Econometric Theory, **35**, 2019, 685–728.
doi:10.1017/S0266466618000336

THE ET INTERVIEW: PROFESSOR HASHEM PESARAN

Interview conducted on April 20, 2017 in San Diego
by Allan Timmermann
University of California, San Diego



Hashem Pesaran, 2013.

Since graduating with a Ph.D. in Economics from Cambridge, Hashem Pesaran has been a prominent contributor to the theoretical econometrics literature, undertaking work on testing of non-nested models, cointegration, forecasting in the presence of model instability, forecasting of multinomial data, and estimation and testing of time-series and panel data models. His work also covers several areas of applied econometrics, including studies of oil prices, prices in financial markets, rational expectations and economic surveys, analysis of panel data models with cross sectional dependence and spillover effects and interdependencies across international economies. Hashem is the founding editor of the *Journal of*

Allan Timmermann wishes to thank Ron Smith for important help in formulating many of the questions and preparing for the interview, and Anna Emerald for help with transcription of the interview and some preliminary editing of the text.

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https://doi.org/10.1017/S0266466618000336 Published online by Cambridge University Press

Applied Econometrics and has served on the editorial boards of numerous journals in economics and econometrics. He is a Fellow of the British Academy, the Econometric Society, and the Journal of Econometrics. Previously, he was head of the Economic Research Department of the *Central Bank of Iran* and Under-Secretary of the *Ministry of Education*, Iran. He has held positions as a Professor of Economics at Cambridge University and at UCLA. He is currently an emeritus Professor of Economics at Cambridge University, a lifetime Fellow of Trinity College, Cambridge, and holds the John Elliott Distinguished Chair in Economics at the University of Southern California.

Good morning. Today is April 20th, 2017 and we are at UCSD in La Jolla, California. My name is Allan Timmermann. I was Hashem Pesaran's Ph.D. student at the University of Cambridge from 1988 through 1991. I have continued to work with Hashem both whilst I was in England and after moving to UCSD in California, where Hashem is now conveniently residing. We continue to stay in contact and work on research ideas. I look forward to this interview which will go through the details of Hashem's long, distinguished, and fascinating career. Hashem gave a talk at UCSD yesterday and so we are taking the chance to conduct the ET interview now that he has taken some time out of his busy schedule. Hashem, let us start by talking about your arrival in England and the early years of your academic career. You arrived in England from Shiraz in Iran. Tell us a bit about your background and your training at the Nemazi School in Shiraz.

First, thank you Allan for arranging this interview. To answer your question, I was fascinated by mathematics from early on when I started high school in Shiraz, where I was born in March of 1946. After completing primary school, I attended the Sharghi, Shahpoor and Nemazi high schools in that order and obtained my school diploma from the Nemazi School. These were very good schools with a number of excellent teachers and students in the areas of mathematics and physics.

We did not have much training in social sciences. For example, there were no classes offered in economics. After the first three years of high school, we had to decide between humanities and natural sciences, with the latter focusing on mathematics, physics, and chemistry. In mathematics, we learned algebra and calculus. There was no discussion of topology or set theory in those days. We developed a very strong foundation in the areas of geometry and algebra, and this was very important for my later mathematical training. The type of mathematics I learned came from the French school of mathematics. For example, in trigonometry there was a lot of emphasis on visualizing objects in three dimensions, which was especially helpful for solving geometrical problems. In addition, we used to solve mathematical problems using Russian texts translated into Farsi, which helped us develop our problem solving skills.

So, you clearly must have been an outstanding student because you won a scholarship from the Bank of Markazi (*The Central Bank of Iran*) *to conduct*

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academic studies in England. What made you interested in going abroad and did you know right from the beginning that you wanted to study econometrics?

In fact, many of us, including myself, were not interested in economics, accounting or banking. Initially, I was offered a place at the Abadan Institute of Technology, now Petroleum University of Technology, located in the south west of the country to become an oil engineer. I started there in the summer of 1964 to attend some preliminary courses in English before the start of the full academic year. Whilst in Abadan, I was invited by Bank Markazi (the Central Bank of Iran) for an interview in Tehran (some 950 kilometers to the northeast of Abadan), and was then awarded (along with 11 others) a full scholarship to study in the United Kingdom. The idea of going to England had more to do with being in a new environment rather than study a specific subject that interested me. The main attraction was the possibility of studying abroad. In fact, we were all expected to become Chartered Accountants, and as it turned out nine out of the original twelve students did become accountants. However, three of us were given permission to continue to Ph.D. programs. Apart from myself, two others went on to do Ph.Ds., one in economics (at UCLA) and one in statistics (at Harvard), and as you know I ended up in Cambridge.

Was there a requirement in the scholarship that you return to Iran after completing your studies?

Yes, that was one of the main conditions. We had to work for the Central Bank or other sponsoring institutions twice as long as the time of our scholarship. If not, we would have to pay back the full amount of the scholarship plus 12% interest. That was the contract.

That was a strong incentive.

Of course. In fact we all returned to Iran before the Revolution, although none of us remained in Iran in the aftermath of the Revolution (February 1979).

Exactly, and so your first stop in England, I believe, was at Salford University. Did the econometrics training you received there lay the ground for your decision to tackle technically challenging topics in econometrics or were you largely self-taught?

As it turned out, seven of us arrived in Manchester in November 1964, and the remaining five joined us subsequently. We were supposed to begin our studies in September. But there was a delay, and by the time we arrived in Manchester, it was November.

We needed to obtain A-levels (General Certificate of Education Advanced Level) to enter university, because the Iranian school diploma was not recognized by the British University system. Our English language skills were not that strong to take A-levels in economics or in natural sciences. As a result, we all ended up doing pure and applied mathematics at the Stretford Technical College next to Manchester United football grounds! The A-level courses were supposed to take 2 years to complete, but to the surprise of our teachers, we all managed to receive good

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grades by the end of the year. The plan was to apply the following year to enter University to do economics.

I think they did not know what to do with us. By the time we got our A level grades (in July) it was too late to formally apply for entry to any university. An opportunity presented itself at the University of Salford which in February 1967 had acquired the status of a University, and it was next door to our college. We were all accepted *en masse*, although three of the group decided to go to London and seek more accredited institutions. As a result, nine of us ended up in the same class in Salford University—a sort of home away from home. It turned out to be an advantage to be together as we benefited from each other's strengths and expertise, a union that has continued. Incidentally, the groups that were sent after us (which included my brother Bahram) mostly ended up at London School of Economics.

It was also fortunate that Salford, being a new university, had decided to introduce a new joint-honors degree in mathematics, statistics, and economics, which was very unusual at that time in England. Back in 1964, Cambridge or Oxford mainly taught political economy and the use of mathematics in economics was not as prevalent—definitely not at the undergraduate level in an integrated way. Salford offered this joint honors program which proved instrumental in getting me interested in the rigorous application of mathematical and statistical methods in economics.

In the second year of my undergraduate studies, I wrote an ALGOL program, a computer program that predated Fortran, for a quantitative analysis of John Hicks's trade cycle. Whilst most economists at the time were focusing on dynamically stable models to explain business cycles, Hicks began with an explosive model but imposed floors and ceilings on the evolution of output to generate sustained cycles without needing continuous shocks as in the propagation mechanisms that had been proposed by Frisch and Slutsky. For my second-year project, I wrote a program to solve Hicks's model using estimates of the saving rate and capital output ratio from the literature—I guess I was a calibrator to begin with! It was, basically, a second order difference equation with explosive roots, but subject to exogenously given floors and ceilings. When the output reached the ceiling, it slowed down and then turned around largely because of its explosive nature. Yet, when output reached the floor it again slowed down crawling along the floor before turning upward, initiating another cycle. The business cycle enthralled me, and also led me to the issue of how best to estimate the parameters of time series models.

Yes, indeed we will come back to those types of nonlinearities that you continued to work on subsequently. I gather you were in Salford for three years from '65 to '68 and then began your Ph.D. studies in Cambridge in 1968. I think Francis Crips supervised you and later Denis Sargan and Jim Durbin examined you. Tell us more about how they helped shape your views.

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Yes, my Salford experience was valuable and taught me that I needed to rely on myself to learn, if needed. Amongst the students in our class I believe I was the only one to receive a first-class honors degree, which paved the way for my entry to a Ph.D. program. As I have already mentioned, we were all supposed to become chartered accountants, and I was pleasantly surprised when I got permission from Bank Markazi to do a Ph.D. I applied to American and British Universities and had to choose between Berkeley and Cambridge. I decided to go to Cambridge because of Marian, my future wife, whom I had met in April 1968 just before my final exams.

In Salford?

Yes, she was a first-year undergraduate student studying languages in Salford when we met. In England I obtained admission to New College, Oxford and Sidney Sussex College, Cambridge. Marian's parents lived near Diss, Norfolk, which is close to Cambridge. So I decided to go to Cambridge! It turned out that many of the key decisions in the early stages of my life were not based on a careful consideration of my various academic options but other personal factors.

When I arrived in Cambridge there were no training programs for Ph.D. students. Francis Cripps was appointed as my supervisor, which is equivalent to the main advisor in the United States. He was expected to direct my studies, but he had very little knowledge of econometrics. He had just finished his BA degree in economics in Cambridge and was appointed as an assistant lecturer in the Faculty of Economics and Politics (now Faculty of Economics). Our meetings were often in the form of discussions and joint-learning, rather than a teacher–student relationship. He was a very nice and supportive supervisor and introduced me to some of the senior Faculty in Cambridge.

The person to whom I was intellectually indebted at Cambridge was David Champernowne who had returned to Trinity College as a Fellow and a Reader after giving up his Professorship in statistics at Oxford University in 1959. He was well known and a very interesting person. He had a close relationship to Alan Turing with whom he walked regularly. Champernowne was particularly interested in computers. In an article he wrote in 1960 in the *Journal of the Royal Statistical Society Series A*, I believe he was the first to show the spurious correlation problem in time series using Monte Carlo experiments, which pre-dates the seminal 1974 *Journal of Econometrics* article by Clive Granger and Paul Newbold.

I also came across another article by Champernowne entitled, "Sampling Theory Applied to Autoregressive Sequences", which provided a comprehensive analysis of time series models using Bayesian and classical approaches that he had published in 1948 in the *Journal of the Royal Statistical Society Series B*. Not surprisingly, he became interested in the research I was doing on time series for my Ph.D. Later, he helped me move to Trinity College, first as a Lector (which is a teaching position without a fellowship), and then as a teaching fellow. He was a major figure in the area of time series, although he did not write much, like many other academics at Cambridge in those days. They were excellent at what they

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were doing but they were not necessarily interested in writing a large number of articles. Champernowne was not only interested in statistical modeling, he was also interested in economics and decision making under uncertainty.



Hashem Pesaran, Ph.D. student in Cambridge (1971).

As I have already mentioned, formal training in Cambridge was negligible. Somebody would tell you to look at this or that article, which was helpful but was not a substitute for a comprehensive course on econometrics or time series. I was complaining about this lack of formal training to Ron Smith with whom, as you know, I have had many research collaborations.[1] In early 1969, I was having tea with him at the Cambridge Buttery, which is underneath the Faculty of Economics where students gather to have snacks and discuss economics and politics and exchange rumors. I grumbled about the lack of Ph.D. training and he suggested that I might take advantage of a rule in the Cambridge University Ph.D. program that allowed us to spend one year of our Ph.D. course at another accredited university as a visiting student.

Ron told me that he had arranged to spend 1969–70 at MIT and encouraged me to do likewise. When I was considering different universities to visit, I noticed

[1] Ron started doing a Ph.D. in 1968 at the same time as me. He is currently at Birkbeck College, London.

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that Zvi Griliches had moved from Chicago to Harvard. He had just become the co-editor of *Econometrica.* As I mentioned, I was very interested in time series analysis and Zvi Griliches had published an article in 1967 on distributive lags. It was relevant to my work on business cycles and time series modelling. I wrote to him to see if I could visit him for a year, and to my surprise, he wrote back almost immediately and invited me to come to Harvard as a full-time Special Student. Bank Markazi also agreed to this move and provided additional funds for Marian (by then my wife) to accompany me.

