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THE ET INTERVIEW: PROFESSOR ARNOLD ZELLNER

Interviewed by Peter E. Rossi

Arnold Zellner stands out among econometricians as distinctive in the breadth of his contributions to many different areas of econometrics. His pioneering work in systems of equations, Bayesian statistics and econometrics, or time series analysis would each have earned him worldwide recognition. In spite of a prodigious volume of theoretical work, Zellner retains a strong interest in applications which has fostered applied work in fisheries conservation, production theory, forecasting, and many other fields. His record of service to both the econometrics and the statistics profession includes such impressive achievements as the founding of two major journals, organizing two NBER/NSF seminar series, and the supervision of over thirty Ph.D. dissertations in economics, finance, econometrics, and statistics.

As a newly minted Ph.D. in economics in the late 1950s, Zellner set to work on the important problems of estimation and hypothesis testing in systems of equations. He soon produced fundamental work on the seemingly unrelated regressions (SUR) model and three-stage least squares. His 1962 article on SURs is one of the most cited articles in econometrics (Arnold often jokes that if he had a nickel for every SUR fitted, he would be a very wealthy man). His work on systems of equations made him aware of the limitations of large-sample theory. This interest in finite-sample results led to a close study of the multivariate analysis literature and contributed to Zellner's attraction to Bayesian analysis in which exact results are possible.

In the early 1960s, Zellner embarked upon a research program to evaluate the usefulness of the Bayesian approach. Since Bayesian statistics was not very well-developed and Bayesian econometrics was practically nonexistent, Zellner developed the field by tackling, one by one, most of the important econometric models. His classic 1971 book, modestly entitled *An Introduction to Bayesian Econometrics*, provides Bayesian solutions for many important estimation problems in econometrics. In addition, the book provides important guidance in the philosophy of science and new approaches to hypothesis testing. More recently, Zellner has added to this line of research by providing exact sample results for the simultaneous equations model through what he calls the "direct Monte Carlo simulation approach," Bayesian and non-Bayesian treatment of regression models with nonnormal errors, and a MELO estimator for the simultaneous equations model which has impressive sampling properties.

Participation in a time series seminar as a visitor to Yale University in the late fifties fueled Zellner's interest in time series analysis. Along with Franz Palm, Zellner established the close link between multivariate time series models and dynamic structural equations models. This link provides new methods of estimation and diagnostics for structural econometric models. It also anticipates much of the linear-quadratic rational expectations literature in which economic models impose nonlinear cross-equation restrictions on vector autoregressions. A keen interest in the problems of seasonal adjustment led Zellner to organize two influential conferences on seasonal adjustment of time series for the Bureau of the Census. Recent projects on forecasting international output series and forecasting turning points continue Zellner's time series research.

Zellner's commitment and service to the econometrics profession is well-known. What is perhaps less well-known is his commitment to his colleagues and students. Colleagues can expect that their papers will be read carefully and that they will receive many insightful and useful comments. Students visit Zellner's office in Rosenwald 205D and return brimming over with many ideas, any one of which, if pursued vigorously, could develop into a Ph.D. thesis. Once a paper or idea impresses Zellner, he will urge his colleagues to learn about it so that the progress of econometrics research may continue.

The hallmark of Zellner's career is a commitment to scholarship, service, and the belief that complicated problems can be solved by the application of a few powerful, simplifying concepts.

It is my privilege to report this interview to you. The interview took place in Arnold Zellner's office at the Graduate School of Business, University of Chicago on May 31, 1988. I have organized the questions into four major categories: 1. Professional Career; 2. Simultaneous Equations Models; 3. Philosophy of Science and Statistical Methodology; and 4. Bayesian Methods.

1. PROFESSIONAL CAREER

How did you get interested in econometrics? What teachers and/or papers were influential in attracting your interest to econometrics?

A "few" years ago when I was a graduate student in physics at Berkeley, I became acquainted with some of my brother Norman's graduate student friends majoring in economics and agricultural economics. From discussions with them, I became convinced that there was a great opportunity to apply mathematical and quantitative techniques in the analysis of economic problems. Norman, who did a Ph.D. in agricultural economics at Berkeley, was very helpful in orienting me with respect to the quantitative study of economics, a subject which was not very well-treated in the four undergraduate economics courses which I took. I've always had a strong interest in economics and business and perhaps if my undergraduate courses had been more quantitatively oriented I might have switched from physics to economics much earlier. But I did make the change later at Berkeley in the 1950s. George M. Kuznets, my thesis chairman, and Ivan M. Lee of Berkeley's Department of Agricultural Economics, and Norman Buchanan, Robert Dorfman, Robert Gordon, and Walter Galenson of Berkeley's Economics Department were influential in stimulating and guiding my interest in economics and econometrics.

The switch that you refer to is between your undergraduate major in physics and graduate program in economics or did it occur after your graduate study in physics? How did that work?

I had had about a year and a half of graduate study in physics at Berkeley before switching into economics. Also, when I was in the army, I worked for a year and a half in biophysics and then came back to finish my graduate studies in economics.

The switch from physics to economics at Berkeley wasn't too hard because I'd had four economics courses as an undergraduate and a considerable amount of math which enabled me to enroll in graduate economics courses. At that time not many students in economics took mathematics or statistics. Anyone who knew a little math, I think, had a decided advantage.

What did you take in statistics as an undergraduate? Did you have statistics courses as part of your physics major?

Physicists were, in fact still are perhaps, babes in the woods when it comes to statistics. In physics you weren't required to take any statistics courses at all. I did get into statistical thermodynamics and statistical mechanics where you do direct probability in maximizing entropy and things of that sort. But when it came to analyzing data, we had no training at all except for a few laboratory sessions introducing us to probable error which few fully understood. Least squares was for me a calculus problem essentially. We had no formal statistical inference training at all. I picked that up in graduate econometrics and statistics courses. These courses seemed easy in one respect, namely, that most everything was assumed normally distributed without much controversy. In physics, theory is used to derive the forms of distributions.

How would you contrast the working atmosphere at Washington, Wisconsin, and Chicago (the three schools at which you have held appointments)?

