate discrimination was to run the reverse regression, that is to say to regress qualifications on salary and gender. The formal justification was based on a single qualification variable—a very simple model with an error in the explanatory variable. There was this old notion that when you have an error in only one of the two variables, then that variable ought to be on the left-hand side. But that reverse regression should be *the* right way to avoid all bias is an astonishing notion. As it turned out it wasn't correct. The case of multiple qualifications is even more problematic.

Can we change the topic again here and move to your time at Stanford? You've spent 4 years at Stanford since 1972. Can you tell us what you worked on, and why Stanford?

As to why Stanford—my first teaching job was out there and I got married when I was first there, and both my wife and I like the climate. We had good memories from my early teaching days, and good experiences each time since. I haven't taught there since the 1950s. I went back the first time because we just wanted to spend a year in that climate again. That was 1972, and I've already told you about the IQ work I started on. There are people that I got acquainted with at Stanford, not just in economics, but also Ingram Olkin, the statistician. He and I did some joint work. I talked a lot to Larry Lau that year. We decided to go back again, and during the years 1976 and 1980, I was at the Center for Advanced Study in the Behavioral Sciences. That's a very unusual environment in which you have social scientists—all visitors, 40 to 45 of them—coming together for a full year. It's an interesting experience because you may know only one or two of the visitors when you come. The others are equally famous, but you have no idea who they are or what they work on, or why they are famous. In 1976 at the Center there were several psychologists and geneticists interested in IQ. That year I continued to work on heritability, so that turned out to be very productive. It's good to talk to noneconomists, and sociologists I find particularly easy to talk to, and it's often easier to do this some place else rather than at your own institution. So I worked on heritability, broadly interpreted because now it also included work on education and income that economists were doing using twin data. The last time I was there was 1985, which I spent at the National Bureau-West. I worked on hypothesis tests for one-sided alternatives and inequality constraints.

What is your view of current research on modeling macroeconomic time series?

I'm not informed on the subject, and my answer will be kind of platitudinous. My general concern about modeling with macroeconomic time series stems from the facts that there is relatively little data, and that the data have been hacked over and over again. I would find it surprising—I would be amazed—to see something new being learned from the analysis of that data. Is it really possible to keep extracting new things from the same old data sets? I used to think that one of the reasons for doing modeling was data reduction, that is, you have a massive batch of data and you want to boil it down into more manageable form. But there's a recent article using macroeconomic time series in a major economics journal. I counted the number of numbers that are recorded in that article—the coefficients and standard errors. There are many more numbers reported, not just more digits, more numbers, more figures reported than there were data observations—20 percent more.

A data expansion . . .

Data expansion is what it was.

Well, an interesting question, I suppose, is whether this transformation has an inverse so the reader with the estimates can recover the data.

A little unfair, because it had parameterized distributed lags and it reported the whole set of lag coefficients, but nevertheless I should think it has an inverse. Or alternatively you could just look up the National Income Accounts to find out what the numbers were.

I would like to have your view on the usefulness of the selectivity literature. There was a period in the late 1970s and early 1980s when every applied labor economist seemed to feel the need to correct for selectivity.

Yes. To get some perspective on this you have to go back to the early 1970s when the question of selectivity arose, certainly not for the first time, but it came up in the context of social experimentation or social economic programs, or educational programs, and specifically with respect to Headstart programs in which the finding had been that there was relatively little effect of Headstart programs. This was work done by educational psychologists. The response to that was that there had been a selection in the assignment—whether a student received the treatment or not—and specifically that the Headstart kids had been those who were taken from the bottom of the barrel. The treatment didn't seem to have a strong effect because it was being applied to kids who were in the worst-off situations. The evidence that was offered for that, that they were the worst-off kids, was simply that they were coming from lower socioeconomic backgrounds, lower parental educational backgrounds. The argument held that you couldn't do anything about this selectivity problem—it was always a legitimate objection to any quasi-experimental finding—a quasi-experiment being something that's not a randomized experiment. There was always a legitimate objection that there was selection and there was no way to do anything about it. You knew there was a possibility of bias, and that was tough, but nothing could be done. It was Glen Cain in particular, my colleague in labor economics, who was not persuaded by that response. In that context (and what is obvious now) what you want to do is model the selection process—this represented great progress. At least that's the necessary thing to do before discussing the direction of bias or trying to remove the bias. Take Jim Heckman's work, giving the first successful method of modeling the selection process, and then you may be half way there to eliminating selectivity bias. Maybe half way. At the time it seemed to some of us like maybe all the way there, if you model the selection process and make a few other assumptions. But, as Heckman realized all the time, there is a serious question about the robustness of the findings to alternative specifications of distribution or functional form. It may be hard now to realize what a tremendous contribution that was in shaping the discussion about treatment effects.