That was very important for my training. I met many econometricians who are now eminent scholars. Gary Chamberlain was one of Zvi's favorite students. Ed Leamer and Christopher Sims were assistant professors at Harvard and Rob Engle was assistant professor at MIT, and I recall that Sims was about to leave Harvard for Minnesota just after I arrived. It was an amazing group of people with whom to interact and from whom to learn.

I attended many lectures, being deprived of a formal training in Cambridge. I attended lectures by Zvi Griliches, Dale Jorgenson, and many others. Dale was also particularly helpful and supportive. It is true that I was already familiar with a large part of the material being taught in the class, but the lectures helped me to identify gaps in my learning and provided a framework for further developments. I also benefited from the general academic atmosphere. Kenneth Arrow was also there at the time, and I remember his seminars, in particular.

I also learned a lot from writing referee reports. Zvi Griliches used to send me articles to review for *Econometrica*, and I had to learn many new materials to write the reports. I used to get one or two articles a month to review, and I had obtained a great deal of experience in reviewing by the time I completed my stay at Harvard.

Back then, the Ph.D. training at Cambridge was more like the genius model, where the best talent was expected to rise to the surface. You had to be very entrepreneurial, effectively patching your own training together.

Yes, that is what I experienced, I guess. At Cambridge we ended up doing research that interested us without any formal instructions but with some one-on-one interactions. As a matter of fact, in addition to my work on time series, I also wrote a few other articles which I did not pursue further after I completed my Ph.D., and which remain unpublished. I wrote an article for Dale Jorgenson's course on consumption. He encouraged me to publish it, and it is a pity that I could not follow Dale's advice after my return to Iran. The unpublished version is still available. I wrote another article entitled, "Instability of the Parameters of the Systematic and Non-Systematic Parts of a Single Equation Model." (March 1970) that built on the work of Dick Quandt on testing for structural breaks in error variances.

Towards the end of my visit at Harvard, in the summer of 1971, I received a letter from the newly appointed Director of the Department of Applied Economics in Cambridge, Wynne Godley, who offered me a Junior Research position

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at the department. Wynne Godley was not an academic by training and came to Cambridge from HM Treasury. I believe he wanted someone with expertise in statistical modeling and econometrics to help him. Frances Cripps worked with Wynne Godley and, being my Ph.D. supervisor, I believe he had recommended me to Wynne.

Upon my return to Cambridge, Trinity College also invited me to teach undergraduate students as a Lector. Essentially, I was like a member of the university after the second year of my Ph.D. I had dining rights at Trinity College, although I do not recall exercising my rights, since no one had told me that I could dine for free, as I would have enjoyed that! But, I did really enjoy supervising the undergraduates. I was allocated 3 Trinity undergraduates doing the Economics Tripos to supervise (give tutorials to). I met them every week during the term and covered a wide range of topics in economics, mathematics, and statistics. I was fortunate to be teaching an amazing group of students. One year, I had three students who all received first class honors in economics. They all went to excellent positions at the OECD, IMF and the Reserve Bank of Australia. It was a good opportunity to be able to interact with talented and hard-working students. Although Cambridge did not provide a formal academic structure, it created an environment in which you could interact with very good people who appreciated what you are doing. However, you really needed to be innovative. You could not passively wait for your advisor/teacher to tell you what to do.

And it was a very fruitful period for the development of time series methods in the early '70s. You mentioned testing for structural breaks, as well as spurious correlation; in the early '70s these ideas were beginning to catch a lot of attention.

Yes. Time series analysis and distributed lag models were big topics in those days with contributions from Larry Klein, Christopher Sims, Robert Shiller, Marc Nerlove, and Peter Schmidt, in addition to Dhrymes, Griliches, and Jorgenson whom I have already mentioned. I had come across the unpublished manuscript of a new book on "Distributed Lags" by Dhrymes that interested me greatly. One of the issues discussed in this book concerned the estimation of the truncation remainder, a problem highlighted by Klein. In my first unpublished article entitled, "Maximum Likelihood Estimation of the Models with Lagged Dependent Variable and Autocorrelated Disturbances", written in 1969, I proposed a new method that avoided the estimation of truncation remainders. In infinite order distributed lag models, at some point you must truncate the lag distribution, which then necessitates considering the error associated with the so-called "truncation remainder". The manuscript by Dhrymes contained a discussion claiming that somehow you could consistently estimate the truncation remainder, whilst in my article I had argued that it is not possible to consistently estimate the history from a finite future.

As it happened, before setting off for Harvard, Dhrymes was attending the Second World Congress of the Econometric Society held in Cambridge in September

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1970. He suggested that we meet to discuss my article and the issue of the truncation remainder. He then encouraged me to revise and submit it to the *International Economic Review* (IER) that he was editing at the time. I worked on the 1969 article during my year at Harvard and produced a new article entitled "The Small Sample problem of Truncation Remainders in the Estimation of Distributed Lag Models with Autocorrelated Errors", which I then submitted formally in November 1971. It was accepted for publication shortly after in February 1972. Thus, I got my first publication in 1973, apparently without any formal review process!

In those days, there was more direct interaction between junior and senior academics. More than now, I think. I was only a second year Ph.D. student when many senior academics paid attention to my research.

Do you think it was because Ph.D. programs were much smaller at the time?

I think so. There were not many Ph.D. students working in the area of theoretical econometrics, and I guess those of us that did stood out. In Cambridge, only a few did Ph.D. in econometrics. I have already mentioned Ron Smith who was doing his Ph.D. on models of durable demands. There were also others at the Department of Applied Economics working on the Cambridge Growth Project under the direction of Dick Stone, such as Mervyn King, Angus Deaton, and Terry Barker, who were mainly interested in applied econometrics.

Right.

In the United Kingdom, the home of time series econometrics was the London School of Economics (LSE), with Denis Sargan and Jim Durbin being the leading researchers in the field. In fact, they were both appointed as my Ph.D. examiners. I vividly recall the day that I took the train from Cambridge to London to be examined by them. I had not met them before that day and was quite apprehensive, but as it turned out I had a relatively easy viva. I recall Denis Sargan, being quiet for most of the exam, just asked me if the exact maximum likelihood method I had developed for estimation of first order moving average processes could be generalized to higher order processes. I responded No! He said, "I thought so", and that was that. I had passed his test. I believe I was the first to propose a simple method for exact maximum likelihood estimation of MA(1) processes, which was then published in the *Review of Economic Studies* in 1973. Jim Durbin went into more details of how my estimation procedure differed from his alternative method that he had published based on truncation of infinite order autoregressive representation of moving average processes. Neither of them asked me about my chapter on non-nested hypotheses testing, which was also subsequently published in *Review of Economic Studies.*

It is also interesting that both Jim Durbin and Denis Sargan had been students at St. John's College, Cambridge, and had obtained their statistical training from Sir Harold Jeffreys, the well-known applied physicist/statistician who did pioneering research in Bayesian analysis.

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Famous for the Jeffreys Prior?

Yes. The same. Durbin had also worked as a Junior Research Officer for Dick Stone at the Department of Applied Economics (DAE). We had a fantastic library in the Department of Applied Economics. I do not know whether you remember it. It was probably before your time.

Yes, in the Austin Robinson Building?

No, that is the Marshall library. The DAE, which was purely a research department, had its own library. It housed many books, journals, and manuscripts in the area of statistics, computing, and econometrics that were recommended by leading researchers who either worked or visited the DAE. I remember margin notes from Koopmans, Houthakker, and Tobin in some of the texts in the DAE library.



Budapest, September, 1972 European Econometric Society Meeting. From left to right: Mervyn King, Terry Baker, Hashem Pesaran, and Angus Deaton.

Now, switching to your time in Iran, this brings us up to about 1973 on the timeline. In 1973, you drove back to Iran. I guess that had been part of the deal with your scholarship. You began working in the Central Bank, quickly rose through the ranks and in fact became the head of the Economic Research Department (1974–1976). Subsequently you served as the Under-Secretary of the Ministry of Education (1976–1978) in Iran. Despite this being a time with lots of distractions, there was political upheaval, economic turmoil, and the threat of SAVAK,

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the Shah's secret police, lurking in the background. How did you manage to continue your research with all of these issues happening at the time?

My wife and I decided to speed up our return to Iran in the summer of 1973 since my wife was already pregnant with our first child. I wanted to make sure that our child was born in Iran, whilst at the time many wealthy Iranians went to a great deal of trouble to have their children born in Europe. I was strongly committed to returning, not just because I was under obligation to do so, but because I wanted to return and help in the development of my country. Also, in those days, as a returning student from abroad, we could import a car without custom duties. I recall purchasing a Peugeot 405 Estate, which I took delivery from Paris and then drove it all the way to Tehran. It took about 7 days. I was fortunate to have the company of Ken Coutts with whom I had shared an office in the DAE.

When I arrived at Bank Markazi, I realized I had a highly technical Ph.D. in Econometrics that involved model selection and time series analysis. Not many people were familiar with these topics in Iran at the time. Soon I realized that I needed to use my knowledge and expertise in econometrics but apply it to relevant problems. One of the problems that attracted me was the trend in income distribution in Iran. Oil prices had been rising since 1970 and Iran was one of the main beneficiaries in the region with a large amount of petro dollars being injected into the economy. The Shah wanted to expand the economy. During the 1960s, the Iranian economy had been growing quite steadily. When I arrived, there were issues surrounding the idea of whether the benefit of rising incomes trickled down to lower income groups in the society, the so-called "trickle down" hypothesis. Even now, we observe a very similar problem in developing countries. It was argued, by many market economists, that the income distribution might worsen initially but it would eventually become better as growth and development took hold. I wanted to know if this was true in the case of Iran, but I could not find any data on the income distribution for Iran.

It was in a way fortunate that when I arrived at the Bank, I was given a desk and more or less was left to my own accord. I guess, they did not know what to do with me—at least initially—and, as a result, for the first six months I could do my own research. I soon learned about the household expenditure surveys conducted by the Bank for the computation of the weights used to produce the Consumer Price Index (CPI), which was one of the functions of Bank Markazi. Household expenditure data from 1964 onward were available on computer tapes. I realized that I could use the expenditure data to obtain a proxy for income inequality. From my earlier work on consumption functions at Harvard, I was already familiar with the idea of the permanent income hypothesis and the fact that consumption could be a more reliable long-term measure of income than income itself, which is subject to significant transitory variations. As it happened, Farhad Mehran, an Iranian Ph.D. student in statistics whom I got to know at Harvard and who also returned to Iran, introduced me to an article written by Gastwirth (1972, *Review of Economics and Statistics*) on computing bounds on

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the Gini coefficient. The outcome was my first article on the Iranian economy, which showed that the trend of the income (expenditure) distribution had worsened systematically during 1964–1971, a period during which Iran had experienced sustained GDP growth of around 6 percent per annum. Politically speaking, that was not something you wanted to advertise or publish. I guess I was naïve as a young researcher, so I presented it at an International Conference held in Persepolis, close to Shiraz, my home-town. I was then invited by Khodad Farmanfarmaian (who at the time was the head of Plan Organization and had previously been the Deputy Governor at the Bank Markazi) to submit the article for inclusion in a volume to be published by Princeton University Press. However, I could not get the required permission from the newly appointed Governor of the Bank, Dr Mohammed Yaganeh (1923–1995), and as a result, that article remains unpublished.

Despite declining my request, Dr Yaganeh was very sympathetic and was very concerned about the possible adverse reactions for the Bank and me from SAVAK (the feared security organization under the Shah). Also, I learned later that Wynn Godley (for whom I worked at DAE) had written to the Bank of England hoping to use their good offices to persuade Bank Markazi to allow me to stay in Cambridge for another year. This did not work out, but apparently, the letter from the Bank of England had been kept on my file at Bank Markazi, and I was told later that this letter was also important in my early and unexpected promotion to head of the Research Department of the Bank in 1974.

