

A Conversation with Arnold Zellner

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Abstract: From the early 1960s onwards, Arnold Zellner has been publishing influential papers in the areas of statistical theory, econometric applications and macroeconomic modelling. This conversation canvasses Zellner's transition from physics to economics, the reason for the renewal of interest in Bayes's theorem in the 20th century, the empirical methodology of science underpinning the Chicago School and the influence of Alfred Marshall on Zellner's recent contributions to macroeconomic modelling. The main insights to have emerged in the course of the conversation centre on the historical influences on Zellner's thinking and his contribution to economic history.

1 Introduction

Arnold Zellner was awarded an *Artium Baccalaureus (cum laude)* in Physics from Harvard University in 1949 and a PhD in economics from the University of California, Berkley, in 1957. In the academic career that followed, he worked at the Universities of Washington and Wisconsin, but his most well known and enduring academic affiliation has been with the University of Chicago, where he was appointed in 1966 as the H. G. B. Alexander Professor of Economics and Statistics in the Graduate School of Business. Zellner has made many influential and pioneering contributions to the theory and practice of econometrics. These range from the international literature on Seemingly Unrelated Regressions (SUR), which emerged following publication of his article 'An efficient method of estimating seemingly unrelated regressions and tests for aggregation bias' (Zellner 1962), to his work on Three Stage Least Squares (3SLS), which followed publication of his joint paper 'Three-Stage Least Squares: Simultaneous Estimation of Simultaneous Equations' (Zellner and Theil 1962). It also extends to his influential work on 'structural econometric modelling, time series analysis' or SEMTSA (Zellner 1994), and the Bayesian approach to econometrics, which was rarely used until he highlighted its potential in a presentation to the *Econometric Society* on 'Bayesian and Non-Bayesian Approaches

in Econometrics' (Zellner 1971a).¹ On the issue of methodology and philosophy of science, Zellner is the lead advocate of a 'sophisticatedly simple' approach (Zellner 2001). His intellectual leadership within the econometric and statistical communities has been further accentuated by his extended service as co-founding-editor of the *Journal of Econometrics*, founding-editor of the *Journal of Business and Economic Statistics* and founder of the *International Society for Bayesian Analysis*. He has held several positions in the *American Statistical Association*, including *President* in 1991. He is currently a distinguished fellow of the *American Economic Association* and a fellow (and former Council member and nominee for president) of the *Econometric Society*.

In recognition of these and other contributions, three extended interviews with Arnold Zellner have already been recorded. The first, published in *Econometric Theory* as 'The ET Interview: Professor Arnold Zellner' (Rossi 1989), focused on Zellner's professional career, the origins of his work on SUR and 3SLS, the philosophy of science with respect to statistical methodologies and, of course, Bayesian econometrics. The second, published under the title 'Professor Zellner: An Interview for the *International Journal of Forecasting*' (García-Ferrer 1998), covered similar ground but put more emphasis on the forecasting problem, as distinct from the econometric testing of models in terms of fit. The third interview, published in abbreviated form in *Amstat News* (Morrissey 2006) and in full on the *Strategy2market* website, deals extensively with family and biographical issues as well as major intellectual influences, institutional influences and his recent work.

The purpose of this interview is not to restate matters already in the public domain. Rather, its intent is to sketch Arnold Zeller's views and experiences in a manner that highlights their historical dimension. In that regard, attention is given to his reaction as a university student to the history of economics and economic history. His views and, where appropriate, his contributions to economic history are also discussed. Other topics of discussion with an historical dimension include the Chicago School and its methodology of science and the influence of Alfred Marshall on Zellner's more recent macroeconomic modelling. The conversation concludes with a brief impression of University life in Australia.

The interview was held on 30 September 2009 at the University of Western Australia (UWA). The text was subsequently edited, in liaison with Arnold Zellner, to reflect the thematic structure that follows.

¹ One of the referee's of this conversation paper reported that "there are four of Zellner's papers that I find genuinely mind-stretching [. The first two come from the era before he devoted himself fully to the complex world of Bayesian estimation in macromodelling

2 University Education: From Physics to Economics

McLure: Professor Zellner, thank you for the opportunity to speak with you today during your visit to UWA. We appreciate it greatly. To differentiate this conversation from your previous interviews, we would like to ask you to reflect on your exposure to the history of ideas and economic history in your university training and discuss the effect that it had on your subsequent work.

