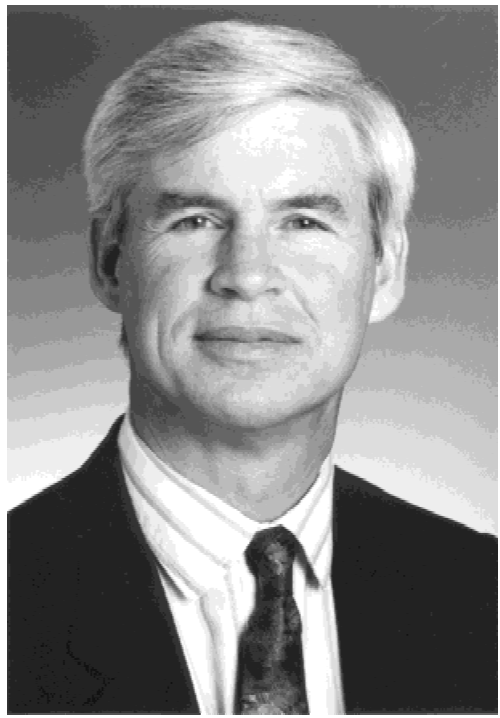
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THE ET INTERVIEW: PROFESSOR ROBERT F. ENGLE

Interviewed by Francis X. Diebold
University of Pennsylvania and NBER
January 2003



Robert F. Engle.

In the past thirty-five years, time-series econometrics developed from infancy to relative maturity. A large part of that development is due to Robert F. Engle, whose work is distinguished by exceptional creativity in the empirical modeling of dynamic economic and financial phenomena. Engle's footsteps

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range widely, from early work on band-spectral regression, testing, and exogeneity through more recent work on cointegration, autoregressive conditional heteroskedasticity (ARCH) models, and ultra-high-frequency financial asset return dynamics. The booming field of financial econometrics, which did not exist twenty-five years ago, is built in large part on the volatility models pioneered by Engle, and their many variations and extensions, which have found widespread application in financial risk management, asset pricing, and asset allocation.

We began the interview in fall 1998 at Spruce in Chicago, the night before the annual NBER/NSF Time Series Seminar; continued in fall 2000 at Tabla in New York; continued again in summer 2001 at the Conference on Market Microstructure and High-Frequency Data in Finance, Sandbjerg Estate, Denmark; and wrapped up by telephone in January 2003.

1. CORNELL: FROM PHYSICS TO ECONOMICS

Let's go back to your graduate student days. I recall that you were a student at Cornell. Could you tell us a bit about that?

It depends on how far back you want to go. When I went to Cornell I went as a physicist. I couldn't decide where to go until the very last minute. In fact—and I'm sure this is totally irrelevant to this interview—I telephoned Berkeley to accept their invitation to go to graduate school in physics there, because Berke-



Young Rob, Swarthmore, Pennsylvania, approximately 1948.

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ley was Berkeley. But no one answered; it was lunchtime or something. So in the meantime I went over and talked to my adviser at Williams, who said, "You should go to Cornell instead." So, when Berkeley called back, I said I just was checking on the application, and then I accepted Cornell.

Both Berkeley and Cornell were physics powerhouses.

Yes. But I was a little ambivalent, I guess, about physics. I'd always figured I would be a physicist, and so I was a part of a team studying superconductivity. We had a big lab down in the basement of Rockefeller Hall, and I spent my first year at Cornell hunkered down there with liquid nitrogen. When spring came, I decided that I had to get out of there. So I went over and talked to the chairman of the economics department, a man named Alfred Kahn, of whom I have always been very fond since that time.

Was that the airline deregulation Kahn?

Exactly. I had many friends who had switched into economics, so I had just wondered whether it would be possible, and he said, "Well, we've just been turned down for one of our graduate fellowships. Do you want it?" and I sat there sort of in shock and said yes.

Had you thought about doing economics in the back of your mind at some point? Or were you straight ahead in physics?

I was straight ahead physics. In fact, I had only taken one economics course as an undergraduate, in my senior year. So, it was a little bit of a surprise that I did it, but I had been becoming progressively more interested in social science, and I was intrigued by the notion of applying myself to the most quantitative social science. This would allow me to use my mathematics and yet still study the interesting problems of modern mankind. It seemed to me that economics could be the path to esoteric academic modeling or to solving practical real world problems, so there was a wide range of career options that I could follow. I started off taking undergraduate economics courses the rest of that spring, and simultaneously I finished my master's thesis in physics. I started the graduate program in economics the following fall.

There are a lot of good econometricians with physics backgrounds: you, Joel Horowitz, Jim Stock, Glenn Rudebusch, John Cochrane, Steve Cosslett, Dan McFadden, . . .

Yes. And Jere Behrman.

I didn't know that, and he is of course my colleague at Penn!

He was two years ahead of me at Williams College and had gone with a physics undergraduate degree directly to MIT. So that was one of the reasons I knew it could be done. Anyway, I think it is a great combination because physicists are continually worried about integrating theory and data, and that's why I think physicists tend to make good econometricians. That's what econometricians do.

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I have always felt that many of the sciences are de facto organized into hostile camps of empiricists and theorists, with most members of each camp surprisingly unaware that it's ultimately the interplay and discipline engendered by the cross-camp competition that fuel scientific progress.

I think theorists and empiricists actually use each other in physics more than might be true in economics. At Cornell, there were a couple of theorists that would wander around the basement to see what the experimentalists were discovering.

Why are the experimentalists always in the basement? That seems to be true across all disciplines. In fact, despite your move from physics to economics, you've never left the basement!

(*Laughing*) That's what I was getting at!

But seriously now, you wound up working with a pioneering econometrician, T.C. Liu. Can you tell us about that? How were you trained by him?

Well, it was very interesting, because Ta Chung was a real dynamo. The year when I was taking econometrics he was in Taiwan helping reform the tax system, and so I took my first econometrics class with Berndt Stigum. It was a very small class, and we went at a high level using Malinvaud's text, which had just appeared in English. The following year when T.C. came back from Taiwan I took the course again, and that time we used Goldberger. Those two books back to back provided a great econometrics background.

T.C. is often credited with a very early and very prescient insight, later refined and amplified by Chris Sims and others, namely, that the identifying restrictions in traditional macroeconometric models are literally incredible and should be abandoned to the extent possible. Did you see those ideas percolating?

You know, I think that's one of the reasons T.C. wanted to get into higher frequency modeling: he wanted to build recursive models. I suppose this is in fact the vector autoregression (VAR) idea in another guise. His monthly model was, I think, almost entirely recursive. But he didn't really discuss the philosophical issues. This actually had been about ten years old by the time, or maybe more by the time I started the graduate program, and I don't remember him complaining about the need to find new instruments, and so forth. He was concerned about what is the best collection of instruments, and that sort of thing, but it wasn't like the way it's presented in the VAR literature, in which nothing is assumed exogenous. I never remember him saying that.

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Tell us about your Ph.D. dissertation work and how you were led to it.