But it was not all smooth sailing at the Bank. Before my appointment as the Director of Research, I was assistant to the Vice-Governor, which meant I had no specific duties and had to function at the pleasure of the Vice-Governor. I found him rather difficult to work with, and on some occasions obstructive. To support my growing family (we had two sons at the time), I used to teach at Tehran University, and at the Institute of Advanced Studies in Statistics after bank hours (4pm onwards). I recall that he withheld giving formal permission and as a result I would not get paid on time and sometimes I had to wait 4–6 months before I was paid! During this period (1973–1974) I also wrote a time series monograph in Persian, which was used for teaching for a number of years after I stopped teaching—given my new duties at the Bank.

And you were very young at the time, you were barely 30.

Yes, I think I just turned 29.

29 years old, and you were the head of the Research Department?

Yes, it was really an amazing experience because as I mentioned at that time Iran's revenues from oil exports had quadrupled and there was a sense of optimism. I recall taking part in many meetings with the IMF and the World Bank, alongside of the Governor, discussing the "Safety Net," which was to provide external assistance to oil importers. I went to the United Nations and helped write a speech (entitled Collaborations and not Confrontations) for Dr. Jamshid Amouzegar (1923–2016) who later became the Prime Minister. I learned a lot about how

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one could combine modeling ideas and formal analysis with policy-oriented outcomes. That was important for my future research agenda and outlook.

You mentioned earlier the type of data you had on consumption and expenditures. Was that the data that also helped you to persuade Angus Deaton to visit you in Iran? *I know that he came to Iran and you collaborated there, I believe, on what later became your 1979 article in Econometrica*?

The article you refer to was in fact published in 1978. Angus was a junior research officer at the DAE when I returned to Cambridge from Harvard. As I have already mentioned, Angus was working on alternative models of expenditures as nonlinear systems, a kind of nonlinear SURE (seemingly unrelated regression equations). I had written an article on model selection and non-nested hypothesis testing, which was published in 1974 in The *Review of Economic Studies*. In this article, I was concerned with testing one single-equation model against another, such as a Keynesian model against a monetarist model of output growth, which I then extended and applied later to a number of problems. Angus became interested in my work on non-nested hypotheses testing and asked if we could generalize it for testing non-nested system of equations of the type that he was concerned with, such as two different demand systems that are not nested within each other. It seemed like a straight forward generalization of my 1974 RES article but it turned out to be a great deal of further work—the derivations turned out to be quite involved, and the computations of the required statistics were not that simple either.

Angus started working on the article, but we needed to meet to go over some of the technical details. I could not leave Iran because I had to join the military as a conscript. He came to Tehran to work on the article, which he then presented at the Helsinki Meeting of the Econometric Society in 1976. As I mentioned, *Econometrica* subsequently published the article in 1978. Angus often reminds me that I was too busy to entertain him—but I believe he did have a memorable visit.

And you later switched from the Central Bank to become Under Secretary of the Ministry of Education. How long did you work in that position?

The offer came towards the end of my military training as a conscript, and I was uncertain about my return to the Bank. The continuation of my work on income distribution had also attracted some attention. In particular, I presented an article on the major determinants of income distribution in Iran at the Aspen Institute in the United States where major social and economic policies were discussed. In that article I considered the geographical disparities in income distribution and related the spatial inequalities to differences in levels of educational attainments, differences in government expenditures across regions, and a number of other factors. That article was then published in J.A. Jacqs (ed.), *Iran, Past, Present and Future*. Through this work and my interactions at conferences and other meetings, I got to know the people who were interested in social issues and were involved in the Department of Education. I was told that the De-

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partment needed to use economic methods in the allocation of teachers across different provinces, and to enhance teachers' welfare. They wanted to have better amenities for teachers to improve teaching. I became very interested in these ideas.

The offer of joining the Ministry of Education also resolved my uncertainty regarding the remaining 1.5 years of my army duties. I ended up in the Ministry of Education in 1976 largely due to the fact that I needed to complete my military service before I could travel abroad—which was what I wanted to do, so that I could interact with other researchers and present my work. As it turned out, a third article I wrote on income distribution was also presented by my co-author, Firouz Gahvari (now at University of Illinois Urbana-Champaign) at a conference in Italy in 1976/77 (the article was published in a volume edited by Richard Stone and William Peterson, 1978).

I was about to ask if we could credit the Iranian revolution for having given us back a great econometrician. But it sounds like you had a long-term plan to return to academia at any rate.

It is true that a number of people wanted me to stay in Cambridge. I received offers from James Meade regarding a teaching position at Christ's College, from Jim Mirrlees to go to Nuffield College, Oxford, in addition to Wynne Godley's offer to stay on at the DAE. Yes, I did want to continue as an academic, even when I returned to Iran. As luck had it, I received a letter in July 1975 from Lord Rab Butler (1902–1982) who was then the Master of Trinity College, offering me a College Lectureship in Economics at Trinity. I wrote back very politely, and thanked him for the offer, but explained that regretfully I was not able to accept since I had to serve in the military, but I added that when my time in the military was over, and if the opportunity was still available, I would be honored to accept the offer. About a year later, Dr. Denis Marrian, who was the Senior Tutor at the time at Trinity, wrote back and informed me that the College had appointed another candidate (Gene Savin), but added that

"…the Committee were unanimous in stating the view that this recommendation did not make it less desirable for the College to elect you to a Lectureship should you become available, and that you should be so informed.

To my knowledge I do not think the Education Committee have ever gone on record in this way before which makes it even more of a pleasure for me to write to you with this information. I need hardly say that we all hope you will find it possible to join our staff.

Should you be unable to take up this position by October 1979, the offer would lapse, but in this case we would still wish to consider the matter again should your circumstances change."

I was pleasantly surprised by the extension, but at the time (1976) did not appreciate the potential value of the option for myself and my family. Ironically, it was only a couple of years later that unrest and demonstrations against the Shah started to spread across the country.

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This was 1979?

Demonstrations had started to build up in April of 1978, and became much more serious starting in August. By September of that year, I knew that we had to leave. My wife was heavily pregnant during the height of the demonstrations in September–October. The question was—how would I manage to leave with a heavily pregnant English wife and two boys? At that time, there was a military government in power and it was not easy to leave the country. However, I managed to arrange for my family to leave Iran when our new-born daughter was six weeks old, with our two boys. I could not leave myself because I was not able to get permission from the Minster in charge. I had to wait for an opportune moment. As luck would have it, when the military government was installed, a young Vice Admiral by the name of Kamal Habibollahi (1930–2016), who was the Commander of the Iranian Navy, was appointed in charge of the Ministry of Education, where I still worked as the Under Secretary. I explained my situation and told him that I was an academic, not a political appointee. I also added that he would probably be much better off if he let me go back to the Central Bank. He agreed. If he had not agreed, I could not have left and most likely I would not have been able to leave for a long time afterwards. It was another stroke of luck!

Also, before departing Iran, I asked my wife to check to see if I was still needed at Trinity, particularly given that I had already missed the beginning of the academic year. I wanted to know as well if the option I was given back in 1976 could still be exercised (as you recall I was given four years to decide). I was delighted to get supporting letters from Alister Sutherland, Tony Wier (then the Senior Tutor), and Robert Neild (then the Chair of the Faculty of Economics and Politics) all welcoming me back should I be able to leave Iran. They accepted and supported me and my family as refugees in November 1978, and I began my Teaching Fellowship on January 1, 1979. Cambridge has been very good about accepting refugees. During the Second World War, many mathematicians from Eastern Europe arrived at Cambridge as exiles. I think I was one of the last refugees at Trinity.

This was in 1979 or in 1978 that you got out of Iran?

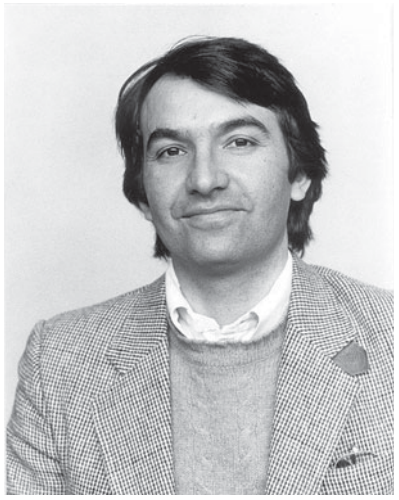
My family left Iran in October ‘78. I arrived in Cambridge at the end of November of ‘78. The Revolution succeeded in February 1979 after the Shah left Iran in January and Ayatollah Khomeini returned from exile in February 1979. Often, I have been asked if I had predicted/expected the Revolution. The simple answer is that I did not. Nevertheless, I realized that what was developing would have irreversible consequences. To make a life-changing decision one does not need exact predictions, when the direction of the change becomes reasonably certain. No matter what the outcomes, it was clear that it would have been very difficult for me and my family to live in Iran as an academically minded professional, with an English wife.

You once described to me how you returned to an empty office at Trinity College with a blank pad of paper and nothing much else, even though your colleagues in

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Cambridge were extremely welcoming. How did it feel to return to econometrics after having experienced so much economic–political upheaval?

Yes. The reason why I had a blank sheet is that although I had completed a number of research articles on time series, I had not initiated any new research. Especially when I started my work as the under-secretary, I was left with little time for research. There were a large number of meetings, committee works, and travels which took me to all corners of Iran in my capacity to see how we could improve teachers' welfare.



Hashem returning to Cambridge from Iran, Trinity College, 1979.

It is true that in 1978 my publications record looked quite respectable, but most of my new research focused on the Iranian economy and did not reflect the significant developments that had taken place in economics and econometrics during the 1973–1978 period. When I arrived in England, I had to decide my research priorities and research areas to focus on. I was only marginally familiar with the developments in the area of time series, distributed lag models, and non-nested hypotheses testing. I had little idea about other major developments, most importantly the rational expectations revolution. This was both good and bad. It was bad because it made me feel paralyzed. I did not know which way to go. It was as if I had to restart the engine. In fact, many colleagues asked me how and in what area I was going to start my research. I was fortunate to receive good advice. Upon my return to Cambridge, both David Hendry (then at LSE) and Peter Phillips, who was then at the University of Birmingham just before he moved to Yale, wrote to me. Colleagues in Cambridge were also very supportive. David Hendry wrote to me about his work on error correction models and was kind enough to send me a

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galley proof of his article, which was to be published in *The Economic Journal* in December 1978. This article was known as DHYS's article, reflecting the initials of the four authors of which Hendry was one.

I believe Rob Engle was visiting LSE at the time, and I heard about his work, although I did not meet him at that time. Peter Phillips was particularly focused on small sample properties of estimators. I was invited to present at LSE, which was a great help to renew my acquaintances and learn about newcomers to the field. I remember Ken Wallis and Graham Mizon, both doing interesting research that caught my attention.

An unexpected break in my research career came about by chance. Geoffrey Heal (now at Columbia Business School) knew me from my time as a Ph.D. student in Cambridge. He had taken up a Chair at Sussex University, and invited me to discuss an article by Robert Barro at a conference he was organizing. The article was about the application of rational expectations to monetary models. Robert Barro and his co-authors were interested in the effects of changes in the money supply on business cycle fluctuations, assuming rational expectations. This was my first introduction to the rational expectations hypothesis (REH). My time in Iran had meant that I was not aware of the important contributions by Muth, Lucas, and Sargent. I approached the REH as an applied econometrician and was surprised by the strength of the assumptions that underlie the REH. In particular, I found the assumption that all agents know the objective true model of the economy to be rather restrictive. In econometrics the issue of model misspecification and model uncertainty had been paramount, and we did not believe any model to be the 'true' model. Then why was it reasonable to assume that agents knew the true model? I realized the importance of modeling expectations but tried to provide a critical account of rational expectations from an econometrician's viewpoint.

In my research on REH, I focused on some of the technical issues, such as the solution and identification of rational expectations model. In this connection I benefited from the contributions of Christian Gouriéroux in France and Ken Wallis in the United Kingdom. I started building my work in the area of rational expectations, which eventually led to my book "The Limits to Rational Expectations", published by Blackwell in 1987, and subsequently reprinted in 1988 and 1989.

