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THE ET INTERVIEW: ADRIAN PAGAN

Interviewed by Christopher L. Skeels
The University of Melbourne



In 1947, Adrian Rodney Pagan was born in the small country town of Mungindi and spent his early years in a rural community nearby. He went on to complete a Bachelor of Economics at the University of Queensland (UQ) in 1968, attaining First Class Honours and being awarded a University Medal, followed by a Ph.D. in Economics at the Australian National University (ANU) in 1972. Thereafter came a series of positions at ANU, the University of Rochester, Nuffield College (Oxford), the University of New South Wales, Queensland University of Technology (QUT), University of Technology Sydney, the University of Sydney, and, most recently, the University of Melbourne. In amongst all of these have been visiting positions at Princeton, CORE (Catholic University of Louvain, Belgium), the Institute for Advanced Studies in Vienna, Yale, Monash University, and Johns Hopkins University.

Over the course of his distinguished career, Adrian has received many honours and distinctions. He is a Fellow of the Econometric Society, the Australian Academy of Social Sciences, Journal of Econometrics, the Society for Financial Econometrics, and the Society for Economic Measurement. He is Medallist Fellow of the Modelling and Simulation Society of Australia, a Journal of Applied Econometrics Distinguished Author, and a Distinguished Fellow of the Economic

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Society of Australia. In 2000, with co-author Mardi Dungey, he was awarded the Best Paper prize by the Economic Record and, in 2001, he was a recipient of the Centenary Medal 'For services to Australian society and the economy.' In addition, from 1995–2000, Adrian served as a Member of the Board of the Reserve Bank of Australia (RBA), the nation's central bank.

Adrian has been very active in service to the profession having served on Editorial Boards of the Journal of Applied Econometrics (where he has also served as both Co-Editor and Associate Editor), Advances in Computational Economics (Kluwer Academic Publishers), Australian Economic Review, Macroeconomic Dynamics, Economic Record, Handbook of Applied Econometrics, and the Journal of Financial Econometrics. He has been Co-Editor for Themes in Modern Econometrics (Cambridge University Press), Advanced Texts in Econometrics (Oxford University Press), and Econometric Theory. He served as Associate Editor for Econometrica, Economics Bulletin, and the Journal of Econometrics, as well as being a member of Advisory Boards for the Centre for International Macroeconomics and Finance (University of Cambridge), and the Centre for Applied Macroeconomic Analysis (CAMA) at the ANU. Finally, he served a term as Co-Chairman of the National Centre of Econometric Research (QUT).

Like the many activities listed above, Adrian's own research career is full of activity and variety. Adrian would probably claim that his primary interest is how best to use econometrics to solve real problems. This quest has taken him across many areas, including: time series analysis, the problem of generated regressors, macroeconometric issues, financial econometrics, nonparametrics, and the list goes on. He is something of a 'swiss army knife' of econometrics. Insatiably curious, one of his great skills is to be able to synthesize seemingly disparate literatures and distil the commonalities, thereby opening the way for others to follow. Naturally gregarious, Adrian loves the hurly burly of lively debate, and is happy for the debate to be on pretty much anything. He is also imbued with a tremendous generosity of spirit that was celebrated by many of his students and colleagues (both past and present) at a Fest in his honour in 2009. I first encountered this side of Adrian at the Econometric Society Australasian Meetings in Sydney in 1984 when he chose to have lunch with a small group of graduate students over the more obvious choice of spending time with the 'big names', something that made a lasting impression on all of us.

On January 26 (Australia Day) 2015, Adrian was gazetted as Officer (AO) in the General Division of the Order of Australia, one of the highest civilian honours available in Australia. The citation read 'For distinguished service to tertiary education as an academic economist, to the development of public policy research, as an author, and through contributions to professional and financial organizations.' What more need be said?

The following interview took place in Adrian's office at the University of Melbourne on the morning of Friday 13 March 2015.

Let's start with a couple of quick questions about the very early years. Most Australian students tend not to move too far away from home for university but, being from the country, you of course had to.

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Of all the choices that you might have made, why UQ and why economics?

Well, I had to move to Brisbane to go to high school and I stayed with my grandparents. There wasn't a lot of choice about universities in those days. There were two universities, the University of Queensland and James Cook University, and the last had just begun. In theory it seemed that country students were supposed to go to James Cook but, for some reason, I managed to avoid that. This could have been because I went to a Brisbane high school, and so they may never have picked up on the fact that I wasn't living in Brisbane. Because I was way down the bottom of Queensland, virtually New South Wales, it was much easier to go to UQ than it was to go to James Cook, as the latter was way up north in Townsville. So that's why I went to the University of Queensland.

And in terms of economics?

Well, being from the country I felt I had to find something that I could do afterwards and accounting seemed to be a possibility, since there were local accounting firms in the country and they were interested in hiring country people. So I enrolled in accounting. I quite liked accounting and did well in it in first year, but what I liked about it was the managerial accounting material. However, the second year of accounting at UQ was on accounting for clubs and societies, and I wasn't terribly interested in that. But at that point there was no alternative. You had to do that topic in second year, so I gave up accounting and took up economics.

Even though, at that time, UQ probably wouldn't be viewed as one of Australia's better economics programs, it has a remarkable record of producing very good economists and your cohort, in particular, was especially strong. Could you reflect a little on important teachers, classmates, and the like? What made the program successful?

It was a very "poetic economics" program. There was virtually no mathematics. Everything had to be done in graphs and I think most of my honours class was somewhat different in that we were interested in using what mathematics we'd learnt at high school to think about the issues. There were some lecturers who were different in this respect. Gus Hooke had come from ANU and he had more of a mathematical background. Mike Alemson, who was actually in a unit that handled external students, was studying the introduction of new products, growth of firms, etc., and he introduced us to a number of dynamic models based upon Robin Marris' book (Marris, 1964). So he had a big influence on me because he was dealing with new material that wasn't in any of the textbooks and it was very interesting. It dealt with questions like why do you see the demand for a new durable good initially grow slowly, then steeply, and finally flatten out? The models being used basically came from physics. So that was very interesting.

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As I said, Gus Hooke bought a lot of new ideas in from ANU when he taught. So there were a few people like that. They were really new additions to the staff at that time and they bought in new and sometimes quite challenging ideas. I think one of the things about the UQ degree was it was very strongly oriented towards policy type issues, particularly current ones. One such issue at the time was international liquidity, and that was essentially what I did my honours thesis on. There was even an examination involving writing about policy issues so you had to be reading the newspapers and following what was going on in economics, rather than just looking at what was going on in the journals. A number of the students who were in my class — Alan Preston, Ted Evans, Peter McCawley — ended up, one way or another, in the public service, even though they could have been excellent academics. Ted was the Secretary of Treasury and (among other things) Alan was the Deputy Secretary.[1] Peter McCawley was an academic for a while but also worked for AusAid and the Asian Development Bank. So I think a lot of that was because we had such a strong training in policy type issues and that's something that you don't get in many honours courses today. It meant you did have to read widely and you had to talk to a lot of people who weren't actually working on the frontiers of economics at the time. I don't think it was such a great training for doing a PhD but it was good if you were going to be a public servant or even get involved in business.

I guess modern honours programs are much more directed towards preparing people for PhD programs and, even then, they're much further from the frontier than I think they were. The gap's just getting larger and larger.

Yes. I think that's true. Alemson's course was of a very high level but mostly they weren't. You did read a lot of material that was quite technical but most of the staff didn't work in this way.

So you mentioned your honours essay a moment ago.[2] Your first publication [6] came out of that, in the *EJ*. Can you tell us a little bit about that?

