Partha Dasgupta (University of Cambridge)

Sir Partha Dasgupta was born in Dhaka (at that time in India) in 1942 and graduated with a BSc in physics from the University of Delhi in 1962 before obtaining both a BA in mathematics and a PhD in economics from the University of Cambridge in 1965 and 1968 respectively. He taught at the London School of Economics between 1971 and 1984 and then moved to the University of Cambridge in 1985 as Professor of Economics. Between 1989 and 1992, he was on leave from the University of Cambridge and served as Professor of Economics, Professor of Philosophy, and Director of the Program in Ethics in Society at Stanford University. He is currently Frank Ramsey Emeritus Professor of Economics at Cambridge, Fellow of St John's College, Cambridge, and Professorial Research Fellow at the Sustainable Consumption Institute, University of Manchester.

Professor Dasgupta's research interests include welfare and development economics, the economics of technological change, population, environmental and resource economics, the theory of games, and the economics of undernutrition. His most-cited articles include, 'Notes on the Measurement of Inequality', *Journal of Economic Theory* (1973), co-authored with Amartya Sen and David Starrett, 'The Optimal Depletion of Exhaustible Resources', *Review of Economic Studies* (1974), co-authored with Geoffrey Heal, 'Industrial Structure and The Nature of Innovative Activity', *Economic Journal* (1980), co-authored with Joseph Stiglitz, 'The Existence of Equilibrium in Discontinuous Economic Games, I: Theory', *Review of Economic Studies* (1986), co-authored with Eric Maskin, and 'Inequality as a Determinant of Malnutrition and Unemployment: Theory', *Economic Journal* (1986), co-authored with Debraj Ray. His books include, *The Control of Resources* (Harvard University Press, 1982), *An Inquiry into Well-Being and Destitution* (Clarendon Press, 1993), *Human Well-Being and the Natural Environment* (Oxford University Press, 2001; revised version, 2004), and *Economics: A Very Short Introduction* (Oxford University Press, 2007).

Professor Dasgupta was elected a Fellow of the Econometric Society in 1975, Fellow of the British Academy in 1989, Member of the Pontifical Academy of Social Sciences in 1997, Member of the Third World Academy of Sciences in 2001, and Fellow of the Royal Society in 2004. He is a Foreign Honorary Member of the American Academy of Arts and Sciences (1991), Foreign Associate of the US National Academy of Sciences (2001), Foreign Member of the American Philosophical Society (2005), and Foreign Member of the Royal Swedish Academy of Sciences (1991). He was named Knight Bachelor by Her Majesty Queen Elizabeth II in her Birthday Honours List in 2002 for "services to economics."

I interviewed Sir Partha Dasgupta at his hotel in Montreal, Canada, where he was attending the World Congress of the Associations of Environmental and Resource Economists. It was the early afternoon of Friday, July 2, 2010.

BACKGROUND INFORMATION

You hold bachelor's degrees in physics and mathematics. How did you end up with a PhD in economics?

I was intending to be a high-energy, particle physicist, but two things made me abandon that ambition. One was that the subject was going through what seemed to me to be an uninspiring patch in the mid-'60s, although that probably reflected my own intellectual shortcomings more than the state of the subject. The other reason was that the Vietnam War was on and, like many other students, I was bothered by it. My friends among the mathematicians at Cambridge weren't interested in the War. A philosopher friend insisted he didn't have enough information to have a view about the War. I found that the only people in college with whom I could have informative discussions on the War and its probable causes were economists, particularly Marxists, who provided me one interpretation, and political scientists, who insisted on another class of interpretations. That was very educational for me.

In my own college at Cambridge, Jim Mirrlees (now Sir James Mirrlees, Nobel Laureate) had done math as a first degree and a PhD in economics. I got to know him through a discussion group we both belonged to, and he encouraged me to shift to economics. And that's what I did, in 1965.

As a student, did any of your professors stand out as being particularly influential or inspirational?

The greatest influence was unquestionably my father, who was however never formally my teacher. He was a professor of economics and a profound educationist. He was also a terrific father. Our home was always filled with visitors: his students, colleagues, and friends. Also, between the ages of 13 and 15, I went to a school (now known as Rajghat Besant School, Varanasi) that was phenomenally good. I came under the spell of several remarkable teachers there. About three months ago, I visited the campus with my wife. We spent a week there. It was an unforgettable experience for us both.

I don't believe there was anybody at university in Delhi who inspired me. But as a PhD student at Cambridge, Jim Mirrlees was a big influence. He had enormous technical abilities and I could tell he asked deep questions.

Why did you decide to pursue an academic career?

That was the influence of my father. I assumed I would be an academic because that's the only life I had known at close quarters. Our home was regularly filled with visitors, who were often distinguished academics. They were invariably kind to me, asked me questions and shared their ideas, even when I was very young. It was but natural that I would be attracted to a life of the mind. And I was. But when I moved to economics I wasn't setting out to change the world or help the poor, or anything so noble. All I wanted to do was to obtain a PhD and become an academic. I belong to a caste in Bengal, India, that nurtures professionals, especially doctors and teachers. My outlook must have been narrow, it never occurred to me to work in the private sector, say for a business firm. If I had joined the private sector, my parents' friends would have merely inferred that I wasn't a serious person, most certainly not a good student [*laughs*].

In the mid to late 1960s, at least in the UK, students of mathematics who had converted to economics (there weren't that many) were viewed with suspicion. Did we have the "horse sense" that was necessary for economics, senior economists would ask. For some years after I obtained my PhD I was unsuccessful in obtaining a tenure-track post. Two of the chapters in my thesis were published in the *Review of Economic Studies* almost immediately, so they must have been reasonable pieces of work. But they were technical papers. As I had little formal training in economics, I was also diffident, and that may have showed. About the time I completed my PhD, that was 1968, I obtained a research fellowship at Cambridge, spent a year at Carnegie Mellon University as a visiting assistant professor, followed by a year as a visiting fellow at the Delhi School of Economics. Then, in the summer of '71, three years down the road, I was appointed to a lectureship at the LSE; but that was after five candidates who had been placed above me had declined the lectureship! If you ask my wife she will tell you that for a long while after we were married she was worried whether I would ever get a job that would enable us to settle down.

As a researcher, which colleagues have been particularly influential or inspirational mentors?