Your undergraduate and early graduate training at Harvard University in the late 1940s was in physics, and it was not until you were a doctoral student that you moved to economics. I would like to start by asking you about your transition from physics and mathematics to economics, a transition which the history of economics tells us has been followed by many important scholars: Augustin Cournot, Léon Walras, Vilfredo Pareto, Alfred Marshall, Irving Fisher, John Maynard Keynes, to name a few.

So my question is; what are the costs and benefits of coming to economics from a background in physics? Obviously it involves strong mathematical training, but what are the issues to look out for when you come from a natural science background to study a social science?

Zellner: In my case the transition went rather smoothly, perhaps because as an undergraduate at Harvard, to meet distributional requirements, I took introductory economics courses in micro and macro economics and O.H. Taylor's wonderful course in the History of Economic Thought, where we read Adam Smith and all the classics, including Karl Marx. His lectures were so illuminating – it was the best course in economics that I took. Also, I took a course in Economic Statistics and found it so unintelligible that I dropped it after a few weeks. In addition, growing up in my father's business introduced me to practical economics. This background together with many courses in applied and theoretical physics and mathematics and two required courses in the philosophy of science, one at Harvard and one at U.C. Berkeley, fortunately, made the transition relatively smooth for me. And in addition, my older brother Norman who completed a Ph.D. in Agricultural Economics at U.C. Berkeley and his graduate student friends were present to provide me with valuable information and advice that made the transition easier for me.

One issue to look out for in making the transition is not to adopt a “superior” attitude *vis-à-vis* other graduate students and faculty just because of a strong background in math and physics. Rather, I adopted a “modest” attitude and was always willing to help fellow economics graduate students and faculty in solving their math and other research problems. Indeed, I served as a graduate research assistant for Professors

Clark Kerr, Robert Gordon and Walter Galenson and helped them with their measurement, mathematical and other problems quite successfully, a wonderful experience involving even a joint publication [Galenson and Zellner (1956)]. This joint work was most important in my opinion, along with my doctoral research project in indicating to others that I was not just a talker about productive economic research but also a producer.

McLure: Is it true to say that you acquired an appreciation of the importance of statistics through your graduate study of economics and not from your study of physics?

Zellner: On the contrary, statistics or data are important in all the sciences, physics, economics, etc. Indeed, in physics we were required to replicate famous experiments in our lab sessions. In one, I replicated the Millikan oil drop experiment to determine the charge on an electron. Our training in physics involved both data and theory, not just theory. However, as in many other fields, good statistical methods for making scientists more effective in learning from their data were and are still much needed. Indeed, Sir Harold Jeffreys published his 1939 book, *Theory of Probability*, to instruct not only his fellow physicists but also all scientists how to learn effectively from data. See the recent issue of *Statistical Science* [2009 volume 24 number 9] for an extensive review of Jeffreys's book on the 70th anniversary of its publication by Prof. C. Robert and his colleagues at the University of Paris and my [Zellner 2009a] and others' invited comments on their review. In my view, while we have much improved methods for learning from data and experience, more work is needed to improve them.

Weber: What about dynamic analysis and the calculus of variations, did you learn about such matters from your studies in physics or did you acquire that later on?

Zellner: Yes, I learned about the calculus of variations in my physics and math courses with many applications. It really is a beautiful part of mathematics and very useful, as you know, in solving important dynamic problems in physics and economics. One focus of my current research, which involves, among other things, a new proof of Bayes's theorem, uses a calculus of variations information theoretic framework and generalizations of it, described in my works published since my 1988 *American Statistician* article [Zellner (1988)] with commentary by Edwin T. Jaynes, Solomon Kullback, Bruce M. Hill and Jose M. Bernardo and my response.

Weber: One of the first books in economics to deal with the calculus of variations is R.G.D. Allen's *Mathematical Analysis for Economists*. Have you used that book?

Zellner: I referred to R.G.D. Allen's book after entering the field of economics to learn about economic applications. It was an excellent introduction to the field.

McLure: Your graduate studies in economics were clearly well grounded in both theory and statistics, but I am also interested to know your exposure to the history of economic ideas and to economic history in your training as an economist. Was history a significant part in your training as an economists?