Well, at the time, international liquidity was a big issue and there were all sorts of plans floating around - the Triffin Plan, the Roosa Plan, etc. so I decided I would do a thesis on international liquidity. If you were a small country you weren't terribly interested in whether there was a dollar glut or not, which is what the international liquidity literature was all about, but you were interested in the question "Do I have enough reserves?" because this was an era of largely fixed exchange rates. So I thought a good topic would be how to determine what level of reserves would be needed in the face of fluctuations in the balance of payments? So that was what [6] was about. The model there just has the balance of payments following an IID process. So I then tried to generalise that in the thesis to a Markov chain, and later still I put it into a growth model in order to

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see if the answers would change. Because Heller (1966) had written a paper in the *Economic Journal* on the same topic my piece commented on it. I was using inventory techniques to think about what the optimum level of the reserves would be. We'd studied inventories in macroeconomics from a numerical perspective. Since I knew what the theory involving the holding of inventories was it seemed logical to apply that.

In 1968, following Honours, you headed down to the ANU to do a Ph.D, beginning an association that lasted, on and off, for over 30 years. This was a time when economics and econometrics at ANU were becoming very strong. Before looking at what you were doing, I was hoping that you might say a few words about the environment there at that time, both in terms of Faculty and fellow students.[3]

When I arrived I felt it was really quite amazing. The library at the University of Queensland had a reasonable collection of material but nothing like ANU had. It was quite overwhelming really. And the other thing that was overwhelming were the tremendous number of seminars and people whose work you had read when you were an undergraduate. Max Corden on effective protection, Noel Butlin on Australian economic history, Bill Phillips of the Phillips curve, and Trevor Swan on growth and open economies. Thus it was very different to what UQ was like. There were lots of students around as well, doing PhDs. Alan Preston and Peter McCawley had come down from UQ with me, but there were others like Ross Garnaut (trade), Geoff Brennan (public goods), Ian McLean (economic history), Des Nicholls, and Peter Robinson (statistics). It really was a very different environment to UQ. ANU also had a lot of visiting academics. I actually went to Takayama's course on optimal control in my first year, as Pontryagin's work was being used a lot by John Pitchford and others. Intellectually it was very alive, with lots of people doing really important work and lots of students around to talk to. There had been a very good group of honours students in Queensland but there was a much bigger group of PhD students at ANU.

As a Ph.D. student you started in the SGS and finished in RSSS. Could you tell us something about the evolution of your thesis and its contribution as well?[4] Given that you'd started off working on reserves in your honours dissertation, was what you ended up doing what you had in mind when you started?

Well, I think I started the honours thesis halfway through third year. You had to have a thesis topic selected before fourth year. Gus Hooke had a great influence on me and he was very useful for suggesting where I might go to do a PhD. He was interested in public debt and he'd worked under John Head at ANU. So Gus convinced me that I should do public finance and go to ANU to work with Head. I didn't want to work on public goods, which is what Head worked on, but dynamic models of tax incidence, i.e., the shifting of taxes in the long run.

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Most of the public finance work at the time was very static. You put a tax on and you got an effect. Maybe you could think of that as the long term result. But you didn't discuss how the long-run came about. My idea was that I would analyse lots of these things through growth models, which I knew quite a bit about, particularly vintage capital models. So that was the idea behind a thesis on dynamic tax incidence.

Head left ANU some months after my arrival. I had to find a new supervisor and, after a few false starts, eventually decided I'd go and have a chat with Bill Phillips. Alan Preston had come from UQ to work with him on optimal control. So I went and saw Bill. He said that since Alan was working on optimal control I needed to do something different. He also said that he had written a paper (his Walras-Bowley lecture) and perhaps I would be interested in looking at that and doing some work in the area. So I took the paper away. But I had never done either statistics or econometrics at UQ, so it was way beyond me. I couldn't figure out what it was about but I thought that perhaps there was something there for me. At one point I also went and talked to Ted Hannan about what I should do and Ted said, "Well, you should look at this paper in the *Annals of Statistics* because I think that it might have something in it that econometrics people could use." This was the paper by Eicker (1963) on robust standard errors in the face of heteroskedasticity that Hal White later extended. I went and looked at it. But I couldn't understand it either since it was all asymptotic theory, so I decided that wasn't going to work. Bill's topic seemed the best bet. So Bill then asked me how much statistics I'd done and the answer was none. So he thought I should go and do Statistics II. I went along to that and eventually I understood some of it. Some of it was a revelation — I can remember when we undergraduates were always told that a t ratio greater than two was what you wanted and I finally understood why they said this. In fact, I was so excited with the discovery that I ran all the way to Alan Preston's house to tell him of it.

So I ended up working on the moving average error models that were the concern of the Walras-Bowley lecture. Because I wasn't really a theorist, I decided to look at computational aspects, as the computer science department was strong on that. After I came across the Gauss-Newton algorithm I could see that was what Bill was doing in his paper. Once I saw that I could also see that there were lots of other ways of doing it — Powell's method, Newton-Raphson, etc. Thinking of it this way made a lot of things simpler to follow, at least to me. Actually Bill thought that was quite a good approach. He quite liked the orientation in terms of looking at different algorithms for maximizing the likelihood. It meant one could do the same thing in the frequency domain, where Ted and Des were deriving three step efficient estimators rather than maximizing the complete likelihood.

I can remember as a student I'd see you give seminars and I always used to think of the way that you tackled a lot of problems as essentially reducing them to OLS, which was how I understand what

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you were doing at the time. The Gauss-Newton method has been something that you've used a lot. My sense is that it's given you a lot of insight into a lot of problems, a lot of techniques and the way they work.

I think that's true. Gauss-Newton was, of course, Marquardt's method and nonlinear least squares. Marquardt's method was very big in the Computer Science Department, and so I think that's where I got it from, but I can't remember exactly. However, very early on I started thinking in terms of Gauss-Newton as it gave OLS in the linear case.

The timing of that would be about right because the Computer Science Department at ANU started life in the Statistics Department, so it's something that Ted generated. I'd have thought they would have been pretty visible back then. You were in the right place to see them.

Yes, it was the right place. Mike Osborne was working on optimisation and so Householder, for example, came out and spoke about the QR decomposition. Householder transformations were all the rage then for overcoming ill-conditioned regression problems. So there were a whole lot of useful things going on in that department.

So, continuing your run of supervisors, in 1969 Bill had his stroke and sometime thereafter he retired back to New Zealand. To what extent do you think that that impacted upon your research direction and overall the development of econometrics at ANU?

Well, I think by then I had the thesis direction. I'd never heard of Box and Jenkins (1970). Bill once told me he wasn't aware of Box and Jenkins's work either, until someone mentioned it after he'd given the Walras-Bowley lecture. Someone had come up and said to him, "Box and Jenkins are working on this, but in the univariate case." Bill was working on systems. So Box and Jenkins' techniques were having an impact and were being applied over in the Bureau of Statistics (by Ken Brewer, in particular), because they were trying to come up with packages for seasonal adjustment, etc. If you are doing a two-sided filter for this then you had to worry about what you were going to do at the end of the sample, and the idea was to use an ARIMA model to forecast forward and so have postsample data. There was even a seminar series in the Bureau of Statistics about Box-Jenkins procedures that I used to attend. So it all fitted together. In the thesis I started working on moving average errors in single equations, then went on to systems, and finally, what would be called today components models. In these two moving average processes were added together and they produced a moving average. So the question was how do you estimate those models? [7] was about that. That work was going back to a univariate problem rather than the systems orientation of Bill's paper.

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After completing your dissertation in 1972 you eventually went to Princeton as a Visiting Research Fellow. There you met Gregory Chow, whom you later interviewed for ET [73]. Could you say a few words about how you came to visit Princeton and about your time there. Was this where your interest in economic policy and control (e.g., [11], [1], [25], [81]) started or had that begun earlier? For instance, you did mention the lectures with Takayama earlier.

