

The Art and Practice of Economics Research: Lessons from Leading Minds
(Edward Elgar Publishing, 2012)

Simon W. Bowmaker, New York University

Interview with Daron Acemođlu

Massachusetts Institute of Technology (July 14, 2010)

Daron Acemoğlu was born in Istanbul, Turkey in 1967 and graduated with a BA in economics from the University of York, England in 1989 before obtaining both an MSc in Mathematical Economics and Econometrics and a PhD in economics from the London School of Economics in 1990 and 1992 respectively. Between 1992 and 1993, he was a Lecturer in Economics at the London School of Economics and then moved to the Massachusetts Institute of Technology, where he currently serves as the Charles P. Kindleberger Professor of Applied Economics.

Professor Acemoğlu's research interests include political economy, economic development, economic growth, economic theory, technology, income and wage inequality, human capital and training, labor economics, and network economics. His most-cited articles include, 'Was Prometheus unbound by chance? Risk, diversification, and growth,' *Journal of Political Economy* (1997), co-authored with Fabrizio Zilibotti, 'Why do Firms Train? Theory and Evidence?' *Quarterly Journal of Economics* (1997), co-authored with Jörn-Steffen Pischke, 'Why do New Technologies Complement Skills? Directed Technical Change and Wage Inequality,' *Quarterly Journal of Economics* (1998), and 'The Colonial Origins of Comparative Development: An Empirical Investigation,' *American Economic Review* (2001), co-authored with Simon Johnson and James Robinson, and 'Reversal of Fortune: Geography and Institutions in the Making of the Modern World Income Distribution,' *Quarterly Journal of Economics* (2002), co-authored with Simon Johnson and James Robinson. His books include, *Economic Origins of Dictatorship and Democracy*, co-authored with James Robinson (Cambridge University Press, 2006), and *Introduction to Modern Economic Growth* (Princeton University Press, 2009).

Professor Acemoğlu's academic awards include the 1996 *Economic Journal's* best paper award for, 'Consumer Confidence and Rational Expectations: Are Agents' Beliefs Consistent with the Theory?', co-authored with Andrew Scott, the inaugural T. W. Shultz Prize from the University of Chicago in 2004, the inaugural Sherwin Rosen Award for outstanding contribution to labor economics in 2004 from the Society of Labor Economists, the Distinguished Science Award from the Turkish Sciences Association in 2006, and the John von Neumann Award from Rajk College, Budapest, in 2007. He was also awarded the John Bates Clark Medal in 2005, given every two years to the best economist in the United States under the age of 40 by the American Economic Association. In 2006, he became an elected fellow of both the American Academy of Arts and Sciences and the Econometric Society.

I interviewed Daron Acemoğlu in his office in the Department of Economics at the Massachusetts Institute of Technology. It was late afternoon of Wednesday, July 14, 2010.

BACKGROUND INFORMATION

Bowmaker What was your attraction to economics?

Acemoğlu: In high school, I was very interested in social issues like inequality, and differences between nations, and I got the impression from reading books that economics was all about answering those kinds of big questions. And so I applied to the University of York in England to study economics and political science. Once I was there, I quickly realized that I did not like political science and that economics was not exactly what I thought it was. But I was sufficiently intrigued by economics and continued with it.

Bowmaker: As a student, which professors were most inspirational or influential and why?

Acemoğlu: I learned a lot from all of my professors at York. I was particularly fortunate as a second-year undergraduate to work with Peter Lambert, who taught me how to work on a research

paper. And then at the London School of Economics as a graduate student, I was lucky to work with great researchers like Kevin Roberts, John Moore, and Charlie Bean.

Bowmaker: Why did you decide to pursue an academic career?

Acemoğlu: In high school, I was thinking about an academic career because I wanted to dig deeper into those topics that I mentioned earlier. I did waiver at points about whether it was the right thing to do. In England, I got the impression that being an academic also meant being poor, which was true in 1989 when I graduated from York. But I really liked the intellectual investigation and freedom that being an academic provides.

Bowmaker: Has your training from England influenced the way that you conduct research?