It seems to be going out of favor, in part, I think, due to the perceived sensitivity of results to minor changes in the specification. Would you comment on the selectivity literature, its sensitivity to specification, and its practical usefulness?

That's only in economics. I think it's only in economics where mechanical selectivity-bias adjustment is going out of style. There's the rest of the world out there. Well, first of all there's economics and there's economics—the economics of the top journal literature and then the economics of consulting firms and program evaluations. Selectivity bias adjustment is certainly strong there. But there's also the education and psychology literature, and the stuff is out there. Presumably it'll be another ten years before they stop or they slow up a bit. I think you often get a false impression of what econometrics is or applied econometrics is if you think about what you see in the top journals. There's an awful lot of stuff going on which is relying on tools that were popular ten, fifteen years ago. It takes a while for that—what is the opposite of diffusion?—that elimination to take place.

Do you have a definition of econometrics that you like? That you use in your teaching, defining the field?

No. What I do stress in teaching—which I covered today actually—is the concept of structural equations. The lecture today came at the stage of the course where we had finished discussing estimation of simultaneous equation models and had developed the relatively complicated methods for estimating parameters of the structural equations. At that point, you wonder what a structural parameter is and why you should want to estimate it. So you go back and look at some of the econometrics textbooks. You read sentences like these: "In the simple Keynesian model β is the marginal propensity to consume parameter, thus β is a crucial parameter and we are particularly concerned with the problem of estimating it as efficiently as the data permit." And "In a fundamental economic sense the property that characterizes the structural system is the assertion that it describes accurately the precise fashion in which all the current endogenous variables and predetermined variables mutually interact." Or "the structural form consists of a system of linear equations relating the current endogenous variables to the predetermined variables and the errors." I always thought that was the reduced form.

Or you read that the "structural parameters refer to the parameters of the original model," or that "the structural form of the model is the form that is derived from economic theory." In another book it says "Equations that describe the structure of the economic model are usually called structural equations, and the coefficients are called structural coefficients."

Well, it's hard to argue with that . . .

Right. But it's hard to find textbook discussions of the notion of autonomy which is the right notion for structural equations. That is, you can visualize one of the equations, or parameters, changing without all the others changing at the same time. That notion you find in the older econometrics books. It's harder to find it in the popular textbooks today.

I was surprised that only one of those mentioned causality.

I'm not surprised. They'd have to explain what causality is. But then even if you go back to the older books—treatises—and look at Marschak (1953), which of course is the Lucas critique. . . . Did you know that? Lucas footnoted it. That is to say, the notion that you want parameters that have autonomy and hold up despite policy changes. But even Marschak has something like "structural equations must refer to individual agents in specified markets. However, to reduce the model to a manageable size, variables referring to single individuals in finely subdivided markets must be grouped with aggregates." Students read the textbooks, I think, but instructors often don't read the book to see what it really says.

What do you give them as a suitable definition?

Well, without trying to define it, I give them the notion that an individual structural equation has to have a life of its own. They have to be able to visualize a world in which that equation stays the same while the other structural equations have changed. After all, if you can represent an economy or some sector by a simultaneous equation model, you can also represent it by its reduced form. The notion that you need the simultaneous model because "the world is really simultaneous" doesn't make any sense, right?