I went to the Econometric Research Program. Chow was an examiner of my PhD thesis. I think Deane Terrell (my final supervisor) had arranged that. Chow produced some money for my appointment and I also won an ANU Postdoctoral Travelling Scholarship and a Fulbright travel grant. At the time Gregory was working on optimal control. He'd actually published a paper in the *American Economic Review* and he was doing things like Kalman filtering, because when you work in stochastic control you had to have the expected value of the state that entered into the control rule. The engineers had worked with state-space forms and shown how you can compute that expectation using the Kalman filter. I'd actually been exposed to optimal control quite a bit. Alan Preston had done his honours thesis on a control topic at UQ and he was now doing linear quadratic control with Bill at ANU. I'd read all Bill's papers, but they were about robust control with simple rules — proportional, integral, and derivative control rules — rather than optimal control. So I thought I was familiar with what Chow was doing. The new thing though was his computing the expected value for the state variable. I'd never heard of the Kalman filter and that was an eye opener to me, as I could see that a lot of the components material I had had trouble with in my thesis could be solved relatively simply with the Kalman filter. So [9] maximized the likelihood using the Kalman filter and then used a Kalman smoother to estimate a permanent component.

In 1974 you returned to ANU, now in the Department of Statistics in the SGS, and entered a remarkably successful phase of your career. Looking at your CV, its pretty clear that throughout much of the 1970s there was a clear focus on dynamic models and, in particular, on moving average disturbance processes. You weren't the only one at ANU working in this area. You wrote papers along these lines with Ray, Deane and, more frequently, Des. You also had a paper on rational distributed lags [15] which, as it happens, can be associated with moving average errors. Alan Preston (1978) wrote an important paper on identification in moving average models around this time. In a book celebrating Bill Phillips' life, you made an observation that Hannan's papers in (1969) and (1971) were almost certainly a response to a debate that he was having with Bill Phillips on identification of parameters in moving average models ([95], p. 421). Phillips had given his Walras-Bowley lecture in 1966, which was just before he came to ANU. To what extent do you think that the focus on dynamic and, in particular, moving average models have grown out of Bill's work that he brought with him to ANU?

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Well, I guess it did. But, as I said, there were a lot of other influences — Ted doing it in the frequency domain, people in the Bureau of Statistics, etc. At that time moving averages were a big issue in time series econometrics. A lot of the visitors to ANU had worked in that area as well, such as Jim Durbin. I don't think that Bill was the sole influence. Bill did give a course on dynamic models, and he did emphasise moving averages there, but I think it was really just part of the milieu. Bill's focus definitely was on systems and that was different. I think that Bill and Ted had debates over the systems aspect. It was about the identification conditions for the system. They were really talking at cross purposes. Bill was interested in taking the old Cowles Commission approach to a structural equation and saying, "When is that identified?" Essentially you ask "How many instruments have I got?" and you can give an answer to that. Ted, however, was concerned with the question, "Is it a unique model?" He wanted to vary the length of all the lags and not just fix them. So he was asking "Is there just one unique model that I can get when allowing for variable lag lengths?" And that led to his identification conditions, which basically said you shouldn't be able to cancel polynomials on the left and right hand side of the equations. In order to get a unique model you had to rule out a common factor that would cancel and which would reduce the orders of both polynomials. So there were two different things going on there and I was certainly very confused by it. Alan Preston (1978) provided a solution to this by saying, "What Ted is talking about is model identification and what Bill is talking about is structural identification." I remember when Alan was writing his article. I kept giving him counter examples which he would demolish. It was a very instructive piece of work and I thought that it clarified a lot of things. Because Bill had the stroke he couldn't quite see what the solution would be. He sort of knew that somewhere or other Ted was answering a different question to what he was answering, but he wasn't quite sure what it was. He actually asked me at one stage to finish the Walras-Bowley lecture (with the missing section on identification), and to publish it jointly with him, because *Econometrica* wanted it. I felt that it was his work and not mine, and so I wouldn't do it. I have a great regret about that. I should have finished it and then not put my name on it. It would have been good if it had come out in *Econometrica,* rather than being just one of those lost papers, which effectively it is now.[5]

In 1977 – 78 you visited in CORE and you obviously made sufficient impact there that you were later asked to contribute to a celebration of their 20th anniversary [60]. Can you say a few words about your time there and the impact, if any, it had on the direction of your research.

Well, I spent a lot of my time there writing the paper on evolving coefficient models [20]. Essentially what I did there was take a state-space model and ask "What are the conditions for maximum likelihood to be consistent and asymptotically normal?" And to do that I had to learn some asymptotics. I had never

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really learnt asymptotics because I never had the training. Statistics II was not good training for this. I taught myself econometrics out of books like Johnston (1963) and Goldberger (1964) and they weren't very big on asymptotics either. So I spent a lot of time at CORE in the library going through all the martingale theorems. This is before Peter Hall and Chris Heyde's book (1980), so you had to go through the basic material. I had a lot of spare time at CORE, and the weather was terrible, so I spent a lot of time in the library and it was very useful. I think it gave me a feeling for how to prove lots of things. After that I used it often for the rest of my career.

CORE was very much into Bayesian work at the time and they used a poly-t approach. You had to have a conjugate prior that matched the likelihood. Poly-t densities were good for that as one could evaluate them numerically. They had a program called the Bayesian Regression Program which I used at one point. But I felt it was user unfriendly. You couldn't do any data transformations and I thought that, unless they made it data friendly, it was never going to be successful. But it worked quite effectively at producing a posterior provided you were happy with this poly-t orientation. And I did write a discussion paper using it to test for serial correlation after Bayesian regression.

I met a lot of people at CORE and even ran a conference. Peter Phillips and Mike Wickens came over to it. In this conference, Van Dijk and Kloek gave their paper on importance sampling for finding a posterior density (Kloek and van Dijk, 1978). There were a lot of good papers at that conference. Jean-Francois Richard was on leave that year but I talked quite a bit to people like Jacques Dreze, Louis Phlips, and Anton Barten. Louis was probably the main empirical guy who was there. There were quite interesting people around as Fellows — Bengt Holstrom and Kumarswamy Velupillai — and I quite liked talking to them. Andrew Chesher and Angus Deaton came over for a few days as well. So I guess I didn't get converted to Bayes, something still true today, but CORE was very important in my thinking about econometrics.

Come the end of the 1970s the impact of your travels and the supervision of PhD students really started to reveal itself in the diversity of interests that characterizes the rest of your career. You did some interesting work on LIML [19], and it was something that you went back to [24], but it never seemed to gain any real momentum. Is there anything in particular that you were looking for or was it just one of those things that came to a natural end as a research topic?

I think I was trying to find a simple way of explaining LIML to students. The traditional treatment was through eigenvalues and I didn't find that simple, so setting it up as an SUR problem, where you write the one structural equation down, add on some other ones from the reduced form, and then estimate by SUR, seemed to be quite a nice thing to do. You could understand what was happening and you could work out formulae which connected two-stage least squares and LIML. I liked that pedagogic aspect of it. I didn't really think it was ever going

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to be a useful tool for numerical analysis. I've used it many times since then, for example with Tony Hall when we looked at estimating vector ARMA processes [24]. More recently I've used it for limited information estimation of DSGE models in the paper with Martin Fukač [124].

LIML has had resurgences over time. For example, Soren Johansen's stuff on cointegration (1989; 1995) and it's obviously been important in the weak instruments literature.

That's exactly right. I haven't seen an eigenvalue treatment of it for many years. I think it's become standard to augment the system and, of course, that was the original Cowles idea. I think the eigenvalue treatment came about because that was an efficient way of computing it. They didn't want to handle big systems. Jerry Hausman criticised the idea in his Handbook chapter (Hausman, 1983) on simultaneous equations, where he said it was not very useful computationally. I never thought I was going to do computations with it. I liked it as a way of thinking but I certainly wasn't going to spend too much time on trying to develop it in other applications.

One of your contributions, around this time, that really had a big impact on the profession was your work with Trevor Breusch on Lagrange Multiplier (LM) tests ([18],[21]). You give some historical perspective on the trinity of likelihood-based tests in your review of Les Godfrey's book [137] but these things always seem clearer in hindsight. I was hoping that you might give us some perspective as to your thinking at the time.