Acemoğlu: Yes. In England, the tradition is very non-technical, so I learned a lot of intuitive economic thinking. That had a big influence on me. Also, apart from just a few complementary disciplines, you don't study anything other than economics during the three-year condensed program. I think that's a bit of a shortcoming, and I had to make up for it by studying mathematics and history by myself.

Bowmaker: As a researcher, which colleagues have been most influential and inspirational?

Acemoğlu: Early on in my career at MIT, I didn't work that much with senior colleagues. I felt more comfortable working with people who were my age, like Fabrizio Zilibotti, Steve Pischke, and Rob Shimer, who was a graduate student here.

MIT is a very friendly, kind, and supportive environment. Olivier Blanchard and Bengt Holmstrom, in particular, were great senior colleagues in terms of giving advice. Although neither do work that is very closely related to my research, it was good to get some perspective about how the profession works.

GENERAL THOUGHTS ON RESEARCH

Bowmaker: There is an increasing emphasis in many economics departments on applied research. Is this true at MIT?

Acemoğlu: MIT has always been at the forefront of applied research, but theory does play some role. My view is that economics is an applied discipline and so the trend that you mention is a very good one. But sometimes when you have such movements, it takes a while for you to find your footing and, right now, we're going through a phase in economics where the exact mixture of empirical and theoretical work is very much in the air. I think there needs to be more synthesis about how to do that best.

Bowmaker: What do you see as the value of "pure" versus "applied" research in economics?

Acemoğlu: Pure research is probably a bigger chunk of research in economics than in many other scientific disciplines because empirical work is hard and multifaceted. But it enables us to have a better conceptual and methodological approach to applied questions that people care about.

Bowmaker: How would you describe the dialogue between theory and empirics in economics?

Acemoğlu: As I've just said, economics is an applied discipline. But the set of questions we're asking are too complex unless we look at them through the lens of a well-defined, narrow, conceptual framework. The most important dialogue between theory and empirics is that theory provides the conceptual framework for us to pose well-defined empirical questions and enables us to interpret the answers. Empirics, on the other hand, does (or should) continuously challenge theory and make us reevaluate whether we have the right ingredients.

Another equally important role of theory is to enable us to go from specific empirical findings towards generalizations, that is, the so-called "external validity" of empirical findings. In the context of complex human interactions, changes in the environment or interventions often lead to reactions and counter reactions. In many economic theories, we can capture those as the equilibrium, or general equilibrium, effects. And so we need to rely on theory to guide us to the important equilibrium effects and then provide us with insights on how to understand and evaluate them. Of course, ultimately, we must use empirical methods to gauge the importance of these equilibrium channels as well.

I realize this is the ideal, and it doesn't always work exactly as I've just described in practice. There is less dialogue between theory and empirics in economics today than I would like to see, and this is both because economics is still a developing, young science and because we tend to be too narrowly specialized.

Bowmaker: How would you characterize your own research agenda, and how has it changed through time?

Acemoğlu: I started out as what people might call an applied theorist; always beginning from a specific question, but trying to provide theoretical answers. And as I went forth in my career, I became more of an empiricist. Right now, my work is half-theoretical, half-empirical. I haven't been much into doing both theory and empirics in one paper because it's not always easy to communicate. But I think ultimately that's what I'd like to do; combine both organically in a single body of work.

Bowmaker: Do you think it is important to have broad research interests?

Acemoğlu: The answer is both "yes" and "no." The answer is "no" if you look at it from the very selfish point of view of how you become successful in the profession. The profession values narrowness for a good reason and a bad reason. The good reason is that narrowness is a way of becoming an expert in a particular area. The bad reason is that, as in every other human activity, being part of a group or camp is valued, and you can achieve that by being narrowly focused.

My own view is that depth is very important. If breadth comes at the expense of not understanding a particular area, or even the background of a particular question, then that's a bad thing. On the other hand, breadth has a very important, often underplayed positive: a lot of ideas come from analogies or by combining them from different realms. And so breadth brings a fresh perspective to certain problems. Also, I work on a broad set of topics because that's what I enjoy; it's a consumption good.

Bowmaker: Do you think there is any difference in the types of work done by researchers at different stages of their careers based on tenure concerns, publication requirements, or other pressures? Should there be a difference?