Zellner: Yes, history was a very significant part of my training as an economist and in my career. As a graduate student in Economics at Berkeley, I was required to take two courses in Economic History, one in European economic history, offered by M. M. Knight and the second in American Economic History, presented by Sanford Mosk. I've always found history to be very interesting and important and found that these excellent courses answered many questions that I had and introduced me to new concepts to explain particular historical developments, e.g. Frederick Jackson Turner's theory of the *American frontier process*. Indeed, when I completed my graduate study at Berkeley and had an appointment in the Department of Economics at the University of Washington in Seattle, I had a chat with a senior colleague, Douglas North, who recruited me to Seattle. He asked me what I had read in Economic History that I found to be valuable. I replied that I really liked Turner's work on the frontier process and its impact on the growth of the American economy. He immediately challenged me to write a paper showing that Turner had anything of value to offer. I accepted the offer at once and wondered why he was so negative *vis-à-vis* Turner and his work. Also, a young colleague in Economic History at the University of Washington, George Murphy agreed to join me on this project. In it we reviewed the huge critical literature on Turner and frontier process and learned that it was based mainly on the argument that Marx made no mention of the frontier process in his work and thus it must be wrong!!! Real science. In our published paper [Murphy and Zellner (1959)], we provided a modern version of the frontier process and concluded that Turner's work and concepts were indeed very valuable. In addition, we had one section devoted to taking apart the critics. The editor asked whether we would mind deleting this section from our paper, which we did, and then learned that he was one of those whom we criticized in that section. I learned many years later that my colleague at Chicago Robert

Fogel was using that part of the paper in his graduate economic history seminar.

3 History of Ideas and ‘Science’: Bayes and Jeffreys

McLure: In your recent lecture (Zellner 2009), you stressed the importance of the ‘unity of science’ principle to scientific endeavours, and the importance of Sir Harold Jeffreys’s classic *Theory of Probability* to both that principle and the re-emergence of Bayesian statistical inference. The Reverend Thomas Bayes’s ‘An Essay Towards Solving a Problem in the Doctrine of Chances’ was published in 1763 – before the publication of Adam Smith’s *Wealth of Nations*. So his ideas on ‘chances’ had an amazing longevity, without experiencing a real breakthrough in applied research for a long time. My question to you on this issue is the obvious historical query: why do you believe that there was a renewal of interest in Bayes’s theorem in the 20th century, with your work on Bayesian econometrics becoming so influential more than two centuries after Bayes’s death?

Zellner: I believe that there was a renewal of interest in Bayes’s theorem in the 20th century because Jeffreys and many others adopted a pragmatic approach in deciding to work out the main estimation, testing, prediction and other problems that arise in scientific work using Bayesian and non-Bayesian methods and to compare the results. In general, Bayesian methods have won these “horse races” by providing excellent finite sample and asymptotic results, helped along by use of powerful computer techniques. You might say the battle is won and now Bayesians are working hard to produce new, improved learning methods, in the endless, iterative process that is called science.

Turkington: When you were a young man, you made significant contributions to what is now regarded as ‘classical’ econometrics; I’m thinking of the SUR model and of 3SLS. Nowadays, you are a very strong advocate of the Bayesian approach in econometrics. Is it right to say that this change in methodology from classical to Bayesian econometrics was brought about under the influence of Jeffreys’s book?

Zellner: I don’t think we were ever introduced to Bayesian methods in the conventional courses I took at Berkley and elsewhere. I started reading Jeffreys’s book in Wisconsin in the late 1960s. It opened up a new vista and I decided to run that approach instead of the other approaches and to study how well it worked in solving econometric estimation, testing, prediction and other inference problems, as explained

in the first chapter of my 1971 Wiley book, *An Introduction to Bayesian Inference in Econometrics*.

McLure: I understand that you have a copy of the Harold Jeffreys and R. A. Fisher correspondence. What is your main purpose in studying these letters: to understand the historical context in which Jeffreys and R. A. Fisher were working, or to deepen your understanding of the underlying logic of the problems at stake?