The trinity of tests was taught in the Econometric Theory and Method graduate course.[6] When Trevor arrived to do a PhD, that was one of the things that I was teaching. In the course I also did two-step estimation methods. In these you started with a consistent estimator, took one step of the Newton-Raphson algorithm (which involved the product of the inverse of the second derivative by the score), and you ended up with an efficient estimator. So the idea was to think of the null hypothesis as providing a consistent estimator of the parameter you wanted to test. Therefore, if you are interested in testing something, say $H_0 : \theta = \theta_0$, taking one step from θ_0 will give you a fully efficient estimator of θ, and you can use that to test whether θ is θ_0. Hence, the idea was to think about developing a diagnostic test from this perspective. I suggested this to Trevor for a topic. Once you write it down like that then you start seeing that it is essentially a product of the information matrix with the score, and that leads you on to the score test. I think that was really Trevor's insight. He either knew or had come across Rao's paper (Rao, 1948). When you go back and look at the mathematics to get out the Lagrange multiplier test you can see that the multipliers are on one side of an equation and the scores on the other. So serial correlation tests could be treated in this way. Although I don't think we realized this until later, Durbin's derivation of the h statistic had a similar motivation. He didn't call it the Lagrange multiplier

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test but that's really what it was. I am a bit vague about how it all came about now but that's how I remember it.

Two papers came out of that work, the Econometrica one [18] and the REStud paper [21]. Historically, the Econometrica one has been the more widely cited, although I believe that the order may have been reversed in recent times. Trevor always maintained he thought the REStud paper was the more important one. Is that a view you share?

I think the *REStud* paper was much more interesting than the other one. When Andrew Harvey's paper in *Econometrica* (1976) came out on estimating a model with heteroscedasticity, he wrote out the information matrix and the scores, and that was all you needed for an LM test. So I actually wrote the first version of it at home, took it in and showed it to Trevor, and he made some great suggestions about how to make it clearer. In about three weeks it was all over. At the time one didn't realise just how many other people had already discovered it as well. It was a nice idea although it took some years to get it published. Then, once you start thinking this way, you realize that there are an awful lot of other things that can be tested with a score test. So why not look at these other applications? That led to the RES paper, which was also written fairly quickly - I think in about two months. One of the reasons it was terminated was that I went off overseas to CORE and I wanted to have a paper to present on the way there. So that was the end of it. We didn't do any more applications, even though there were obviously lots that you might do. In retrospect I think it's a pity that I went away at that time, because if I hadn't done that we probably would have added more applications to the paper. Maybe not. It was already a pretty long paper.

Buse (1982) presented a famous graphical comparison of the LR, Wald, and LM tests. You also had a graphical description of these tests, which offers some interesting insights into the relationships between the three tests that aren't obvious from Adolf's diagram. Sadly your diagram was hidden in a paper that seemingly had nothing to do with hypothesis testing [25] and, as far as I can tell, it pretty much disappeared without a trace.[7] I have tried to capture its essence in Figure 1 and was hoping that you might give some background to its development and comment on its pros and cons relative to Adolf's diagram.

The diagram arose out of a conference at ANU on "Economy-wide Modelling: The Way Ahead." One of my arguments in the paper was that most of the macro models in Australia had never been checked very thoroughly. They were written down, estimated, a few simple tests were applied, and then they were used. And so I was arguing that you really needed to test these equations more thoroughly, and that there were a lot of diagnostic tests out there that you could use. So I felt I had to try to provide a simple explanation of why the score test was a logical way

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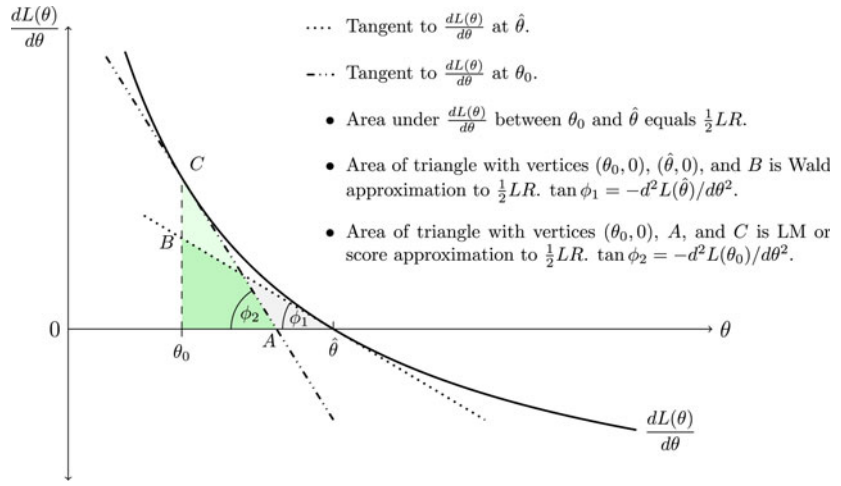


FIGURE 1. Pagan's graphical comparison of the Wald, LM, and LR tests.

of generating them, and the diagram basically tried to capture the way the score test worked. That's the reason why the derivative appears on the Y axis. The log-likelihood was then going to be the area under a curve. Because I was talking to economists, and they knew about consumer surplus computations, it was natural to talk about triangle approximations to the area under the curve. So it seemed obvious to write down the triangle approximations and to see what happened. Well, you get the LM test out of one of them and the Wald test out of the other. That seemed rather neat and I thought it was a good way of explaining to the conference why there were three tests. I don't think it had any impact whatever but it was good for the day. Certainly, as you said no one really ever took it up, so it clearly wasn't terribly popular. I have never really thought much about it. I used to use it for teaching in Rochester and I think I used it at Oxford when I was teaching there. Adolf's graph has certainly been much more widely used. My focus at the conference was on diagnostic tests and the trinity of tests came out of it as a by-product. A lot of people at the conference would have known about likelihood ratio tests so it was natural to say that all we're doing is approximating areas in different ways, and computationally it's much simpler to do the LM test than it is to try to do likelihood ratio tests.

While we're still talking about LM tests, I'm interested in your views on the impact of environment in the development of big ideas. So you mentioned before that Durbin had been out at ANU in the early '70s and Ray Byron, who was also at ANU, had written some papers that used both LM and Wald tests in the context of simultaneous equations models (Byron, 1972, 1974), which clearly had a lot less impact on the profession than your work did, but it does speak a little bit to the vibrancy of ANU as a research centre at the time. Did the environment

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contribute to the motivation for thinking about LM tests and testing problems more generally? Was it simply a reflection of the way the literature had been developing? Or was it a combination of all of these things, or something else entirely?

I think Ray was very influential in my finding out about Lagrange Multiplier tests and seeing how they worked. Without Ray's work I would never have known about them. And the test became part of the courses that I was teaching because I thought that this was a very nice way of thinking about other ways of testing rather than just the likelihood ratio test. I feel that the difference in our approaches was that Ray's work was very much about testing hypotheses from demand systems, such as separability, whereas I was more interested in using it for diagnostic purposes. That's what seemed to me the biggest problem with macro modelling of the day — the lack of testing about whether the assumptions being made were correct. So I wasn't as interested in testing economic hypotheses as he was. Ray was really the driving force for my learning about LM tests. I think Ray had lots of very good ideas and you could learn a great deal from his work. One of the problems he worked on was structural equations with uncorrelated shocks. That was unusual at the time and his referees were really very negative about it. Yet later that's how Sims (1980) solved the identification problem — structural VARs have uncorrelated shocks. I think it was more successful for Sims as he talked about shocks (rather than equation errors) and impulse responses. Then it's natural to say that the shocks should be uncorrelated, because you can't



FIGURE 2. Department of Economics, University of Rochester, Sept. 1989.
Back Row: Mary Finn, Barbra Mace, Bill Brown, Hanan Jacoby, Rangarajan Sundaram, Marcus Berliant, Jeffery Banks, Siu Fai Leung, Kenneth McLaughlin. Middle Row: John Boyd, Alan Stockman, Ronald Jones, Eric Hanushek, Lionel McKenzie, Walter Oi, Adrian Pagan. Front Row: Michael Wolkoff, Stanley Engerman, William Thomson, Charles Phelps, Bruce Hansen, Changyong Rhee.