This work also reflects your fascination with applied econometrics which has been a hallmark of your academic career. Does this interest go back to your time at the Central Bank and the Ministry of Education in Iran? *It sounds like even early on as an undergraduate student, you were fascinated with business cycles.*

Yes. I had always been interested in applied research. As I have already mentioned, the applied motivation behind my time series and modelling research was to learn about business cycles. However, when I returned to Iran, I realized that there are many more, equally important, applied problems to consider. I realized

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that economics and econometrics are tools for understanding and decision making, with far reaching implications for all areas of social and economic activities. But my experience in Iran also highlighted the importance of having the data in the first place to promote applied econometrics.

I also became fully aware of the importance of teamwork for empirical analyses—you need a variety of skills in economics, statistics, conducting of surveys, data compilations, and computing. It was also clear that for applied research to be socially acceptable and useful, it should be readily replicable. To carry out good applied research one should respect pure theoretical analysis as well as data analysis—both are important in problem solving. Those engaged in pure mathematics and statistics also provide an important service, since without their contributions empirical analysis cannot be based on a sound and rigorous foundation.

Yes, I know you were an early proponent of replicability of empirical results at the Journal of Applied Econometrics (*JAE*). *The journal had a section dedicated to replicability of empirical results and insisted, as far as possible, that authors make their computer codes and data available in an electronic archive maintained by the journal.*

Definitely. One of the main reasons for starting the *Journal of Applied Econometrics* was to fill a gap in the area of replication—I had noticed that in the case of many applied articles the data or codes used to produce the results were not available, even for articles published in top economic journals.

At the time, I was an Associate Editor of *Econometrica* on the invitation of Angus Deaton who had just become one of the Co-Editors. However, I soon realized that despite significant efforts on the part of researchers such as Angus Deaton, the Econometric Society, with many of its influential groups in theory, was not ready to take on the challenge of replicability in empirical economic research. I stepped down as Associate Editor after only one year and started the *Journal of Applied Econometrics* (JAE) in 1986, because I was not convinced that any of the existing economic journals would make replicability the cornerstone of their decision to accept an article. I published an editorial in the first issue, which made replicability the main distinguishing feature of JAE. Although it took a few years to make this idea fully operational, thanks to the relentless efforts of James Mackinnon and many others, we set up the JAE Data Archive and slowly but surely showed the profession that the idea of replicability is in fact feasible and can be done without too much cost. Naturally, advances in data storage and internet access were also enormously important.

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JAE Meeting in Cambridge (2001). Hashem Pesaran (editor), Melvyn Weeks (assistant editor), John Rust, John Geweke, Gill Smith (front) and Rob Engle (JAE Lecturer).



Athens Nov 2003. Conference in memory of Michael Magdallinos. From right to left: Anna Staszewska, Karim Abadir, Hashem Pesaran and Phoebus Dhrymes.

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I also wanted to have an outlet where we could publish serious applied econometric work. While there were already a number of economic journals, I thought there was still space for a journal that emphasized applied econometrics. There was a lot of opposition to this idea too. Many people wrote unsolicited letters to Wiley (the publisher of JAE) opposing the foundation of JAE.

The focus on replicability was indeed early for the economics profession. I remember the emphasis on this in your editorial in 1986 in the first issue of Journal of Applied Econometrics.

JAE's success owes a great deal to many dedicated colleagues. Adrian Pagan from Australian National University, Meghnad Desai from LSE, and Rob Engle, from USCD at the time, were the founding Co-Editors. Later, Mark Watson from Northwestern University replaced Engle. They were an amazing group of colleagues to work with. John Geweke also came aboard, just to name a few early contributors to JAE. Pravin Trivedi (Software Review Editor 1988–1992) made important contributions and was instrumental in developing a software review section to provide JAE readers with critical evaluations of new econometrics software and packages. James MacKinnon helped develop the replication section and as already mentioned, he was in charge of the JAE Data Archive (at Queen's University, Canada). He also took over from Pravin as the software review editor. I am pleased to say that the replication policy of JAE has continued under the new editor, Barbara Rossi, who took over from me in 2012.

The formation of JAE also coincided with the development of Microfit (the time series software program I co-developed with my brother Bahram Pesaran, also an econometrican by training and a Ph.D. student of Denis Sargan). In 1987, Oxford University Press released the first version of Microfit under the title Data-FIT. It turned out that the name Data-FIT was already used by a physical fitness company!

Over the 1984–1987 period, I was preoccupied on three fronts: establishing the JAE developing an econometrics software package, whilst at the same time trying to do my research on the general problem of expectations formation and further development of non-nested hypotheses testing.

Let's talk a little bit more about your early work. Some of your early econometric work was on testing non-nested models. Maybe you can tell us about this work and how you got interested in the topic and the key issues as you see them.

In the early 1970s there was considerable interest in alternative explanations of the same given phenomena, such as Keynesian *versus* Monetary models, lifecycle *versus* Keynesian consumption functions, just to name two important examples. In the case of consumption functions, from an empirical viewpoint, the main difference between the two models was in the specification of the dynamics of the relationship between (labor) income and consumption, which in turn related to the problem of distributed lags that I had earlier worked on under the supervision of Zvi Griliches. I was interested in comparing rival models.

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But I soon realized that "rival" models are not special cases of one another. Usually, classical statistical inference is concerned with testing nested hypotheses, which can be characterized as parametric restrictions on a sufficiently general model. I thought this approach might not be the most efficient way of testing non-nested models. In order to apply the standard testing approaches to the problem of non-nested hypotheses, one needed to build an artificial model which encompassed (nested) both rival models, as was the case when one considered Keynesian and monetarist models of consumption. It was not clear to me that such a mixture (hybrid) model was of interest in itself and might not be acceptable or theoretically coherent if the more specialized models that are nested with this hybrid model are both rejected. Unlike model selection which ends up selecting one of the models under consideration, hypothesis testing could end up with no specific model being selected, with a realization that a new model might be needed to explain the data.

In the course of my research, I accidently came across the work of David Cox, who had published two articles in the *Proceedings of Berkeley Symposium 1961–1962*. After a careful reading of these two articles and some related publications, I realized that Cox had in fact provided a neat solution to the problem of non-nested (or separate) hypotheses testing. However, the examples he had considered were very simple by comparison to the types of rival models being consider in the econometrics literature. I introduced Cox's idea into econometrics, by developing tests of non-nested regression models. I then submitted my article to the *Review of Economic Studies* for consideration. David Hendry was the Editor. He wrote back with two negative referee reports both recommending rejection. Still, he gave me the opportunity to respond to the referees. I was told that the referees were both eminent figures in the statistical literature. As it turned out, I was lucky, since I could easily refute the claim made against my derivations in one of the reports. It turned out that it was the referee who had made an error in his/her derivations and not me! *The Review* accepted the article. Later, as noted earlier, in collaboration with Angus Deaton, we extended Cox's test to non-nested systems of equations, and Aneuryn-Evans and Angus Deaton also extended the test to linear versus nonlinear models, followed by many other important contributions from James MacKinnon, Mike McAleer, Christian Gouriéroux, David Hendry, and Graham Mizon, just to mention a few.

Contributions in this area slowed down, not because the issue of testing rival models was not important, but because many researchers were not interested anymore in comparing one theory against another. Interest shifted to the problem of model selection, rather than hypotheses testing. As you know, the two problems are not the same. Model selection searches for an approximating model, not necessarily a causal or a theory-consistent model. It does not aim to find the truth, but a model which is (in some sense) sufficiently close to the truth. What might be referred to as the "pseudo-true" model (a concept that I have developed further with Alex Chudik and George Kapetanios in a forthcoming article (in *Econometrica*) on variable selection in high dimensional systems. The main idea behind

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hypothesis testing is to investigate the validity (or statistical adequacy) of a maintained hypothesis, like being a Keynesian, and then check to see if there is any statistically significant evidence against the Keynesian position, say in the direction of the classical full employment model. Model selection does not address this important problem. The two approaches complement each other. Hopefully, there will be a return to non-nested hypotheses testing in future, as alternative macro models are developed.

It is certainly true that there was a very active research agenda at the time in which monetarist versus Keynesian models was being discussed. And, of course, the rational expectation hypothesis played a key role in laying out, in a rigorous manner, expectations that were model-consistent and less ad hoc than in earlier contributions. This, in turn, helped economists write down macroeconomic models that could be tested econometrically. The rational expectations hypothesis also turned out to be extremely fruitful in your work. Not only did it give rise to your book on Limits to Rational Expectations that was published in 1989, but also through many of your articles published in the Journal of Econometrics and elsewhere at the time. How did the book come about and why did you choose that title?

The Limits to Rational Expectations, was first published in the United Kingdom in 1987 and then printed in paperback in the United States in 1989. The book arose from an article, which I had published in 1982 on Keynesian versus monetarist models as developed by Robert Barro and others. As mentioned earlier, I had come across Barro's work on the new monetarist rational expectations model in early 1979 after I returned to Cambridge. Barro had also published an article with Mark Rush, which was a very influential work at the time. It claimed that unanticipated money supply shocks are the main source of business cycle fluctuations, and macro-economic policy has no lasting impact on level of output or the rate of unemployment (the so-called policy ineffectiveness proposition). With my Cambridge background, I was not comfortable with this explanation, and got interested in a formal evaluation of Barro's model (against a Keynesian alternative). My work on non-nested hypotheses testing proved to be particularly helpful in this regard, because it allowed me to test a Keynesian model against the classical model that Barro was advocating. It turned out that both the new classical and the Keynesian explanations of unemployment get rejected when tested against each other. This article was published as the lead article in the September issue of the *Economic Journal* (*EJ*).

The article attracted some attention at the time, and I was approached by René Olivieri from Blackwell (now merged with Wiley) who had seen my *EJ* article and wanted to know if I would be interested in writing a monograph on it, thereby expanding the analysis of the Keynesian *versus* new classical/monetarist models. Initially, I was not that interested. As you know, in economics we tend to be more focused on publishing journal articles rather than books, particularly at the start of our career. I was also busy with other more theoretical work on rational

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expectations. However, after some exchanges, I agreed to write a book, which focused on the rational expectations component of the *EJ* article and not the non-nested testing part. The Econometric Society had already invited me to write a monograph on non-nested hypotheses testing, which I had to put to one side to write the book on rational expectations.

In the process of working on the book, I also came across studies of survey data on expectations, which had been studied by psychologists. I soon realized that there was an extensive literature on expectations outside of economics, which was different from how economists approached expectations. The psychology literature approached the problem of expectations formation from a much more general perspective, mainly from a behavioral viewpoint. In that sense, some of the ideas and concepts I developed in the book on behavioral uncertainty, as opposed to exogenous uncertainty, had the beginnings of behavioral economics, which is quite topical nowadays. I realized the importance of strategic interactions in the process of expectations formation and the importance of survey expectations for the analysis of expectations. One's expectation may depend on what others are expecting, and in this regard the issue of the Keynesian beauty contest became relevant and important. I considered heterogeneity of expectations formally and discussed the issue of infinite regress that arises in such contexts. The book was not only about rational expectations. It also covered other models of expectations formation (adaptive, regressive, etc.) and how to measure and test different models of expectation formations.

There was also the problem of how to entitle the book. In selecting the title, I was influenced by the eminent Cambridge philosopher Ludwig Wittgenstein. He had argued that in order to figure out what something is, one needs to know what it is not. Understanding the limits of an idea was, therefore, important, as this helped us to arrive at a more critical evaluation of those ideas. With this in mind, I entitled the book "The Limits to" and not "The Limits of" Rational Expectations. In that sense, the book was not intended as a critique of the REH as such, rather as a clarification of its foundations and the limits to its applicability.