My dissertation was very much along the lines of T.C.'s research, which was on temporal aggregation, basically asking, "What's the relationship between macro models estimated at different data frequencies?" T.C. had already built an annual model and a quarterly model, and he was working on a monthly model, and so that was what I was trying to analyze and reconcile, from both theoretical and empirical viewpoints. The key issue was, if you started out with a certain high-frequency (say, monthly) dynamic model and assumed it to be true, and you aggregated to a lower frequency (say, annual), then what would the lower frequency model look like? You ended up being able to talk about the time aggregation problem in the frequency domain, and work out moments of aggregated data when the whole thing was dynamic, and it had to do with integrating over the spectrum, stuff like that, and the answer was messy. But what T.C. had observed, I think, was that the lag lengths were affected by aggregation; they got shorter, and that's what I was trying to characterize rigorously. I also noticed that the long-run effects seemed to be approximately invariant to temporal aggregation, which is related to some much later work on cointegration.

Let's wrap up the Cornell days. Is there anything else in your mind that you remember as shaping your later thinking, any other faculty or students who had a particular influence?

Well, my committee included John Fei as well as Bernt Stigum, and of course T.C. Liu, and it was really a very good committee, because they all brought different points of view. I think I didn't really get the economic intuition for model building as well from T.C. as I should have—that kind of came later when I was sort of building it on my own—but I think he gave a great background for how the statistics and the underlying model fit together. And I also took a statistics class, with Jack Kiefer, which gave me really a wonderful idea of how statistics actually worked and what the statistical decision theory problem was. He and Wolfowitz were the key people over in the statistics department, and it was fascinating, because physicists didn't actually treat statistics very carefully—they just sort of "did it."

Yes—do an experiment and find the answer. Little need to worry about quantifying uncertainty.

That's right. I didn't really learn statistics until I became an economist!

2. MIT, BAND-SPECTRAL REGRESSION, AND URBAN ECONOMICS

Now, on to MIT. How did you land your first job at MIT?

I have no idea really how I got the job. It seemed most miraculous because I didn't even go to the meetings. It was the year of the Chicago riots, and the meetings were canceled, and there was an alternative meeting in Philadelphia,

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the "gray market" as they called it, and I went there and had a little interview with Cary Brown, who was the chairman of MIT's department at the time. And then I went to MIT and gave a little discussion of my dissertation with just some faculty members, and I did the same thing at Yale, and both of them somehow came up with job offers.

What did they like about you and your work?

I think one of the things that impressed them was that I knew things from my physics background that had been useful in analyzing this time aggregation problem, like contour integrals and stuff like that, and they thought, "Oh, anyone who can do that can probably do something else." I'm not sure whether they were right, but at least I could do contour integrals!

Do you remember anything of your visits to Yale and MIT?

Mark Nerlove was at Yale, but he took me aside and said, "I want to tell you, I'm not promising to be here for much longer!" I also met Ken Wallis, Chuck Bischoff, and Jim Tobin at Yale. At MIT there were Duncan Foley and Ed Kuh.

Let's get back to your interest in spectral methods and your eventual creation of band-spectral regression. Do you think physicists make particularly good time-series econometricians? Fourier analysis and related ideas are surely a natural passageway into time series.

Well, that's what I did my dissertation on; I mean spectral analysis was a big part of it. They were tools that I already knew, so it was helpful. I guess physicists don't do much cross section, do they?

Did your dissertation develop band-spectral regression methods?

No, they came later, at MIT.

What was the thought process that led to band-spectral regression? How did the work progress, and how it was received?

The idea for band-spectrum regression probably came during summer vacation. I hadn't realized that I was supposed to work all summer after my first year as assistant professor, so my wife and I bought a car, went to Europe, and spent the entire summer in Europe traveling around, ending up at the World Congress in Cambridge, where I was on the program with Phoebus Dhrymes and Chris Sims and Ken Wallis was the chair of the session. So, as part of the summer I had brought along Jenkins and Watts, which is the book on spectral analysis that I like the best, and we stayed for a couple of weeks in a Spanish resort hotel where every morning I sat out on the deck looking out over the Mediterranean reading Jenkins and Watts, which is sort of a wild thing to do, but it actually really appealed to me, and I love doing that sort of thing. I was working on these Hannan efficient estimators, and so forth, and just trying to write all this stuff in different forms, and all of a sudden band-spectrum regression just emerged as very simple yet useful idea. I actually called it partial-

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Camping with Marianne in Maine, 1972.

spectrum regression because it was regression of a part of a spectrum, but Manny Parzen took a look at it, said I had given it the wrong name, and changed it to band-spectral regression. Really the first draft came when I was visiting at Cornell, which I did the second or third year when I was at MIT, because my wife was completing her master's degree in psychology there. That's when I wrote it, and that's where I got the first feedback on it—I didn't really get much feedback on it when I was at MIT.

I recall that Ben McCallum and some others criticized band-spectral methods from a rational expectations viewpoint, in that rational expectations tend to produce relationships and restrictions that hold across all frequencies, not just certain frequency bands. What is your view, twenty years later, on all that?

Certainly the McCallum critique yields useful insight. And it's related to the reason I stopped working on band-spectral methods, which is that one interpretation of band-spectrum regression is as a diagnostic, a check on whether a static model is well specified. That is, a dynamic model can be well fit at all frequencies just by finding the right coefficients, and that's in fact what the cross spectrum does. The cross spectrum tells you exactly in the frequency domain what the relationship between two series is, and then you transform that back into the time domain and you get a distributed lag model. That's what these estimators really did. So what I was interpreting as different coefficients at low and high frequencies could also be interpreted as whether the static model was misspecified and whether there really should have been a distributed lag model.

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I see. So it ultimately boiled down to a specification test for adequacy of dynamic specification.

Yes, and in that sense it was less interesting. The time domain provided plenty of ways of testing that already. But then, on the other hand, low frequencies are particularly interesting, and we only recently have fully appreciated why they are so particularly interesting, because they carry the long-run information in them. In modern language that is the cointegrating information. And cointegrating relationships are of course static.

You mentioned that you didn't really get much feedback on your band-spectral work from the folks at MIT. Please elaborate.

The person that I probably talked with the most about these sorts of things was Chris Sims, who was at Harvard for my first year, and so Chris and I would get together and talk about a lot of frequency-domain stuff because he was very interested in the frequency domain, but then he left after the first year, and so I don't know that I had a lot of people to talk to about it.

Let's stay with that a bit. Some would say that MIT, and Cambridge more generally, has never found time-series econometrics appealing. Is that correct? And if so, why, and how do you feel about it?

Yes, I have a lot of feelings about that. It certainly is inhospitable in an intellectual sense for the time-series people who have been there. Everybody was very nice to me at MIT and Harvard, and I feel a great deal of fondness for all those people. But I didn't feel any support, really, for interest in time series. That was certainly true, and I think that's the reason I spent so much of my time doing urban economics at MIT. There was the big urban economics project which Frank Fisher and Jerry Rothenberg were doing, and I got involved in that and spent most of my research time doing that, but in the back of my mind there was still this time-series thing that I wanted to do more of. The person probably who was most interested in my time series work at MIT was Ed Kuh; he really encouraged and supported me. He had the Troll econometrics software project going at that time. He had me involved in that group, and they programmed up versions of band-spectrum regression and Hannan efficient estimators.