At the time I joined the LSE, it had a phenomenal economics department. (It still does.) Bauer, Gorman, Hahn, Johnson, Morishima, Sargan, and Sen are a formidable list of names, by any standard. (Hahn had left for Cambridge, but visited for a day every two weeks.) None of them was particularly interested in my research interests, though. Maybe that was because I didn't have any particular interests in those days. Intellectually I was still quite rootless. But my senior colleagues were supportive of the young. I was left alone to get on with my work, which, however, wasn't much. I think being left alone was good for my development; it meant I didn't get depressed that I wasn't producing papers by the week. I was influenced more by my contemporaries, especially Joseph Stiglitz, whom I met way back in '65 when I had just moved to economics. He was inspiring even then, brimming with ideas. The contrast with me was all the more sharp because I rarely <u>had</u> an idea. I owe Stiglitz an un-repayable debt because he made me feel as though I was contributing to our joint work, even while I was unsure what I was bringing to the proverbial table. Geoff Heal was another contemporary whose work and engagement I found exciting. We collaborated all through the 1970s in developing the economics of exhaustible resources.

Among my senior colleagues at the LSE, I saw much of Amartya Sen, from whom I learnt how one might interpret economic development. He had style and a flair for polemics. I read pretty much everything he wrote at that time. In recent years our visions of what economics should be about have diverged somewhat. That may be why we haven't seen much of each other. As far as I can judge he feels development economics should get closer to moral philosophy and has influenced international agencies and charities to adopt that position, whereas I am convinced the subject's greatest weakness lies in that it's not informed by the natural sciences, especially ecology. I don't think the failure of official development economics to successfully address extreme poverty and demographic distress in the poorest countries has had anything to do with not knowing what poverty or justice mean, rather it seems to me the answer lies in the fact that professionals have neglected to uncover the pathways that determine the poverty-population-environment nexus. If you read Sen's famous 1999 book, Development as Freedom and his recent book, The Idea of Justice, and my 1993 book, An Inquiry into Well-Being and Destitution and my 2001 book, Human Well-Being and the Natural Environment, you will see what I mean. For example, in his book on justice, Sen makes it his central point (or so it has been read by reviewers in UK newspapers and literary magazines) to criticize Rawls' theory of justice on grounds that the theory characterizes the just society, the attainment of which presupposes a well-ordered society; whereas a useful theory should be able to provide a moral ranking of unjust societies too, even dysfunctional societies. I don't know whether Sen's charge against Rawls will be found by experts to stick, what I do know is that his view of what theories of justice should offer is bread and butter in modern welfare economics. The idea of a social welfare function, now over 70 years old, does precisely that. It ranks all alternatives; it doesn't merely identify what's judged by the theory of justice to be the best. Theories of the Second Best, constructed by James Meade in 1955, are an illustration of what I mean. But even the usage of the term "second best" carries with it the thought that the society under study is nearly just. So it struck me some years ago that what needed doing was to apply the idea of a social welfare function to rework welfare economics and develop a unified theory of policy evaluation that covers not only Utopia (the ideally ordered society) and Agathotopia (Meade's name for a Good Enough society), but also Kakotopia (the name I gave to dysfunctional societies). In my 2001 book I just mentioned, I did that, and it required of me to study a number of socio-ecological pathways that sustain dysfunctional societies. It seems to me that's where the hard work lies, unearthing further pathways that are bound to be site specific and time specific. But I found no reference to that applied-theoretic work in Sen's book on justice. But at the time I speak of, the 1970s at the LSE, I didn't know much about development economics, certainly I didn't know then the way I would subsequently come to frame and study the state of affairs called poverty.

By the mid-1970s I had worked on several fields. One reason I moved fields then and have continued to do so is that I haven't had a proper training in economics. Working on a field has been my way of getting acquainted with it. For example, when I started working on industrial organization and technological change with Stiglitz (that was in 1975 or thereabouts), I had little prior knowledge of the subject. Ignorance may have been a help, though. As I didn't know the literature, I wasn't minded to make an advance on someone else's work. Stiglitz and I simply chatted about what might drive an entrepreneur to innovate. Once we had arrived at a formulation, I was sufficiently intrigued to read Schumpeter and Scherer, who were very much worth reading of course; but it was as well I hadn't read them before. Their style was very different from the one Stiglitz and I adopted in our attempt to understand the character of technological competition.

Ignorance has helped my work over and over again. For example, even after completing the first paper Geoff Heal and I wrote together, on the optimal depletion of exhaustible resources, I didn't know of Hotelling's now-famous paper of 1931. In this instance even my coauthor didn't know it. We learnt of that paper from Robert Solow. My guess is that if we'd read the paper before starting our work, we would have modeled the problem as an extension of Hotelling's work, which was entirely Marshallian, partial equilibrium. Heal and I knew some capital and growth theory, so we found it natural to embed the exhaustible resource in a larger economy. I like to think our paper helped frame the contemporary literature on sustainable development.

GENERAL THOUGHTS ON RESEARCH

There is an increasing emphasis at many economics departments on applied research. Is this true at Cambridge?

Yes and I am all for it. I certainly tried to bring more applied people into my department in Cambridge when I was Chairman. I felt we were particularly weak there, especially in applied micro-econometrics. Traditionally, the Faculty of Economics at Cambridge has been of a highly theoretical bent. One reason is that, at least since World War II, there was a separate department called the Department of Applied Economics (DAE), which had been established owing to Keynes' urgings, essentially to advise him on the kind of numerical figures he needed for his own work. I can only think Cambridge was a most patriarchal society (*laughs*). The DAE built its reputation on its first Director Richard Stone's innovative work on consumption and the social accounting framework that's needed to describe an economy's doings. It may be that because the DAE was in the same building as the Faculty of Economics, appointments in the economics department, which did most of the lecturing, were mainly in economic theory. When I was a student, the great names were Joan Robinson, Nicholas Kaldor, and Piero Sraffa, who were all theorists. It makes me blush even to think of what Robinson, Kaldor, and Sraffa thought applied economics amounts to. They really were hard-line Mandarins. I think Austin Robinson was the only applied economist of note in the Faculty when I was doing my PhD. James Meade was also in the Faculty, and he straddled both theory and empirical policy with enormous distinction, but the politics in the place at that time was so virulent that he remained an outsider even while occupying the Professorship of Political Economy.

As you know, applied economics (by which I mean applied micro economics) has grown by leaps and bounds in the last 30 to 40 years, but our department is not yet a balanced one. We are pretty strong in microeconomic theory, not so strong in applied microeconomics. Macroeconomics remains a mystery to me. Meanwhile the DAE has closed. The quality of its research had deteriorated. Like most other think-tanks, it survived on soft money, which meant it had to chase research programs that others were interested in. That doesn't do much for the university it inhabits. That's not to say there aren't outstanding research centres built on soft money. The Institute for Fiscal Studies is excellent, but that's in London.