These three institutions are impressive universities and all of them fortunately provided me with complete freedom to do the research that I wanted to do for which I'm most grateful. The most significant difference I think among the three institutions is that both Washington and Wisconsin had very large undergraduate programs, whereas Chicago is mainly a graduate institution. That I think is the biggest difference. At the University of Chicago, I am very fortunate to have had the opportunity to interact with many colleagues in economics and business since most are directly involved in research and don't have heavy undergraduate teaching loads. You get more of a research atmosphere in economics here at Chicago perhaps than at other places. What I like about Chicago particularly, and it's true too at Washington and Wisconsin, is that there's a strong interaction between theory and application.

Very few econometricians find their home in a business school. What particular advantages or disadvantages has this afforded you?

It's hard for me to generalize since I have had a position in just one business school since 1966. I think our school is unique in emphasizing the discipline approach to business and economic problems and we have, as you know, various disciplines very strongly represented in the school – economics, econometrics, statistics, management science, and behavioral science. Having these various disciplines represented in the school has had a very beneficial broadening effect on me. I think that's rather unique with our School of Business. Then another feature of Chicago's School of Business which appeals to me very much is its interactive nature. Since being here, I've had very close interaction with the Department of Economics, particularly in the Doctoral Econometrics Program. We've had a joint doctoral program in econometrics for many years and many of my students come from the Economics Department since all my courses are cross listed. At Chicago's School of Business, I think it's almost like having an appointment in a University within the University. It's a very exciting environment. We have financial economists, labor economists, microeconomists, marketing researchers, accountants, and others. My interaction on econometric topics with colleagues in different areas has been very stimulating.

Is there any one place or time in your career which you single out as the most productive or stimulating?

I've been fortunate in the sense that almost always I've had a number of projects underway about which I've been very enthusiastic. Some people call me an optimist because I'm very optimistic about getting results on almost any project. However, there are a few projects which stand out that were really exciting for me. One of them was the two-year study that I did at the University of Washington with Jimmy Crutchfield in the 1950s to evaluate the world's most famous fishery conservation program, the International Pacific Halibut Fishery Conservation Program. That was a very exciting and useful project. Then in the early 1960s at Wisconsin and at Chicago over the years, I've worked on a comparative evaluation of the Bayesian and other approaches to econometrics and statistics. That has been a very exciting experience involving interaction with a number of colleagues and students. Also, while at the University of Washington, I got the idea of writing systems in single-equation form and that gave me the seed for the idea of the seemingly unrelated regression paper that I did later and the three-stage least-squares paper that I did later jointly with Hans Theil. I had these ideas expressed in a paper in the IER in 1961 and took a year's leave as a Fulbright professor in the Econometric Institute and the Netherlands School of Economics in Rotterdam for a year. During that year, I had the good fortune to complete the work on the seemingly unrelated regression problem and on three-stage least squares. These were several very exciting experiences. Another one was in the construction of the Susquehannah River Basin Economy Model in the 1960s done in consulting work with the Battelle Memorial Institute. And then a very stimulating research experience was the joint work I did with Franz Palm in the early 1970s in the time series area. We emphasized and analyzed the link between statistical multiple time series models and dynamic structural econometric models and suggested a synthesis of the two approaches. I still find it a very exciting topic and I'm currently working on it. These are some of the items that come to mind but there are many other projects. Current topics involve work on a new derivation of Bayes' Theorem and its interpretation, information-processing, and our international forecasting and modeling project in which the SEMSTA approach that Palm and I developed is being used along with Bayesian shrinkage and turning-point forecasting techniques.

It is very rare that one person should have been involved with the founding of two major journals in econometrics/statistics, the *JBES* and the *JE*. Why did you co-found the *JE*? Why did you found the *JBES*? In particular, how is the mission of the *JBES* different from that of *JE*? Has the focus of *JE* remained true to your original intent?

As regards the *Journal of Econometrics*, Dennis Aigner and I founded it in the early 1970s mainly because we thought that a journal devoted to econometric statistics and econometric statistical methodology and applications was needed. Some journals in the econometric area had come to stress mathematical economics at the expense of econometrics. We thought there was an opportunity to develop a new journal to reflect work in that area—a growing amount of work, I should add. Currently, we are very happy with the journal. The journal has grown in size. We now have three volumes a year including special annals issues that treat specific topics in depth.

As regards the *Journal of Business and Economic Statistics*, the idea to found this journal emerged from two very successful applied time series conferences, one on seasonal analysis and adjustment and the other an extension of the first. These were held under the auspices of the Bureau of the Census, the American Statistical Association, and the National Bureau of Economic Research and brought together leading researchers from academia, industry, and government. These fruitful conferences and the wonderful papers presented at them suggested to me that this sort of interaction would be useful to foster, particularly in a journal devoted to a combination of theory and application. That was the way the *Journal of Business and Economic Statistics* got started. It took a couple of years to convince the ASA to found the journal and since it's been founded it's been successful. I take great pleasure and enjoyment from innovating, in general, and in setting up these two journals. It involved a lot of hard work with a good deal of satisfaction in seeing them started and quite successful.

2. SIMULTANEOUS EQUATIONS MODELS

It would be fair to say that your work on the SUR model is one of the most frequently cited and used techniques in econometrics today. How did you get the idea for SUR?

The origins essentially arose from my efforts to understand multivariate analysis; in particular, the multivariate regression model and other multivariate systems. As is usual when I try to seek understanding, I try to simplify problems. On a rainy night in Seattle in about 1956 or 1957, I somehow got the idea of algebraically writing a multivariate regression model in single-equation form. When I figured out how to do that, everything fell into place because then many univariate results could be carried over to apply to the multivariate system and the analysis of the multivariate system is much simplified, notationally, algebraically, and conceptually. I first applied the idea to the analysis of autocorrelated errors in multivariate regression in a 1961 IER paper, and then I noticed in analyses of panel data, in contrast to the traditional regression model, that different matrices of independent variables appeared in each equation. On applying generalized least squares to the system, it turned out that the generalized least-squares estimator differed from the equation-by-equation least-squares estimator. That was the tip-off that you could get some added precision in analyzing the equations jointly rather than one by one. The only fly in the ointment was that the generalized least-squares estimator for the system involved the unknown covariance matrix of the error terms. However, by substituting a consistent estimate, that provided a large-sample solution to the problem. Currently, Bayesian methods provide finite-sample solutions to SUR estimation, prediction, and testing problems.