Were they included in the Troll package?

Yes, both were included in the Troll package. He was a very supportive friend, and I really enjoyed working with him a lot.

Related to MIT, one thing I've noticed in your work from the early days through to the present is a fondness for the Berndt, Hall, and Hausman method of numerical optimization. Does your fondness for it stem, at least in part, from conversations with Jerry Hausman during your MIT days?

No. I think it's laziness.

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Pardon me?

I'm afraid it's laziness. I learned it long ago, and I've just stuck with it. Certainly, though, I had a great time with Jerry, and I think very highly of him. The best econometrics conversations that I ever had at MIT were probably with Jerry, where he said "Tell me something—Why do you think this is true?" and we'd really go through these things, and we've remained friends over the years.

Were you also thinking about macroeconomics at the time and the potential role of band-spectral techniques in getting at low-frequency macrodynamics, and so on?

Yes, that's right, very much so. I taught undergraduate macroeconomics all the time I was at MIT, and really I was very interested in macro modeling. I had been interested in that for my dissertation as well. This was the period when the revolution of what I suppose we would call "the demise of the big models" was going on. The St. Louis model was the new kid on the block and was doing well, and everybody wanted to know about monetarism, and the Sargent–Lucas criticisms, and so forth, were big issues. So a lot of the interesting econometric issues in macroeconomic modeling were right there, but there really were no econometricians at MIT or Harvard who were interested in those kinds of questions.

Is there anything else you would like to mention about urban economics? You already mentioned getting involved because people like Frank Fisher and others at MIT were thinking about it. Was that the genesis of your work in urban economics, or did you have previous interests along those lines? And are you still in one way or another pursuing research in urban economics?

Right now I don't think I'm doing any more urban, but I had kept it going until pretty recently with just the occasional paper. I had some really interesting students in the area, such as Ed Coulson. I did a little paper on growth controls a few years ago with Richard Carson and Peter Navarro, which grew out of the classes that I taught [82]. I taught urban economics classes until two years ago, every year. So I kept trying to think about how the models and the data fit together. I still feel like there is wonderful data in urban economics that provides a great place for econometric analysis. In urban economics we have time series by local areas, and wonderful cross sections, and my sense is that they have not been analyzed in a very systematic econometric way.

Do you think that recent advances in spatial econometric methods, such as Tim Conley's spatial generalized method of moments (GMM), will have payoffs in urban contexts?

You would think so. The space turns out to be complicated, because whenever you look at it up close, it isn't very linear due to transportation costs. Then you've got to worry about mode of transportation—freeways and all these kinds

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of things—so the abstract models have to be changed a lot to take them to the data, and I don't know whether spatial correlations in and of themselves are actually interesting. But maybe they are. It's just that I don't know how interesting the stuff I've seen so far turns out to be.

3. THRIVING IN SAN DIEGO

So, a few years go by, you get on a plane, and you're in San Diego. That must have been a fascinating time. I guess Clive Granger had been there for at least a little while, although maybe not a long while.

He had come once as a visitor, visiting Dan Orr, his old friend and then he had taken a permanent job really just the year before I came. I had seen him at one of Arnold Zellner's conferences on seasonality in Washington, and I had been looking around at various places and asked him if there were any jobs in San Diego, and he said, "Oh, sure. Come on out." So anyway, I came out in February and stayed in this nice hotel right on the beach and just decided, "Wait a minute. What am I doing in Boston? I should be here." So, anyway, I was very pleased to go, and it was one of these decisions—sort of like my switch into economics—which was really a big decision but it just felt like it was the right thing to do. It turned out to be great.

When you arrived was Clive the econometrics group, or were there also other people?

There was a strong econometrics group there in addition to Clive. There was John Hooper, Dennis Smalllwood, and Dick Attiyeh, who had been colleagues together at Yale before they came out, and Ramu Ramanathan, a Minnesota Ph.D. who was also a good econometrician. A short time later, Hal White came out, which was great. We came very close to hiring Nick Kiefer as well.

Wow, I didn't know anything about that.

It was really tragic, looking back on it, that Hal and Nick were available at the same time. We had only one slot.

The development of San Diego econometrics has been amazing, obviously, since you and Clive joined. What's the secret?

Well, I was hired as an urban economist.

I see. Hire econometricians but under different labels!

Jim Hamilton was hired as a macroeconomist. I guess Hal was actually hired as an econometrician! But I think the secret really is that San Diego's educational strategy was that there are really only three subjects in economics: micro, macro, and econometrics. And so it made sense to build strength in any of them. In contrast, if you think of there being ten subjects in economics, then you only want one econometrician. Or maybe a half. And that's not enough. It

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was really having enough of us in one place at the same time that made it so productive. Students came because they wanted to work with us. You know, we fed off each other. We wrote papers together. Seminars were interesting. There were plenty of audiences. It just takes a certain critical mass to make things happen. And it was really quite different from my Boston experience.

What do you think of San Diego macroeconomics? That seems to be a more recent sort of blossoming, with Hamilton, Flavin, the Rameys, den Haan, and so on. How did that happen?

That's exactly the same sort of thing. From the very beginning we always said, "We want to build an applied group, and really macro would be our first choice as to what the applied group would be." We struggled and struggled, trying to find the right people, and couldn't hire them, and then we hired Valerie and Garey, who are terrific, and then we hired Jim and Marjorie because they had been on leave in San Diego and liked it, and it was again one of these very fortuitous circumstances that everybody was very anxious to have them come, and then Wouter den Haan helped it all come together. I feel that it has become a really lively place in macro.

I agree. And most recently I guess it's quite a lively place in finance, with Allan Timmermann and, of course, Bruce Lehmann.

Allan has been great. And Bruce Lehmann is great. I mean, Bruce doesn't actually write that much, but he's such a resource, and he's so very active and lively, and he has such good comments on everything. It's great having him there. And it's also great having Alex Kane, although he doesn't pop into the department all that often, but Bruce comes to all of our econometrics workshops. Also, because Hal, Clive, Allan, and I are all interested in financial econometrics, the econometrics workshop has taken on quite a strong flavor of finance.

San Diego students have also been great. Who stands out in your mind? Who surprised you?

I've had great students. And it's really one of the pleasures about San Diego, it's one of the reasons that a few years ago I decided not to leave, because I've really thought that the students that I've had are so good. Who has surprised me? You mean about how well they've done in the profession?

No. I mean surprised you by stimulating you and quickly emerging as colleagues.

You know, I feel like that is something I really look for in a student. At the beginning I am telling them what to look for and what to do, step by step. Toward the end, the best students are telling me how it works and what we ought to do and how to go from there. And that's when I know they're really going to go out and do well in the profession, and I've had some students who have done wonderfully well at that. Some of whom you know very well and others of whom are not very well known but who I think are really terrific and

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have the potential to make wonderful contributions. But I guess I'd better not produce a list of names, or I'll get in trouble!

Let's talk about your work at the time. Your early work on testing, particularly Lagrange multiplier testing, was very influential. Did your London School of Economics (LSE) visits and your discussions with David Hendry influence you to move in that direction?