Acemoğlu: I'll answer the first question! [*Laughs.*] Before I had tenure, I was told that I was pursuing a strategy that would destroy me; I was working on too many topics and I should narrow it down. That's the advice that many people get. Without tenure concerns, I think there would be more diversity in what an individual does.

Bowmaker: In the end, do you think the profession has helped to bring out and shape your research for the best?

Acemoğlu: I think so. It's a great profession for pursuing different things. It's relatively open, combines mathematical and theoretical ideas with data, and opens up lots of frontiers. It's also been blessed by having some amazing leaders in many different generations that have really helped it go forward. I have benefited tremendously from being in this area, and I can imagine that, if I had ended up choosing something else, I would have been much more miserable.

IDEA GENERATION

Bowmaker: Where do you get your research ideas?

Acemoğlu: There isn't a fixed formula. Sometimes you read an article in the newspaper that gives you an idea about topic X, you start thinking about topic X, and then that suddenly gives you an idea about topic Y. I generate a lot of ideas by thinking in terms of analogies. And, of course, there is also the natural progression of science; ideas are very often based on those of other researchers, and you push them further.

Bowmaker: At what point does an idea become a project that you devote resources to?

Acemoğlu: Good question. Most of the time, I use natural selection. When I get an idea, I'm normally very busy, and so if it's a good idea, it will recur to me, and if it's not a good idea, it will die down. Sometimes an idea comes to mind that is so relevant that I can create the time to work on it, but usually it takes several years for me to go from the idea stage to the project stage. In many cases, I don't even know where it will lead. Any initial probing that I do, such as background reading, is to see whether the idea has any useful direction. And so many of the ideas that don't turn into projects are those that I don't feel will mature into something that's concrete; they are a little loose.

IDEA EXECUTION

Bowmaker: What makes a good theoretical paper?

Acemoğlu: I think there are two kinds of good theoretical papers, and the profession values one kind versus another a little more but, in my opinion, they're both extremely useful. One kind looks at the existing problem and resolves open issues or takes it to another level in terms of depth of analysis. The obvious example would be Arrow-Debreu competitive equilibrium. Going back to Leon Walras and Adam Smith, people had some intuition about why it had to exist, and that it should have some optimal properties, but Kenneth Arrow and Gerard Debreu nailed it.¹ First of all,

¹ K.J. Arrow and G. Debreu (1954), 'Existence of an equilibrium for a competitive economy,' *Econometrica*, Vol. 22, No.3, pp. 265-290.

they closed some open questions like the existence of equilibrium using a theorem, but in doing that, they also took it to a higher and deeper level of analysis by defining what the commodities are, and what the structure of equilibrium has to be. I'm just picking that example because it's one that will be familiar to many people, but it's an extremely valuable contribution.

Another kind of theoretical contribution poses a new question, brings some formalism to that question, and broadens our horizon. That could be because, in posing the question, it generates some surprising answers that one would not have expected or, just by formalizing the question and what it answers, it might change our perspective on the issue. Sometimes people make the comment that some of the mathematics in economics is superfluous because you can say certain things in words, not in math. There is some truth to that, but I think by saying them in math, you can often make those things more explicit, and that enables people to build on them.

Bowmaker: What makes a good empirical paper?

Acemoğlu: Again, I would say there are two kinds of good empirical papers. One I would call an answer paper, and the other a challenge paper. The answer paper is the more straightforward one. It will answer a well-posed empirical question: What is the elasticity of labor supply? What is the value of one more year of schooling to an individual or to society? What is the effect of subsidies on school building? But the question can be asked at different levels of depth, such as at a purely descriptive level, which wouldn't be my favorite, or you can add some theoretical structure so that the empirical exercise gives generalizable answers or external validity.

There are also challenge papers that reject a well-known theory or put a puzzle on the table. Those kinds of empirical papers are very useful because they encourage people to dig deeper into a field. For example, there is a fairly simple theory of the equity-premium, and [Rajnish] Mehra and [Edward] Prescott have written an empirical paper, which is essentially descriptive, showing that the model doesn't fit the data.² Perhaps, it's a little "inside baseball," this type of research often has a bit of that feel. On the other hand, it has led to a very large literature, and we have learned something from that process of probing deeper.