On the whole, it is fair to say that the SEM has attracted a great deal of econometric research following your early work on 3SLS. However, linear SEMs are not very widely used in applied economic research. What accounts for the declining use of SEMs?

For many years, considerable attention has been given to the linear simultaneous equation model, theoretically and in applications. And at present, linear supply and demand systems are used to analyze markets quite broadly and there are many other problems that come up in the linear simultaneous equation form. Also, as you know, there have been some major developments in the analysis of the SEM system from the Bayesian point of view. Now through the use of direct Monte Carlo simulation and Monte Carlo numerical integration, we can provide finite-sample analyses for SEM systems. So there's been a fair amount of continuing work on those systems. Also what you call a linear simultaneous equation model is linear in the structural coefficients and error terms but it's not linear in the reduced form coefficients. You have the unrestricted reduced form subject to some nonlinear restrictions involving the structural coefficients and the reduced form coefficients. When we look at this simultaneous equation model in that form, it is in the form of certain rational expectations models. Some rational expectations models are in fact forms of the linear simultaneous equation model. And then there's an overlap between the linear simultaneous equation model and errors in the variables models. Thus, I don't think it's entirely accurate to say that interest has declined in the general SEM system but that new variants of the SEM have come to the fore.

Do rational expectations models pose unique econometric problems? Or are these models simply special cases of nonlinear regression models?

As I pointed out before, there are certain rational expectations models that are special cases of nonlinear regression models. In particular, you can view some of these models as unrestricted reduced form equations subject to nonlinear restrictions and that's exactly the form of the linear simultaneous equation problem. So in part, I agree with you that there are cases in which that can happen. Do rational expectations models pose unique econometric problems? I would say definitely yes—more on formulation of the models than on the statistical implementation, though on both scores. For example, many rational expectations models assume that everyone holds the same expectations. Then too, often it is assumed that everyone is in agreement about what is the true model. However, these assumptions have been relaxed in certain analyses. Some have allowed for dispersion of beliefs. Others have allowed for the possibility that several models are entertained. If you go ahead with the problem allowing for dispersion of beliefs and the possibility that several models are entertained rather than just one, I think one will have to consider the use of prior distributions and posterior probabilities on models and then combine results by averaging over beliefs and over models. That will involve new econometric methodology which will draw heavily on the Bayesian model selection literature.

3. PHILOSOPHY OF SCIENCE AND STATISTICAL METHODOLOGY

You have often stressed the importance of simple models in scientific inference. How did you come to form this view? After all, you have been involved in several large-scale modeling projects.

To be more accurate, I have emphasized the importance of "sophisticatedly simple" models. Some simple models are just absurd. Thus, I refer to models that are "sophisticatedly simple." I have come to the view that sophisticatedly simple models are to be preferred because they seem to work better in explanation and prediction. I can list a number of relatively simple models that have performed well in explanation and prediction but I find it hard to find any complicated models that work well in explanation and prediction in any area of science. Let me give you a few sophisticatedly simple models that seem to work well. From physics, $s = 1/2gt^2$. That works like a charm. Another one is $E = mc^2$. That is very simple and works very well too. I can mention many more simple equations from physics that work very, very well. Now from economics, we have supply and demand models that are relatively simple. If it weren't for them, I think economists would be having a really hard time. Similarly, some consumption theories, for example Friedman's consumption theory, are very simple and work fairly well. Arbitrage relations are relatively simple and very important. When I look at economics and other sciences, I see a number of sophisticatedly simple models that work fairly well. I have yet to see one complicated model that works well in explanation and prediction. You mentioned my work on the Susquehannah River Basin economy. That was a big model because we had a lot of detail to take into account. We had to subregionalize the Susquehannah River Basin economy. We had many industries, a water sector, an employment sector, and a demographic sector. But we could boil that model down into nine equations. The workings of the model were very simple. We had much detail because we disaggregated by age group, by region, by industry, and the water sector was part of the model, and so forth. Even though we had a lot of detail, we tried to keep everything as simple as possible. The other project I worked on with a large model was one of the macroeconometric models of the U.S. economy. I urged the model builders to simplify their model and, to make the point very dramatically, Steve Peck and I did a paper in which we simulated the model. The simulation experiments produced a number of surprises. The model builders did not know that they had built into their model certain features that showed up in the simulation experiments. So there I tried to urge them to simplify their model and see what they gained or lost by complicating their model. Some years later, I was told that workers at the Federal Reserve Board developed a core model. At the time their large model had 179 equations. Their core model was much, much smaller and attempted to simplify the big, complicated model – 179 equations, many of them nonlinear stochastic difference equations – a very complicated model.

Some statisticians regard it as fundamentally impossible to test causal theories without the use of experimental data. For this reason, they regard empirical work in economics as irrelevant to the testing of competing economic theories. Do you agree that it is impossible to test economic theories using *one sample* of passively collected economic data?

I agree that if you have good experimental data and a lot of it, it makes testing theories much easier. I'm all for experimental data. There are a number of experiments that can be performed to shed light on the validity of competing theories, but as you say most of our data are passively generated by the system and it takes a good deal of skill to test alternative economic theories using one sample of passively collected economic data. Now if you just have one sample, I think the real task is to quantify accurately the uncertainty regarding competing theories. Given the information in the data, to what extent do the prior odds change as you analyze the data, and transform the prior odds to the posterior odds? For example, if you start one-to-one on two theories and the data move you to 1.5 to 1, you have a slight preference for one theory vis-à-vis the other. On the other hand, there is often no need to limit analyses to one sample. There's always the possibility of getting more samples. For example, in our current forecasting work, we have data for 18 countries and in the IMF-IFS data base they have data for 100 or more countries. So I think sometimes in testing theories we all tend to be lazy. We don't collect enough data. There is much more data out there many times than one would think. It takes a lot more work to collect the data and there's usually more than one sample of data relevant for testing important economic theories.