Well, my relationship with David probably started when I first went to MIT. I had met him, as well as Ken Wallis, that first summer when I was on the way to the Cambridge meetings. David didn't actually go, but I met him briefly, and I felt like LSE was the place where time series was most interesting in those days, and I did my very best to spend enough time with the people there: Sargan, Durbin, and Mizon, as well as David and Ken. Each visit was stimulating. So I spent a quarter there sharing an office with Chuck Nelson in 1975. He was on his way from Chicago to Washington, and I was on my way from MIT to San Diego, and I was thinking about testing, and Ken said, "By the way, you might like to see this paper by Berndt and Savin on the inequality between Wald, Lagrange multiplier (LM), and likelihood ratio tests." And so I got very interested in testing, LM tests in particular, and ended up writing the LM paper that appeared in the *Handbook of Econometrics* [47].

Related, tell us a bit about your work on exogeneity, also done in your early San Diego days. Engle, Hendry, and Richard [40] is a fascinating paper—perhaps something of an outlier relative to your overall research program but a tremendously influential one-paper critical mass, and it involves David Hendry. Tell us a little bit about the path that led to that work, including your relationship with David and how that has influenced you.

I think it must also have been when I was at LSE. I went to CORE to give a talk on a paper that I had done on unemployment, which involved causality tests. Afterward, Jean-Francois Richard said, "You know, you think you're talking about causality, but you're really talking about exogeneity." I said, "No, no, no." He said, "Well, Koopmans would have called your concept exogeneity." I said, "I don't think he did." So anyway we pulled out Koopmans, and we looked at this and realized that in fact there were different concepts of exogeneity that could be formulated—which we would later call weak and strong exogeneity or even super exogeneity—none of which was Granger causality. Simultaneously, Chris Sims was busily pushing causality tests as a way of assessing exogeneity. Neither of us liked that idea, and so we spent hours discussing this and decided that really we should write a paper on it—just a little note because, really, how do you write a paper on a definition? You just write a little note.

How did Hendry get involved, and how did the work progress?

Jean-Francois and David were good friends, and he said "Oh, should we get David involved?" and I said, "Great." The three of us spent a lot of time on

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this, and we worked and reworked. Jean-Francois really helped the mathematical structure, and David kept pushing to extend the scope and depth of the paper. The main thing was figuring out how all these different concepts could be defined and how they all fit together. That really made it come to life. And then—I don't remember whether it was the same year or the next year—Ken Wallis had one of his summer institutes on time series and dynamic macroeconometrics, and Chris Sims was there, as well as Hendry, Richard, and I, and we spent hours and hours talking about all these different concepts. That really helped to focus our thinking, and then we wrote up the paper. The paper was controversial, and it was rejected a few times by *Econometrica* before we finally managed to get them to take it.

Clearly your time at LSE was highly influential on your research. What other places and people outside of San Diego have been most important to your research?

Gourieroux and Monfort in Paris have put their fingers on so many interesting problems, often very early, and put their stamp on them. I think they've made a really positive contribution to the profession and to my thinking. And I always have a good time talking to Adrian Pagan. Adrian is really the prototypical tough critic and insightful econometrician. If you can get him to agree that something's interesting, you've really made an accomplishment.

4. COINTEGRATION

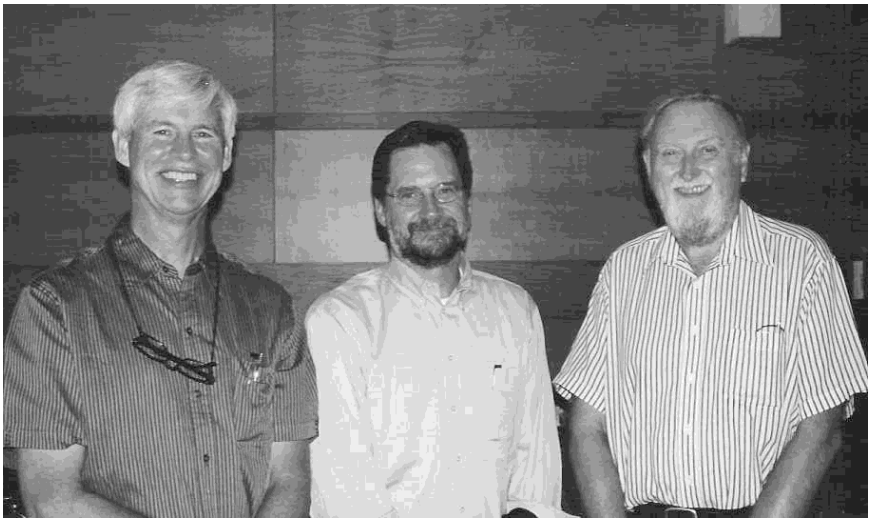
Cointegration.

Cointegration.

What an amazing ride from 1980 to the present—certainly one of the key developments of the last twenty years in econometrics and empirical macroeconomics. How did it all happen?

Well, Clive had been formulating the problem for some time, and he had proposed some definitions of cointegration early on, and he and Andy Weiss wrote a paper which was actually the first attempt to try to test it and get it to connect with the error correction model, but in fact I don't think that paper actually hangs together quite right. It's a good attack on the problem, but there are some formulation issues that really didn't work. So, what happened was that I got an idea of a slightly different way of writing it down which gave rise to the test statistic we proposed in our paper, and of course also gave rise to the two-step estimation method which follows directly from the test construction. Clive had done the proof of what we call in there the Granger representation theorem, which in my view was not a very tight proof, although I suppose people would say that what we finally published wasn't very tight either. But in any case, it was a little tighter. So we decided to write this joint paper, which was first

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Rob, Mark Watson, and Clive Granger at Clive's Festschrift celebration, Santiago, Spain, 1999.

presented at the NSF/NBER time-series seminar in two sessions, one session on the theory and one session on the testing and estimation. It took place, I think, at UC Davis, and the reaction was not as enthusiastic as one might have imagined, but a lot of discussion ensued. Later the paper appeared as Engle and Granger [63].

You mentioned the NSF/NBER time series seminar. That's a fascinating seminar, particularly in that it brings together the statistical and econometric sides of applied time series in the Box–Jenkins tradition. One aspect of the statistical side is reduced-rank regression. Were you aware of reduced-rank regression at the time?

Not really. I wasn't exactly aware of reduced-rank regression, but I was aware of the Box–Tiao paper on the maximal correlation coefficient between multivariate series. But that work was for stationary processes. There was no unit root distribution in that, although the framework of course was the same as, or almost the same as, the one that Johannsen eventually used. The insight of Soren Johansen and Greg Reinsel that cointegration was a reduced-rank problem was new to us, and of course we thought it was very good and powerful.

How did you meet and get to know Soren Johansen?

I'm trying to remember when I first met Soren. I remember him being in San Diego for a visit and talking about all the things that we could do with cointe-

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gration, like I(2) problems and separability, and many other sorts of extensions of the simple cointegration model—that must have been after he wrote his first paper. But I think I knew him before I saw the paper. And I did a week in Copenhagen at one point, talking about cointegration, ARCH, and stuff like that, but I knew him before that too. So when did I meet him? You know, I really can't quite remember. I bet it was at one of the European summer meetings.