Bowmaker: When you hit a "brick wall" on a project, do you continue to work on the problem, or do you take a break and work on something else?

Acemoğlu: I do one of three things. Sometimes you keep on pushing a bit more because, if you give up too early, you'll never tackle difficult enough questions. More often, I take a break and come back to it six months later or two weeks later. But the third thing is that you might hit a brick wall because the problem is not well posed, and so it's useful to think about how you would change the questions. One example would be the early work that I was doing on human capital in labor markets; training and technology adoption and their interactions, and how this might lead to human capital externalities and so to underinvestment. I came to those problems thinking about unemployment; I had a hunch that a firm's choice of technology and training of workers had a lot to do with unemployment. I still believe that's true, but once I started working on those topics, I realized that, firstly, unemployment was not so interesting from a theoretical point of view, and secondly, the papers that I was writing on the topic were just paying lip service to unemployment.

² Mehra, R. and E.C. Prescott (1985), 'The Equity Premium: A Puzzle,' *Journal of Monetary Economics*, Vol. 15, pp. 145-161.

Bowmaker: Related to the previous question, when a project isn't going to turn out as hoped, do you scrap it or aim to send the work to a lower-tier or field journal?

Acemoglu: I scrap a project for every one that I complete. It's very time-consuming to finish a project.

Bowmaker: What would you say has been the biggest change, in the course of your career, in how your research fields conduct research?

Acemoglu: I developed the courage to work on the things that originally attracted me to economics, and I was fortunate that the profession was already undergoing changes in that direction. When I was in the PhD program at the LSE, there was a field called political economy, although it was only dealing with a subset of issues. But then I met Jim Robinson, who was like-minded, and we started investing more in the field, and I think it has been fairly well received. That's been the biggest change in my own career trajectory.

I would also say that the profession is much more open to answering other kinds of big questions. Of course, it's not an isolated event. The Gary Becker revolution, which showed that economics could ask social questions that were outside the subject's narrow realm, was very important, and the major work that people did in political economy before then was obviously quite a big precursor.

Other economists might say, "Oh, the biggest change is that we now have much better empirical tools." I think that's important, but it's not a qualitative change. People had empirical tools before, and were aware of their pros and cons. And so while it's great that we now have faster computers and can run bigger regressions, the bigger change in my view is the conceptual structure that we bring to a problem.

THE WRITING PROCESS

Bowmaker: Which aspect of the writing process do you find most difficult?

Acemoglu: Writing in general is difficult, and in economics it takes a lot of learning. You have to be clear so that you don't force the reader to read the text several times. And you have to be economical, despite the fact that papers in our profession are overly long. It's not an easy process, and I found it very painful at first.

Bowmaker: What steps have you taken during your career to improve the quality of your writing?

Acemoglu: I just worked on it over and over again; that's the only way you can do it. I think it also helps having read a lot. I find generally that two kinds of students have great difficulty in writing well. There are those who haven't read much in literature or in history; their ability to process long prose has not been developed because they haven't invested enough. And there are those who haven't written enough mathematics. You can immediately see that there's a mathematical immaturity in some writings and a mathematical maturity in some others, which, again, is an entirely acquired trait.

Bowmaker: Who proofreads your writing?

Acemoğlu: Sometimes I'll ask my assistant or my co-authors, including graduate students, to proofread it for grammar but, most of the time, nobody does it. I think it might help if I had more proofreading.

Bowmaker: How do you split up the writing tasks among co-authors?

Acemoğlu: That is very specific to the project and to the co-author. Some co-authors may not be very experienced at writing, so I may end up doing quite a bit of it.

COLLABORATION

Bowmaker: When you work with co-authors, how do you decide with whom to work?

Acemoğlu: Early in my career, it was people from the same PhD program, but once I started going to conferences and meeting others with a lot of common points, that also developed into co-authorships. Then, as your time becomes increasingly scarce, it becomes a good option to co-author with students and split the work, otherwise you end up doing everything and it just becomes infeasible at some point. Of course, I'm blessed that MIT students are fantastic.