Many statisticians scoff at hypothesis testing as a means of testing model adequacy and advocate the increased use of graphic methods. Economists, on the other hand, persist in the parametric restriction approach to testing of economic theories. How do you stand on this issue?

First off, some of my best friends are statisticians and they often scoff at many things. The fact that they scoff at many things doesn't mean that what they scoff at is necessarily unsatisfactory. Some have suggested that instead of hypothesis testing that we all compute confidence intervals rather than test sharp nulls or precise nulls. Now you know the old joke about the statistician. He has his head in the refrigerator and his feet in the oven and says "on average I'm comfortable." Then he computes a confidence interval and says "I'm uncertain." Years ago, Jeffreys in his *Theory of Probability* discussed hypothesis testing at length. More recently, Berger and others in *JASA* (1987) and *Statistical Science* (1987) reexamine this issue. Many scientists, including physicists and economists, want to test sharp null hypotheses. Is the income elasticity of permanent consumption with respect to permanent income equal to one? Others want to test hypotheses such as that a variable has no effect on the dependent variable in a regression equation. Some people say these hypotheses are not realistic but that's really not a very sensible reaction. If a person wants to test such a hypothesis, he should have good methods for doing it. As you know, there are good methods for analyzing sharp nulls. Now if you have no reason for entertaining sharp nulls, then you don't want to be testing sharp nulls. You want to pursue an estimation approach and get the point estimate and standard error and measure the size of the effect. If the theory suggests a sharp null, then you should be able to test that very well.

I'm sympathetic with graphic methods but at present, graphic methods are really pretty much an art. What we're trying to do in econometric and statistic methodology is to turn art into science. So there's really great room for the development of scientific graphical methods—a theory of graphics. I think Persi Diaconis has attempted a contribution in that direction. There's a great opportunity to make graphic methods scientific, scientific in the sense that two people given the same data and priors will come up with approximately the same conclusions using satisfactory graphic methods.

You have always emphasized the close link between economic theory and econometric models. Doesn't the increased specialization of econometrics preclude this important mixture?

From Adam Smith's writings, I've had great respect for the effectiveness of specialization. There's no question about it that specialization can lead to enhanced output and production, but the question asks whether we want to forego a close link with economic theory and I guess that's what you mean by econometric models—applied econometric work. Is that what you mean?

Yes. I mean that the econometric specification should be closely tied to the economic theory. The economic theory should be aimed at generating an econometric specification, not just some sort of a loose suggestion of what variables would belong in the equation.

We have to distinguish a couple of situations here. In some analyses of data there is no satisfactory economic theory. We have this problem currently in certain macro areas. We go at the problem empirically. We do what you might call exploratory data analysis—to define models that forecast well. Forecasting and prediction are very important activities. If you can do those things well, it is quite an achievement. So we take it as an exploratory data analysis problem to find out what relationships work well in prediction using heuristic information about economics and so forth. Then when we have something that works well, we back up and try to get the economic theory to explain why it works well. So that's one situation where you don't have well-formulated and precise economic theory. You have a lot of background information about the subject but you don't have a well-formulated mathematical economic theory. And then exploratory data analysis of various types can dredge up all kinds of interesting facts and relationships that may work well in forecasting that theorists can then attempt to explain. This is emphasized in the structural econometric modeling time series analysis approach that Palm and I put forward. If you do have a well-formulated mathematical economic theory, it would be stupid not to take account of that theory in approaching the data, just to see if it works well would be of great interest. It really is a tremendous achievement to have a mathematical economic theory that works well in explanation and prediction. So its a very different situation to have a full blown, well-developed mathematical economic theory that works well in explanation and prediction. It would be very exciting to implement a theory like that. Now I think Friedman, to a certain extent in his book, *A Theory of the Consumption Function*, had a fairly well-developed, rather simple, model of consumer behavior and then implemented it with data throughout his book and made a number of predictions (eight or ten predictions near the end of his book) and told others what data to gather to test further implications of his theory. It's the only work in economics, I think, in which an author has made a list of predictions and told others how to perform the analyses and predicted the outcomes. So a lot depends upon the quality of the theory, the quality of the data as to what you do in applied econometric work, and one has to be very flexible. If the theory isn't there you can't insist on using theory. So there an exploratory data analysis approach could be very fruitful. But I do think, as I indicated, that a close interaction between applied workers and theoreticians is very fruitful in promoting the work of both. That's not to say that everybody should be involved in such work but perhaps a larger fraction of the profession should be involved in the close interaction between applied work and theory—of course not everybody.

4. BAYESIAN METHODS

How did you become a Bayesian? Are there any particular milestones associated with your "conversion" to the Bayesian philosophy?

the dynamic case you might expect different solutions to emerge. I've got some results along these lines where I allow for differing quality of the sample and prior information used in inference problems, and I'm currently working on other problems in this area. So my interest in the Bayesian approach hasn't been a religious interest. It's been a scientific interest and certainly motivated a lot by my curiosity to see which of several different approaches works better or best in solving econometric problems—a very pragmatic interest as well as an intellectual interest has motivated my work in this area.

What prompted you to write your classic book on Bayesian econometrics? Why did you find it necessary to write a book instead of articles?

At the time, Bayesian articles were published in many leading journals but scattered around the literature, and I felt they weren't having the impact that a book would have. Also, I felt that bringing together the results that we had at that time would be very useful for others so they could see and view the book as a report of our progress in the comparative evaluation of alternative approaches in econometrics. The book was in the nature of a progress report to the profession. I've always taken the responsibility to the profession very seriously. Since society is making funds available to support us while we're doing our research, it's very important to be accountable. So the book was an effort to be a little bit accountable—not too much, but a little bit.

What has changed in Bayesian econometrics since the publication of your book?