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

Both are valuable. I'm not a believer in "relevant" theory, though. It's hard to tell in advance when, if ever, good theory will turn out to be useful in practical, policy terms. Take the case of Frank Ramsey's 1928 paper. Ramsey asked how much of an economy's national income should be saved.[1] It was a highly mathematical, esoteric piece of work. For a long while the paper languished, probably because the world entered a depression and nobody was interested in the long run. But after World War II, people became interested in the long-term development of nations, such as India, and Ramsey's was the obvious theoretical tool for one class of questions, concerning the optimal magnitude and composition of investment activity over time. So Ramsey's question and the way he framed it became useful even to economists with a huge interest in policy, such as Jan Tinbergen. At the time I was working on my PhD, my teachers such as Joan Robinson used to think Ramsey's paper was about how many angels are able to dance on the head of a pin. Recently the paper has made another return in the economics of climate change. Ramsey's paper contains the only machinery available for thinking about the long-term trade-offs.

My father once said that if you see a piece of theory that looks directly applicable, you should be suspicious. I think he meant that if the theory is so designed that the gap between its formulation and application is small, there should be a suspicion the theory may have been doctored to suit the answer desired by its authors or their patrons. The advantage of maintaining a certain distance between theory and policy is that it encourages the author to seek deep answers, not shallow ones. I'm not saying all theoretical papers should be like that, but it's the more esoteric type of theoretical work that gets criticized for their lack of "relevance". My father provided a sophisticated defense of pure theory.

How would you describe your own research agenda and how has it changed over time?

[1] Ramsey F.P. (1928), 'A Mathematical Theory of Saving', *Economic Journal*, Vol. 38, No. 152, pp. 543-559.

Most of my work has been on what is often called "applied theory". No one is the best judge of their own work, but I believe much of my work has sprung from the ground up, motivated by some phenomenon out there that demands an investigation. Of course, being a theorist by temperament and training, I pretty soon lift the phenomenon up many miles, so that it may even become unrecognizable by the time I am done with it, but I like to think it's still likely to be useful to someone concerned with the phenomenon.

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

It's a matter of personal taste, nothing more. Gerard Debreu is a good example of someone who did foundational work, but never took interest in anything other than a narrow set of very abstract problems. And Wassily Leontief appeared to me to be rather dull (input-output tables, not much else), but I only met him when he was quite old. Debreu is one extreme. At the other end is Kenneth Arrow, who is interested in a huge number of problems and can explain why we should be interested in them. And of course, he has written fundamental papers on pretty much any subject he has touched. In 1975 I came across, quite by chance, his short book *The Limits of Organization,* and it transformed my work. I had known Arrow's work on social choice, general equilibrium, technical progress, health, and economic externalities, of course, but as I read that little book of his, I could feel that at last I knew what basic research in the social sciences amounts to and how to go about it. Among economic theorists of my generation Joe Stiglitz has the widest reach in terms of research interests. He is simply phenomenal.

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?

The answer to the first part of the question is "yes." The American PhD program is very much like an apprenticeship, which England is now mimicking. Students tend to take their supervisors' research lead. This means that at an early stage, you are shaped by someone else's style of research. And there is no question that, intellectually, we are history-dependent. Our capital stock is created by the time we're 27 or 28, and it takes quite some time to overcome it and break out on one's own.

The answer to the second part of the question is also "yes." Many years ago Bob Solow put it nicely. If I remember him correctly, he said the really hard problems in the social sciences relate to policy. That however looks easy, which is why even taxi drivers with no training in economics spout on it. Solow said the technical stuff is relatively easy, although seemingly very difficult. He also said he liked young economists to get their fingers burnt in the technical stuff and wouldn't trust someone with the policy stuff if he or she hadn't undergone the technical test.

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

I think so. I have been very lucky and the profession has been good to me; but in an unusual way. Judging by citations, or rather the lack of them, most of my solo work has gone unnoticed, but by the remarks my colleagues make, I have the sense they approve of the titles of my publications. Recently I had to prepare Introductions for a pair of volumes of my collected papers that Oxford University Press will be publishing, so it made me reflect on what others were doing when I worked on a particular set of problems and why I chose to work on them and how I framed the problems and why. I guess such reflective moments are a sign of getting old! In drafting the Introductions it came to me that I have a non-standard way of framing social problems. For example, I have written extensively on the poverty-population-environment interface. But it hasn't had the slightest impact on development economists or on environmental and resource economists. And the papers on population and fertility behaviour have gone unnoted by economic demographers. It may be that I am remorseless in trying to link seemingly disparate features of daily life, and because we economists are trained to consider them only piece by piece, one at a time, my analyses probably appears alien to my colleagues. For example, if I'm studying the way rural people use natural resources (e.g. disappearing forests), I can't resist modeling such other human activities in the world of the poor as reproduction. The problem for me is that the typical environmental economist is unfamiliar with the word "poverty", the development economist won't know how to spell "environment", and the economic demographer thinks fertility depends entirely on the value of time. So I face a problem. What continues to surprise me though is that this intellectual distance I feel that separates me from my colleagues hasn't made me an outsider: I have enjoyed more than my fair share of honours.

One advantage of framing problems in a quirky fashion, it's not a conscious decision of course, is that I've been able to get on with my thinking without having to compete with others. You will notice from my CV that I have many papers on the same subject. One reason I have done this is that when working on my own I have rarely arrived at an understanding of the phenomenon I was studying in one paper; it's been almost always incremental. Discovery for me has usually meant a growing realization, rarely a revelation.

I have been able to indulge in that slow process because I was aware I wouldn't be beaten to the post by somebody else – nobody else would be working on my problems, most certainly no one would have framed the problems in the way I do! So, I have had a very, very lucky life. Colleagues seem to approve my work, even though mostly they don't read any of it (*laughs*).

IDEA GENERATION

Where do you get your research ideas?

By observation, I guess. On one occasion in the early-'80s, when passing through Calcutta on my way to visit my parents in Santiniketan, I noticed that the baby of a mother beggar on the sidewalk was being molested by flies. I thought, "That's odd. Why isn't the baby swatting the flies?" Then it dawned on me that the baby was conserving energy. That eventually triggered my joint work with Debraj Ray on malnutrition and the capacity to work. Of course, he had been thinking along similar lines before we met at Stanford, which is how we came to collaborate, but it was a casual observation that led me to seek a theory that would cover what I had observed. When Ray and I discovered we had been thinking along similar lines, we closed the deal, so to speak, and produced our analysis.