Zellner: My interest in this correspondence was in part motivated by my curiosity to learn how these two intellectual giants would interact in discussing sensitive, controversial scientific matters. The answer is that both were extremely courteous but very stubborn about changing their basic positions. I found little in the correspondence that would deepen my understanding of the issues. I just learned how both formalized their positions and on occasion would request the other to consider changing his position. And rarely, if ever, did the request receive a positive response. They were indeed “divided for life.”

Weber: Why did you turn to time series analysis at an early stage in your career, at a time when with few exceptions - for example Frank Ramsey (1928) and Harold Hotelling (1931) - established economic theory was static? Your studies of physics covered dynamic analysis, including differential equations, the calculus of variations and stochastic processes (random walk, Wiener process and Brownian motion). Does your background in physics explain your early interest in time series analysis?

Zellner: In my past economic research in the late 1950s, joint with my senior colleague at the University of Washington, James Crutchfield, we agreed to do a study to evaluate the world's most famous fishery conservation program. The International Pacific Halibut Conservation Program, established under a treaty between Canada and the U.S. in the 1920s helped to save the Pacific halibut fishery from extinction, as occurred to the Atlantic halibut fishery that did not institute a conservation program. The marine biologists, who in the main formulated the conservation program, built their models using differential equations governing the population growth. It was not too hard to combine the marine biologists' and marine economists' models into a model that is useful in analysing the development of an unregulated fishery and a fishery subject to certain conservation measures. Not only continuous differential equations were used by the marine biologists, but mixed continuous-discrete equations, now called “chaotic” equations, were also employed. All of this stimulated my continuing interest in time series dynamic models. In addition, I was

asked to review Quenouille's book on Multiple Time Series models and got introduced to his very ingenious representation of multivariate time series models using matrix polynomial lag operators. It occurred to me that his representation could be used in analysing our dynamic simultaneous equations models. I have spent many years, along with Franz Palm and others developing our "structural econometric modelling, time series analysis" (SEMTSA) approach for analysing and building dynamic models. Thus, my interest in time series is not just theoretical but also pragmatic since its methods provide many valuable results.

4 The Chicago School

McLure: There is often discussion along the lines that economics has a religious or ideological element. I raise the matter because I was at a seminar recently at which a former Premier of Western Australia told the audience that "economics is religion" and it is nothing more – 'it's a belief!' To what extent are "schools of thought" differentiated by 'religion' and by 'science'? Also, would you place your own work within the corpus of the Chicago "school of thought", if there is such a thing, and would you say ideology plays any role in defining that School?

Zellner: I believe that the Chicago School, influenced by Milton Friedman, Theodore Schultz, Carl Christ, and many others, too numerous to name, has been in the forefront of the movement to subject economic propositions to empirical testing using data, to the extent that it has almost become commonplace, apparently, however, unknown to your former Premier of Western Australia. Where propositions cannot be empirically tested, of course there will be uncertainty. This is, however, not unique to economics. Physicists, for example, have conflicting evidence about Albert Einstein's theory from space station observations that show the universe is expanding at an increasing rate, not a decreasing rate as predicted by Einstein's theory. Until new evidence and new theories become available, it appears that physicists will have to put up with some uncertainty ... religiously or secularly? As I remarked to my good friend George Judge recently, an important basic issue is whether it is possible to transform the religious area of study into a scientific discipline. I shall not try to answer this question but will leave it for your thought and discussion over several glasses of great Australian brew.

McLure: Just on Chicago generally, sometimes it's seen as leading and disseminating an ideological view about free trade and liberty in political matters, social matters and economic matters, which is contrary to the view that Chicago ultimately fostered a scientifically-oriented approach grounded in empirical testing. How do you react to such comments?

Zellner: Frankly, the individuals making those comments have in the main never participated in the famous Chicago workshops where everything is open for discussion and presentation of empirical evidence. Perhaps we need more tours of Chicago workshops, whether in money, industrial organization, etc. to show how they operate. And indeed, there is much empirical evidence relating to the comparative performance of "Mrs. Thatcher-like laissez-faire" economies and "socialist-type" economies that supports the hypothesis that the former perform better.

McLure: In response to his famous *Essays in Positive Economics*, Milton Friedman (1953) received some critical comments over the proposition that deductions may be derived from assumptions which are unrealistic if the resulting model predicts well. In your view, is it appropriate for economists to entertain assumptions that are unrealistic? Also, do you think that ontological complexities, which may call into question the objective character of certain observed and recorded 'facts', are an important constraint on positive economics and the 'unity of science' approach?