It was not that the REH was not interesting. Mathematically, a model of expectations was required to complete decision rules that involved expectational variables. Survey expectations were not enough to fill the gap. Tom Sargent emphasized this point to me during a conference in Bristol. For example, to solve an infinite horizon optimization problem, one needs to have a model that generates expectations at all horizons into the future. It is very unlikely that an individual would know their expectations at all points in the future, and even more importantly, it is unlikely that one could elicit such expectations for many periods ahead from surveys. Therefore, a formal model of expectations formation is needed. The main issue is to come up with a model of expectations formation which is coherent, and at the same time empirically adequate. REH did meet the first requirement of being mathematically coherent, but did not match the data where different individuals, often faced with the same information, ended up with different expectations, due to differences in cognitive abilities, degrees of inattention,

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learning, and loss functions. Some of these issues are discussed in the book, and simple alternative models to RE are considered. Over the past two decades behavioral economics and the literature on rational inattention advanced by Christopher Sims and Michael Woodford have made important progress in coming up with viable alternatives to RE.

Let's talk about how you have impacted the academic profession in various ways. There are a lot of points on that list. We talked earlier about how you became the founding editor of the Journal of Applied Econometrics in 1986. In fact, you remained the editor for around 30 years before passing the editorial duties to Barbara Rossi. Tell us about what made you decide to start a new journal, and whether you think the Journal has succeeded in its mission.

As I have already mentioned, my reasons behind founding the JAE were twofold. One was to provide an outlet for the application of econometric methods, which were being developed quite rapidly at the time, to economic problems in a rigorous manner. Seen from this perspective, it was a general-purpose journal. It was not like field journals such as Energy Journal or the Journal of Monetary Economics that focused on sub-areas of economics. JAE would publish articles in all areas, micro and macro, finance and energy, environment and health economics. It was important that the econometric community have exposure to all areas of economics through a unified methodology and publication outlet. The second aim of the journal was to address the "reputation" issue. It was clear that empirical research could only be trusted and used in the service of society if the research outcome could be subject to public scrutiny with the researchers themselves made socially accountable. This required the possibility of verification and replication. The aim was not to police the profession in any way. Many individuals were interested, like Ph.D. students, in learning about other people's research and using their data. The scientists I knew at Trinity were quite familiar with the practice of replicating the results of an experiment across a number of laboratories. However, when I discussed the idea with economists they were generally skeptical. Either they thought that replicability was not needed, or they believed it would be too costly to implement; with many authors unwilling to share their data and computer codes.

I was convinced that with advances in computer technology and data storage facilities, and with the increasing emphasis in the profession on citations, the time was right for requiring replicability of articles published in *JAE*. We focused on the narrow sense of replication, meaning the requirement that published results can be readily reproduced from the data set, computer codes, and the readme file provided by the author(s).

As I explained earlier, many great econometricians joined the Journal, which was very important for its success. When we set up the Journal, I wanted to use some of the profits, not for my own benefit, but to help the applied econometrics profession. I suggested this to Wiley. I presume Wiley did not believe that *JAE* was going to make much money, and readily agreed to allocate 10% of the profits

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of the *JAE* or £2,000, whichever was greater, to a research fund. As a result, with the success of the Journal we also received significant funds for promotion of applied econometrics, which helped us to organize lectures, conferences, as well as providing financial support for conferences and workshops organized by others in the profession. The research fund gave the Journal maneuverability to promote areas of research in applied econometrics that went beyond just providing an outlet for publication.

Established in 2011, the International Association of Applied Econometrics (IAAE) now oversees these activities. The Association continues to organize and sponsor other organizations' conferences. It has recently established Student Prizes, awarded to the best article presentation given by a Ph.D. student at the IAAE Annual Conference.

Let's talk, in fact, about this. You are one of the founders of the International Association of Applied Econometrics (*IAAE*) *and you are currently also chairing the Board. This association has only been around for a few years but has already hosted a number of very successful international conferences. What do you see as the main scope for this Association*?

We created the Association in order to take over some of the activities that were part of the *JAE*. I also realized that it is best to separate the editorial activities of *JAE* from its other activities, as the Journal became more successful and the editorial task expanded significantly. I had decided to step down as the Editor of *JAE* to take on new responsibilities at USC—in particular as the Director of the newly founded USC Dornsife Institute for New Economic Thinking, and it was important that my replacement would not be hampered by all the noneditorial activities of the JAE. The foundation of IAAE fitted well with the plans we had for the handover of the *JAE* to a new Editor.

As I mentioned, IAAE has taken over many of the noneditorial activities of the JAE. The first IAAE conference was held at Queen Mary's College, which exceeded our expectations. At the last conference in Milan, we had over 900 submissions. We continue to support Ph.D. students. For example, we give scholarships to students who attend the conferences. We created a poster session because we thought young people would want to come and meet leading figures in their respective areas.

This association is creating a forum, which brings all econometricians together. Over the last 20 years, we have had many conferences. They are mainly in specialized areas such as Bayesian econometrics, time series, forecasting, nonlinear modeling, and program evaluation. All these topics are important, but you still need an umbrella organization. *The Econometric Society* used to perform this function when I was a young researcher. At the European Econometrics Society meetings, I met eminent econometricians such as Denis Sargan, Jim Durbin, David Hendry, Ken Wallis, and many others. But with the expansion of economics, theorists, and econometricians do not mix as they used to during my junior years, and not everyone goes to these meetings.

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This led me to consider why *The Econometric Society* plays a very fundamental role for the whole profession. I think it is a good idea to have a general association, like the *International Association for Applied Econometrics*, which includes all areas of economics that use empirical methods. Replicability is important in that area. Having that forum and allowing younger people to come and mix and learn from other more senior researchers is important.

It is very difficult for everybody to be an econometrician and to become a Fellow of *The Econometric Society.* So we are also planning to establish Fellowships at IAAE to recognize achievements in the general area of econometrics (theory and applied). We continue to sponsor conferences. We provide at least 10 to 12 conference grants to smaller groups annually. We are also hoping to have regional conferences. Marcelle Chauvet (one of the Directors of IAAE who also serves as the Secretary of the Association) has contributed a great deal to the success of IAAE during these initial formative years.

Besides your work with the IAAE, you have also been involved with the founding of the International Iranian Economics Association (*IIEA*)*, which is one of the smaller associations that you mentioned. You were President of this organization from 2013 to 2016. So tell us a little bit about your work with this organization and their objectives.*

As with IAAE, the motivation behind the *International Iranian Economics Association* (IIEA) came mainly from young Iranian researchers who were interested in working on the Iranian economy. There was a demand for an international network that could bring together students and scholars. Many Iranians inside the country do not benefit from the technical expertise that is available externally. It is important to bring them together. The main idea was to encourage research on the Iranian economy. I was President of IIEA for three years and stepped down in June of 2016. I am still an ex-officio member of the Board of IIEA, and plan to attend some of the meetings.

IIEA has also been active in organizing panel discussions on the Iranian economy, which has brought experts from inside and outside of Iran together. I was fortunate enough to have many good Iranian Ph.D. students interested in the Iranian economy starting with Massoud Karshenas (from Cambridge, now at SOAS), Adnan Mazarei (from UCLA, now a Deputy Director at IMF), Mahdi Barakchian (from Cambridge, now at Sharif University), and Kamiar Mohaddes (from Cambridge now at Girton College, Cambridge).

At USC you have become involved with the Center for Applied Financial Economics (*CAFE*)*. What is the aim of this*?

I moved to USC as a full-time Professor when I retired from my position at Cambridge, although I am a lifetime Fellow at Trinity College and spend summer breaks in Cambridge. One reason for my move to USC was to continue my empirical research, which requires some degree of research infrastructure. *The Center of Applied Financial Economics* (CAFE) serves as a network for my research in the area of financial econometrics. It also connects to the expertise at USC and

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elsewhere. Over the past few years, a great deal of my time has been allocated to the development and management of the *USC Dornsife Institute for New Economic Thinking* (*INET*). This Institute with six postdoctoral researchers and three Ph.D. students conducts a great deal of research and networking activities. We have weekly group meetings, seminars, weekly visitors, workshops, and conferences. The Institute, the Center, and the research groups I have been developing at USC have a very similar structure to the research groups I formed in Cambridge. I was lucky enough to obtain funding from the Economic and Social Research Council (ESRC) at Cambridge, which allowed me to attract a large of number of talented postdocs to Cambridge.

The postdocs at Cambridge included Yongcheol Shin (University of York) and Kyung So Im (University of Central Florida), both from Michigan State University who had done their Ph.D. with Peter Schmidt and Jeff Wooldridge, Vanessa Smith (from University of Nottingham now at University of York), Francisco Ruge-Murcia (a student of Jim Hamilton, and now at McGill), Takashi Yamagata (from University of Manchester, now at University of York), and Natalia Bailey (from Queen Mary, University of London now at Monash University). My Ph. D. students from Cambridge were Elisa Tosetti (Brunel University), Andreas Pick (Eramus School of Economics, Rotterdam), James Mitchell (Warwick Business School), and George Kapetanios (King's College, University of London). Other Ph.D. students who worked closely with me, but were not postdocs, included yourself and Alex Chudik (now at the Federal Reserve Bank of Dallas).

Applied research requires teamwork. Many different skills are required that also need to be coordinated. Creating research teams is critical for the success of a research program. I was lucky to be given the funding and fortunate enough to work with such a large number of young, talented, and hard-working researchers.

Your work with Ph.D. students and postdocs has been an integral part of your impact on the economics and econometrics profession. They have gone on to successful jobs today in academia, government, international organizations, and in the private sector. Do you have a particular philosophy for tutoring Ph.D. students? *You mentioned the importance of having weekly meetings, at least at the beginning, to help students form ideas.*

I often act like a mirror. By asking the students to articulate their interests and their aims, they become more conscious of their research interests and the types of skills and data sets they require to deliver. It also helps them not to get too diverted. Most of the students who approach me are interested in applied work, although they can get diverted. For example, I had a very good student, Adam Brandenburger (now at NYU Stern School of Business) who came to Cambridge specifically to do Econometrics. I do not know whether he was your contemporary?

He preceded me.

OK. Adam Brandenburger worked on learning and game theory and then produced one of the major articles that formalized the concept of "common

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knowledge". He did not continue with applied econometrics, although he was interested in it. Therefore, it could be the other way around as well. I think those people who succeed and work with me are the ones who really have a desire to learn about an area that interests them. They are then willing to learn in order to produce a credible and rigorous answer that they can present in a formal fashion.

Yet another way in which you have influenced the economics profession even beyond academia is through your work with your brother, Bahram, with whom you designed a time series econometrics software application originally called Datafit, subsequently Microfit. I certainly remember using Microfit extensively in my early applied work. You were teaching a course, I think, for Financial Asset Management and Engineering (*FAME*) *in Geneva, which made use of this software. Microfit was a menu-driven computer program that predated a number of subsequent application packages. It had great documentation and came with a thick manual that was very detailed with formulas for the various econometric tests, like tests for structural breaks, etc. So how do you see the current role for econometric application programs like Microfit and, nowadays, Stata, SAS, Eviews, etc*? *Is it about lowering the cost of entry for people who want to do applied work*?

I thought software packages were important since they helped to "demystify econometrics" for practitioners. Econometrics is a difficult subject to learn with a wide range of potential applications in economics. You need to know economics, mathematics, statistics, computing, data processing, and data analysis, as we discussed before. Unless you somehow provide an easy and interactive computing platform, many may end up using simple statistical tools such as OLS that might not be appropriate. Admittedly, interactive econometric software packages are also subject to misuse that could be damaging. If you provide a package that generates complicated results by pressing a few buttons, users might not know how best to interpret the results. Some might also abuse the power of software packages. The problem of misguided use or abuse of interactive software packages relates back to the issue of social accountability of research that I raised earlier. We cannot police the research and its output. We can only hope that public exposure, the requirement of replicability and its strict enforcement, will allow other researchers to evaluate publicly released research outcomes. Having social accountability together with easily understandable and accessible software would lead to the democratization of the subject area. We need to have easily accessible (shareware) packages together with social accountability.