Bowmaker: When you do work with co-authors from outside your university, how do you prefer to interact with them (e-mail, phone, or face-to-face meetings)?

Acemoğlu: Certain things are much easier face-to-face, but I'm not a big fan of writing papers that way. Being some distance apart is certainly not a big constraint in my view.

RESEARCH ASSISTANCE AND FUNDING

Bowmaker: How do you use undergraduate and graduate research assistants?

Acemoğlu: I used undergraduate RAs early in my career, but I decided that wasn't for me. You need to be a very organized person to have undergraduate RAs because they make a lot of mistakes. And they don't get things unless you explain it in a very, very clear manner, and I'm just not very good at that.

I use graduate RAs for two purposes. One is that when I have a paper that has mathematical derivations, I hire a graduate RA to check over it. The other purpose is for data work. Because I started as a theorist, I never became extremely quick at manipulating datasets. And so I have always relied on graduate RAs to put the data together. Often if the graduate student is heavily involved in the project, I will make them a co-author.

Bowmaker: How important is funding for getting your work done?

Acemoğlu: If I didn't have any funding, I wouldn't have any RAs, and that would be a problem. But it's not a do-or-die thing. It's not like in engineering or physics where you need a big lab and a hierarchy of post docs, PhD students, and undergraduates, although some people in economics have turned it into that.

Bowmaker: Do you have any advice for a young scholar on the funding process?

Acemoğlu: Not really. The NSF [National Science Foundation] is organized a bit like a journal; you submit and you get rejected [*laughs*]. But over the last decade, I've seen lots of alternatives for funding become available. I haven't made much use of them, but a lot of my colleagues have taken funding from NIH [National Institutes of Health] or from private sources and foundations. Being entrepreneurial helps. But I think too much effort is spent on funding, which is perhaps an inevitable consequence of empirical research becoming more important.

SEMINAR PARTICIPATION AND NETWORKING

Bowmaker: What are the benefits to attending a seminar that is closely related to your work versus one that is not closely related?

Acemoğlu: I like the ones that are not so closely related to my work; I find that I learn more from those. But I view all seminars as a learning experience, and I try to get as much out of them as I can.

Bowmaker: How important is professional networking to success in research?

Acemoğlu: Unfortunately, it's very important and an inevitable aspect of human relations. It's worse in some fields because you have to network with funding bodies and so on. In economics, it's about being part of a club, and sometimes that matters in our profession.

Bowmaker: How does the researcher without extensive networks succeed?

Acemoğlu: It's very difficult. There are some very talented people in the UK and other parts of Europe, but they follow leading research indirectly by reading journals. You don't always get the full picture when you just look at an article, and you don't become known by doing that. And people who are part of a network tend to get better treatment in journals.

Bowmaker: To what extent is the absence of departmental colleagues working in one's research area a major disadvantage?

Acemoğlu: It depends on how narrow or broad is your area. If you're a theorist, and there's nobody else in the department who's doing any theory, then that would be a major disadvantage; you wouldn't get the right conversations and you wouldn't be exposed to the right ideas. But if you're a decision theorist in a department where there are game theorists and contract theorists, I don't think it would be a big deal. In fact, it's good to talk to people who have different perspectives.

COMMUNICATION OF RESEARCH

Bowmaker: How do you find the right balance between communicating your research at an early stage versus the close-to-finished stage?

Acemoğlu: In economics, the issue is a little different from medicine, where being first is very important. Unfortunately, journals take a long time to publish articles in our field, and so you have no choice but to post them on your website. Then the question is when to do this. I think first impressions matter. If you do it too early, people will read your paper when it's not polished in a

way that tells the story appropriately, and that might backfire. And so I don't post my papers until I'm fairly comfortable with them.

Bowmaker: What are the unique challenges to giving a seminar and how do you overcome them?

Acemoğlu: The main reason why you give a seminar in economics is to advertise the work. Originally, I thought the point was to get comments, but I now believe it's the networking; people get to see the work, they get to understand it, and you get to clarify what they don't understand. And once the work becomes known in seminars, it gets easier reception in journals, and people are more likely to adopt it for teaching. You can write great papers and post them on your website or submit them to journals, but that won't have the same impact as a seminar.