Of course we've solved many more problems from the Bayesian point of view, and the Bayesian solutions have compared very favorably with non-Bayesian solutions. Also there are many more Bayesian econometricians and statisticians now, including the Chairman of Harvard's Statistics Department, Don Rubin, the Director of the University of Minnesota's School of Statistics, Seymour Geisser, and the President of Carnegie-Mellon University, Dick Cyert. Thus, we have many, many more Bayesians now. Really in the 1960s, the number of Bayesians was very limited as well as the number of Bayesian publications. So that's a radical difference now as compared with 1971. The number of Bayesians has increased, the number of Bayesian publications has increased, the number of problems that are solved by Bayesian methods has increased, etc. It's a very different situation now than it was in the 1960s when Bayesians were very few and far between.

We now have more powerful computer techniques. That's perhaps currently the most important factor. We can compute posterior distributions for whatever problem we want by numerical integration or direct Monte Carlo simulation approaches and so forth. So progress on the computational side is very impressive and important. Earlier, we often had to use asymptotic

As I mentioned earlier, in the early 1960s I embarked on a program of research to evaluate the Bayesian approach to inference and decision making relative to other approaches. So I never got converted. I started work on the problem and if the Bayesian approach had produced silly answers or poor answers, I would have dropped it like a hot potato. Over the years (about 26 years, I guess), I found that the Bayesian approach has been very fruitful and useful. It's still not perfect but no approach to inference is perfect. You can always develop additional systems that may be better. But as it stands now, I think the Bayesian approach comes off very well relative to current competitors. As regards milestones in the development of my interest in the Bayesian approach, in the early 1960s I participated in a University of Wisconsin Statistics Department seminar devoted to a reading of Harold Jeffreys' book, *Theory of Probability*. As you know, he's a leading natural scientist who along with Laplace and some others viewed the Bayesian approach as appropriate for statistical work in all areas of science and produced Bayesian solutions to many statistical problems. This caused me to rethink my own approach to econometrics. Then in 1962, I stumbled onto the *JRSS* paper by Barnard, Winston, and Jenkins on likelihood inference in time series analysis which emphasized that a weighted likelihood approach yields exact finite-sample inferences for time series problems. I was very worried about asymptotic approximations even way back then in 1962, so seeing that Barnard, Winston, and Jenkins had produced this weighted likelihood approach had a great impact on me. Their weighting functions could be interpreted as prior distributions and thus you had a complete finite-sample analysis of time series problems. I thought it would be worthwhile to do more research to make explicit comparisons between Bayesian and non-Bayesian inference and decision techniques which I've done over the years. Pursuing this comparative approach has made my research exciting and interesting to many different parties, including myself.

Currently, the Bayesian learning model is one model of learning. The fact that the Bayesian approach has a learning model embedded in it is quite unique among systems of inference. But there is a possibility that one can improve on the Bayesian learning model and I'm currently trying to get some results in this area by considering optimal information processing, in which we have an information criterion functional and optimize it subject to side conditions. The solution produced using calculus of variations techniques is an optimal information processing rule. Under certain conditions, it turns out that Bayes' Theorem is an optimal information processing rule, but that's under static conditions. If you change the conditions of the problem to make the problem more dynamic, it is conceivable that other solutions may emerge and they may be improved learning models. So solutions can change just as they change in firm problems. Static optimization leads to certain rules for the firm. If you use a dynamic model with costs of adjustment built in, you get other rules for profit maximization. Thus, going from the static case to approximations because we couldn't compute posterior distributions. Now for many, many problems, we can compute finite-sample posterior distributions for parameters, predictive densities for future observations, optimal point estimates, posterior odds for evaluating models, hypotheses, etc. That's a big difference. On the conceptual side, I think people are much, much more sophisticated now than they were back in the 1960s about Bayesian analysis. At that time, you might say the field was rather thin and anyone with a decided viewpoint had an inordinate effect on the market—the thin market. By now we've digested a lot of the controversial issues about uninformative priors, about utility theory and the expected utility hypothesis, Jeffreys' improper priors, Jeffreys' and Laplace's approaches to hypothesis testing, and other topics. For example, "Is Jeffreys a Necessarist?" That was argued until people were blue in the face years ago. But now I think people are much more sophisticated about these issues.

Let me also add along those lines that your work on the MELO estimation approach in structural equation models was not yet developed in the book and I think it really fills an important gap that was not covered.

It is obvious that the work of Sir Harold Jeffreys has greatly influenced you. How did you discover Jeffreys' work and why do you find it so much more persuasive than the work of Savage and de Finetti?

As I mentioned earlier, there was a seminar in the Department of Statistics at Wisconsin devoted to a reading of Jeffreys' work with George Box, Irwin Guttman, George Tiao, Norman Draper, Mervyn Stone, and some others involved in the reading of chapters in the book. We spent a whole semester reading that book. I should add that I was assigned to report on the most difficult chapter of the book, the chapter on hypothesis testing, and I came away from that seminar very impressed with Jeffreys as a philosopher of science, an applied mathematician, a statistician, a mathematician, a geophysicist, an astronomer—a man of all parts, you might say. It was very impressive to me that a person could cover so many different areas so knowingly. You know Good has reviewed that book and stated that Jeffreys has more of value on the philosophy of science as it relates to probability theory than all the professional philosophers have written about the subject lumped together. This is in a review that appeared some years ago. So it's a very impressive book on the philosophical side, on the theoretical inference side, and then also on the applied side. He had what are now called M estimators in his *Theory of Probability* years ago. He had nonparametric Bayesian analysis in his book *Theory of Probability*—how to make inferences about the median of a population given independent observations from a population and his contributions and applications in a number of different areas have been very impressive. I also find very impressive Jeffreys' many contributions to different areas of science and his keen interest in applications along with the philosophy of science. As regards Savage and de Finetti, they came at the problem in the way that Ramsey came at the problem—through the expected utility hypothesis. They started with utility theory and developed inference based on utility theory. Jeffreys considered the Ramsey-Bayes approach and then in a few pages decided that, while you could develop the subject that way, he preferred to develop it in the way that he did with probability, a fundamental concept and then the axioms of probability theory giving the rules for manipulating probabilities and leading to Bayes' Theorem. Then at the end if you want to introduce a utility function you can do that and maximize expected utility. But Jeffreys and I currently find utility theory somewhat controversial—let's put it that way. Mark Machina and others have written about the current status of the expected utility hypothesis and it's not as secure I think, as Savage and de Finetti believed earlier. Jeffreys avoided this problem by introducing probability as a fundamental concept on which to build and avoided reliance on the expected utility hypothesis that's central in Savage's and de Finetti's work.