That's when I first was exposed to him. I remember seeing him and his flamboyant and enthusiastic style, just so thrilled by the beautiful geometric structure of cointegration.

That's right. He was very much into the aesthetics of the statistics, and he was happy to assume finite autoregressions in order to get a beautiful theory, as opposed to approximating some infinite autoregression, in which case the order would have to grow appropriately with the sample size. Soren has always been very interesting to talk to, and I thought from the beginning that his paper was really very interesting. The idea of reduced-rank regression was very natural.

Peter Phillips is another key contributor to the cointegration literature. In your view, how do Peter's contributions fit in—the functional central limit theory approach and the triangular representation, for example—and what strikes you about them?

Well, Peter developed the functional central limit theorem approach to doing the unit root asymptotics. The functional CLT was of course well developed in statistics, but he found it and brought it into time-series econometrics and showed that you could derive the Dickey–Fuller distributions using it. He also introduced the triangular representation that simplifies the analysis of cointegrated systems because you're no longer testing for the existence of cointegrating vectors or how many there are; instead, you're only estimating the model. But of course then it doesn't really solve the problem that we and Johansen were trying to solve: how to test for cointegration. Also there's the issue of normalization. One of the things about Johansen's method, of course, is that there's no normalization involved, and that's both strength and a weakness. On the one hand, you never have the awkwardness of having normalized on something that truly doesn't belong in the model, but on the other hand, it can be hard to interpret the cointegrating vectors.

We've already talked a bit about David Hendry's influence on your thinking, in the context of your work on exogeneity. A second ago you mentioned the Granger representation theorem, which of course characterizes the intimate relationship between models of cointegration and models

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of "error correction" popular in the LSE tradition. Did Hendry influence your thinking on cointegration?

Absolutely. David and I have had long conversations on error correction and the LSE tradition, going back to the work of Denis Sargan. He's a great econometrician and a great friend.

Very good. Now that we've talked about cointegration, let's move to common features. If two variables are integrated but there exists a linear combination that is not, we say that they're cointegrated. More generally, if two variables have property *X* but there exists a linear combination that does not, we say that they have common feature *X*. Sounds like an obvious logical progression with wide applicability. Do you want to say anything about common features?

I was pretty enthusiastic about common features as an organizing concept for a lot of multivariate analysis. I don't feel that it has actually caught on in the way that I thought it might. It just seemed natural to me that in high-dimensional systems you'd really want to look at things that were common across a bunch of different series. I think maybe the two most attractive applications—besides cointegration—have been the volatility models, which are really tests of the factor-ARCH model, where you ask whether there are linear combinations of returns which have no ARCH in them, and some of the common trend/common cycle models, in which some restrictions are associated with the unit roots and some restrictions are just associated with stationary serial correlation. But at any rate the common feature idea has not actually been picked up as much as I thought it might.

Why do you think that's the case, especially given that cointegration was such a hit?

Cointegration explains some things that we didn't really have a good theory for, like why static regressions actually give a pretty good estimate for the long-run effects: you build the best dynamic model you can, and lo and behold the long-run effect is the same thing as you had from the static model. Or when you take principal components of things, how the first component might explain 99% of the variance or something like that. Cointegration fits those stylized facts. I think some of the other factor models or common feature models might do that too, but we're not so familiar with those stylized facts, like how many seasonal features are there really? Is seasonality really the same for all series? Or some kinds of nonlinear errors, are they really the same for all series? In terms of the factor ARCH model, I think the world is more complicated than just having one or two volatility factors to explain, say, global volatilities. Perhaps a more realistic situation is twenty countries and ten factors, and that's a hard thing to detect in practice.

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How do you view cointegration in its relation to macroeconomics and finance? How useful are cointegrating techniques in those areas?

Cointegration is really an econometric technique which is designed first and foremost for analysis of macroeconomic data. And I think that the short-run dynamics of macroeconomic systems are often thought to be kind of the gloss on top of the fundamental, long-run driving forces. Cointegration is exactly a method which is designed to look for long-run behavior without being too distracted by the short-run movements. So, I think it is the sort of generic tool of choice for macro modeling and forecasting. It's also the natural completion of the band-spectrum regression idea, where you think that the long-run relations are what you see with the low-frequency data and the high-frequency part is dynamics around the low-frequency movements. And this connection, I always thought you could make a little more rigorously, but in fact, Peter Phillips was the one that has proposed estimators of cointegrating relations using just the low-frequency components, and I think not surprisingly, has better performance than using the whole spectrum.

What do you think about the use of cointegration methods in finance?

Cointegration among asset returns implies that at least one return can be predicted based on the others, so in an efficient markets world one generally would not expect cointegration. But there are exceptions in the sense that some asset prices are not total return prices, and so the first difference of the price is not the total return, such as a bond with a coupon payment or a stock with a dividend payment—then you can have cointegration in prices and still not have return predictability. And that's one of the reasons you see cointegration between some bond market prices, because in fact it's just the coupon payment which is giving you the predictability. But short of that, cointegration has something of an appeal to financial people; that is, if prices deviate from where they are on average they are eventually going to come back, and cointegration might be a way you can detect that they are going eventually to come back. This suggests the potential profitability of portfolio strategies based on trading against prices that deviate from their "normal" values. Some people seem to think that all you have to do is have patience and you'll make the profits. I'm not sure that sort of enthusiasm is warranted. When finance people find evidence of cointegration, it's often after running many cointegrating relations, often with relatively short sample periods, and perhaps even fiddling with the sample periods. All this suggests some data mining, which would invalidate tests for cointegration. In fact, many applications I've seen have not even really tested for cointegration; instead they just sort of observe it or hope for it. So, I think it's easy to abuse cointegration in financial settings; "statistical arbitrage" is not as easy as it sounds. I've actually done some recent research looking at whether you can tell when cointegrating relations are breaking down. Effectively the approach says that you may have cointegration for a while, but then you'll get

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big shocks to the system, and those will be permanent shocks, and they'll move the cointegrating relationship to a new place. And so you no longer get the reversion to the old equilibrium.

5. ARCH AND FINANCIAL ECONOMETRICS

Let's move to ARCH. I don't know where to begin—it's been a tremendous quarter century. Can you tell us how you started thinking about it, your role in its development, and its future.

Well, generalized autoregressive conditional heteroskedasticity (GARCH) or ARCH is one of these LSE inventions that I attribute to my great sabbatical time at LSE and the conversations there. I've taken sabbatical time at LSE twice. The first time was really when the LM tests and exogeneity were done. The second time I did ARCH. And ARCH was a problem that actually was started and finished while I was on leave at LSE. A lot of the discussions I had over lunch and coffee were with David on issues of "How do you interpret these things; how do you formulate them; what are the theorems?" And with Durbin, Sargan, and others around, too, there was just lots of input, and I really appreciated all the feedback I had. When I finally got the ARCH model formulated so you could do it as an iterated set of least-squares regressions, David said, "Okay, I guess we can do it." So we had the programmer code it, and we tried it out, and the results seemed promising. The name ARCH was actually David's suggestion, and the ARCH paper turned out to be the first paper they put in their new working paper series.

What led you to think about volatility dynamics?

