

An Interview with Avinash Dixit

Forthcoming in the Royal Economic Society Newsletter

Andrew Oswald interviewing Avinash Dixit of Princeton University in 2007 on the event of his receiving an honorary doctorate from Warwick University

I = Interviewer (Andrew Oswald)

R = Respondent (Avinash Dixit)

I So why are you an economist, Avinash?

R Largely by a series of accidents -- starting out as a mathematics undergraduate who liked maths but not pure maths all that much, nor physics applications, I began hunting round for other kinds of applied maths and discovered economics. Very fortunately, my tutor at Cambridge put me onto Samuelson's Foundations and gradually I got interested.

I You have worked across an unusually large range of areas. Is one article or book a particular favourite?

R That's almost like children, right, asking which of your children you like best (Oswald laughs). Really I should say it's the one I am working on right now, and somehow funnily enough that's almost always the case, whatever one is working on. But the work on irreversible investment and uncertainty was particular fun to do, and ended up being a big problem and also a nice application of techniques.

I How did you get into that area?

R While reading what people like Paul Krugman and others were doing, and thinking that there had to be a rigorous mathematical way to do this, and looking round to find the right techniques. And it just all came together -- again a series of accidents.

I That's a couple of times that you have mentioned techniques or methods, perhaps because in a sense that's how you came to go into economics. Have you sometimes in a problem been driven by, let's say, a down-to-earth idea, rather than a method?

R Yes. However, it's when the two come together that it becomes most interesting for me. A lot of people make a distinction between maths and intuition, but for me really the two should come together. I think this is when one gets the best understanding.

- I Some people, Avinash, would not entirely agree. Plenty of social scientists would say -- including me, I guess, though perhaps it is the Methodist-preacher blood coming from my grandfathers -- do their work because in some small way they hope to make the world a better place. They might not use those exact words, but that's a way to paraphrase it. A third group of scholars and scientists are interested in unlocking complicated puzzles per se; they are problem-solvers. A fourth group want to shape actions in society and so on. Do any of those ring bells with you?
- R Yes. It's of course valuable, especially for economists, to do work that will benefit society. But it's also valuable to do simply what is research of good scholarly quality and sometimes there is a kind of trade-off between the two. Should one deliberately start by saying, hey, I am going to work on something of social value? It just might not be someone's comparative advantage to do such work. My belief is that there are lots of economists around, and others might be better at doing that. Overall, the subject as a whole, and even socially valuable work as a whole, will progress best if people follow their comparative advantage. I had always believed this but thought I should keep it to myself because most people regard it as a "politically incorrect" attitude. But I decided that I should go public with this view when I found a beautiful and cogent argument in its defence, with great examples, in *Surely You're Joking, Mr. Feynman*. If it works for Richard Feynman, it can't be a bad way to do research (even if my achievements fall way short of Feynman's).
- I You must see many young economists as well as older ones. Perhaps they ask you for advice. What would have been useful for you to have been told by the older Dixit whispering into the ear of the younger Dixit (if such a thing were possible with some kind of time machine)? For what it is worth, my own bit of advice to incoming Warwick PhD students across all disciplines -- I have to give a speech to them during part of a general training day at the start of each new academic year -- is: if everyone likes your work then you can be sure you haven't done anything important (simply because to move a discipline forward one has to go against established ideas, and to some degree against those who have made the ideas famous, and it is only human that, if only subconsciously, those individuals aren't keen on that).
- R Do whatever you find most fun to do and whatever you are best at doing. I don't see much point in torturing oneself, hating every moment of the work, just because one feels it's got some social value somewhere out there. That's usually a guarantee, I think, for doing mediocre work.
- I So you would actually emphasise to young researchers they should enjoy their research lives first and let the social purpose or the policy ramifications come second?
- R Along with even more selfish goals, like is this going to be the best way to get tenure. All the other valuable things will be by-products.