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change a demand shock to study impacts if it isn't uncorrelated with the supply shock. Ray's idea of making these shocks uncorrelated went back to Wold and he didn't have much luck with it either. Ray was a very influential figure at ANU in terms of introducing new ideas, particularly from LSE where he had done his Ph.D. I'm sure that they taught LM tests there as well. Bringing it back was really important.

While I was preparing for this, a number of people commented on your generated regressors paper [38] along the lines that this was you at your creative best. Could you provide some background as to what led to that paper?

I was working at that time on volatility in inflation. We had high inflation rates in Australia and they were actually quite variable. So I went to a conference at Warwick in 1981, which was on international economics, and at some stage they said. "Well, we have a little bit of space in the program. Could anyone give a seminar on something?" So I said, "I can give this talk on modeling volatility in inflation." I gave the talk and Ken Wallis says, "But you've used estimated residuals here and you should make an allowance for them in computing standard errors." And I said, "No I've shown that you don't have to," although that wasn't in the seminar. And he said, "Sims says that you have to make an allowance for it." And I thought, "That's peculiar." But it worried me. That night I had to go and do my laundry, so I sat in the laundry shop at the University of Warwick and decided I'd look at why Sims would say that. After a while I realised that what Sims was talking about was not estimating the volatility parameters, that's the second moment, but rather the first moment. For the first moment it was generally true that you have to make an allowance for the fact that you've estimated parameters. There are a few exceptions. But for second moments you don't. So it all came out in the three hours it took to do the laundry. By the time it finished I had a paper written on it and it was just a matter of cleaning it up and sending it off. It took a long time to get it published. Not because anyone was rejecting it but for some reason they couldn't find any referees. So it was just Ken's comments that brought it about. Without them I would have written a paper about volatility but would never have thought about these other issues being a complication.

In 1985 you visited Yale and then in 1986 you left the ANU to take a position at the University of Rochester. Why then? Why Rochester?

Well, the first question is why I left ANU. Firstly, I'd finished the inflation project and so was looking for another project to do at ANU. I suggested econometric work on an open economy, which I thought was a good topic, but no one was interested in it. I started work on it with Jenny Anderson who constructed a long real exchange rate index for Australia, going back to the 1860s or so. But there were gaps in this series. And then it turned out Ian McKenzie had done his

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PhD at MIT and he'd constructed a long real exchange rate index too. Moreover, he had managed to fill in the gaps because Harvard's Widener Library had all the missing data that the ANU library didn't. So Ian's paper came out in the *Economic Record* (McKenzie, 1986). This left me feeling that I was in a bit of a rut.

I went to Yale on leave for half a year and Yale had excellent students who I taught — Joon Park, Sam Ouliaris, Dean Corbae, and Steve Durlauf. I rather liked the atmosphere and it seemed that it might be worth looking for a position in the U.S. Rochester was just an accident. Barro rang me one day and said he'd heard I was going to Chicago to give a talk, so why didn't I come back through Rochester. My initial reaction was negative. I then changed my mind. And when I went to give the seminar in Rochester I thought it was an extremely good place. At that point in time there were a large number of very well known people there. Moreover, there were a lot of good junior macro people. You really couldn't help but come away thinking that this was a terrific place. It was a small department, very cohesive, and with very good people among both senior and junior ranks. Paul Romer, for example, took me to dinner. He later went to Stanford, but he was the type of person that they had there at the junior level. I just thought that this was a good place to go to intellectually and, unlike a lot of US departments, they seemed very cohesive. In terms of schooling for the children it was excellent and the countryside around Rochester was really quite beautiful. So all these factors suggested that this was a good time to go.

Rochester's one of those places that's always struck me as having a good recruiting team. So they don't necessarily have the profile of some other institutions but they've been pretty good at picking up people straight out of their PhDs, who've gone on to be much more successful than we might associate with them.

I think one of the reasons was that they offered graduate teaching to younger Faculty. In a lot of top universities the new appointments only teach undergraduates. The teaching load was reasonable and it was a place you could actually make a mark much more readily than in some other places. Rochester worked hard at integrating the junior staff. That's what impressed me about the place when I went there.

The pastoral care was good.

That's a good word, pastoral care, because I felt it was like that.

Around this time your applied work seemed to move from a focus on macroeconomic problems to more financial problems. Was this symptomatic of the US academic environment at the time or just a natural evolution of your interests?

I'd been working on the volatility of inflation, and the paper with Pravin and Tony [34] was about that. In it we wrote out models that were like ARCH models.

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The only difference being that we didn't use residuals as driving volatility. Instead we used a function of the actual dependent variable. Someone wrote to us and said Engle had written a paper on the same thing, that is, about the volatility of inflation. This was his ARCH model (Engle, 1982). I looked at it and thought, "Well that's a different way of looking at it" because, in terms of a regression $y_t = x_t\beta + e_t$, we were looking at y_{t-1} as affecting volatility and he was looking at e_{t-1}. So we then compared what happens if you used these different forms. That is, since the ARCH term e_{t-1}^2 involves y_{t-1}^2, $y_{t-1}x_{t-1}$ and x_{t-1}^2, do the cross product terms add anything to using just y_{t-1}^2, as we effectively had? We found it didn't, and so we thought our variant was better.

ARCH didn't really work for inflation. In our work, we also only found pretty weak evidence of volatility depending on the level of inflation. It wasn't until Bollerslev (1986) bought out the GARCH model with a finance application that there seemed to be very strong effects. So that's why I became interested in returns as the effect seemed very strong. The other reason I got into financial issues was Bill Schwert. One day Bill suggested we might write a paper about volatility in returns using GARCH-type models along with work I'd done on nonparametrics. So that is [61]. It was my second introduction to financial data. I'd written a paper with Aman Ullah on the risk premium [52] but this was really where I got more involved in it than I had been before.

Similarly, at about this time, your theoretical work became much less classical in nature, with much more emphasis on simulation, calibration, and, ultimately, nonparametric methods. I guess some of that reflects the developments in the statistics literature, although I doubt that calibration figures too heavily there. Was there any other reason driving this development of your interests?

I think calibration was because the early work in freshwater macro at Rochester used calibration. The King, Plosser, and Rebelo (1988a; 1988b) paper , which was one of the foundation papers in the real business cycle literature, used calibrated models. Either the first or the second week that I was in Rochester this was given as a seminar paper, and I remember going to it and thinking that you could do more estimation stuff here rather than calibration. However for what they were doing it seemed that calibration was appropriate — basically you just get some parameter values from what is like a method of moments approach. That seemed a reasonable thing to do. So I got into calibration because people were doing it, but at the same time I really felt that they should be doing better. They should be using more estimation methods and that's what the paper with Fabio Canova and Mary Finn [70] was about. It used VECM-type methods to say something about parameters in these RBC models. I guess this is what I was on about when I produced the slide in the Tokyo World Congress session that said — "Just say no to Calibration." I did get a following in Japan for a few years with that slide, as it featured a rising sun.

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The nonparametric work probably came about more through being friends with Aman Ullah. Aman was relatively close to Rochester at Western Ontario. Simulation, I think, is an old interest. It goes back to the models of the '70s and the '80s. People were simulating macro models in Australia all the time. When you come to a complete system model it's very hard to devise tests of it. So I've often quoted from Chris Higgins' statement that the best way to check it was "to simulate early and simulate often."[8] That way you found out whether it was dynamically unstable and whether the exogeneity assumptions being made were sensible. By the last I mean that in these models the money supply was often assumed to be exogenous. But how can you keep money supply exogenous? It's not exogenous and that would show up in the simulation. You'd see some crazy simulation results that came from that invalid assumption, and the only way you learnt about that was by simulating the model. A single equation wouldn't tell you there was anything wrong. It was how they fitted together that caused the problems. And so I was interested in simulation very early on for what it could teach you.