The fact that misuse and abuse of software econometric packages are possible, does not necessarily mean that such outcomes do not arise if we did not have interactive software packages. A good example is the ARDL (Auto Regressive Distributed Lag) module which is included in Microfit 5.0. In my view it is better to use the ARDL option in Microfit than OLS when analyzing time series data, even if users are not that familiar with the complex time series theory that underline ARDL modelling. Cointegration is another example. Its underlying theory

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is complicated, but it does arise in applied work and we do need simple computing procedures to apply cointegration techniques to time series or panel data sets. Applied econometricians are routinely encountering empirical problems that involve unit root and near unit root processes with economic relations that are subject to structural breaks. Unit roots and near unit roots are still issues that concern econometricians. Microfit's program includes recursive modeling and rolling window estimation. I see the use of software packages and data sharing platforms as steps towards democratizing the use of data, packages and by sharing of the results online.

I think this is increasingly happening nowadays as people share codes written in Matlab, R, Python, and other coding languages. Let's turn to your work on economic forecasting. One thing people may not know is that for a while you sat on the advisory committee to the UK meteorological office. How would you compare weather forecasting to economic forecasting and should we feel inferior as economists? *What can we learn from weather forecasters more broadly*?

That is an interesting question. Julian Hunt (now Lord Hunt), who in 1992 was appointed the Chief Executive of the British Meteorological Office, invited me to join the meteorological committee of the Met Office. He was a colleague of mine at Trinity. He was appointed after the Met Office had been made an executive agency in April 1990 to make the Met activities more commercial under Mrs. Thatcher's government. Given the fact that the Met Office was under the jurisdiction of the Ministry of Defense, I recall that I had to get security clearance with my name appearing in Hansard (The Official Report of the Parliament). So one of the questions put to us was whether buying an expensive super computer (such as Cray) for forecasting is worthwhile—namely if the expected benefit of improvements in weather forecasting that could result from the use of the more expensive computers outweigh the costs involved.

This type of question, which I know you have also worked on, was a very practical one. It involves decision-based forecast evaluation. I was very much surprised by how much information one needs to answer this seemingly simple question. It was not just about the accuracy of the forecasts. Later I formalized some of these issues in a couple of articles that I wrote with Clive Granger. I guess my experience at the Met was my first encounter with the analysis of Big Data. I learned that they use a great deal of data (95% of which were imported) to produce very short-term-forecasts. There have been clear gains in the accuracy of 24-hour ahead forecasts, with 80% forecast accuracy. Similar advances have also been made in predicting the eye of a hurricane worldwide. I am not sure if we could make similar claims about the forecast accuracy of output growth or inflation worldwide.

As you know we also spent a great deal of time on forecasting and analyzing financial markets after I left Cambridge for UCLA. I worked with Simon Potter (now Executive Vice President of the Market Group at the Federal Reserve Bank in New York) on nonlinear modeling, which I thought was also important in

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forecasting. I was not sure whether nonlinear modeling was going to be helpful to forecasting. Although major progress has taken place over the last three decades, there is still much more to be done. We are able to consider many more variables, using penalized regression techniques such as Lasso, or factor analysis using shrinkage methods. The trouble is that these methods help on average because they focus on predicting the mean. As a result, they are not good at predicting important events that are often relatively rare. Similarly, shrinkage methods work because they put small weights on the more difficult events to forecast.

I was also told by Howell Tong (the author of *Nonlinear Time Series*, OUP, 1993) that nonlinearity is effective in improving in-sample fit, and often fails in out-of-sample forecasting, I guess due to over-fitting, and since the probability of regime switches tends to be small. There is a great deal to learn. We do not really know whether using better and bigger data sets have helped improve forecasting in economics or finance. We hope that improvements have been made, but I believe we are short of documentation. The *ex post* evidence often provided does not answer the type of *ex ante* forecasts issues that I learned were being answered about weather forecasting during my time at the Met Office. Do you know of any articles that address the issue of forecast improvement in economics or finance?

I think Philip Hans Franses conducted a survey of academics that addressed this issue. He found that roughly two-thirds of the participants thought that economists had made modest improvements to economic forecasting over the previous three decades.[2] *Of course, it is difficult to demonstrate that we have systemically gotten better or worse because of events, like the global financial crisis in 2008 and 2009, during which the target variable becomes more difficult to predict. I recall you once told me that one of the things that greatly helped improve the precision of the UK weather forecasts was merging the data from the United Kingdom with data from the French side of the channel. Do you think something similar could happen in economics with maybe granular network data or data on financial institutions' leverage, liabilities and, maybe, counterparty risk exposures*?

Yes, particularly as more spatial and network data become available, connections could be through space or other channels such as social or economic networks. But to incorporate network information in forecasting we need to know how networks are formed, and we might even need to model how network connections change over time. We have not gotten there yet. Some network theorists are now working on endogenizing the connections, but these attempts are limited in scope at the moment. As Internet data become available, it might be possible to use network or spatial structure to improve forecasting.

As an example, consider the problem of predicting the incidence of influenza as it diffuses across time and space. If you know that influenza is spreading in northern California, for example, most likely it will travel to southern California.

[2] See P.H.B. Frances, 2004, "Do we think we make better forecasts than in the past? A survey of Academics." Interfaces, vol. 34, 6, 466–468.

https://doi.org/10.1017/S0266466618000336 Published online by Cambridge University Press

Therefore, diffusion across space and networks and the delay in such diffusion could help in forecasting. If you do not have spatial data and you only make use of data from Southern California, you deprive yourself of an important source of information and that could be inefficient. That is why I have spent some time recently on understanding financial and production networks and how they are likely to help us in forecasting.

The way the financial markets pool information is quite complex. In fact, if markets were efficient, one would not need to know the network structure. One only needs to condition on past prices. That is the view championed, for example, by Eugene Fama. However, I do not think this is true. While it could be true for most of the time, there are episodes where the structure of financial networks could be helpful as well.

More recently, I have developed tests of market efficiency, which clearly show the existence of such episodic patterns of market inefficiency. There are many periods, possibly 90–95% of the times, where it might be sufficient to just use past prices to forecast market returns. However, there are also periods during which past prices might not be enough. Network structure, connections, and other types of data could help. The problem is that we do not know how to identify such episodes in real time. This is another area where more research is needed and could be promising.

We have, therefore, two problems to solve: one is to model the interconnections and the other is to figure out under what conditions they become important. It is not like weather forecasting. The main difference is due to the strategic nature of interactions that take place in the market, which seems to be absent in climatic models. Market interactions can alter beliefs and expectations, which could be accompanied by sudden switches from one kind of state or regime to another. One hopes that by using larger data sets, we would be able to develop models of cross-sectional dependence for use in forecasting and policy analysis.

The trouble is that most of the econometric literature on panels, for example, has always assumed, until recently, that all errors are cross-sectionally independently distributed; yielding, basically, no network effect. Even when such dependencies are acknowledged, it is assumed that they do not alter the consistency of the estimators and one only needs to cluster the standard errors to accommodate for error cross-sectional dependence.

A large of part of my research over the past decade has been concerned with the problem of testing for cross sectional dependence, measuring the strength of cross dependence, and estimation and inference in panel data models subject to cross-sectional error dependence.

One of the questions that you have written on extensively is market efficiency. In fact, this is an area that you know a lot about not only as an academic but also from your work in the financial market. Unusual for an econometrician, you worked for a hedge fund for two years from 2000 to 2002. Tell us a bit about this

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experience and the ways in which your training as an econometrician helped you in this position.

Well, you are the guilty party in that because, if you remember, we published an article in *The Journal of Finance*, in which we introduced the idea of recursive modeling.

Yes, in 1995.

1995, yes. You carried out all the computations, which were relatively heavy at the time.

Yes, even though it is quite trivial using today's computing power.

This article led to a great deal of further research. The hedge fund wanted me to develop an automated trading system based on the recursive modeling procedure that we had advanced in the *Journal of Finance* article. I agreed to take a sabbatical leave from Cambridge and work on this project with the help of my brother (Bahram). We also had other researchers in the group. I believe we were one of the first in the industry to develop a fully automated trading system, from the raw data stage to the forecasts, and then direct to the trade orders, which were then sent as emails to traders for implementation. We built a complete trading system using econometric and risk management techniques which was then tested rigorously by independent researchers (who were not in our group).

This was an eye-opening exercise for me and taught me a great deal about application of econometric methods in real time. I also gained a practical understanding of how markets function. It became clear that the main issue was not whether financial markets are efficient or not, but under what conditions they become less efficient, and therefore predictable. We knew that one could make money when the markets became less efficient or inefficient. It was clear from our joint work, that when market volatility rises significantly, learning becomes more difficult, which would give rise to some profitable opportunities. When I looked at the trading patterns of the model, we realized that this was exactly the case. We did not know about Lasso at the time, so we developed our own procedures to deal with a large number of predictors that we encountered.

We filtered the data in many ways before using them for forecasting because we knew that unit root and spurious correlation were a problem. This contrasts with the current empirical applications using Lasso that tend to include all sorts of variables in the penalized regressions without checking them for persistence and other nonstandard features. Economic and finance data are quite complex and cover all sorts of variables. One needs to carefully consider the choice of variables to include in the forecasting exercise. Just because one is using Lasso, it does not mean one should include any variable that comes to mind indiscriminately.

In the late 1990s most researchers considered only a few variables for forecasting. It was also clear that our recursive modeling was not operational if you had, say 60 variables, since this would give rise to 2^{60} possible model combinations, and in such cases recursive modelling was not applicable. That is why we had to think of other ways to forecast with many variables. We used a precur-

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sor to the new one covariate at a time multiple testing (OCMT) procedure that I have recently developed with Alex Chudik and George Kapetanios.[3] OCMT is much more flexible and faster than Lasso. It also deals with serially correlated regressors.

You talked about automated trading systems and I know you list real-time modeling in economics and finance as one of your current areas of research. This was, of course, part of the experience working for a hedge fund that you just described. How far do you think we can go in terms of automatic real-time forecast of the state of the economy and which types of data do you think will be potential game changers there?

I think you have to be realistic about the limitations of these methods because of the very different sources of data that are available nowadays. I do not think a universal forecasting method can exist. A method that might work for forecasting output growth, could do poorly for forecasting asset prices, as you probably know. We need to understand the nature of the forecasting problem reasonably well, the frequency and the horizon of the forecasts, and the loss function associated with the forecasting exercise. The loss function matters under misspecification, which is the rule rather than the exception. In a hedge fund environment, one tends to focus on high frequency forecasts, which might require different methods from those required to generate quarterly and yearly forecasts. One needs to be eclectic and understand the forecasting environment first before embarking on the forecasting exercise. There is no general theory that I could recommend. I believe analysis of structural breaks, whether in the form of time-varying coefficients, discrete change, or regime-switching models are important and should be considered. Most economies go through periods of episodic change, and these changes tend to occur often when they are least expected.

Modeling this kind of switch is very difficult. For example, I do not think Markov switching models would be successful because of their recurring nature. As you know, with Andreas Pick and others I have been trying to develop forecasting procedures that are reasonably robust to structural change. This is done by considering alternatives to exponential down-weighting of observations. Such procedures provide a compromise between forecasting in low and high volatility periods. The outcome is intended to be better on average, although the procedure might not work well in periods of calm when compared to standard expanding windows forecasts.

Perhaps, less bad is the appropriate term here.

Exactly. To be more specific, suppose high volatility occurs with a small probability, but it is not known when this occurs. The aim is to derive a forecasting strategy which is reasonably robust to this type of low probability event, by integrating/averaging out the alternative forecasts obtained under different volatility scenarios. Therefore, the resultant forecast would not be as efficient as if you

[3] Postscript: This article is now published in *Econometrica* (July 2018).

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had rightly assumed the world was stable and, in fact, it turned out to be stable. You still might not forecast well, but hopefully your error on average would be less than using expanding or rolling windows. I think there are some possibilities along these lines.

You are hedging model breakdowns.