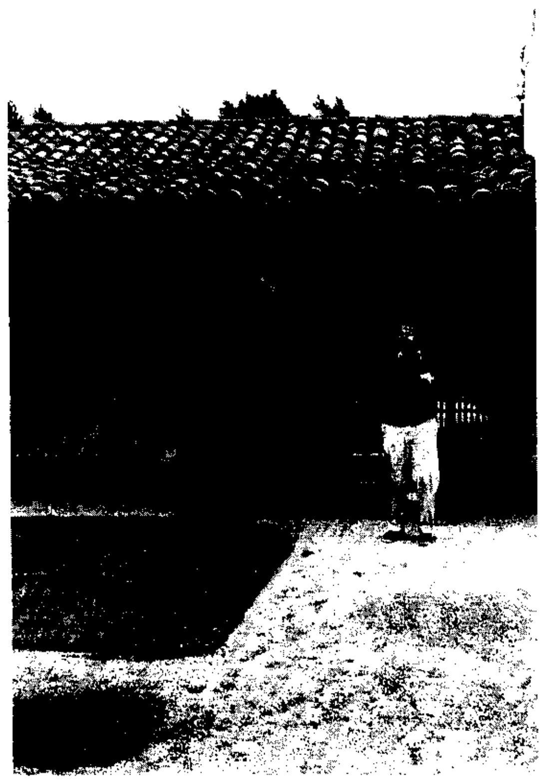
Right.

Because you could just as well use the reduced form. And then why is a structural form more interesting? What does it do that the reduced form doesn't do? What it does is, allegedly, give you equations that could be changed separately. The demand curve could shift without the supply curve shifting.

What about the view that structure is really just nonlinear restrictions on the pi matrix?

Well, that's one position, that the entire content in a structural model is simply in the restrictions, if any, that it implies on the reduced form – that's true. That gives priority to the reduced form. There is another notion, that if one structural parameter changes, a lot of parameters will change in the reduced form. But the rest of the structure won't change. That's a distinct idea from the restrictions. And, of course, the concept of autonomy is not confined to simultaneous equation models.

I do think there's some tendency to introduce simultaneity when there's no reason for it, when a multivariate regression model would suffice. You have exogenous variables and you have endogenous variables and that's the way it ought to be, but there's some effort to invent a simultaneous structure. That is to say, you feel obligated to have the endogenous variables depending



California 1958

on each other because, after all, they are determined jointly, and so you retro-fit equations that have no plausible autonomy.

You might try this out some time, if you teach linear simultaneous equation models and use the simple two-equation Keynesian model as an example. When you're finished with that, go back and ask the students, what is β in the consumption function (that's the structural parameter)? They'll say that it's the change in consumption when income changes by one unit. But of course it's not. The expected value of consumption given income in that model, after all, is a different linear function of income. It's the thing that least squares will estimate, if you let it. The slope in *that* linear function is obviously the right answer to the question: If income changes by a unit, what's your guess as to what happens to consumption? That's β^*. So the question is, what's β? I think that can generate a good discussion.

That's a good exercise. So, you're not going to give me a Goldberger definition of econometrics?

Well, what I wrote in my 1964 text was "Econometrics may be defined as the social science in which the tools of economic theory, mathematics, and statistical inference are applied to the analysis of economic phenomena. Its main objective is to give empirical content to economic theory; econometrics in fact encompasses a wide range of activities aimed at accomplishing this objective." But nowadays my definition would be that econometrics is what econometricians do.

What fields in econometrics do you see as currently the most promising research areas?

One is nonparametric, or weakly, parametric modeling, that is an approach which provides the ability to do statistical inference without excess, *ad hoc*, assumptions on functional form or on distributions. And the revival of the method of moments is an exciting thing. It offers a lot more flexibility. It is a revival of course . . . it goes back to Pearson, at least.

That's an interesting comment. I tend to like to do things in terms of the likelihood function and think in terms of probability distributions for data.

Well, going beyond the regression, that is, beyond expected values, beyond the conditional expectation function, that's important. But you can get at a probability distribution without the likelihood function by considering several moments. One of the other active fields considers the other features of a distribution, like the conditional median function. Those are estimable by various kinds of methods of moments, all without looking at the likelihood function.

When did you first encounter Bayesian ideas? Why did you not find them persuasive?

I'll tell you when I first encountered Bayesian ideas—this you're not going to believe. Theil and I did a paper on pure and mixed statistical estimation, which spells out how you can combine prior information that a parameter lies within a bounded interval with information from the sample to get a better estimate than you would get from the sample alone. After that was published, somebody asked me why we didn't mention Bayes and I said, who? Now I'm sure Theil knew who Bayes was, and even I had heard the name, but I had no idea about Bayesian theory. Why didn't I find Bayesian theory persuasive? Well, in a sense everybody's a Bayesian, we use information beyond the sample. The question is whether inclusion of prior information should be formalized. Formalizing may sound like a good idea, but maybe it's not a good idea. I like Manski's argument, which I will paraphrase. The objection to classical statistical inference is that it's nonsense. But everybody knows that. So if you follow the style I use, which would be classical statistical inference, you don't take it too seriously—you don't take it literally. If you use a Bayesian procedure, I get the impression you really have to believe the implications—you've already committed yourself to everything. You don't have the opportunity for waffling afterwards because you've already put everything in, and you have to take it literally from there on out. That may not be a virtue, but a disadvantage. Of course, a deeper reason for *my* not adopting a Bayesian approach was the computational and mathematical complexity of incorporating prior information.