- I If you look back on the way economics was done, and the substance of economics, when you were a younger man, and compare it to now, how do you see what's happened in economics, and perhaps in social science?
- R In many ways, economics is in good shape, especially because of the things like game theory and information economics on the theoretical side and a whole lot of econometric and computational methods on the empirical side are some things that one would never have thought possible when I started out. And they have really changed our thinking. Just to give one example, think of Michael Spence's idea of market signalling. We now see signals all around us, and when I was a graduate student the concept simply did not exist. That has been one of the most important things to come along in the last 40 years.
- I Are there any other one or two things that really come to your mind as having surprised and shaped your subsequent thinking?
- R I think the incremental work of improving theory has been going in the right direction. But let mention a negative thing which I am powerless to do anything about. A big difference that has taken place in the last 40 years is that when I started the typical working paper, double-spaced, was 20 to 25 pages. But now they are 60 to 70 pages, and as I get older and my eye sight deteriorates especially, I find this a terrible thing. I wish people would put their ideas in a punchier, simpler way.
- I Let me take you back to, you mention, the Spence model, the work of Michael Spence. If you had to think of a small number of other big intellectual influences on you, or things that you think have really changed our way of thinking, what might come to mind?
- R For me, something that was not actually taught in graduate school, but was coming along at the same time, was Tom Schelling's work on strategic moves – commitments, threats and promises – and their credibility. That to me made game theory come alive in a way that even beautiful proofs of the minimax theorem or Nash's equilibrium theorem never really could. And I learned it by reading Schelling's books, not in any courses.
- I It might be interesting if you could look to the future. Obviously it is very hard to know, but do you have any forecasts for us or a sense of where we might be 40 or 50 years from now?
- R I think trying to speculate in that way is usually a mistake. But the first thing that comes to my mind as a good thing to happen will be, so to speak, delineating the frontier between behavioural economics and conventional neoclassical economics. Behavioural economists, particularly in lab experiments, have found departures from many orthodox economic behavioural assumptions. And others, like John List in particular, have found in field experiments that among experienced traders in markets sometimes orthodox economic theory applies quite well. So a combination of experiments and theorising, one hopes, will sort that out over the coming years.

- I So, let's see if I understand, are you implying that field experiments, actual experiments in the world, will be very important to economists in the future?
- R My belief is that both field and lab experiments have some substance in them and which one is closer to being right depends on circumstances, what kind of context one is looking at. So a more systematic understanding of what's right, or whether some mixture is right in variety of particular context or applications, I think is an important thing that should be on research agenda.
- I Finally, I think we could say that there has been a kind of broadening of economics towards other social sciences. The edges now appear to me, anyway, to be rather blurred. Do you have a view about whether you think that's a good and natural thing, and where we might end up in the future?
- R Absolutely: I think that's an excellent development. What is particularly good is that, unlike in days where at least some economists set out on a kind of conquest of the rest of social sciences, these days there seems to be a genuine merger rather than a takeover. Economists have much to learn from case studies of historians, sociologists, political scientists, lawyers, and even anthropologists, and those scholars can learn methods of theoretical and empirical economic analysis to benefit those other social sciences. It may be possible, although that's perhaps an unlikely and very distant goal, that social sciences that split into a half-dozen disciplines a 100 years ago might even, at least to some extent, eventually come back together.
- I That is an interesting idea. Thank you very much, Avinash. Is there anything else you would like to add? Perhaps we will persuade the Royal Economic Society to print it up as a kind of interview in some form or other. Anything else you would like to say, for the record?
- R I don't know if either they or you will want it printed, but here is my reaction as I read, say, page 50 of the 70 page paper. I am reminded of the character Elaine in the show *Seinfeld*. (I: I am not a big TV person, I confess). She is watching the movie of *The English Patient*. Finally she bursts out "quit telling your stupid story about your stupid desert and just die". That's what I think when I am on page 50 of a 70 page paper. Even a really good one!

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