If you travel by train in West Bengal, you will notice that every village has a pond, supplying water for drinking, washing, and cultivating root crops. On several such journeys I observed that villagers have built their homes very close to one another around their pond. Why? One answer is that you have more land for cultivation if you crowd the huts. It occurred to me that another possible answer was that closeness would enable people to observe each other's behaviour easily. We know of the old adage that in the third world there's no privacy. But maybe you don't enjoy privacy because life there is built on social norms. There are few private property rights to those commons, so presumably communities have had to devise norms of behavior. And norms of behavior involve sanctions for misbehavior. But how do you know somebody has misbehaved? You have to observe it. Those problems led me to the then nascent literature on social capital, and I tried to understand the concept in terms of modern resource allocation theory.

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

I've never had a project in the sense most people mean by a project. I've never applied for a research grant. My guess is that you have to have a fairly well-defined notion of what you want to accomplish when you apply for a grant. But mostly I've not even been able to frame the question I was tackling until locating the answer. So, by the time I might have been in a position to apply for a grant, I'd completed the paper and moved on to a new set of problems, ones that I would be unable to articulate. Of course, I have enjoyed grants indirectly. For several years Joe Stiglitz included me in his grant applications, but it was he who had an idea of where we would be heading.

My research practices are very old-fashioned. I do all the ancillary work that's needed to be done in preparing a paper: reading other people's work, referencing, checking citations, proof-reading, the whole works. Even now I don't Google for references; I go to the library and browse. The latter is a pleasure in itself. In the course of browsing I frequently find very interesting things to read, material I didn't know existed. My book, *An Inquiry into Well-Being and Destitution*, has about 65 pages of references. Believe me, I read, or at the very least glanced at, each of the items mentioned, all in libraries. For certain chapters I used to walk to the library of Addenbrooke's Hospital (our University hospital), quite a distance from the University Library, because that's where I could browse the literature on clinical under-nutrition. In describing my long standing work habit, I am neither apologizing nor bragging. It's how I have always worked. I have always felt chasing material is part of my job.

IDEA EXECUTION

What makes a good theoretical paper?

It should have a surprise.

What makes a good empirical paper?

Good applied work doesn't necessarily have to have a surprise because you may be engaged in repeating a previous investigation in a different geographical location. That can be extremely valuable work. You may discover subtle differences from the findings of previous investigators, and that might suggest that the phenomenon is site-specific, a frequent characteristic of phenomena in the social sciences and challenging to the theorist. Often it may be that you are investigating the same phenomenon others have examined, but you are deploying better tools; and so on. For example, the theoretical models Kenneth Arrow, Karl Goran Maler, and I have been developing over the past few years show that that wealth changes rather than movements in GDP per capita are the true indicators of the progress and regress of nations. But then, what is wealth? It must be the value of all capital assets of an economy. Does that include natural capital? Of course it does. So, if a national income accountant claims that the savings ratio in Brazil is nearly 15%, we should respond by insisting that the statistic doesn't take into account the forests that are being razed there. That's depreciation and should be deducted from savings. If accountants buy the argument, they would repeat the exercise by deducting forest depletion. The research wouldn't be novel in the conventional sense, but it would be illuminating and useful.

When you hit a "brickwall" on a paper, do you continue to work on the problem or do you take a break from it and work on something else?

I take a break and then, USUALLY, serendipitously, I get an answer. Eric Maskin and I once worked on a paper that took us ten years to complete. It was on the existence of equilibrium in games in which payoff functions are discontinuities. It was very esoteric stuff in game theory (not the sort you would bore your partner with), but Maskin and I thought it was important to determine whether such games possess Nash equilibria (in mixed strategies).   Pretty quickly we managed to prove an existence theorem, but it was only for symmetric games, meaning that players were assumed to be identical. Now we could have tried to publish that result, in fact all the then existing theoretical models with discontinuous payoff functions were symmetrical, which is a perfectly sensible modeling strategy to adopt when trying to capture something else about the phenomena out there in the world; but Maskin and I chose not to submit our result for publication. And the reason we didn't is that we knew we hadn't dug deep enough, we still didn't understand the underlying structure of the problem. So, we sat on the problem for some more time. Then, in one set of interchanges we found a simple trick that enabled us to prove the result in its generality.

Related to the previous question, when it appears that a project isn't going to turn out as hoped, do you scrap it or aim to send the paper to a second-tier journal?

I have been enormously lucky. I've rarely been involved in a paper that hasn't eventually been published. There have of course been occasions when a submission didn't get accepted, but I always interpreted rejection to mean I hadn't drafted the work well. That meant working on the problem some more and improving the exposition. But I don't think I have entirely abandoned any work. And I've also had amazing luck with editors. Over 40 years I have found journal editors almost always to be fair and encouraging. Journal editors generally get a bad press, so there was one occasion I can't help recalling, to illustrate how shrewd and fair-minded editors can be:

In the mid 1980s my friend Debraj Ray and I developed a timeless general equilibrium model in a world where nutrition affects productivity, a project I mentioned earlier. There were some interesting technical problems that the model threw up (having to do with non-convexities in nutrition-to-productivity transformation possibilities), and it showed, among other things, how and why equilibrium allocations can violate horizontal equity, in the sense that very similar people end up with vastly different utility levels. Arrow-Debreu equilibria, as you know, satisfy the principle of horizontal equity. Ray and I showed that in a rich world the principle would be maintained, but not in a poor world. And we identified several other properties of the model, each of which spoke to the world we believed we knew in India. So we felt we had understood something of importance about the nature of poverty; and we submitted the paper to the Economic Journal. In return we got a referee's report that was 8 pages long in A4, single spaced paper, offering as many reasons as you care to number as to why the paper should be rejected. The referee basically had sat down and asked how many reasons he could think of for not liking the paper. Ray and I could tell the referee was technically proficient, but we could also tell that he had little imagination and suffered from an inability to discover general truths from non-standard models. Now you would think the Editor, who was the economic historian Charles Feinstein, would have written to me to ask why I had wasted his time submitting such a shoddy piece of work. But he didn't. He smelt something not right in the report, the referee had gone for over-kill, so he wrote to say that, obviously, he couldn't accept the paper as it was drafted, but that he would publish it if Ray and I re-wrote it, having dealt with all the reasons the referee had collated for recommending rejection. Ray and I did that, and the paper was published in two installments. I don't know if many people have read the paper, but it has been the basis on which I have tried to understand poverty traps.

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

People are a lot tenser now about research than they were in my time. I can see that amongst young colleagues. Life for the researcher is harder today. There is far greater competition. Moreover, family life has changed beyond recognition. And remember, economics remains a male profession. In UK economics departments, women average round 10 percent of senior appointments. Responsibilities at home among males have changed enormously and that adds to the pressure. I like to think I was a good father and husband, but the division of labor between my wife and I, one that we reached without thinking, would be unthinkable today.