It turned out to be a marriage of a couple of different ideas that I was really struggling with. One strand was trying to get variances into macroeconomic models, because some people thought that it was actually not the expected value of economic variables but rather their variability that was relevant for business cycle analysis. This was basically Milton Friedman's Nobel lecture, but I was looking for ways of tying it in with rational expectations macroeconomics. A second strand is that in everything I did I was repeatedly impressed by the importance of the conditional distribution and how it simplified the way you think about building models. And I suppose the third strand is that before I went on leave to LSE Clive and I were talking about bilinear models and he showed me a test statistic which had a lot of power, he thought, to detect bilinearity. It was what we now know as the ARCH test, regressing squared residuals on past squared residuals. I had some sort of model up on my computer and he said, "Square those residuals and get an autoregression." I did and was very impressed to see that the R-squared when multiplied by the sample size was quite large. So, having done all this work on LM tests, I thought to myself, "This is not the LM test for the bilinear model. So what is it the LM test for?" So,

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putting that question together with the attempt to try to find time-varying variances, I realized that it was the ARCH model. So, I think I would say I discovered the model from the test, rather than the other way around.

How about GARCH? How did Tim Bollerslev develop that? Was it just the obvious progression, or were there difficulties involved?

Well, David Hendry was involved in that one, too! David was concerned, and I think actually it was Steven Hall that prompted him on this or maybe Steven Taylor, I can't remember exactly, that GARCH looked not like an autoregression but rather like a moving average. David was in San Diego at the time, and we struggled with it a little bit, and the question was how could you put a lagged dependent variable into an ARCH model. Tim was very interested, so we talked about it with him, and the next day Tim came with it all worked out. He said, "Well, you could do it this way, and here's the conditions for stationarity," and the next thing we knew, he had programmed it, and he was very, very quick working out all the details. It was really a wonderful simplification of the ARCH model because the parameterization had been such a stumbling block early in the pure ARCH model, and it just appeared to be much simpler in the GARCH framework. So, I think in some ways David deserves some credit for the GARCH variation.

It's interesting to see that ARCH started out with an eye toward macroeconomic applications—you mentioned the Friedman lecture—but quickly moved into finance. With the benefit of hindsight, it seems clear that finance is really the natural place for GARCH applications. Volatilities of financial asset returns are clearly forecastable, and that has lots of implications for finance. What's your view on that—the development of GARCH from a financial perspective. Where has it been, and where is it going?

Well, of course I was trying to find this trade-off between risk and return in macroeconomics, but risk and return is much more a trade-off in finance, which I sort of recognized, but I didn't know very much finance. David Lilien was one of the people who said, "You know, you really ought to apply this to finance," and that's when he and Russell Robins and I wrote the ARCH-in-mean paper [62], which was trying explicitly to measure this risk-return trade-off. I think the paper that really kicked it off in finance was the French–Schwert–Stambaugh paper, which was done without any input, or interaction, with me. I think it made the finance community realize how interesting this was. That paper was published in 1987; the ARCH-in-mean paper I think was also 1987 and the original ARCH paper was 1982, and it was written in 1979, so there was really a lot of time in there before it caught on and actually made the migration to finance, which gave me a lot of time to work on ARCH variations, including integrated GARCH, which turned out I don't think to be such a good idea,

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factor GARCH models, and so on. We got a lot of research done before it got so popular, and that was very helpful.

What are your views on ARCH and its contribution to the emerging, or perhaps emerged, financial risk-management industry?

It's interesting how I've gotten into that. I've been asked periodically to talk to finance groups and this started probably ten years ago, and at first I had no idea what they would be interested in. So in fact, I got invited to talk at a conference called "Volatility Models" and another conference on correlation, and actually the second was especially puzzling to me because I had no idea what it was that you would ask somebody to talk about in correlation. So I asked them for a few references and found out what the finance questions about correlation really were. In any case, I think that GARCH is a very natural tool for doing risk management, and I think the idea of "how do you measure and quantify market risk?" is exactly one of the real strengths of GARCH models. They give you the ability to talk about risk when it's varying over time in a way that most other methods so far really have not been able to do. And, you know, this includes in particular the multivariate notions where you're talking about portfolios which have assets with time-varying correlations.

Let's switch for a second to asset pricing, in particular derivatives pricing, options being a leading example. The volatility dynamics literature in general and the GARCH literature in particular have made important contributions there. Can you describe the genesis of your thinking along those lines and your views on the future of derivatives pricing under time-varying volatility?

All options-based derivatives require some sort of volatility number because they're more valuable when volatility is higher. It is typical to quote the price of options in terms of volatility. So it's surprising that volatility models weren't a very important part of the initial work on options pricing. In fact one view that I like is that building better and better volatility models is like doing fundamental analysis. We're trying to understand what the fundamental value of an option really is, regardless of where it is being priced today. We can view GARCH models as facilitating that fundamental analysis. That line of thinking leads you to think about the relationship between implied volatilities, which arise from trading, and GARCH volatilities. But in a sense that misses a key feature, which is that if the real world has time-varying volatility, then it's not clear how you ought to price options, so the link between implied and GARCH is not as close as you'd like to think. In fact, finance literature has a whole series of options pricing papers on how you would do this under various settings. We now have several different versions of how you actually ought to price options if GARCH is in fact not just an approximation but really the true underlying data-generating process. And these reveal some strong similarities with actual option prices but are certainly not as close as you might like for

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actual applications. Hence I think one of the missing features in present analyses is investigation of risk premia. That is, in a GARCH world options are no longer redundant assets, and therefore they may be priced with a risk premium. And this risk premium comes from some sort of pricing kernel which must price not only the underlying asset and the option but everything else, too. So the question is, "What sort of pricing kernel can actually rationalize options prices in a GARCH environment?" In a recent paper that Josh Rosenberg and I wrote, we looked at this pricing kernel as not being a constant of nature but actually having time-varying risk aversion, so we would allow the possibility that agents are sometimes more risk-adverse than others. And by matching the options prices with the GARCH forecasts, you can see that there are periods when agents seem to be more risk-adverse than others, and this gives you a full representation of the options in this particular underlying index. It's possible that the analysis suffers from overparameterization, but it's an interesting way of investigating the issues.

Let's move to financial market microstructure. What can we learn about market microstructure effects from high-frequency returns, and what can we learn about the dynamics in high-frequency returns from market microstructure? Where is the literature, in your view? Does the potential remain latent, or has it been realized? Are we in the middle of it all right now?

I think we're in the middle of it. I think it's a fascinating field for an econometrician, but it's also a fascinating field from an economic point of view because the fundamental issue in market microstructure is how we get from some people knowing something to the efficient market hypothesis. How do prices incorporate information? And what institutional structures facilitate that? How long does it take? How efficient are markets, anyway? Essentially what market microstructure recognizes is that agents are continuously doing an inference problem, trying to figure out what the price ought to be given what they see around them. What they see around them are trades. People buying, people selling, as well as public information, and so the econometrician has the same information the agents have, or at least an appreciable subset of it, and he can try to figure out how this inference problem really works. So, by the time we get to market microstructure, we're back in an arena which is a little closer to the macroeconomic arena we talked about earlier, where prices don't yet reflect our information. There is predictability in prices, but it's predictability only over hours or minutes or even seconds, and the interesting question is how quickly we move to the new equilibrium.