The most common objection to Bayesian methods is that some sort of prior is required and some regard this as a diversion from the task of data analysis. How do you answer these objections?

The task of data analysis is a pretty loose term. I don't know exactly what you mean by that. I've kidded some data analysts about their poking through data without theory as really going back to the stone ages. I mentioned the data analytic approach earlier. That is in need of theory really. Data analysis—Do you mean by that descriptive statistical analysis? Descriptive statistics?

I don't subscribe to this view but I think if I could paraphrase it, they would say that you have to introspect and produce this prior distribution or density and that time spent on introspection may be more profitably spent on plotting the data or fitting models to the data or something like that which is what I think they mean by data analysis.

Well Bayesians of course plot data or look at the data. They don't spend all their time thinking of priors. Also, I should add that non-Bayesians sit around thinking about restrictions on simultaneous equation models. That's prior information. Others think about what to assume about the error terms properties. That's many times prior information. Others sit around thinking about how to formulate a model for the observations. That involves a tremendous amount of prior information. Others worry about how to choose a significance level and the power of their tests which involves a lot of prior information. So the thought that non-Bayesians are not using prior information I find hard to believe. The statements of non-Bayesians, John Tukey, David Freedman, Eric Lehmann, and others indicate that non-Bayesians use a lot of prior information—judgment in the choice of functional forms for relationships, judgment in accepting "reasonable estimates," judgment in the assumptions about error terms' properties, judgment about the total form of the model, etc. And then let's take the non-Bayesians who work with time-varying parameter models or random coefficient models. They put a distribution on the parameters just as Bayesians put a prior distribution on them. Is the distribution of the random parameters a prior or is it part of the model? Well if you like time-varying parameter models and you put an ARMA process on the time-varying parameters, you're involved I think in formulating your prior beliefs about parameter variation. In multivariate regression, you may have regression coefficient vectors that you assume normally distributed around some mean vector. Is that part of the likelihood function or the prior information? Non-Bayesians worry about whether to assume the vectors normally distributed, independently distributed, or identically distributed, just as Bayesians worry about a prior distribution. In my opinion, the amount of time spent on prior information is probably not far different for Bayesians and non-Bayesians.

Recent papers by Diaconis and Freedman and Sims have convinced some that Bayesian methods have some difficulty when applied to nonparametric problems or problems with an infinite number of parameters. Do you agree with this conclusion?

Which conclusion? That they have convinced some?

No, I think we can agree with that.

Well the people they've convinced I don't think know much about nonparametrics and models with an infinite number of parameters. First off, tell me about one infinite parameter model that works well in explanation and prediction. Can you name one? I can't.

Well, people use histograms quite a bit.

For explanation and prediction?

Not in prediction.

I said both, explanation and prediction. Because those are two vital activities in science—explanation and prediction. Prediction is involved in definitions of causality. Prediction is involved in definitions of induction. And explanation is what we're trying to do—learn about how things work. I, frankly, can't think of any infinite parameter models that work well in prediction and explanation, in economics or any field of science. Now when some are faced with these models with an infinite number of parameters, the natural inclination is to put a hyper-distribution on the parameters and reduce the problem to a model with a finite number of parameters. As regards the implication of these articles for Bayes and non-Bayes controversies, it carries on in a tradition. If we go back a few years, we had the marginalization paradox. If we go back further, we had some other mathematical quirk. Over the years, it's been one mathematical argument after another purporting to show that the Bayesian approach is defective. It goes back to R.A. Fisher too, and others. And yet the Bayesian approach continues on. These arguments are, I think in part, contrived. Certainly, the marginalization argument as answered by Ed Jaynes is a contrived criticism of the Bayesian approach. Now let me take an example to illustrate what I mean. Let's take an infinite parameter model; the simplest model I can think of: $y_i = \theta_i + u_i$, where u_i is an error term, θ_i is the mean of y_i, and i runs from 1 to n. As n increases, the number of θ's increases. So when we have n observations, we have n θ's, with $n + 1$ observations, we have $n + 1$ θ's, etc. If we think the error terms are independently normally distributed with zero means and say constant variance, 1 or σ^2, the maximum likelihood estimate for each θ_i is y_i. It is also a diffuse prior Bayes' estimate of θ_i. Now this estimate, $\theta_i = y_i$, has the property that is inconsistent. That's the nature of these arguments that these people are putting forward. A procedure leads to inconsistent estimates, and, therefore, it's defective. In this case, maximum likelihood and Bayes lead to inconsistent estimates. I would say that the fact that they lead to inconsistent estimates is a virtue and not a defect in both approaches because you have just one observation per parameter. To have the distribution of the estimator become degenerate in the parameter value in a situation like this would be all wrong. Here the estimator for θ_i is y_i. You have just one y_i and clearly the estimator can't have a distribution degenerate into θ_i. It shouldn't, given this. So you can see that maximum likelihood and Bayes with diffuse or proper priors for each individual θ_i. leading to inconsistent estimates is a virtue rather than a vice and really leads to my greater confidence in those approaches.

Let's think about what I said before about putting a hyper-distribution on the parameters. Suppose y_i is estimated total income for an economy in a particular year, the ith year estimated by blowing up a survey estimate. θ_i is the true population income for the country that year, and u_i is the error term of the ith year, the year of the survey. θ_i is a parameter to the survey statistician, total income. What do we do in economics? What do macroeconomists do? They model the θ_i's. That's macroeconomics. So therefore they put hyper-distributions on the θ_i's (I hope successfully). So it's really very traditional to put a distribution on the θ_i's and explain the variation of the θ_i's through time. And that gets around the infinite number of parameters problem because then you can introduce a finite number of hyperparameters and make inferences about them. So as I say, I can't think of any models with an infinite number of parameters that have been effective in economics or any other science. Maybe you can think of some. I should add that Jeffreys discusses this problem in one of the sections of his book—adding a parameter for each observation. And he says that's not satisfactory because the law changes and with each observation you get a new parameter or if you go at it in terms of polynomial expansions, if you keep increas-

ing the degree of a polynomial as the sample size increases, the nature of your law can change tremendously from one polynomial to another. So unless you can convince me that there are virtues in applying infinite parameter models in explanation and prediction, I don't think I'll be tempted to use them.