THE WRITING PROCESS

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

I used to find writing difficult, but having gained experience over the years I find it much easier now. The word processor has of course helped. I frequently take the lead in writing a first draft when working in collaboration, largely because I enjoy composing papers. In the process of drafting, based on notes, I at last begin to understand the point of the paper we have been working on (*laughs*).

COLLABORATION

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

If you look at my CV, you will find an enormous amount of collaborative work. Swapping ideas is always good and it also encourages friendship. Conferences are terrific breeding grounds for collaborative research and my guess is that some personal relationships do then develop. But in my case, the causal chain has been the reverse. Almost always the collaboration starts over a conversation with a friend, maybe over a drink, an idea comes up, and then we work on it together. Joe (Stiglitz), Eric (Maskin), Karl-Goran (Maler) and Geoff (Heal) were friends first; collaboration came later. In the case of Ken Arrow, collaboration began many years after we first met, but that's because I used to be terrified of him. It was no fault of his, but for a long time I found conversations with him an agony. It slowly dawned on me that the problem was with me, that Arrow believes everyone is as deep and quick as he. That's the only intellectual error I have ever known him to make, but once I realized he wouldn't notice my intellectual shortcomings, I found it possible to collaborate with him! It's been not only a privilege, but a wholly pleasurable experience.

How do you interact with your co-authors (by e-mail, phone, or face-to-face meetings)?

With Maler it's been face to face discussions, but that's because we have met frequently over the years in connection with the teaching programmes he and I helped to initiate in South Asia and sub-Saharan Africa. With Maskin, too, it's never on the phone or by e-mail, it's always been face-to-face; but that's because over the years he and his wife Gayle have made it a point to stay in touch with us, as have my wife Carol and I with them. Maskin and I have a discussion and then we do our writing separately. We're about to write a paper on a problem where we don't know which of two models we ought to use to illustrate the point we want to make. He has one, I have another. But we will write down both models and then decide which best makes the points we want to make.

With Stiglitz it used to be walks in Oxford or Princeton or while he cooked supper. He would talk nineteen to the dozen, throwing out one model after another to capture a phenomenon we agreed was worth understanding. With Ken Arrow it's been a meeting or two where we have discussed a problem, followed by e-mail exchanges on how best to model the phenomenon, or as in a 5-way paper we have just completed (with Larry Goulder, Kevin Mumford, and Kirsten Oleson), most of the discussions were held over conference calls.

SEMINAR PARTICIPATION AND NETWORKING

How important is networking to success in research?

It's very important. It was very important even in times long gone. Isolation is never a good thing. I remember talking to Fred Hoyle, the great astrophysicist, who courted notoriety. As we all know, he held on to the steady state theory of the universe. He never gave up on it, partly I believe because he chose to be isolated. I am told by friends who know, that his best papers were early collaborative efforts, like his work on how heavy elements are cooked up in the stars. But when I met him, it must have been ten years ago, he lived out of reach from university campuses. On the occasion we sat next to each other at dinner at St John's College, I asked him if he didn't feel isolated. And he replied, "Oh, no, that's the advantage – I don't get contaminated by other people's ideas." I thought it was sad that such a powerful mind could be so wrong.

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

I don't think it's a major disadvantage. As I mentioned earlier, at the LSE, I was isolated in terms of the work I was doing, but I always had access to the great minds there. Conversations with powerful minds, even if they don't work on the problems you work on, is always helpful. It keeps you alert and prevents you from becoming sloppy. They set the standard, if you see what I mean. For example, in Frank Hahn's presence one could never say anything remotely imprecise; he would tell you in a booming voice that you had slackened your intellectual muscles, maybe even that you had been educated beyond your natural limits. And who wants to be told that in public? Moreover, even though my colleagues at the LSE, and later at Stanford, didn't work on my problems, they were world experts in their fields of expertise. I could, and did, pick their brains for what to read. When I was at Stanford in 1989-91, working on my book on Well-Being and Destitution, it's not that any of my colleagues had much interest in the subject, but I could always collar them on the corridor for a quick tutorial on some technical matter I hadn't understood, or needed a reference to a paper that would explain something I wanted to understand. My colleagues saved me hours of work by telling me what to read and explaining something I had not understood.

COMMUNICATION OF RESEARCH

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

It has to be a pretty finished paper before I put it up on my website. If you have that option, you should exploit it. In the past, you had to rely on being part of a discussion paper series that was then mailed to a restricted number of people. Being able to retrieve other people's writings easily today is an enormous boon.

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

I haven't been worried about seminars. I think I'm fairly articulate; in any case, I like teaching, and I'm generally not shy to talk about my own work. Of course, there have been occasions when a seminar has gone badly; but that's generally been because I wasn't particularly proud of the quality of the paper I was presenting. If you don't find your own work exciting, your audience will know that pretty quickly, which is when you start wondering when the seminar will end.

Do you have any advice for a young scholar on giving a seminar?

Be excited about your paper. Of course, the problem is somewhat the other way in America, where there is abundant self-confidence. And so the advice I would give to young scholars there is, don't overrate your self. Very often, I hear seminars where the presenter thinks he (it's still usually a "he") has solved the world's greatest problem; worse, he often seems to be selling a product. Overconfidence in the quality of your own work can distort your notion of what is genuinely important work. I'm not saying you should be humble, but it's one thing to be self-confident and at ease with yourself, it's another to think that you are the greatest. Just read a page or two of Arrow and you will realize you are not.

PUBLICATION

How do you decide upon the appropriate journal to send your work to? Related, whom do you view as the readership of your research?

At my age, I write quite a number of papers that are invited ones. You write in a style appropriate for the occasion. But on the whole, I have tended to send my research papers to journals where the reader is more likely to be interested in what I am doing. It's a marriage; there is a natural place for most articles.

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

Today, there is an obsession with the top five journals and I think it's absolutely dreadful. It's stalling progress. I feel so bad for young scholars because they are convinced they have to submit their work to *Econometrica* or to the *American Economic Review*, where there is more than 95 percent chance it will be rejected; that too after two years. It can then be that after three years into your first job you still haven't got a publication. At the end of the day, it's the quality of the paper that matters rather than where it has been published. The problem is, people, especially those are on appointments and tenure committees, don't appear to have confidence in judging a paper for its quality. So they look for quality by the journal in which it was published.