PUBLICATION

Bowmaker: How do you decide upon the appropriate journal to send your research to? Relatedly, whom do you view as the readership of your research?

Acemoğlu: Most of my research papers are directed at other academic economists. Sometimes I write expository things like entries into the *Handbook of Economic Growth* or *Handbook of Labor Economics*, which are also read by graduate students. And I have written things for the broader public, such as undergraduates and even non-economists.

The incentive structure in the profession is such that highly coveted publications are very valuable— that's why they're highly coveted [*laughs*]. And so everybody has an incentive to send their articles to *Econometrica*, the *AER* and the *QJE*. When I feel that my papers have a shot, that's what I try to do.

Bowmaker: How would you best describe your approach to dealing with a "revise and resubmit" request from a journal? How about an outright rejection?

Acemoğlu: Rejections happen all the time and you get very upset at the referees who didn't understand your paper, and at the editor who didn't treat you very well. A "revise and resubmit" is a good outcome; no paper ever gets in straightaway. And so I believe in trying as hard as possible to be responsive and clear in the revision to satisfy the referees. That's the respectful thing to do when people have actually taken time to read your paper, and also because sometimes they might have a point that you didn't see. My papers tend to improve after refereeing.

Bowmaker: Do you think that the current structure of the publication process in economics facilitates or impedes scientific understanding and knowledge production?

Acemoğlu: I think it's important that there is a hierarchy of journals because nobody has the time to read even one thousandth of the papers that are being produced. But there are two major problems with the process. One is that the economics profession has a terrible record in terms of speed in dealing with papers; it's despicably bad. There's no justification whatsoever for a journal to take one year on a paper. It's totally irresponsible of the editors. I'm an editor myself [of *Econometrica*], and I know that it's not that hard to make sure that you respond to things on time. It's awful behavior to be playing with people's careers, particularly junior authors. I just cannot understand it. As I said earlier, the profession does well on many, many other dimensions, but this aspect of it just infuriates me.

The other thing is that refereeing sometimes becomes very strategic. I see this as both an author and as an editor. People are very open to work that is a minor improvement on a particular research line that they are pushing, and they are very closed to anything that's outside that line. Again, bad editorship comes in there; you can condone it implicitly by not doing anything about it. I think it makes it really hard for new ideas to break into good journals, and can result in articles in those journals being full of meaningless extensions of past work.

Bowmaker: What have been your best and worst experiences during the publication process?

Acemoğlu: I have had many experiences that were awful. As a junior author, I had a paper at the *Review of Economic Studies* that went to four rounds of revision in almost four years and then a new editor came in and rejected it right away. For somebody at the beginning of his career, I felt like everything was going to collapse.

My best experience was having my first paper accepted in one of the top journals. That was more than three years after finishing my PhD—quite a while—and so when the *QJE* published my paper on human capital externalities, I felt like I'd finally broken the barrier.³

REFEREEING AND EDITING

Bowmaker: What would you say are the benefits to refereeing?

Acemoğlu: None. I enjoy reading papers, but being a referee requires you to read very carefully a paper that you may or may not be interested in. Refereeing is public service; the profession could not function if nobody did it.

Bowmaker: Why did you decide to become an editor of *Econometrica*?

Acemoğlu: Again, I view it as public service. I think the profession suffers in the hands of bad editors and, if I express that opinion, I feel that it's incumbent on me to do something about it when offered the opportunity.

Bowmaker: What would you say are the benefits to editing?

Acemoğlu: I don't see any benefit whatsoever; people hate you because you reject their papers.

TIME MANAGEMENT

Bowmaker: How do you divide up your working day, both in terms of quantity and timing of different kinds of work?

Acemoğlu: I feel like I have a fireman's approach—there are fires to be put out—but I've lost control. Most of the time during the day, I'm unable to do anything other than answer e-mail. And so I often have to stay up at night in order to get research done. I don't have good time management.

³ Acemoğlu, D. (1996), 'A Microfoundation for Social Increasing Returns in Human Capital Accumulation,' *Quarterly Journal of Economics*, Vol. 111, No. 3, pp. 779-804.