Recent surveys suggest that the influence of Bayesian ideas has been increasing in both statistics and econometrics. However, in econometrics only a small fraction of applied work uses Bayesian methods. What accounts for the slow diffusion of Bayesian ideas into empirical work?

I'm glad to hear you say that theoretical and methodological Bayesian work has infiltrated econometrics to that extent. On the applied side, keep in mind that many of the middle-aged applied econometricians working today (middle-aged and above) were trained in a period when they rarely were introduced to Bayesian methods. That's one fact, that a large number of applied workers have never been exposed to Bayesian methods. Some of the younger people like Geweke, Litterman, and others are very prominent in applied Bayesian work. Sims has become quite Bayesian. He's doing applied Bayesian work. Leamer, for example, got exposed to Bayesian methods at Michigan and at Harvard. He studied I think with Bruce Hill at Michigan and at Harvard with colleagues there, Dempster, and others. So the younger econometricians have been exposed to Bayesian methods. Years ago, the textbooks had very little Bayesian material in them. Then another factor impeding some of the applied work is the lack of computer programs. Bayesian computer programs are really very much needed. In the last five or ten years, some rather good Bayesian computer programs have come on the scene. Litterman's RATS program has some Bayesian capabilities and is available on both mainframes and PCs. There's some limited Bayesian capabilities in the SAS programs. The time series group at Warwick has a Bayesian analysis in time series program, BATS. We've just finished a PC version of our BRAP program. Up until recently, the computing situation has not been favorable for wide-scale application of Bayesian methods. But in recent years, these programs have come upon the scene and many more people seem to be experimenting with Bayesian methods at the present, including the Federal Reserve Bank of Minneapolis which operates a Bayesian vector autoregression forecasting model due to Litterman. Now even in government circles, Bayesian methods are coming into use—at the Federal Reserve Board, at the Federal Reserve Bank of Minneapolis, the Department of Agriculture, and other places. The Japanese under Akaike have a Bayesian seasonal adjustment computer program. Thus, progress is being made.

Computing costs continue to decline at an exponential rate for the third decade in a row. What implications does this have for the future of Bayesian econometrics and econometrics in general?

For Bayesian econometrics, I think this will certainly speed up the adoption of Monte Carlo numerical integration techniques, direct Monte Carlo simulation techniques for computing posterior distributions and predictive densities, and will allow more interactive computing where users can experiment more with different models for the observations and different priors to go along with the model and be able to characterize model uncertainty better. I think improvements on the computing side, as I mentioned two or three times earlier, are key for promoting more use of Bayesian methods. As regards econometrics in general, the improved computing and also not just computing but new techniques for information storage, transfer and processing will have a great impact on the production of data and its utilization. You know years ago data were transcribed by hand and stored in books and written records. They were very inaccessible in warehouses, etc. Over the years the data storage, data retrieval, and the data gathering systems have all been computerized and that's going to have more of an impact on econometrics than the developments I mentioned earlier. Getting into data more easily, having more extensive data sets, data sets that can be stored readily and readily available, and having the capabilities to do on-line surveys or on-line field experiments will, I think, make econometrics much more effective. Getting further data generated that way and ready for use and able to be processed – that will be extremely important for the future of econometrics in general. I really think getting more and plentiful data of high quality is a high-priority item. As you know, I've worked in the last couple of years to try to promote improvements in data and accessibility of data. In the past, I think econometrics has really been hamstrung by the fact that we worked with very limited data. As you mentioned before in one of your questions, one sample of data oftentimes is all we have and the data may be highly aggregated, seasonally adjusted by some mysterious process with computed values in the series that you don't understand and subject to all kinds of systematic errors and from that series we're supposed to make good, precise inferences about whether theory A or theory B explains the data. Well, with improvements on the data side, we'll get much cleaner data and data with much more information. That I think will lead us to discover new facts, new features of economic behavior, and feedback on economic theory and give us a better understanding of economic behavior. So I really think improvements on the data front are really required and they will have a first-order impact on progress in econometrics as well as, of course, on methodological and theoretical improvements, too.

5. MISCELLANEOUS

Over the years, you have had a very large number of doctoral students in econometrics, economics, finance, and statistics. What has been your philosophy of doctoral education and why have you been so successful in stimulating large numbers of students?

I think the basic element of my philosophy is the importance of the doctoral programs in our system. Young people it seems to me, about the doctoral age, are just ready to make their most basic contributions to research and thus providing a setting in which they can work effectively on their research problems is extremely important. Doctoral programs should, in my opinion, enable a person to realize his aspirations in research. Here at Chicago, particularly, we're training research scholars and the doctoral program here should function to allow young people to realize themselves in research and if they plan to go on in teaching, in teaching. Now to do that you can't have a doctoral program with too many restrictions, as you know. Here in Chicago, we're very flexible. We try to keep the number of restrictions down to a minimum, prodded along by Harry Roberts who would like to eliminate all restrictions. Over the years, that's been a distinguishing feature of our program. It has very few requirements. We've been very flexible to allow students to do what they want to do.