The practice has so annoyed me, it's now arrived in Cambridge, that some years ago I ran an experiment to judge how top the top 5 journals are. As you know, over the years there has been a big increase in the number of economics anthologies. The publisher Edward Elgar has produced more than 100 anthologies, on various themes in economics. What they do is to print about 500 copies and sell them at a very high price to libraries. They are clever to commission well-known people as editors. Those anthologies are very useful to university libraries in poor countries. They can't afford books or journals, but at a stretch they can afford anthologies, which give students and scholars the opportunity to read the classics in their field. For teaching purposes in a third-world country, they are invaluable.

What I did was to peruse a dozen Edward Elgar anthologies. After all, if experts have edited anthologies, they could be relied upon to know what's stood the test of time. My very cursory research suggested that the major journals in economics are overrated. Most of the papers in those anthologies were published in journals other than the top 5. The point it seems to me is a simple one. The top 5 journals publish excellent articles on currently fashionable topics. The signaling effect of ability is certainly strong. But papers that may have lasting value, or are novel, get crowded out by good but standard-quality papers on hot topics. I suspect some of today's best papers are appearing in second-tier journals. It would be interesting if someone were to do a more thorough study of anthologies than I was able to do.

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

I've never had a quarrel with an editor. There have of course been instances where my submission has been rejected and where I could have written a letter showing that the referee was perhaps illiterate; worse, prejudiced. But I never felt the need to do that. What I took away from a rejection was that I (or I and my co-author) had not drafted the paper well. Usually I have re-drafted a rejected paper and published it elsewhere, sometimes in a better journal.

In 1996, you helped to establish the journal, Environment and Development Economics. Part of its purpose is to provide an opportunity for scholars in developing countries to publish their findings in an international journal. Do you think there should be more examples of journals like this?

Yes, of course. But if the journal is going to be any good, submissions must go through the same screening process that other journals insist on. You mustn't introduce affirmative action. How do you achieve that? You need to ensure that three things happen. First, the editor mustn't necessarily chuck a paper in the way he or she would have if it were a standard journal. If there is a semblance of an idea in the submission, the editor needs to be sympathetic and should ask referees not only to referee, but also to act as mentors. Secondly, you have to build up a body of academics who are willing to be those mentors. And third, you need funds to enable authors to spend time with one of their mentors so as to be able to complete their paper for publication.

That's how it's been working at the interface of the journal Environment and Development Economics and the South Asian Network for Development and Environmental Economics (SANDEE). SANDEE has in its roster such outstanding economists as Enamul Haque, Subhrendu Pattanayak, Priya Shyamsundar, E.S. Somanathan, and Jeff Vincent. They give a lot of their time to teaching and training young scholars from Bangladesh, India, Pakistan, Nepal, and Sri Lanka. Karl-Goran Maler and I have also been engaged in that work, as we had been involved in obtaining the funds for starting SANDEE. Collectively, we have been hugely successful. Journal articles (in Environment and Development Economics; even the Proceedings of the National Academy of Sciences) and collections of articles on selected themes have been published by scholars who entered the international academic community first by attending SANDEE teaching and training workshops. It's the most exciting venture I have ever been involved in.

But building capacity in poor regions takes patience, time, and a great deal of good will. And it requires a collegiate atmosphere. SANDEE's director, Priya Shyamsundar, is an outstanding environmental economist in her own right, but is also simply out of the world as a leader, mentor, and administrator. Maler and I are in awe of her. We do whatever she asks us to do, whenever.

Perhaps the most striking example of success is the case of a woman economist, Saudamini Das, who came from an out-of-the-way place in the intellectually unpromising state of Orissa. She had a bit of economics training, had raised a family, and then sought to understand the role of mangroves, which are an important form of natural capital in hurricane ridden Orissa. She attended a SANDEE teaching and research workshop, was successful in obtaining a grant from SANDEE (we are talking of at most $12,000, so this is research on the cheap) and eventually produced a joint paper with her mentor at SANDEE, Jeff Vincent, who is one of the best minds in environmental and resource economics. The paper was published last year in the *Proceedings of the National Academy of Science*. This is research that came from the heart, to put it one way. Das knew that mangroves protect coastal villagers. Every NGO or international organization I know will agree that mangroves are an important form of natural capital. But how important are they? Do we have any quantitative feel for how much of a buffering capacity it offers to shorelines? Das and Vincent used data on the effect of the Indonesian Tsunami on coastal villages to show us how to estimate the social worth of mangroves. Theirs is a very important paper.

BOOK WRITING

You have written numerous books. Do you enjoy the process?

Yes, I enjoy the art of writing, and books enable me to understand the subject on which I had been working. As I told you earlier, all of my understanding is incremental; I've never had a eureka moment. Articles are of necessity narrow in focus. If you want to understand a complex phenomenon, you want to break it up into small bits and publish articles on those small bits. Putting them together in the form of a book enables you to puts those bits together, explore the way they feed one other. When I've finished writing a book, I know a lot more about the subject. Writings books has been a way I have tried to educate myself. Economists are writing more books now than they did 40 years ago. That's good news.

Tell me about writing A Very Short Introduction to Economics.

That was a curious experience. It took me eight years to complete it, but not for reasons you might think. I signed the contract with Oxford University Press in 1998 or '99, but I didn't know how to write it. I asked several people for advice on how to squeeze economics into 160 small pages, but the advice I received didn't match my temperament. So I sat on the book for several years. The delay was so great that I got into trouble with the department at Oxford University Press responsible for the VSI series. They had huge expectations for the series, it had become very successful; they were aiming for more than 200 titles, but seven years had gone by and they were still missing the economics title. Meanwhile, as I didn't know how I was going to frame economics for the book, I did what comes naturally to me: go into denial and continue working on other things.

Then out of the blue, sometime in 2005, Tim Gowers, a distinguished mathematician at Cambridge (he is a Fields Medalist), asked me to write a chapter on Mathematics and Economic Reasoning for the *Princeton Companion to Mathematics* he was editing. Naturally, I was flattered; I didn't even dream of saying "no". However, I was required to pack my chapter into 15 printed pages of admittedly a large size book. That concentrated my mind. I thought, "How do I give the flavor to a mathematician of what our subject in about in 15 pages?" Once I cracked that problem, I knew it would be the model I'd use for *Economics: VSI*. If you read it, you will see that it reflects all the prejudices and convictions I have laid bare before you the last two hours.

REFEREEING AND EDITING

What would you say are the benefits to refereeing?

You learn something new, but I've been a bad referee all my life. I think it's because of my lack of training in economics. I've been learning 'on the hoof', so I don't have that much of a command over the literature at any moment to be able to be a good referee. I am likely to say, "This is not a very interesting paper because it's rather obvious." Somebody else might say, "But it's not published anywhere in the literature." And I'm then likely to say, "Well, maybe it's just as well it's not in the published literature because it's so obvious." I feel nervous refereeing.