After the book with Aman [4] you seem to have largely lost interest in nonparametrics as a research topic. Why is that?

Well, I thought that for most time series there weren't enough observations for making effective use of nonparametric techniques. If you think of a conditional mean you need to find the expected value of Y given X. It's fine if there is just one X, maybe even three, but if you get ten then it's just impossible. You need enormous numbers of observations, so I didn't really see it had any great application in macro type work. An exception was when you were simulating a macro model. Then you can get a lot of simulated data and can do many things with it. For empirical work I didn't think nonparametrics was terribly useful. Finance is where it looked like it could be useful, but financial data have got a lot of problems with it — there's very strong dependence and quite a lot of so-called outliers. The estimated leverage effect that Bill Schwert and I talked about a lot [61] was due to six observations, and nonparametric methods are very sensitive to those sort of outliers. They will drive the curve right through them and it isn't really a global characteristic at all. It's just a very local one coming from those observations. I've seen that in some nonlinear macro models [116] — Markov switching models driven by one observation in the 1950s. Once you've used those observations it can change the nature of the whole model. I think if I'd been doing lots of micro data work I might have had a very different attitude, because that's where it gets used a lot more. However, I'm not sure it has got used as much as I would have expected. If I go to a typical seminar around here there's lots of micro work done, but nonparametrics and semiparametrics doesn't seem to get used a great deal. There are plenty of techniques out there but I think they're rather hard to understand. People aren't comfortable with the fact they haven't got a good understanding of them.

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We had Jeff Racine out last year giving talks and he extols the virtues of nonparametrics. But one thing that he has done, which I think might have an impact, is they've got very user friendly software that they've written for R, so it makes it much more accessible I think.

Certainly the software has to be user friendly. It's interesting it hasn't broken into any of the big packages though. You can estimate a conditional mean in almost all packages but a lot of the nonparametric methods that are in them seem to date from 10 to 15 years ago. This literature is still developing but I suspect it is just filling in gaps now.

In 1992 you returned to the ANU, this time to Economics, RSSS. Not long after that you were appointed to the Board of the RBA, which was presumably a very different experience to academic life. Could you provide some insight as to what that entailed?

Well, firstly the Board met to set interest rates once every month. I guess the experience was really like dealing with big data. The number of series that you'd be presented with at a Board meeting, or with papers that came before the meeting, was huge. The Board papers provided an interpretation of the data and the Reserve Bank generally had a recommendation that the Board could decide whether they wanted to adopt or not. Sometimes the interpretation was based on a few key series and there could be other interpretations, so you can have debates over the recommendation. It was very different to academia because it's a much greyer area. You actually learn a lot about having to deal with big data. One of the things you learn is that you need a framework, and a relatively simple one at that. A complicated one wouldn't give you a reasonable answer.

Not surprisingly, around this time your applied interests moved back to macroeconomics, with a focus on the business cycle, factor models, DSGE modelling and the like. Would you like to say a few words about how you see that area developing?

First, I don't think my interests moved back. They were always there. For example, DSGE modelling was a feature of Rochester. I think my experience of DSGE modelling started there. But I didn't see DSGE modelling as much different to regular macro modelling. There are some differences — a smaller number of parameters, maybe more optimization, and the shocks were the exogenous variables. I've written about this perception in a number of places, for example [128].

I see DSGE model developments as a continuum rather than something that's totally new, even though a lot of people present it in the latter way. I don't think it's a good idea to treat it as revolutionary and that has been been one of the biggest problems with it. I think today we are moving back towards the older models. People are now saying, "Well, we don't want to just have rational expectations. We want to make some allowance for expectations to depend on either survey data or a strong dependence on the past." There is much more interest in getting

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a closer fit of DSGE models to the data today — the first RBC models didn't really look much at the data except for checking that they could replicate a few moments. But then people started to add on habit persistence, inertia in pricing, etcetera in order to make the model fit the data, and that's been the trend. As you keep adding on these extra constraints DSGE models start looking more and more like the old models that they replaced. Except they're still smaller.

I started working on issues with DSGE models in a lecture I gave to the Canadian Econometric Study Group in 1991 (Canova, Finn, and Pagan, 1991). One of the questions of the day was why the old large-scale models could produce cycles that were just like those from RBC models? And the answer was that you had a unit root in technology and that produced a unit root in the level of output which was a key feature in producing the business cycle. So it was present in the old models as well as DSGE ones.

Until recently, I don't think I've spent much time with factor models. I've always thought of them as just a special type of state-space form. The latter goes back to Princeton days and [9]. There I set up a permanent-transitory decomposition in a state-space form, estimated it by maximum likelihood, and then did Kalman smoothing to extract the components. So the factor was the permanent component. When you get lots of series with a common factor it is a different story, because then you start going to principal components and so on, and that's the more recent development.

You've been interested in what we now call 'weak instruments' for a long time, and you talked about it in that paper with Canova and Finn back in (1991), but it was not until much later that weak instruments ever explicitly appeared in a title of a paper of yours and that was published in Russian [120]. How did that come about?

Weak instruments was clearly not my term. I guess it came from an early piece of Stock's,[9] but I don't remember when the first time I ever heard that term was. The paper in Russian came about because I'd written a piece on weak instruments, for a course I think. Stanislav (Anatolyev) wrote to me from the New Economics School and he said he'd seen this somewhere and what had I done with it? I said I hadn't done anything with it and he said, "Well, we've got this new journal and could you submit it to that?" I made a few additions to it and that was it.

In 1996 you presented the Shann Lecture,[10] parts of which appeared in [83].[11] In some sense this talk fore-shadowed some of your more important contributions over the coming years. Could you say a few words about the themes of the talk and how those ideas were subsequently developed.

The Shann lecture was mainly about the history of business cycles in Australia and the relative contribution of various shocks. A small part of it provided a rather simplistic analysis of what factors determine turning points in a series. After it had been given Don Harding gave me a call and wondered if I might be interested in

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doing some more work on this with him at the University of Melbourne. Don and I were struck by comments at the time that the type of work done by Burns and Mitchell (1946) had been discarded by many macroeconomists as it had no statistical foundation. Our work provides a foundation and it began with [100] and [103]. Once we started we kept seeing new things that needed to be analyzed, such as how to combine together turning points in many series to find a single set and how to analyse the relationship between them [117]. Our central theme has always been that data on expansions and contractions found with turning points provided an informative way for summarizing macroeconomic data. Just like a variance provides different information to that in the first and third moments so turning points give a different cut to the data that should be looked at by macro economists. The material was summarized and extended in the Tinbergen Lectures in Econometrics delivered in 2014 [143].

Your term on the RBA Board ended in 2000 and thereafter you seem to have become a little restless professionally, holding positions at Nuffield College, Oxford and then shuffling around various Australian universities, as well as a variety of visiting positions, before finally landing here in Melbourne. Was there any particular reason for this?

When I left the RBA Board I had some time on my hands. I either had to go back to ANU full-time or I could use that time to go somewhere else. At the time I thought that the Research School of Social Sciences wasn't terribly interested in economics people like me, and I had the feeling that sooner or later economics would have to leave from RSSS.

Which it did.

Which it did. I think there was no real prospect of hiring anyone in macro econometrics in RSSS. So most of the people with whom I was working were either people in the Faculties or in the Research School of Pacific Studies — such as Mardi Dungey and Warwick McKibbin. So I thought I might as well use the time by going somewhere else. Oxford was a nice possibility, because their short terms meant that you could fit the appointment into the time that was available. My wife was still working in Canberra so I could keep the ANU connection. After Oxford finished, New South Wales had a position available because Ron Bewley had gone to the Commonwealth Bank, and so it went on. I think the best thing to say was that I wasn't ultimately going to stay at ANU unless there were some changes. The Research School of Economics had been floated at that point, and I did feel it was something that I would stay at ANU for, but it didn't happen until quite a few years later.