Yes. Working at a hedge fund taught me the importance of robust inference and decision making as compared to often-unattainable optimal procedures. I also became aware of the importance of relating the information pooling feature of markets, which was well known in finance, to econometric analysis of panels, which was less known or recognized. In the case of panel data models with many variables (say securities) over many time periods, one can eliminate the effects of idiosyncratic errors by simple or weighted averaging, very much in the same way idiosyncratic risk gets diversified by using portfolios. The basic idea was simple and involved working with standard panel regressions but also adding cross-section averages, subsequently referred to as cross-section augmented regressions, which I developed further with Alex Chudik. These contributions extended my earlier work on the Correlated Common Effects estimator to dynamic panel regression models.

My first article on this topic appeared in *Econometrica* in 2006. The article was a direct consequence of my understanding of how markets work. This work also related nicely to my earlier research on global vector autoregressions (GVAR) which was used to analyze economic interdependencies across countries. The key was to recognize that since the world economy, individuals, firms, and markets are interconnected, they should not be modelled as closed systems, rather as a collection of interrelated entities. But this in turn raised the difficult modelling issue of how to estimate large dimensional systems that are connected. I showed that the problem could be solved by using cross-section averages as sufficient statistics and estimate panel regressions conditional on these cross-section averages. For forecasting, you then needed to model the individual equations (disaggregated part) with an additional model for the cross-section averages (the aggregated or common part).

In terms of the timeline, your work on GVAR models came between your interest in time series analysis and your work on panel models. Tell us about global GVARs and how they evolved, for example, through collaboration with organizations such as the European Central Bank (ECB).

The idea of GVAR came to me in 1998, when I was a consultant to Oliver Wyman, a credit-consulting company. Frank Diebold, whom I got to know during my visit to the University of Pennsylvania in the Fall of 1993, introduced me to Til Schuermann who was in charge of the Oliver Wyman Institute and wanted someone to help them with econometric modelling in Europe. The year before I started as a consultant to the Oliver Wyman Institute, there had been a major financial crisis in Southeast Asia in 1997 that shattered many economies in South East Asia, particularly Indonesia and Thailand, without any notable lasting effects

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on the United States or Europe. Oliver Wyman wanted to develop credit risk models that could cope better with such (possibly) localized crises, and in Europe they were interested in the effects of financial crises on loan portfolios of major banks in Germany, in particular.

In those days, there was very little econometrics in risk management models. They tended to use unconditional models, which involved means, variances, and covariance matrices estimated on a large number of asset returns. They relied on bootstrapping from the historical distribution, in order to characterize market and credit risks.

In 1998, I accompanied a team from Oliver Wyman during their consultation visits to one of the major German banks, who were worried about a similar financial crisis occurring in Turkey, as they had exposure to Turkish counterparties. The question was how to quantify the impact of a shock in Turkey or in other regions on the loan portfolio distribution of the bank. To adequately address their concern we soon realized that we needed to develop a globally interconnected economy model, which naturally led us the GVAR.

Till Schuermann, Scott Weiner, and I developed the first GVAR model using excel sheets. In 1999 I left Oliver Wyman, but it was agreed that we could publish the results of the GVAR model and its application to the bank loan portfolio. The article was presented as the *Journal of Business Economics and Statistics Invited Address at the Joint Statistical Meetings, San Francisco, August 2003*, and subsequently published in JBES in 2004 together with comments by Badi Baltagi, Soren Johansen, Ken Wallis and others, followed by a Rejoinder by us.

The ECB's (European Central Bank) interest in GVAR came later, after I returned to Cambridge in 2001. They were interested in learning about how US economic shocks affect Europe as a whole and the different member countries within the Euro Area. The matter was first discussed in a meeting held in Cambridge with Sean Holly (the Director of Research in Cambridge), and Vanessa Smith who had joined us as a post doc. We agreed to collaborate on the condition that ECB would allow us to publicly release the data and the codes.

Filippo di Mauro, who was the Managing Director in charge of the project, understood the importance of the GVAR and gave us his full support right from the start. He was instrumental in the success of the project from data compilation to writing up and dissemination of the results. As you can imagine, a great deal of resources was needed to compile, update, and extend the GVAR data set. The number of countries was increased to 33 and the data updated to the end of 2003. We also had to deal with the introduction of the euro in 1999 which was right in the middle of our data set. The GVAR computer code was also written afresh from Excel to Gauss (and later to Matlab). In this connection Vanessa Smith put in a great deal of effort to make the codes comprehensive and user friendly. Later she was assisted by Alssandro Galesi. The results of this project were published in *Journal of Applied Econometrics* in 2007.

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Later I also helped with the development of the GVAR model for the Bank of Switzerland (Katrin Assenmacher), and the American Development Bank in Washington (with Alessandro Rebucci and Ambrogio Cesa-Bianchi). Others have also developed GVAR type models at the World Bank, the IMF, and elsewhere. The data and the codes are made available online and managed by Vanessa Smith. https://sites.google.com/site/gvarmodelling/gvar-toolbox.

There are also courses on GVAR provided by Vanessa through Economic Modelling. The GVAR data set gets updated quite regularly—the latest data set was prepared with the help of Kamiar Mohaddes (University of Cambridge) and Mehdi Raissi (IMF) and covers the period 1979(1)–2016(4).

In addition to cross country applications, GVAR has also been used for forecasting, analyses of financial interconnections, the effects of commodity prices on the global economy, and the effects of food price shocks on inflation, just to name a few. Some of these applications are included in the GVAR Handbook (edited by di Mauro and Pesaran, 2013), with further details provided in the survey article (Theory and Practice of GVAR Modelling) written jointly with Alex Chudik and published in the *Journal of Economic Surveys*. The GVAR framework can also be used for the analysis of networks, although this is an area which has not been adequately explored.

How would you compare GVAR models to other approaches that are currently very popular amongst Central Banks such as Dynamic Stochastic General Equilibrium (*DSGE*) *models*? *How useful are DSGE models in your opinion*?

The DSGE model is a theoretical construct used for the analysis of interconnections between a few macro variables within a single country, often viewed as a closed economy. But there have been some attempts at IMF to extend the DSGE framework to a multi-country setting, which in my view naturally leads to what can be labelled DSGE-GVAR. We did such an exercise in the article written jointly with Stephane Dees, Vanessa Smith, and Ron Smith, entitled "Constructing multi-country rational expectations models", which was published in 2014 in the *Oxford Bulletin of Economics and Statistics.* The reason why the current mainstream DSGE models do not interface with the GVAR is due solely to the single country nature of these models. I believe DSGE models will start to have GVAR features as soon as they are considered in a multi-country setting. I do not think we have made enough progress in this area. I guess, 30 years ago when the US economy was a truly dominant economy in the world system, maybe it was reasonable for Sims and others to consider the US economy as a closed system containing only 6 variables. This approach is no longer applicable to the analysis of many macro or financial problems, even in the case of the United States. In my work on the Iranian economy, it was clear from the outset that we must study Iran in relation to the rest of the world. I view a multi-country framework as the key to the analysis of business cycle fluctuations, a problem that attracted me, as I mentioned earlier, to time series and econometrics when I was an undergraduate

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student in Salford. International economic relations studies fully recognize the importance of multi-country and multicultural-settings that operate within and across all nations. But DSGE modelling is lagging behind. I am sure that when the DSGE literature begins to seriously consider the multi-country aspects of business cycle fluctuations, a GVAR structure of the type that I have developed will become central to such an endeavor.

You have written articles that adopt a Bayesian perspective. What is your view on Bayesian methods?

Well, as you know, I am very interested in Bayesian analysis. One example is the article we wrote jointly with Davide Pettenuzzo, which employed a Bayesian numerical method to deal with structural breaks, which was published in *Review of Economic Studies* in 2006. More recently, I have written an article with Gary Koop and Ron Smith (published in JBES, 2013) which considers the problem of identification in Bayesian DSGE models. This article has been important for the development of my ideas about Bayesian analysis and Bayesian learning. In Bayesian analysis identification is often judged by a comparison of the posterior distribution of a parameter with its prior. However, priors and posteriors can differ even when the parameter in question is not identified, or weakly identified. For identification it is not sufficient that the prior differs from the posterior; one also needs to consider the rate at which the posterior precision rises with the sample size. For example, for the parameter of interest to be $\sqrt{T}$ identified then the posterior precision must rise linearly with the sample size, T. The same also applies to weakly identified parameters—namely the posterior precision of such parameters will not rise linearly with T; they could be rising with T but their rate of increase will be slower than T. In view of this theoretical insight we proposed two Bayesian indicators of identification which I believe could be potentially important for empirical analysis of Bayesian models.

I have always been attracted to Bayesian analysis as it represents a coherent framework from a personal point of view. As an individual, you need to have priors since you do not have enough data ever because you have a fixed history that you can rely on. On a personal level, I am definitely Bayesian. Bayesian learning makes a lot of sense. When you came to Cambridge, I suggested to you to work on Bayesian learning which you did successfully with a QJE publication in 1993, as I am sure you remember.

Yes.

So I am not against Bayesian analysis. The problem is that when one is concerned with inference or a counterfactual exercise, the use of Bayesian analysis could become problematic. For inference, the question is—how much data is required before one can become sufficiently confident about the truth of a given hypothesis? In such cases, priors should not play a role, and Bayesian and Classical inference must eventually coincide; and hence identification becomes as important whether one follows a Bayesian or a Classical approach. The fact that the posterior distribution always exists even for nonidentified parameters is not the

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point. There is also the issue of social accountability and social communication of the results. On a personal level, there is no restriction on the choice of the priors—so long as they are proper. But for social communication the choice of the prior matters, and one cannot simply state that we learn since the posterior differs from the prior. This point is also related to the issue of the robustness of Bayesian results (inference) to the choice of the priors. The article with Gary and Ron was also relevant in this respect as it showed the nonrobustness of Bayesian inference to the choice of the prior when dealing with nonidentified or weakly identified parameters.

We talked earlier about time series econometrics and GVARs. Much of your work in recent years has been on panel modeling. What generated your initial interest in panel data and what synergies do you see between time series modeling and the analysis of panel data?

My interest in panels goes back to my joint work with Ron Smith, and my experience with predicting many securities at the same point in time. There is also the issue of financial networks and interdependence, I have already alluded to, that are important features to be exploited when forecasting.

In fact, GVAR is also a modeling approach for high-dimensional panel data sets. However, because the time dimension is relatively large and the number of countries are relatively small, one could rely on time series techniques to estimate the country-specific models in the GVAR. But, even with 30 countries, that would involve modeling 435 connections across the countries, which meant dealing with large-dimensional covariance matrices. The issue of how to test for and model such interconnections also became important. The panel aspects of the problem became of importance because of the large number of countries involved. This in turn led me to the analysis of cross-sectional dependence in classical panel data models where the time dimension is fixed and the cross dimension (N) is very large but the slopes across the units are restricted to being homogeneous.

My article with Ron Smith on heterogeneity in dynamic panels was published in 1995 in the *Journal of Econometrics*. At that time we did not pay attention to error cross-sectional dependence. I knew dynamics and heterogeneity were important. We showed that you could end up with standard panel estimators that are inconsistent even when N and T are large, if one ignored slope heterogeneity in the case of dynamic panels.

When I went to the hedge fund, I realized the importance of interconnections and cross-dependence, and upon my return to Cambridge in 2001, I started working on testing and modelling error cross-sectional dependence in panels. The GVAR research (which we had started in 1998) turned out to be exactly the appropriate framework to deal with all three features (dynamics, heterogeneity, and cross dependence). Dynamics and heterogeneity were important due to historical, social and cultural differences across countries. There was also dependence across countries through trade, finance, cultural, and political change.

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Most theoretical results in economics relate to the long-run equilibrium, and are generally silent on the speed with which individual economies move towards their equilibrium. It is, therefore, clear that one needs to allow for heterogeneity simply because there are differences in institutions and the way different individuals or societies react to shocks. We must take into account these dynamic differences even if the long-run outcomes turn out to be the same across countries.