You have never been an editor of a journal. Is that for the same reason?

I think my colleagues realized that I wasn't a very reliable referee as well. I'm not disciplined enough to say on Mondays and Tuesdays, I will work on the journal, and on Wednesdays, I will get back to research. My research life contaminates everything else, even when I was chairman of my department at Cambridge. I was a diligent chairman and I had a very, very clear vision of where I wanted to see my Department go. I was raised in an academic household, so I was fully prepared to be Chair in my department at Cambridge. That meant I didn't agonize over decisions. Consequently I continued to publish during my tenure. I see the world through a particular lens, and that's a bad thing for an editor; an editor is supposed to an Olympian (*laughs*).

TIME MANAGEMENT

How do you divide up your working day both in terms of quantity and timing of different kinds of work? And how do you balance your personal life and professional life?

I had some very, very lucky breaks in terms of my genes; I can concentrate no matter how noisy is the environment. And I don't need to be comfortable when at work. For example, I've never had a study at home. I've very often worked on a problem or drafted a paper, sitting at the dining table with small children running round, even one of them sitting on my lap. If my wife were here, she would tell you there's never been a time at home when our children were told to be quiet because "father is working." They were always running around or sitting on my lap when I was working. My family life never interfered with my research and my research certainly never interfered with my family life. Even today, when I'm washing up, I might be thinking about a problem while my wife and our family friends are sitting at the dining table, chatting.

My office door is always open, people are drifting in and out, and I can switch on and off. I'm not bragging about it; it's a fact. But I am rather grateful that my genes allow me to do that. We're a very close family, and if any of my children write to me about anything, it's unthinkable that I would not respond immediately; it doesn't matter what I'm doing. My wife often asks, "What do you do at the office? Do you ever work?" She asks because she can't imagine how I could be at academic work and would nevertheless be able to set everything aside the moment an e-mail arrives from one of our children. That doesn't mean I am efficient with other matters; I'm not. If it's an invitation to a conference, that will go in the hold bin, because the e-mail is impersonal.

Do you also find it easy to balance multiple research papers?

Yes, because I've got this wide-ranging, interconnected body of research. Everything is tied up with everything else, or so it seems to me to be so in the social world.

Do you have a sense of the optimal number of papers that you could be working on at any one time?

No. I've never been able to plan my research and don't suppose it would have been a good thing if I had. In the first 20 years of my academic life, my publications appeared in bunches. In the early-'80s, I published quite a number of papers, but then there was a fallow period. On the work on technological competition that I did with Joe (Stiglitz), we produced 7-8 papers out of one massive manuscript we had created for ourselves. But that manuscript took a couple of years. We then produced a string of papers out of that. I am from a fortunate generation in the UK. I got tenure pretty quickly and easily. It didn't bother me when I was publishing nothing, even before receiving tenure.

REFLECTIONS AND THE FUTURE OF ECONOMICS

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

The economics of asymmetric information is one big one. There are two strands to that literature: mechanism design when the agents are asymmetrically informed, and analysis of markets .under asymmetric information. But usually when economists are asked to explain asymmetric information, they take examples from the latter. I'll do the same here.

It's not that people didn't know that information was asymmetrically distributed – of course, they did – but as there was no canonical formulation, the profession was waiting for the right language in which to talk "information". Just to give you an idea of how difficult the matter was, in the 1960s a number of very fine economists thought the way into the economics of information would require first of all a measure of information (e.g. the Shannon measure). But that didn't seem to lead anywhere: the social world requires a different treatment from the world of communication. Kenneth Arrow was the first to realize, at least in a published form, that we should bypass that obsession and model an economy in which different people knew different things. To my mind his 1963 on health economics and the medical profession is the real origin of the economics of asymmetric information. If you read it you will find it had everything, but for algebra, that was in Akerlof's famous 'lemons' paper (in Arrow read "quacks" for Akerlof's "lemons"). But it went beyond the lemons example by offering an explanation for why the market for medical practitioners never collapsed. Arrow suggested that medical associations monitor quality and that you need institutions to control quality. At a time when most economists viewed such associations as creating cartels, Arrow's analysis must have been a revelation.

The person who carried out the bulk of the next stage of work on asymmetric information in markets is Joe Stiglitz. Stiglitz relentlessly pursued the problem, basically by re-constructing price theory. It's interesting that no single paper of his on the subject nailed things down, it's only when you put them together (studying markets for credit, insurance, labour, capital), that you begin to make connection to an enormous number of features of the world which were beyond the reach of economic analysis until then. Of course, Stiglitz was essentially studying the same model, but after having given a different name to the market being modeled. It was very Stiglitzian (*laughs*). But it was necessary he did it that way. He was trying to produce a canonical model; and he succeeded.

What are the biggest challenges facing your research fields?

It's best to respond by noting it's not just *my* research field, but the biggest challenge in economics.

Bringing Nature into economics will prove to be the biggest challenge, largely because whenever Nature is mentioned, the hard boiled economist says "externalities" and suppresses a yawn. Economics has established bad cultural practices. The profession doesn't reward someone who may be doing vital work estimating those yawn-generating externalities in, say, a situation where forests in the uplands of a watershed are being cut down and damaging farmers downstream. The profession rewards empirical work in socially acceptable fields, such as education, health, labour, insurance, and various industries producing private goods. But when it comes to natural capital, they give it a thumbs-down. It's very hard for empirical environmental and resource economists to get jobs in leading economics department. The natural sciences are far more sophisticated in their appreciation of good applied work. In the case of upstream deforestation, the economist has to obtain data from scratch because the government doesn't publish data on the subject; he or she has to collaborate with hydrologists, soil scientists, and agronomists if they are to estimate the "externalities". If there has been a recurrent theme in my own work, it's been the attempt to introduce Nature (natural capital) into economics in a seamless way; in many ways to re-construct economics. Sustainable development is a buzz world among intellectuals. But that doesn't make it a bogus word. Until economists take Nature seriously, we will not know how current policy will affect future people. We have to understand humanity's relationship with Nature at different levels of economic development. In order to do that, we need to make contact with neighboring disciplines. The profession isn't prepared to do that as yet.

If we want to understand, say, poverty in the Third World, we need to engage with anthropologists and ecologists, because they have gained insights from years of experience. I have found engaging with them very, very fruitful. If we want to understand rural life, we need to engage with geographers too, because they have developed tools about the landscape. It's taken me years to appreciate how deeply interconnected our social systems are with the natural system, and how we have also isolated ourselves from Nature via the market. We need to be constantly aware of the unintended consequences of that isolation.