Usually, students have general ideas and I think the faculty's responsibility is to give them guidance in trying to get these general ideas more precise and formulated in such a way that they can answer specific, important problems relating to their general area of interest. So as a teacher, as a thesis advisor, this is what I try to do with the students who work with me—try to find out what their interests are, try to lead them to the literature so they can see what others have done in that area, and then to help them formulate their research in such a way that it produces new, useful, and important results. What is often very important in this situation is confidence in what one's doing. It is very important to try to generate confidence in the student who has no experience or limited experience in research. They can't do the impossible (sometimes solving research problems does appear to be impossible) and keeping one's confidence up is very important. It's extremely important to have an optimistic outlook—I can solve any problem. I try to steer students to simple important problems—the problems that are important and simplest to solve. If you have an important problem that's very hard to solve, maybe if you have an equally important problem that's easier to solve, it would be good to start with that one and then work toward solving the other problem. The experience in solving one problem may be helpful in solving the second, equally important problem which is more difficult. Oftentimes, I suggest to students that they form a matrix of problems ordered by degree of importance and by degree of complexity with the hope there's a simple important problem they can solve and then I urge them to work on that. If you have an important problem that's easy to solve, you might as well work on it first.

Are there any problems in econometrics which you regret not working on?

Well, I've worked on a broad range of econometric topics and about the only one I thought was perhaps too hard or too difficult is the econometrics of world economies and that's always fascinated me, the relative growth of economies, some develop, others decline. Working on that range of problems has always had an attraction for me. I've thought those problems were very difficult and I really thought that my efforts were better directed toward simpler problems, ones that are easier to solve. But currently we are working on the international forecasting problem. We may get into some other problems that involve the world economy. I'm very excited about what we've done already in the international forecasting area—forecasting rates of growth of real GNP for 18 countries and forecasting turning points in the rates of growth of these 18 countries. And I expect, within the next few years, we'll push that work further into other areas. So maybe after all these years I'll get into the area in which I've had an interest since graduate school days. I did take courses in economic development with Norman Buchanan and just thought the problems were too hard at that time.

What recent articles or books are likely to emerge as especially useful or insightful in the future?

That's a hard thing to predict. So take these answers with a few grains of salt, perhaps many grains of salt. First, I really think that the work on Bayesian time-varying parameter state-space modeling techniques described for example in the West/Harrison/Mignon paper in JASA, March 1985, has already been important and probably will be more so in the years ahead. As you know parameters are not always constant in life. Aggregation effects, Lucas effects, and other things can cause parameters to change. At the macro level and even at the micro level, when we did our fishery conservation work, the conservation regulations caused the seasonal pattern of fish migration to change. Some of the parameters of the fishes' migration behavior changed. You can get changing parameters at the macro or micro level and having good models for coping with time-varying parameters, I think will be very important. Richard Highfield in his thesis work at Chicago used some time-varying parameter models in building a quarterly forecasting model for the U.S. economy. Then, second, I think that there will be some steps forward in improving our capability of learning from data. Currently, we do it mostly, particularly non-Bayesians, in an informal way. Bayesians use Bayes' Theorem as a learning model. Well, Robin Hogarth and the late Hilly Einhorn, our colleagues in psychology here in the School, and David Grether, Tversky, and Kahneman and others are studying how people learn from data. I fully believe that we'll get better learning models that will help us a good deal in econometrics and statistics. With better learning models, we'll be better able to learn more effectively from our data.

Then testing in econometrics is currently in terrible shape, I think. We don't test hypotheses very well. And I think the work of Berger and his students, Jeffreys' work in his *Theory of Probability*, and some of your work in the production area where you're testing alternative models for production—that sort of work will come to the fore in the future to be reflected in changed techniques for evaluating alternative models. Then too, the work on model selection that you've done and others have done which involves getting posterior probabilities on alternative models and carrying along several models rather than just choosing "the best" model is very important because as Draper and Hodges and some others have been emphasizing, just relying on one model when there's model uncertainty doesn't give you a correct measure of the uncertainty of your inferences. It's very important to allow for model uncertainty in measuring the precision of forecasts and estimating parameters. You get different estimates with different models. That uncertainty should be reflected in your overall standard error or measure of precision. I think that will be important. Let me get back to my favorite author, Harold Jeffreys. I have promises from more and more people, for example, Arthur Goldberger and his colleagues, that they're going to read the *Theory* *of Probability*. That more people will read the *Theory of Probability* and *Scientific Inference*, I think will really change the way they look at science in general and applied statistics and econometrics in particular. I think that Jeffreys' *Theory of Probability* and *Scientific Inference* will be among the most influential books in the future.

How can we improve the teaching of introductory econometrics and statistics so as to attract more interest in these fields?

With respect to introductory econometrics and statistics, I think most people have a keen interest in learning about the economy and economics in general if the subjects are presented in a reasonable way. With respect to econometrics and statistics, I think having really good data bases available for undergraduate students to plot and analyze would be very helpful and then too, the relationship of the data to economics is sometimes not well taught at the undergraduate level. Students are given theory they can't relate to data and data they can't relate to theory. If you can't bring the two together, you're not going to have effective econometrics at the undergraduate level. Bringing the two together really takes some work and I think would generate a lot of enthusiasm for both econometrics and economic statistics.

Now there is a question about how econometrics and statistics are taught at the undergraduate level and some claim, in particular, John Hey who has a new textbook out entitled *Data in Doubt: An Introduction to Bayesian Statistical Inference for Economists*, that teaching statistics and econometrics from the non-Bayesian point of view is "unnatural" to quote his preface. In his teaching experience over the years, he found the results very unsatisfactory until a few years ago when in desperation, he changed to the Bayesian approach. He claims that the Bayesian approach cleared up most of his problems. The students found it natural and took to it with great success. Whether that's specific in his teaching experience or general is a question in my mind. I really would like to do some experiments to check whether his experience is unique to him or is more generally encountered. I haven't seen those experiments done so I'll just leave it to your own prior views as to whether you want to extrapolate from his experience to the general population or not. Now this is a case in which you have to use a lot of judgment. But it is interesting to look at his book and to see how he's developed mainly a diffuse prior Bayesian approach to the standard problems of making inferences about means, proportions, simple regression, multiple regression, and introductory simultaneous equation models. He's taught that in upper division courses for some years now and the book is into its second edition, so apparently he's convinced some people that it works. I don't know if the conclusion holds in general or not, but it's interesting. Then getting the students onto PCs, I think will change the situation a good deal. That will enable them to plot data, do analyses more readily, and get into the subject much more easily. Extensive use of PCs will be a decided positive factor in undergraduate econometrics and statistics courses, in my opinion.

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