I've always thought that one of your great talents is synthesizing disparate material into a coherent framework. Examples include your

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'Polynomial Distributed Lags' paper with Pravin [16], your 'Regression Diagnostics' paper with Tony [30], your 'Two Stage and Related Estimator' paper [46], and your 'Diagnostic Testing' paper with Frank Vella [58]. You have also written numerous survey articles over the years (e.g., [31], [37], [50] and [76], [59], [71], and [79], among others). Has this sort of activity been a conscious decision as part of your research agenda or simply one of those things?

I think the three papers you mention all came out of teaching. At that time many modellers, such as those at the Reserve Bank of Australia, were using polynomial distributed lags and these were almost treated as if they were a free good. They would write down a polynomial (say a quadratic), add in some end point restrictions, and that'd be it. And so Pravin Trivedi and I looked at this procedure.[12] The question we asked was "Can you test these restrictions?" Most of it probably came out of the Case Studies in Applied Econometrics course in the M.Ec degree as you had to suggest how could one look at whether this was a valid specification or not.[13] One couldn't help thinking that it was too good to be true. It's so obvious nowadays why it's not a free good, but not so obvious then.

I taught microeconometrics in Rochester for a year and needed to think about a good framework to put together diagnostic testing for that. So the generalized method of moments framework was obviously the way to go because I thought Whitney Newey's (1984) sequential method of moments paper was a very nice bit of work and it could be utilized very effectively. The diagnostic tests paper with Tony was pretty much the same story. I actually think my paper [40] was a better



FIGURE 3. Les Oxley, Peter Phillips, Werner Ploberger, and Adrian Pagan. New Zealand Econometric Study Group Meeting, Hamilton, 1999.

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way to do it, but that came afterwards. The reason I think it was better was that you can add on a suitable variable to the maintained model and get a diagnostic test for any estimator. It's much easier than doing residual diagnostics. As Tony and I found it was very hard to work out the distributions of some tests using that approach when instrumental variables were used. With the variable addition idea all you had to do was add the variable on to the model and apply IV to the augmented model. Subsequently, I've always used it as the way of teaching diagnostics.

Throughout most of your career you have had streams of both theoretical and applied econometric work running concurrently. It would seem that these two activities have gone hand in hand. For example, in the early 1990s, much of your empirical work focussed on stock market volatility while your theoretical work focussed on heteroskedasticity. To what extent do you think that your theoretical work has been driven by practical considerations arising from your applied interests? How important do you think it is to have both activities as part of a research portfolio?

I do think the theoretical work was driven by applied issues. For example, all the volatility material came out of the question of whether the level of inflation affected the amount of volatility in it and how do we go about formulating that in an econometric way? Because there was volatility in Australia inflation, and inflation was rising, it was natural to look at this. I think some people are tool makers and some people are tool users, and in a way I'm a bit of a mixture. Perhaps a better description would be that I am a tool modifier. The tools that I make are generally driven by the need to look at something and I don't really go into tool making without thinking about what its use will be at the end. Sign restrictions in SVARs are a good example. I had done work in SVARs and, when the sign restriction material came along, I wanted to know how it worked. It seemed to give people answers with minimal assumptions and was like a free lunch. So that makes you ask what is the theory involved in it? I hadn't realized from the early work that there was no unique model, as only one model was ever presented. So the issues coming from multiple models was what Renee Fry and I wrote about in [132]. I think the one exception to this principle was the work that I did at CORE, when I sat down to learn some asymptotic theory just to be able to establish normality and consistency of state-space form parameter estimators. That was really a theoretical piece and not driven by any particular application. But I can't think of many other cases where I've not been driven by an application to try to do theoretical work.

Over the journey you have tried lots of different things but you seem to have resisted the temptation to work with Bayesian methods. Any particular reason or just the way things panned out?

As I said earlier, I didn't resist the temptation. I did do Bayesian work at CORE. But I didn't like the conjugate prior stuff. I thought that just meant you were

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dreaming up priors for computational reasons. When Kloek and Van Dijk came along with importance sampling it seemed that now you possibly could have very general priors. And that made it much more appealing. The problem I still have with it is that increasingly I get the impression that the priors we see, or the way they're quantified anyway, are being used to get the answers that people want. This certainly seems to be the case in macro work. I also get bothered by comments that I see along the line that, with Bayesian methods, you don't have to have a correct specification of the model. Well, there is a likelihood in Bayesian analysis and it is about the world not your beliefs. You cannot get away from this. In large samples the prior is dominated and so the Bayesian estimator converges to maximum likelihood. So everything that can go wrong with maximum likelihood (such as model mis-specification) has to apply equally to Bayesian methods. I like some parts of it, for example where you write down a likelihood, add on the log of the prior, and maximize to get the Bayes mode. I'm quite happy with a Bayes mode. It can be thought of as smoothed maximum likelihood. You're smoothing the likelihood function by using the prior to fill in the holes. At that point I would be just as happy taking the second derivatives of this criterion function and treating it like normal maximum likelihood. The engineers often do that and call it regularized likelihood analysis. It seems to me that for many years we've always added penalty functions on to likelihoods which impose some constraints and that stop us searching in certain parts of the parameter space. Basically, I think a prior is a neat way of doing that but I don't want to go on to posteriors etc.

My last problem with Bayesian methods is discovery. In the old days wrong signs on coefficients could often tell you that the model being estimated was badly specified. Putting on a prior which says this can never happen mostly means that you never learn that the model you are working with is wrong.

Presumably you're not a big fan of empirical priors, where the same data are used to formulate a prior that is then going to be used in the rest of the analysis?

I think Bayes would turn over in his grave with the idea that data should determine the prior.

You first arrived in Canberra at a time when it started developing as a wine region. As I understand these things, you, Deane, and Tony were all fairly knowledgeable about the subject. Deane now runs his vineyard, Tony lost interest, and your reputation for wine knowledge continues to grow around the world. I presume that didn't spring from Mungindi, so how did that interest develop?

Well, in Queensland I was a beer drinker and the beer was branded XXXX. Canberra's climate was a bit different to Queensland though and drinking beer in Canberra wasn't quite as attractive as drinking something else. I guess I might have got into whiskies but they were expensive. There weren't any micro breweries in Canberra in those days, of course. When I first arrived in '68 it was

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literally New South Wales beers and I can't say I ever liked them, so that was a problem. Parties had wine and there was a lot of backyard bottling, which made it cheap. I started getting a bit interested in the history of vineyards as I've always had a strong interest in history. It even turned out that my great-grandfather was a vigneron in the Shepperton region and taught some well-known wine makers how to do it. But the move from quantity to quality was really through Noel Butlin. When Janet and I started dating I would go to their place for lunch and he would have some very good wine sent to him from a CEO of a wine company who had visited his Economic History department. What I didn't know at the time was the wine we were drinking was very famous. I thought we were drinking average wine but we were drinking a wine that won a large number of gold medals at wine shows. So you did start to notice the difference between this and what you drank most days. Once you're onto the quality stuff I think it's very hard to go back to the other.

With the wisdom of hindsight, have there been moments/people that were pivotal to your career that we haven't already discussed?