Did the comments on the Theil-Goldberger paper on pure and mixed estimation lead you to read about Bayesian ideas?

It was either that or my colleague at the time, Arnold Zellner. I think I found the approach too technical.

Looking back over the last few decades, would you comment on what you feel have been the truly major advances?

I would think the—this is not quite econometric theory—the development and broad availability of micro data bases is a major advance. It's a different world than it was 30 to 35 years ago. The computing facilities changed dramatically, of course. The fact that we have gotten away from the linear simultaneous equation model as the end point of econometrics. . . .

The first two items you mentioned, the computing and the micro data in a sense are exogenous in the field . . .

Those were exogenous to the field. Perhaps the third was more endogenous—I like to think there is more willingness, for example, to look at latent variable models. That's really coming from Milton Friedman, who showed the power of a latent variable model to reconcile conflicting evidence. The fact that you can work with unobserved variables—do you see that as one of these big advances? Perhaps you don't have a picture of what econometrics was like say in 1965. . . .

I'm not sure that I really do because these days, due to your work and Anderson's, we think of the simultaneous equation model as an unobserved variable model, so it all seems to fit together.

Yes, that's true. That's really Ted Anderson's work. You know, he and Herman Rubin wrote the classic papers on simultaneous equations estimation, and shortly after that they wrote the classic paper on factor analysis, and they knew very well at the time they were the same. This is about 1950. But the development of econometrics got away from multivariate analysis. I think that was the introduction of two-stage and three-stage least squares that pulled simultaneous equation estimation away from being a theory of restrictions on a pi matrix, which would have kept it as a special case of multivariate regression.

Could you comment on the major weaknesses in econometrics over the years, especially continuing weaknesses?

Covering applied econometrics?

Sure.

I'm bothered by some trivial things such as reporting coefficients to 6 decimal places, which occupies journal space, whereas a physicist or chemist would report 2 digits. We report 6 because it looks more scientific and because the journals will publish it, even if it's based on 2-digit data, and even though half the digits are leading zeros. There is some pretension in applied economic research in that it claims to have much stronger findings than are seriously there. Related to that is a weakness in econometric theory—we really don't have a good theory of how to do empirical research when you're doing model selection at the same time. Ed Leamer is one of the few who writes about the issue. What guidance do you give to someone doing empirical research in the case where there's no tightly defined economic optimization model that everybody agrees is relevant? Perhaps other fields, or data analysts, do a better job at model selection. You read a journal article and you know that there's been a lot of fishing and you're skeptical of the results reported. And when somebody says "this is the best of the 25 equations I fit," you don't take that as a positive recommendation. But we really don't know how to guide somebody properly on the interpretation of the final results.

As a field we miss out a little bit because we don't do decision theory at all. If one thinks of econometrics as an intersection between statistics and economics, decision theory would fall naturally to econometrics, but in fact it seems to be the province of theorists. Do you think that econometricians would be better off as a field if we trained econometricians in decision theory?

Possibly. But we're going to have a problem, namely, what is the cost and benefit of a research finding in economics? If a policymaker is going to use results seriously, then I can visualize how much money it would cost if I fouled things up. But the way it is now, the output of the process is an article published in a journal. Apart from the benefits to the individual of the article being published, and the cost of writing it up and getting it published, it's not clear to me what factors should enter into formal decisionmaking. The only place we get close to decision theory is in hypothesis testing, and that's not a good scene. It's a bad scene altogether.

Looking back over your own work, what articles emerge as your favorites?