You've worked on duration models lately, in particular models of durations between trades, estimated using transactions data. What is your view on the links between those duration models and various market microstructure models? In particular, what is the ability of those models

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to illuminate aspects of market microstructure that might be economically important?

Well, the data, of course, are irregularly spaced, and so the econometrician has to do something about that, and there are various solutions, but it seems to me the ideal solution is to use all the information and not to aggregate it out, which forces the econometrician to somehow estimate a model with irregularly spaced timing intervals. Now, this might be thought of as just a nuisance, and some models treat it as just a nuisance, but I think that one of the things that we see in the research is that in fact the information available in these durations, which is available not only to us but to market participants, tells something about the state of the market. And so the durations between trades actually inform people of what's happening. That is, if you picture the New York Stock Exchange and the people clustered all around the specialist, jumping, shouting, screaming, and raising their hands, what you're going to see on the tape is a lot of trades all clustered together, and the market behaves rather differently when the trades are close together than when they're spread out. And so that by looking at the timing of trades you learn something about the state of the market; when the trades are close together there's information flow, whether public or private, and a lot of agents are looking at each other trying to figure out what to do. And as soon as you see this kind of herding behavior the market behaves in sort of an illiquid way and volatility is high, bid ask spreads tend to be high, and I think the market tends to have high costs of doing business at those times, bad execution.

This is very reminiscent of Peter Clark's work, in terms of information flow, links to volatility, and so on.

That's right. Peter Clark certainly proposed this general class of models. He didn't really have a way of tying it to observables. It was more of a theoretical construct, but I think the asymmetric information models do tie up to trades in a very nice way. For example, in the Easley and O'Hara model, intervals between trades get very short with information flow, because any informed trader who gets the chance to trade will trade, whereas when there is no information flow informed traders find that it's not profitable to trade. So you've got times between trades changing endogenously based on optimization; this is in fact also a way that informed traders can be sort of single minded, because they're really trying to trade as fast as they can.

Continuing with the Clark theme but from a volatility as opposed to duration modeling perspective, once one allows for serially correlated information arrival, one arrives at the stochastic volatility model. What are your views on GARCH vs. stochastic volatility?

In a GARCH model, the variance is measurable with respect to observed information, whereas in a stochastic volatility model it's driven by a latent variable

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and immeasurable with respect to observed information. And somehow economists, and possibly statisticians as well, seem to feel that the unmeasurable ones are more natural and more structural, which is actually a feeling that I've never understood. Measurability with respect to a latent variable doesn't suggest that they're more natural, and doesn't suggest that they fit the data, and really doesn't suggest anything. The feeling seems to be that stochastic volatility models are more natural because they're discrete-time analogs of diffusion models, but the analogy is superficial and doesn't ensure that stochastic volatility models are in any sense "good."

What do you think about the emergence of financial econometrics. Has it emerged? If so why now and not thirty years ago, and where will we be ten years from now? And if it has emerged, why has it emerged? What is financial econometrics?

Well, I think financial econometrics has definitely emerged. It is a very rapidly growing area of econometrics, and I guess there are a couple of reasons for it. One is that financial theories are very precise and very much amenable to testing. Another reason is that the data are very high quality, especially compared to the data we are used to in macro, labor, and some of the other areas where there is a lot more concern about the data quality. And the third reason is that there are a lot of rewards to people who study it. There are lucrative job opportunities for people who decide not to be academics, so it makes it a good topic for dissertations by people who are not certain that they want to go into academics.

The question of why financial econometrics didn't emerge thirty years ago is really interesting. And I am not sure that it really didn't. But it didn't really attract the attention of econometricians so early. But there was a lot of work being done. A lot of it was done on the street. A lot of stock selection models were set up, and a lot of portfolio models were set up. They probably were not as sophisticated as today's models, but they served the function. I think from an academic point of view financial econometrics was viewed as trying to beat the market, which in fact was a task which blew immediately into the face of financial theory, and therefore it was kind of disreputable. By studying and turning our attention to risk and portfolios, derivatives and all these kinds of things, all of a sudden it is now consistent with theory as opposed to being in conflict with it.

What do you think of the journals and collections that have emerged, such as the *Journal of Empirical Finance*, the *Journal of Financial Econometrics*, the *Handbook of Financial Econometrics*, and so on?

I think there is room for these journals, and they clearly reflect the congealing and maturation of the field. I think there is financial econometrics which is more complicated or more abstract than what is a natural candidate for the main finance journals. On the other hand, I think a lot of financial econometrics ends

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up appearing in economics and econometrics journals rather than in the finance or financial econometrics journals.

What do you think about the communication in general between what you might call financial econometricians and the broader empirical finance community?

I think there is a big gap.

Why, and what can we do to narrow it?

Well, there is probably a gap because the culture is different. Empirical finance people typically come from finance departments, and econometricians typically come from economics departments, and each sees the other as relatively unsophisticated. Empirical finance people see the econometricians as tremendously unsophisticated people, because they don't know how the markets work and how the data is constructed and what are the important questions. I think cross-fertilization is tremendously valuable. That's why I had conferences in San Diego quite a few times where I tried to get half of the audience to be finance people and half of the audience to be econometricians, and to get them to talk to each other.

Three of the pillars of modern financial econometrics are asset pricing, portfolio allocation, and risk management. What do you think are the interesting questions for future research in those areas?

I guess I think of your three pillars as all being asset pricing, because asset prices are determined by some trade-off between risk and returns, no matter what kind of model you have in mind. I think asset pricing is an area that appeals to econometricians, because the data is very good and the theories make strong predictions. So I think that those are different ways of looking at the asset pricing problem, and I think they are all very interesting. I think that risk management has provided great impetus to financial econometrics, because it is a real problem that people try to solve every day, and I think its extensions to credit risk and liquidity risk are also very fruitful areas for financial econometric research which remain underdeveloped.

6. NEW YORK UNIVERSITY AND NEW YORK CITY

You recently moved to the Stern School of Business at New York University. How do you find the research environment in a business school as opposed to an economics department?

One of the fascinating things for me, now being in the Stern School at NYU, is to have all these finance colleagues. It really gives you a different perspective on the interesting questions and the quality of the data and what are the kinds of issues you have to develop your models for.

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What do you think about the environment for training graduate students, in general, in economics departments vs. business schools?

I don't know enough business schools to make a general statement, but it seems that most business schools are clearly much more focused on the MBA program than on the Ph.D. program, whereas the mission of economics departments that I know of is typically not at all focused on master's students but on Ph.D. students. So it seems to me that Ph.D. training happens very largely in economics departments as opposed to business schools. That being said, some top people do come from business schools.

There is a real issue that Ph.D. students have to face now, much more so than twenty years ago—Ph.D. students in econometrics and financial econometrics in particular—which is whether they want jobs in business schools or in economics departments. What do you see as the relevant aspects of the situation, and how should students decide?