I realized that we must take the issue of interdependence much more seriously than was typically done in empirical analysis. From my work with Ron Smith, I was also aware that to allow for (i) dynamics, (ii) heterogeneity, and (iii) cross-sectional dependence, we needed panel data sets with both large N (cross section) and large T (time dimension). The issue of the relative expansion rates of N and T was discussed more formally in an important article by Peter Philips and Roger Moon (*Econometrica*, 1999) which further developed my work with Ron Smith (*Journal of Econometrics*, 1995). Their work highlighted the importance of letting both N and T tend to infinity jointly.

I also realized it is important that we test for cross-dependence in large panels, and distinguish between factor and spatial or network dependence, and investigate whether the cross dependence is strong or weak. Over the last 10 years I have focused on development of concepts, tests, and estimators to address all these aspects of large panels.

Your work seeks to, in some sense, provide an integrated account of time series and panel data econometrics. As part of that objective, you recently published a volume of over 1,000 pages on time series and panel econometrics. Tell us how this book came about, how long it took for you to write the book, and what led you to work on it?

I have to admit, I am a little embarrassed by the size of the book which prevents students from carrying it to the classroom! Many years ago, I had the idea of writing a text book with Adrian Pagan but we were both very busy and decided not to follow it through. The main reason for writing the book was to make my notes written over the past 25 years (for the lectures that I had given at Cambridge, UCLA USC, University of Pennsylvania, and for Microfit courses running annually over many years) more widely available. I was often asked if the lecture notes could be circulated, and soon realized that I had little control on the dissemination of my lecture notes, and I thought the best solution was to publish the notes as a book. Of course, this is easier said than done. As you know from your own experience, putting together a large book can be quite time consuming and at times frustrating. As soon as you finish one part of the book, you realize that some of the other materials that you had completed need updating and/or extending. The literature moves on fast.

There are three parts to the book. It is designed to help students and researchers at different stages of their learning in time series and panel data econometrics. For example, undergraduates start with the first two chapters, but they will not be able to follow the book if they skip ahead and go to Chapters 6, 7, or 8. A

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Ph.D. student could begin with these more advanced chapters, but return to the earlier, less advanced chapters to refresh what they had already learned in the past. I thought it was better to write a book that was not just for undergraduates or post-graduates, time series people or panel people, but to try to bring these two audiences and dimensions together. The book does not just integrate time series and panel data. It also operates at different levels. I am hopeful that this aspect of the book is also recognized by the readers.

Panel data econometricians have done enormous work over the past 40 years. Cheng Hsiao, my colleague at USC, has an excellent book which is widely used, which he has revised recently. It now also includes chapters on large N & T panel data problems. New editions of the well-known text by Badi Baltagi also cover large N and T panels. I view my book as another contribution, which in many ways complements these excellent texts.

Your work covers many different areas—far more than most economists or econometricians ever get to work on. What makes you work in so many different fields?

I am by nature curious and optimistic, which has led me to look into many different problems, although they end up being related, in some way or another. If I come across an interesting problem, I tend to follow up and try to learn more about it. Of course, there is a limit to how much you can learn and I am aware that one cannot be too diversified in academic life. I do not think young researchers should work in many areas, especially at the start of their career. You really need to learn and show that you know a specific area well. But, as one's career develops, as an educator, researcher or consultant, comes social responsibility and the need to contribute to policy debates and issues of public interest that lies in our area of expertise.

For example, coming from the Middle East, I was attracted to learn about oil markets. I worked on oil and the Middle East and published an article in the *Economic Journal* in 1990 on the development and exploration of oil fields, with application to North Sea oil fields. I received the Royal Economic Society prize for this article, which was not an econometrics contribution as such, but was driven by my general interest. I was especially pleased since the prize money was tax free (in the United Kingdom) and arrived at an opportune moment when our young family was growing.

I have also written a number of articles on the Iranian economy and continue to do so as there is considerable demand for commentary on the Iranian economy, both inside and outside of Iran. Over a number of years, I have been fortunate to work with very good Ph.D. students, some of whom you know. Massoud Karshenas was my first Iranian Ph.D. student who did research on the Iranian economy. He is now an internationally recognized Professor at SOAS, University of London. More recently, Kamiar Mohaddes has been working with me on the application of global modeling to the oil market. For example, we have an article on how to do counterfactual analysis of oil sanctions, such as those recently im-

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posed on Iran. For this purpose, we used the GVAR approach to investigate the effects of sanctions on the Iranian economy. To sum it up, most of my research on Iran stems from my original interest in oil markets and the Iranian economy, and my love of econometrics.

Do you experience synergies and cross-fertilization from working on different topics, perhaps even at the same time?

Yes, sometimes, but not always. For example, in the case of modeling the Iranian economy, the GVAR modeling approach becomes particularly relevant, since Iran is a small open economy, which is very much dependent on the international price of crude oil. However, when you are dealing with specific sectoral or institutional issues, the synergy is less apparent. Consider, for example, the role of OPEC in the determination of oil prices. To investigate this problem one needs specific knowledge of the history and the institutional features of OPEC and the major oil companies, known as the Seven Sisters. There are no shortcuts.

Another example is my work on the 1979 Revolution and its aftermath for the Iranian economy. My first article in this area, written in 1980, was in fact published first under a pseudonym, as I was not sure about its implications for my family in Iran. The article appeared as a commentary in the *Cambridge Journal of Economics* under the name of "Thomas Walton." But the referees of the article knew I was the author and ended up revealing my identity when I attended a conference on Iran in Chicago a few years later. A revised and updated version of the 1980 article has now been published under my own name.

Despite having worked in so many different areas, do you have a favorite article?

Favorite? This is like asking if you have a favorite child, right? It is a difficult question to answer.

I would like to start with my 1974 RES article, which introduced the idea of non-nested hypotheses testing into econometrics, and the 1995 JoE article with Ron Smith which was the first to consider slope heterogeneity in dynamic panel data models, followed by the 1995 *Journal of Finance* article (written with you), which I believe was the first attempt to introduce recursive modelling in finance. The 2004 JBES article with Til Schuermann and Scott Weiner, which introduced the GVAR modeling approach, and the 2006 *Econometrica* article which introduced the CCE estimators for panel data models with multiple factor error structure. Other articles that come to mind include my 1981 JoE article on identification of rational expectations models, my article on system of dependent capitalism in pre- and post-revolutionary Iran published in the *International Journal of Middle East Studies* in 1982, my 1999 *Journal of Economic Growth* article with Michael Binder on stochastic growth models, my articles with Alex Chudik on high-dimensional VARs, the JBES article on identification of Bayesian models with Gary Koop and Ron Smith, and articles on tests of weak cross-sectional de-

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pendence (the 2004 WP version, and the related 2015 article published in *Econometric Reviews*). Finally, I should also mention my most highly cited article, written with Im and Shin, on panel unit-root tests, published in the *Journal of Econometrics* in 2003.

Looking ahead, how do you think that the access to "big data" will affect applied econometrics and which areas of applied econometrics do you find particularly exciting at this point in time?

That is difficult to predict. I think big data has already started to have some influence on the way empirical research is carried out, and this has started to impact both the theory and the practice of econometrics. There are numerous conferences on applications of Big Data Analytics and their use for the analysis of economic problems. However, big data has different meanings to different people. The GVAR approach can be viewed as a Big Data technique, but it does not meet the engineering standard as characterized in various machine-learning techniques now used widely across disciplines. In economics increasing availability of large household and firm data that can be integrated to macro-prudential analysis could play a very important role in the future and is likely to revolutionize the way macro-econometrics is practiced. But, it is important to bear in mind that the use of big data for economic policy analyses and macroeconomic forecasting relates back to what we discussed earlier.

Large data has existed and has always been a problem in the past. For example, we have data across regions over many years. We have had data on house prices across 374 Metropolitan Areas in the United States over many months. However, when you have to predict home prices at an aggregate level, many still use simple single equation time series models. We need to combine aggregated and disaggregated data at all levels for better forecasting and policy analysis.

You are suggesting more emphasis on disaggregated data?

Yes, we need to make better and more innovative use of disaggregated data. I wrote an article in *Econometrica* with Richard Pierse and Manmohan Kumar in 1989 on the aggregation problem. I was interested to know when forecasting an aggregate, would it better to forecast the aggregate directly or its components and then aggregate back to the total? At that time, we were not aware of common factors or network structures and did not know how to exploit such features in forecasting from the bottom up. In cases where there are only weak connections across units that form an aggregate, it is often the case that forecasting aggregates directly might be a sensible strategy. The aggregation problem is less serious if there are no connections, or if the connections are weak. This is an area that benefits from further research.

It is clear that the analysis of large data sets presents its own problems. Standard statistical methods need not be applicable. One often has to carry out many tests, and multiple testing becomes an issue. When you have a lot of data, there are a lot of connections. High dimensional econometric or statistical methods become an important part of the analysis of economic data at the macro level. These issues

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are also relevant to finance. If your interest is to predict intraday movements of asset prices, it is difficult to figure out how big data can be helpful in that setting. If we observe data for a few seconds or minutes during the day, the time interval might be too short when considering a very large number of covariates. Clearly, one can apply high-dimensional techniques for financial forecasting in the case of daily, monthly, or quarterly time series.

We need to examine both commonalities and idiosyncratic features of the data and decide which aspects are most relevant for our analyses or forecasting exercise. I do not mean idiosyncratic effects that are purely random, but unit-specific effects that could be related through social or economic networks (banks, companies, production, etc.). As an example, I would like to mention the article I presented here at UCSD, entitled "Double-Question Survey Measures for the Analysis of Financial Bubbles and Crashes", written with Ida Johnsson, which makes use of new survey data to measure bubble and crash indicators from the disparity between individual expectations and their valuation beliefs.[4] We argue that using aggregate data to understand bubbles or crashes might not be that fruitful for policy purposes, due to the sudden and abrupt nature of such phenomena. Instead, we propose a new double-question survey whereby an individual is presented with two sets of questions; one on beliefs about current asset values and another on price expectations. Leading indicators of bubbles and crashes are proposed and their potential value is illustrated in an out-of-sample forecasting exercise where it is shown that forecasts of house price changes that make use of bubble and crash indicators perform significantly better than a benchmark model that only uses lagged and expected house price changes. The main advantage of survey-based measures of bubbles and crashes is their spatial and timely nature.

We need to think about other types of new data to compensate for the limitations of macro data. More use can be made of administrative data. There are many such data sets but unfortunately, for confidentiality reasons, they are not made publicly available.

Why not share such data?

Yes, I am in favor of data sharing. Privacy is important, but I am confident that it would be possible to mask the identities of the respondents and share the data sets. I believe more can be done on this front. The problem is accentuated since there is no incentive for the private sector to share their data either, and it is often protected even more vigorously than the administrative data. Government agencies and the private sector should consider releasing such large data sets for public use, possibly with some time delays.

We have seen a vast growth in the types of data that are becoming available to economists in recent years, and you mentioned some of the challenges in the econometric modeling of new data structures. We see nowadays that many of our econometrics-trained Ph.D. students are recruited not just to academia, but also

[4] This paper is forthcoming in the Journal of Business Economics and Statistics.

https://doi.org/10.1017/S0266466618000336 Published online by Cambridge University Press

join hedge funds or work for Amazon, Google, etc. Is this a golden age to be an applied econometrician?

In terms of economists going to the financial sector to work, this is not new. I welcome it and think there are many new opportunities for our graduates to make more money and get recognition outside of academia. However, that does not necessarily mean that our profession is going to benefit from it, unless there is private sector support for independent research that can be published, and there is at least some sharing of data. Private companies also have public responsibilities, particularly in the new world of internet and data harvesting. Some of the students that we train go to these big companies as econometricians and the students and the companies that employ them both benefit, as they should. But it would be good if our profession could also benefit from such associations. This might need innovative partnerships between financial companies and universities, of the sort we have witnessed in the areas of computing, medicine and engineering.

As a profession, we also need to adapt our teaching material to reflect changes that are taking place around us in the society at large. We cannot just teach the same techniques year after year. The problem is particularly acute at the undergraduate level, where changes to the curriculum are likely to encounter resistance and could be complicated.

Thank you.

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