I think there are lots of people who have had an influence. I'm not sure that anyone is really pivotal. If you work with someone you do change your attitudes and so they all have little impacts on you. There is no doubt some have more impact than others. Bill Phillips was one of these for his attitudes towards econometrics. Deane Terrell was another for the way he operated and supervised people. Gregory Chow introduced me to material I had never seen. Then there were a sequence of co-authors — Pravin Trivedi, David Hendry, and Ray Byron. They all changed what you focused on and what you thought was important, and without them you'd be a very different sort of researcher. Going to Rochester and encountering macro economists like Bob King changes your perspective as well. He was very knowledgeable about what the Cowles Commission did, as well as business cycles from a turning point viewpoint. Even people like Bob Gregory and Fred Gruen, who seem a long way from econometrics, had an impact. Both Bob and Fred had a way of doing applied work that influenced what I did. Moreover, Fred told me to write shorter sentences, and so I wrote shorter sentences. All the people I've just mentioned have had an influence on what I do, and the way I think about things, and it would have been very different if I'd never met them.

Again, with hindsight, were there opportunities that you missed because, at the time, you didn't recognise them for what they were?

I think there were opportunities that I've missed, often from not following them up in later work. Let me give you some examples. At Princeton I did varying coefficients regression. I wrote a paper when I came back for the Australian Economists Conference on how to use varying coefficient models to handle instability in participation rate equations, seasonal patterns, etc. No one was interested

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in it at the conference so I stopped working on it. In retrospect that was a big mistake. I should have spent more time on it. Another was the LM test for nonlinearity that I did at CORE. I sent it out to referees but they hated it. They couldn't see why you'd want to do a test for nonlinear models, particularly since I was only doing it for bilinear and nonlinear moving average models. I thought it was interesting as I found nonlinearity in IBM stock returns and things like that. But no one else did, so I stopped working on that as well. Weak instruments are other work that I never really followed up on either, even though I'd done a paper for the Canadian study group meeting pointing out that there were weak instruments in RBC models, and that would have an impact upon the distribution of GMM estimators of the parameters. The argument given for this was Peter Phillips' work on partially identified models (Phillips, 1989). I probably discussed this with him sometime before and it stuck in my memory. I remember going to a conference, I think it was in 1992, where presentations were given about bad results from GMM. I said that it's all due to weak instruments, but my remark was treated with a lot of scepticism. I could have followed that up but I didn't. The paper with John Robertson [87] was the only thing I ever published on it. Still I think everyone will have sometime in their career when they could have followed a theme that they had worked on and then didn't. Looking back it seems a mistake.



FIGURE 4. Adrian and Friends at the Pagan Fest, Sydney, July 2009.

Front: Tony Hall, Mardi Dungey, Janet Pagan, Adrian Pagan, Aman Ullah, Heather Anderson, Eric Renault, Jiti Gao, Denise Osborn, Ólan Henry. Second Row: Pravin Trivedi, Trevor Breusch, Ken Wallis, Eric Hanushek, Bruce Chapman, George Mailith, Warwick McKibben, Bill Griffiths, Bill Schworm. Third Row: Frank Vella, Farshid Vahid, Tom Cooley, David Gruen, Don Harding, Ron Bewley, Louis Maccini. Back Row: Martin Fukač, Ross Williams, George Tauchen, Chris Kent, Chris Skeels, Hashem Pesaran, Colin Cameron, Jan Jacobs, Michael Binder.

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But you would not have done something else. So it's not clear that these were bad outcomes.

If you were starting out now what do you think would be the most interesting avenues for study?

I don't know that I could even answer that because, as you get older, you tend to focus on things that you already know, since it's just much harder to absorb a lot of new material. When I started at ANU I spent a huge amount of time going to seminars, reading journals etc. Seminars that weren't just in economics but in political science, history etc. I spent hours in the Menzies Library reading *Management Science*. Very interesting papers on components models by Nerlove and others were regularly appearing in *Management Science*. When you do all this I think that you often find something where you feel "That's interesting and I think I can do something with it." What might interest me today is not what's going to interest a lot of younger people. In many cases I'm going to be working on something that others can't see any real future in. So I just don't think I have any recommendations except that young researchers should spend more time attending many seminars and seeing what it is that really gets them excited. In a macro conference last year, when I had to give my reflections on the past, I said that the thing that I remember most strikingly when I was a Ph.D. student were people like Chris Higgins coming back from doing a Ph.D overseas. They had worked on techniques for building macroeconometric models with Larry Klein. I thought this was a really interesting area to get involved in. And then it was natural to ask what can I do that's different to what they're doing? So that was why I was interested in testing models and that led on to other things.

Congratulations on the AO and thank you for your time.

NOTES

1. The positions described here, and like descriptions appearing subsequently, are in the Australian Public Service.

2. Adrian's Honours Thesis was entitled *A Theory of Optimal Reserves and the Adequacy of International Liquidity in the Developing Countries.*

3. Given that ANU has played such an important role in Adrian's professional life it is probably worth saying a few words about its peculiar structure. The ANU was created in Canberra in 1946 under an Act of Federal Parliament, the only Australian public university with a Federal rather than a State charter. Its purpose was two-fold, to be an intellectual resource from which Government might seek advice, hence its location in the nation's capital, and to be responsible for research and research training, which at that time would have been largely restricted to the Universities of Melbourne and Sydney. As such, the ANU had no undergraduate responsibilities. It was originally comprised of four schools: the John Curtin School of Medical Research, and the Research Schools of Physical Sciences (RSPhysS), Social Sciences (RSSS), and Pacific Studies (RSPacS). It was not until 1960, when the ANU amalgamated with Canberra University College (CUC), that ANU had undergraduate responsibilities. CUC, established in 1930, had been a college of the University of Melbourne (en route to becoming a uni-

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versity in its own right) and was comprised of Faculties of Arts, Economics, Law, and Science. In the (somewhat uneasily) amalgamated institution, the original ANU became the Institute for Advanced Studies (IAS) and CUC became the School of General Studies (SGS), later called The Faculties. There was much duplication of disciplines across the IAS and the SGS. For example, the Department of Statistics had been established in the Faculty of Economics with Ted Hannan [45] as the foundation Chair in 1959. (It is worth noting that it was responsible for the teaching of statistics across the SGS, as well as econometrics under the guidance of R.D. (Deane) Terrell.) The Faculty also included separate departments of Economics and Economic History, together with a Department of Accounting and Finance. The IAS contained a very strong statistics group under the leadership of P.A.P. (Pat) Moran (in RSPhysS), together with strong economics programs in RSSS and also in RSPacS. At that time Economics (RSSS) was under the leadership of Trevor Swan, with faculty including, amongst others, A. W. H. (Bill) Phillips, R.P. (Ray) Byron, and the economic historian N.G. (Noel) Butlin, while RSPacS counted amongst its faculty H.W. (Heinz) Arndt, W.M. (Max) Corden, P.D. (Peter) Drysdale, and later R.G. (Ross) Garnaut. Later in the interview Adrian makes reference to the Research School of Economics (RSE), which is an amalgam of Economics, RSSS and the Department of Economics from The Faculties, the latter having absorbed the Department of Economic History and the Econometrics group from the Department of Statistics at an earlier point in time. The RSE resides in The Faculties, but other economics programs still can be found in elsewhere in the University.

4. Adrian's Ph.D. Thesis was entitled *A Study of Estimation Procedures for Time Series Models in Economics.*

5. This paper was eventually published in two tributes to Phillips, both of which contain contributions from Adrian: [14] in Bergstrom, Catt, Peston, and Silverstone (1978), and [94] and [95] in Leeson (2000).

6. Econometric Theory and Method was the highest level econometric theory subject taught at ANU at that time.

7. The diagram first appeared in an unpublished conference address (Pagan, 1981).

8. Chris Higgins was later Secretary of Treasury.

9. This is a reference to J.H. (Jim) Stock.

10. The Shann Memorial Lecture is an annual event held by the University of Western Australia (UWA), in conjunction with the Economic Society of Australia, in honour of the distinguished economist Edwin Shann, who was a foundation professor at UWA and the first occupant of the Chair of Economics at that university.

11. A more complete version of the lecture can be found in Pagan (1996).

12. Pravin is P.K. Trivedi.

13. Case Studies in Applied Econometrics is the capstone econometrics subject in the Master of Economics Program at ANU.

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