This is very much related to what I was talking about before, which is the difficulty of bridging the gap between the finance community and the econometrics community. And I think that is an issue that graduate students are going to face. If they get their Ph.D. in econometrics, they might very well end up finding a job in a business school. But, it is not an easy step; some business schools are very reticent to hire non–business school trained Ph.D.s because of exactly this divide that we've been talking about. I find that many of my Ph.D. students now working in a business school initially got a job in an economics department and then after a couple of years moved to a business school. So they would do it sideways but not straight ahead. So when I talk to a Ph.D. student, I typically ask them, are they interested in a job in a business school or in an economics department? To some extent that shapes their dissertation topic. I don't think that is unreasonable, and I think there are many dissertation topics which would go both ways, although there are certainly quite a few topics that would only be interesting in a business school or would only be interesting in an economics department.

The statistics department at NYU is in the business school. What do you think is the role of a statistics department in a business school as opposed to a statistics department elsewhere in the university? For example, at both NYU and Penn the (only) statistics department is in the business school. Should statistics departments in such situations effectively be econometrics departments?

Stat departments have always been political, and hence the question is very complicated. Sometimes stat departments are in math departments, in which case they become very theoretical and not very useful. But as soon as they move into the subject areas, then they are in competition with the subject area. But certainly I think it makes sense for statisticians in business schools to be

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involved in financial econometrics. Then they can maintain their mission as stat departments and also do something useful for the business school.

Let's move from NYU to NYC. New York and London are the financial capitals of the world. Does that aspect of being in New York influence your research?

I typically do research on problems I think are interesting and where there are intellectual payoffs, but I am motivated in my payoffs by problems that I think people would really want to solve. Over the last five or so years I've done a lot of work in market microstructure, and I've never found anybody in the financial markets who is very interested in it. But in New York I've discovered that within the city there are a lot of people who are trying to solve exactly the problems that I think are interesting, whereas in other places people don't readily see that. They don't work on the time scale of market microstructure; they don't work on the actual volume scale of the market maker. And so really New York is unique in being able to provide a setting where some of my research actually finds its natural place. So, I am having a great time here, I must say.

What do you think of the interaction between consulting and research, and how does New York factor into that?

I think that research that has no application is sort of boring. And yet, if you do too much consulting with no research implications, that is boring too. So I think there is really a nice balance that you can, if you are lucky, maintain where your research informs your consulting and your consulting informs your research. And so I am always much more enthusiastic about consulting when publication of the results is a natural outcome. It is surprising how many consulting projects want exactly that. They want to bring in an academic, because they want to publicize what they've found and what they are interested in. That is my first choice in consulting. I think that actually enriches both academic and consulting arrangements.

We're drawing to a close. Are there any concluding remarks that you would like to add?

Just that I think it is an exciting time to be studying financial econometrics. One of the things in financial markets that makes it even more interesting to study econometrics is speed. Financial markets are getting more and more computer oriented; they are moving faster and faster, and it is getting to be impossible for an individual to keep up. An individual trader or market maker can't actually survey all the possibilities and make optimal decisions. That forces you to go to the computer and do statistical things. So, my feeling is that, in the future, financial econometrics is going to set up automated market makers and automated brokers and traders who in fact have strategies which are designed to accomplish well-defined objectives, in well-defined market environments.

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When you say automated market makers, this basically means electronic markets using algorithms to match buyers and sellers?

Right, but if you place your order with an electronic broker, he needs to be able to survey markets all over the world, some of which are open, some of which are closed. And he needs to be able to assess different ways of purchasing a particular commodity, for example, as an ADR or as index futures. And an individual broker doesn't really have access to that. The screen simply can't convey all that information. So I think there is scope for statistical optimizing, which will work pretty well. It's really not a question of designing a better chess player; rather, it's a question of recognizing what the uncertainties are and making the best decisions given the uncertainties.

7. THE FUTURE OF ECONOMETRICS

What's your view on the interaction between theory and data in the advancement of science?

I think it's the best part of our profession. The idea that you can build models from theory and that you can build models from data and work to make them mesh is really what every econometrician's supposed to do, and I think it's what our profession's about, and I think it's too bad if you take either of those ingredients out of it. Many people would think that my work was either not grounded enough in theory or maybe not grounded enough in data, but I really think that it's that subtle balance between the two that makes the important contributions.

I agree. But it often seems to me that econometricians routinely view paying attention to theory as part of what they're supposed to do, and for good reason, but that very few theorists view paying attention to data as part of what they're supposed to do. What do you think of that assertion?

I've been asked by theorists, "What's the empirical evidence on this question or that question?" But I must admit, not very often. I think that the best theory must have data and stylized facts to support its importance just like the best econometric work has got to have theory to support it. I am skeptical of empirical work that spends a lot of time deriving the model from theory and then doesn't test to see whether it actually fits the data. I think that you've only started the research at that point. Almost every time I've done an empirical project I've found that the data had something surprising in it, and I think that the best work is work that looks at that surprising information, figures out whether to take it seriously, and then alters the theory in some way to be consistent with the empirical results. A model purely born of theory doesn't have that strength.

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Rob's favorite Australian café, Sydney, 2001.

San Diego is obviously a key current center of econometrics research. What other pockets of intellectual activity intrigue you, and will they be stable in the long run?

Berkeley and Yale are very strong too. We were very lucky actually at San Diego because we had a lot of stability, really excellent faculty, excellent graduate students, and tremendous support by the administration in bringing in good graduate students who want to do econometrics and supporting the econometrics program and not thinking of it as being something that's too big or too powerful or something like that. I think the same thing has happened at Yale. I don't want to compare it with San Diego, but I think Yale has the most stimulating time-series program in the country. There are a lot of other places with good groups doing cross-sectional modeling and nonparametrics, and that whole class of tools used in labor and industrial organization. I think the Cambridge area with Harvard/MIT really is excellent in that area of econometrics.

And what of the future of econometrics itself? The time-series half of the *Handbook of Econometrics* that you and Dan McFadden edited was a great distillation of the 1980s and 1990s. But where will we be ten years from now?

I think that an awful lot of the econometrics we're working on these days and in the future will concern nonlinearity of one sort or another. But I think that the class of nonlinear models is so general that general treatments are boring; instead I prefer nonlinear models tailored to particular situations. And I think the most common types, the most successful types, of nonlinearities are when the nonlinearity is actually associated with the dependent variable. One inter-

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pretation of the ARCH model is that it is associated with the dependent variable, as a model for squared returns. Other important nonlinear models like that are logit, probit, and related models, which are clearly nonlinear because the dependent variable is discrete or censored. Duration are also naturally modeled nonlinearly, so I think that there will be a growing collection of nonlinear models, but I won't be too surprised if we find that they're more focused on different types of data and analysis. And I would also think that there are lots of interesting generalizations of Markov-switching models that could be usefully entertained.

What about the future of financial econometrics vs. macroeconometrics? A colleague joked to me the other day that time-series econometricians have won finance but lost macro. Do you agree?

I think that the decline of empirical macro is temporary. I mean, I think there's no substitute for empirical macroeconomics, and maybe the models in the past were too simple, but I just can't believe that you can have a viable macroeconomic profession without serious empirical time-series econometric analysis, so I think that empirical macro will be back.

Thank you, Rob.

Thank you, Frank.

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