We've got to really engage with a whole group of different, but related disciplines. We're not doing enough of that at the moment, and we don't have the willingness; our entire training process and subsequent career go against it. I can't help thinking that we economists are missing the most significant problems of our time, or for that matter of anybody's time, by avoiding them.

How did you feel about being awarded a knighthood for "services to economics"? What would you say has been your biggest contribution to economics?

I was totally surprised on receiving the letter from the Prime Minister's Office, in May 2002. I was surprised because I had never consulted for governments, in fact I didn't know any government officials. The recommendation must have come from the UK's Economic and Social Research Council. When I showed her the letter, my wife took some time to digest the question I was asked, namely whether I would accept a knighthood. The question didn't arise. I was very pleased with that recognition, it seemed to me to be an affirmation of my research, but it has had no effect on my life.

An Inquiry into Well-being and Destitution (1993) is unquestionably the work with which I am most satisfied. Working toward it made me understand the social world in a way I couldn't have by reading anything else. I wrote it over a 4 year period, start to finish, and it knocked me out. Unconsciously I wanted to change the way economics is understood, but of course I wasn't about to write a methodological work, I focused on well-being and destitution as my object of study with which to re-write economics. I was writing the book as a letter (a very long letter!) to my father, who I knew was going to die soon. The book wasn't finished when he died, so I wrote a memoir for him as an introduction to the book. *Economics: A Very Short Introduction* (2007) resembles that earlier work, but it's a whole lot briefer.

Do you have any professional regrets?

I don't think so, largely because I've never taken my professional life that seriously, qua professional life. That explains a good deal of my answers to your previous questions. Research for me has never really been research; it's been an engagement with life. And my work has never been compartmentalized from the rest of my life. Of course, if you ask my wife, she will say, "There were periods when he was impossible to live with; when I would talk to him in those moments, it was clear he wasn't listening." But that's inevitable; any person who's engaged in research, no matter how compartmentalized he or she is, will have moments when they're slightly disconnected.

I've never had a big agenda and I've never wanted to change the world. It's been self-indulgence all the way; I've wanted to understand the social world, and the way economists handled it wasn't good enough for me, which is why I was led to geographers, anthropologists, nutritionists, ecologists, and development biologists. And I've had enormous help from some of the greatest minds in those disciplines, scientists like Paul Ehrlich, Jack Goody, and John Waterlow. Whenever I have written, seeking guidance, sometimes to scholars whom I had never met, they have responded handsomely. And of course, I have had enormous help from my professional colleagues. My co-authors in particular have taught me a great deal.

I also don't think I've made a wrong move in terms of employment. In 1977, when I was at the LSE, I turned down a very fine offer from Princeton, mainly because I was hoping to become a Professor at the LSE, where my father had done his Ph.D and my father-in-law had been a Professor. I should say it wasn't competition with my elders, it was a matter of seeing through an intergenerational agreement, if you see what I mean; carrying the proverbial torch. For a couple of years I regretted not moving to Princeton. I also wanted to live in a campus environment, and London doesn't provide that. On the other hand, London was exciting, and my wife and I enjoyed an active social life. But, when in 1984 Cambridge approached me with the offer of a chair, both my wife and I knew we were going to accept it. She had grown up in Cambridge and I had been a student. That was an easy decision.

Do you have any professional ambitions?

No. It goes without saying I did want to become a professor. Once I became one, at the LSE in 1978, that ambition was fulfilled. From then on, what was important for me, professionally that is, is that I continue to explore the social world.

OUP is publishing my collected papers in the autumn. About ten years ago, I turned down the offer, saying I didn't see the purpose. I thought that those who did it did it either as a vanity project or because they felt that their creative period had come to an end. (I was dead wrong, of course.) But then two years ago, I had to undergo major surgery for cancer. I was given two weeks' notice and was told there was about a 4 percent risk of fatality at the operating table, not to mention that there could be further problems. When I learned that, I thought, "Well, if I'm dead, then it's dead (the volume)." But another (worse for me, personally) possibility was that I would survive but the experience would dampen my curiosity about life and the social world round me. If that were to happen, I thought editing my collected papers would be no bad thing. So I informed OUP that I was willing. They sent me a contract immediately. However, within two weeks of the operation, even though I could hardly do anything physical, I found myself reading a textbook on Earth Science. While lying in the hospital, a day following my operation, I had realized I knew little formal about the mathematics underlying plate tectonics. Recognition that I had reverted to being a student cheered me up no end. However, I began to regret that I signed that contract! But a deal is a deal, and I have done part of my job producing the two volumes. As I said, I was quite wrong earlier. I enjoyed collecting the articles and writing the Introductions.

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

At one level economics is in a very good state today. The last 30-40 years has seen extremely fruitful progress in both theoretical and applied work. Before then, the applied-theory divide was enormous. Theorists knew little about what applied people did, and applied economists couldn't understand the point in theory. Today, most theorists know something about the applied work to which their theory relates, and applied economics has changed beyond belief because of the development of advanced econometric techniques.

But there is a huge downside to the state of affairs. Good people usually do good research, but they don't necessarily work on the most important problems. And economists can misread the social and natural world so badly that even good people end up doing flippant research. The profession even rewards such work. I have already alluded to the fact that our profession is dismissive of really hard, empirical work on environmental externalities. Let me elaborate on it. Take the enormous literature that has been built up over the past two decades and more on endogenous growth. I find most of it wholly unreal. Here is the present world, heading for a population of more than 9 billion by the middle of the century, everyone wanting to enjoy the lifestyle of, if not Dubai's Sheiks, but certainly the average income of a resident in a high middle-income country. But the environmental requirements of such a state of affairs would require 3 to 4 Earths. We economists don't even begin to appreciate that fact. We simply postulate technological progress and think that Nature's constraints can always be overcome through education and research. How have we come to such a pass? We have after all only about 250 years of experience of what we now call the modern world, which seems a moment in a 11,000 years of human "history". Economists as a profession don't want to think about population and it doesn't want to take Nature seriously. I can only conclude that we have detached ourselves from the world. None of that would matter if we economists weren't enormously influential. But we are. The language we use seeps into the journalistic and political world. Economic growth, wealth, markets, and technological progress are expressions we have fashioned. We help others to go into denial about possible adverse futures of human societies, because we are smart enough and articulate enough to say knowledge and ingenuity will solve all problems. Periodically we write to say that "Malthus was a false prophet" (a quote from a recent issue of the *Economist*). And it's the economics profession that identified "externalities". Put all the terms I have just mentioned together and you get a contradiction in the economist's favoured model of the long run. That's not just ironic, it's tragic.