Zellner: Theories and data may not be ideal, as in economics and many other areas of science. As a result, we work hard to produce better theories and data in the unending interactive process called science or learning from our data and experience. Would that all were perfect, but it's not and we have to improve the situation that involves a lot of hard work and ingenuity. For example, the Bayesian learning model has been shown to work well on a broad range of econometric and statistical problems ... but that doesn't imply that it's perfect and explains why, as mentioned above, I am working to improve the Bayesian learning model, Bayes's theorem.

5 Marshallian Macroeconomic Model and Macro-Forecasting

McLure: Your current work on macro-modelling includes the adjective "Marshallian": you label the approach the 'Marshallian Macroeconomic Model' (Zellner and Israilevich 2005). How did you come to Marshall in the first place and in what sense does he influence your current macro modelling?

Zellner: Well a lot of the courses on price theory that I took as a graduate and undergraduate were the best courses. The Marshallian model for the competitive industry was so impressive. Marshall didn't just define an equilibrium, he also showed you how you get from one equilibrium to another through the entry and exit of firms. That's the part of economic theory that really impressed me. As I mentioned in my lecture, in our MMM, we disaggregate by industrial sectors, for example, agriculture, mining, durables, services, etc. and for each sector we have a product market involving demand, supply, entry-exit equations, just as Marshall employed many years ago. Upon aggregating the supply function over the firms, the number of firms appears in our supply equation, an endogenous variable that is left out of many macro models. So you can see that our sector product and factor markets are formulated to include entry and exit of firms since the economy is not in equilibrium period by period.

Weber: The econometric approach that you favour is 'sophisticatedly simple', one starts with a simple model and progressively works to a more complicated model when the simple model does not perform too well. Now that approach has been criticised on the grounds that it includes an element of circularity, for a model that is constructed in this way will fit the data well in the end. How do you respond to that criticism?²

Zellner: Perhaps I did not emphasize enough the need for extensive predictive testing of models and using modern model selection techniques for evaluating alternative models. With just one sample of data, while it's better than nothing there still is a need to use model selection techniques, such as posterior odds, that incorporate a penalty for "over-fitting." Still, usually, the more good data the better.

Weber: You have previously raised the issue of success, or otherwise, of forecasting in science. As a proposition, do you generally agree that the

² A referee for this conversation observed: "I'm puzzled the interviewers didn't raise with Zellner the contrast between [David] Hendry's (step-down) 'general to specific' model building strategy as contrasted with Zellner's (step-up?) prescription 'Keep it sophisticatedly simple'". This was put to Zellner, who replied: "as regards Hendry, we have indeed disagreed about how to produce good models and in a recent issue of the *Journal of Econometrics*, an article [by Helmut Lutkepohl (2007)] appeared indicating that in his applied work Hendry uses a 'specific' to 'general' approach, NOT a 'general' to 'specific approach'. I sent the article to Hendry for comment but did not receive a response".

global financial crisis and economic recession of the past two years wasn't well forecast by economists?

Zellner: Econometric models and economists in general have not been very good at forecasting downturns and upturns in our economies. Indeed, just a few years ago at the American Economic Association meeting in San Diego, Alan Greenspan, the then Chairman of the Federal Reserve Board, made a presentation on difficulties associated with making monetary policy in the U.S.. One main difficulty that he mentioned, along with 18 others, was that he did not have a macroeconomic model that forecasts well. This has been a problem world-wide and I am hoping that our Marshallian macroeconomic model will be a solution.

6 A Few Final Words

McLure: Thank you Professor Zellner, we appreciate you spending the time with us today to share your insights on the historical dimension to your scholarship. I would like to bring our conversation to a close by asking if you have any general reflections on Australian universities based on your visits to this country.

Zellner: My wife and I have thoroughly enjoyed all our visits to Australia over the years, and there were quite a few. We visited Monash in the past and enjoyed the University and colleagues there very much. The intellectual interactions as well as the monthly "wine dinners" at Max King's home were memorable! Then too our wonderful sight-seeing tours, as in Perth on our recent visit, are unforgettable. Thanks to you and your colleagues for your warm hospitality, beautiful natural sights and lovely culture. Keep up the great work.

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