I thought about this question in advance so I'm going to mention four that I really like. There's a 1971 *Econometrica* article with Olkin on minimum distance estimation which shows the numerical equivalence of maximum likelihood and minimum distance estimation for a single structural equation in a linear simultaneous equation model. It's a short note, and quite elegant – the elegance is all from Olkin. What happened was that I had derived it by very clumsy means and sent a copy to Olkin. He wrote me a letter back sketching the right – the elegant – way to prove it and that ended up being a joint article. Then there's my 1972 paper on structural equation methods in the social sciences, which is an article in praise of Sewall Wright. This was an invited lecture of the European Econometrics Society Meetings in Barcelona. What I did there was propagandize for econometricians talking to sociologists, who were already working with latent variable models. It had some historical material, and it also had a somewhat nonscientific literary quality which I liked. My all-time favorite is the 1979 *Economica* article on heritability in which I summarized my work on the genetic determination of IQ and the genetic determination of income. There's just one more paper on my list, which is "Linear regression after selection," published in 1981, but circulated several years earlier.

How did you pick your research questions? Is there a "Goldberger approach" to econometrics?

Well, nowadays I pick research questions when I get irritated. I have a nitpicking style. If there's something that irritates me, or if I find something pretentious in somebody's interpretation of results, I may feel compelled to look into the topic. And I work on something if it's fun – I think that's an important consideration. The IQ work was certainly stimulated by irritation.

So was the work on the Coleman Report which Glen Cain and I wrote up a few years ago. This is a report on public and private high schools, and the conclusion being pushed was that private schools were much better than public ones. Consequently, we should be giving school vouchers or tuition tax credits to encourage private education. It was highly politicized at the time. Both Glen and I were suspicious of drawing such a clear implication from an uncontrolled experiment, particularly when there's selection going on. That is, presumably the kids who go to the private high schools are sent there by their parents, who are pushing them—providing an environment that's more stimulating to education.

Nowadays, I seem to be doing secondary analysis of major empirical research and trying to pick holes. Well, I started out as an accountant, and I've ended up as an auditor. I'm obviously not a major empirical researcher in the sense of doing primary data analysis. I'm not a high-powered econometric theorist, so I work in the middle. Literally, I haven't done major empirical estimation, that is, taken a raw data set and analyzed it, for at least 15 years now.

Your introduction to functional form and utility referred to an upcoming study of a worldwide demand. . . .

Yes, Ted Gamaletsos, my research assistant, and I published an article in the *European Economic Review* on the cross-country comparison of consumer expenditure patterns. We used aggregate level data for about 15 countries using Stone-Geary and indirect addilog specifications. That's one of the largest data sets that I've personally been involved with.

Although your students have processed millions of observations. . . .

Right.

Let me follow up on an earlier question and ask you what you regard as currently the really important open questions for econometricians to work on?

The one I mentioned, which is how do you actually do empirical research when you don't begin with a tightly written optimization model. You are trying to learn something about behavior from the data set: What's the right way to do that? I think people who do empirical research know a lot more about the right things to do—the right tracks to follow—but it's hard to formalize that. There are certain empirical researchers whom you trust. What is it that they do differently than other people? That's not a specifically econometric question, but it is a statistical inference question.

I guess I want to see if I can sharpen that and see what advice you would give new junior econometricians starting out in independent research.

Well, I'm inclined to say that they should avoid narrowness and be receptive to different approaches, especially from the empirical side. But that may just be a reflection of my own tastes. Maybe narrowness – or specialization – is desirable at an early stage? The breadth can always come later on.

I have a question that my colleague George Jakubson suggested for you. Some areas of current econometric research seem to have little to do with applied work. What kind of relationship do you think there should be between econometric research and applied work?

I'm not bothered or disturbed by that lack of connection. There are econometric theorists around whom you're going to need sometime – it's the same with mathematical economists – and you want to keep them happy. If they want to do their thing you have to let them do their thing.

Your former students rave about your teaching. . . .

Even my present students do. . . .

Do you have a philosophy of teaching?

No, I just work hard at it, and prepare heavily, and put a lot of time into it. Obviously you want to convey the interpretations as well as the mechanics. I lay on the mechanics heavily and the students begin to realize that, even if they're only interested in interpreting the results or applying the results, it helps an awful lot to master the mechanics. I try to have something for everybody, going through theoretical details, but also giving empirical examples. I think it's just mostly hard work.

How do you see the position of economics in the social sciences?

It's right in there. Not as dominant as we'd like to think. There is an economic style of thinking that's very useful. As far as the seriousness with which data are taken, we're behind sociology on that. Economics has nice things about it: cleverness, use of theory, but we do put a premium on cuteness in a way that other social scientists don't.

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