Bowmaker: How do you balance multiple research projects?

Acemoğlu: I'm pretty good at that actually. I form a hierarchy in my mind as to which project is more urgent, and I don't have difficulty in switching from one to the other.

Bowmaker: What is the optimal number of projects you could be working on at any one time?

Acemoğlu: Whatever it is, I know that I'm beyond it. It would definitely be more than one because you get bored with one; it becomes too engrossing and all consuming.

Bowmaker: How many projects are you working on at the moment?

Acemoğlu: Probably about 20.

Bowmaker: How do you balance your personal and professional lives?

Acemoğlu: What personal life? My wife is also an academic, so we're both used to our personal lives being encroached upon by obligations. Things often fall out of balance to the point where my personal life becomes a residual claimant. And now I have a two-month-old son, so sleep has become a residual claimant.

REFLECTIONS AND THE FUTURE OF ECONOMICS

Bowmaker: What have been the most important findings and contributions in your research fields during the course of your career?

Acemoğlu: Going back a little before my career started, I think the most important innovations were made in the 1980s. The incomplete information game theory revolution deeply changed economics. Many areas have been swept by that; for example, we have a greater understanding of organizations and contracts. And then came the work on endogenous technological change. That's still a very, very fruitful area for further research and many questions remain unanswered, but I think we now have a framework that goes beyond what the non-economists bring to the table. I would also say that the empirical literature was absolutely transformed by more use of micro data and greater attention to the issues of causality and interpretation of estimates.

The one that I would pick after my career started is the political economy perspective where, instead of policy being treated as exogenous or conducted by benevolent governments, we think of where policy comes from, where the institutions that lead to policies come from, how we can link them to their historical roots, and how they are related to long-run economic growth. That paradigm shift has been quite important in my view.

Bowmaker: What are the main challenges facing your research fields?

Acemoğlu: I think we have a relatively poor understanding of how credit markets work, as the recent credit crisis has demonstrated. And I also think the general area of short-term macro is a wasteland. But I'm a little less optimistic about how much progress we're going to make there because the problems are so difficult.

Bowmaker: What are the strengths and weaknesses of your own research?

Acemoğlu: I'll give you weaknesses. The biggest weakness is that I still don't understand most of the questions that I work on. That's what keeps me going. I'm deeply passionate about political economy, but if you ask me, "You've done all this work on political economy; if the World Bank or a government comes to you and says, "How should we use your research in order to improve something?" I would be very hard-pressed. There is a big gulf between concept and practice.

Bowmaker: Do you have any professional regrets?

Acemoğlu: I wish I had learned more in my undergraduate and graduate days. I spend a lot of my time reading because there are big holes in my knowledge. I always look at our PhD program here at MIT and think, "Wow, I wish I'd taken that program." The London School of Economics was very good by European standards, and it's improved a lot, but it was really subpar compared to the US institutions in terms of what it invested in the students. That was something that I was able to make up for with hard work, but I still feel, "Oh, God, there is a hole in my training here."

I just find the British system very strange. Economics is a technical discipline and, to come up to speed, you need to have lectures where you go through problems. I don't think it's the job of a teacher to give you comments on an essay in a tutorial. Obviously, some people will disagree with that, but you can put that in the book—I don't mind.

Acemoğlu: Do you have any professional ambitions?

Acemoğlu: Just to keep on doing what I'm doing. I like what I'm doing.

Bowmaker: How would you describe the state of economics today? Are you optimistic about its future?

Acemoğlu: The financial crisis has been good for us—of course, it's been terrible for society and the world would have been a better place without it, so I'm not trying to minimize that—but it has been like a wake-up call for our profession. To a non-trivial fraction of the profession, it has been a reminder that we know very little, and that there are lots of important questions. It has also increased people's interest in economics. And so that makes me optimistic that there will be plenty of interesting research to come. I think the empirical tools of economics are really being used, and there is also a useful debate about how to combine empirics and theory. That was long overdue and, hopefully, it will have an impact on how economics is done. And I'm very encouraged by the fact that a lot of young, talented people are doing political economy, and thinking about institutions and the big questions in economics.