

Puzzles and paradoxes: a life in applied economics

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Starting out

My father believed in education, and he liked to measure things. He grew up in a mining village in Yorkshire, between the first and second World Wars. He was bright and motivated, but the school system was designed, not for education, but to produce workers for the “pit,” and only one child in each cohort was allowed to progress to high school. Not my father, who got in the line to be a miner, then was drafted into the army in 1939, and drafted out again with tuberculosis before war’s end. In the easy labor market of those days, he got a job with a firm of civil engineers. The managing partner was impressed by my father’s skill with a slide rule and a theodolite, and was prepared to ignore his lack of formal education. My father went to night school at what is now the Heriot-Watt University in Edinburgh, graduating after very many years as a civil engineer. He married my mother, the daughter of a carpenter. She had a great gift for storytelling; it is said that Sir Walter Scott would walk over from Abbotsford to our house in Bowden to share tales with one of her ancestors. Even so, she did not share her spouse’s view of education; she found it hard to see me with a book when I could have been using my hands. But my father was determined that I should be educated properly, and set his heart on sending me to Fettes College, a famous public school (in the British sense) in Edinburgh, whose annual fees were well in excess of his salary, even once he became the water supply engineer for the county of Roxburgh in the Scottish borders. Then, and perhaps even now, there were schoolteachers in the Scottish state schools who were prepared to coach a bright kid for a scholarship that would take him away, and to do so in their own time.

So I went to Fettes at 13, as one of two scholarship boys in my year—Sir William Fettes had left his fortune to give a public school education to the children of the poor, but there was only this remnant of the intent (and the original endowment) by 1959. Fettes had all of the resources to provide a great education, and in those days, sent most of the graduating class to Oxford or Cambridge, as did, for example, Lawrenceville Academy to Princeton in the United States. I was one of the Cambridge lot, I played the piano, the pipe organ, and the double bass, I was a pretty good second row forward—which is how I got into Cambridge (“Fitzwilliam needs second row forwards, Mr. Deaton,” the senior tutor told me at my interview)—and I was a mathematician of sorts in my spare time. But I had no idea what I wanted to be or to do; the rugby at Cambridge was serious and brutal, and the mathematics was appallingly taught, in huge classes by ancients in mildewed gowns whose sinecures depended only on their never publishing their yellowed notes. I quickly drifted away from both rugby and mathematics, tried to become a philosopher of science, but was refused by my college tutor, and instead adopted into a largely pointless student life of card-playing and drinking. Eventually, my college, losing patience with my aimlessness, told me that I could leave, or stop pretending to study mathematics. What to do? “Well, there is only one thing for people like you. . . . economics.” I should have preferred to leave—but doubted that I could explain to my father, who already felt that I was not making enough of the opportunities that

I had, and that he had lacked—so I accepted the inevitable, and set off for the Marshall Library, where the aimlessness came to a surprised and delighted end.

I found economics much more to my taste than mathematics. I was helped by the little mathematics that I had learned, though it hardly got me through David Champernowne's econometrics course in which the first (French) edition of Malinvaud was the sole text, or indeed the mathematical economics exam in which Jim Mirrlees set all of the parts of Diamond and Mirrlees that neither he nor Peter had been able to figure out¹, most of which were ill-posed and insoluble. But lectures at Cambridge were like the books in the Marshall library, varied, sometimes interesting, but entirely optional, and the important thing was reading and writing essays, which were regularly read and discussed by college-appointed supervisors. I found that the material was interesting—Samuelson's *Principles* was terrific—and I found that I could write, indeed that I could write with clarity and with a good deal of pleasure, a lasting benefit of the one-on-one teaching at Fettes.

That single year of (involuntary) undergraduate economics, reading Modigliani and Brumberg on life-cycle saving, Hahn and Matthews on economic growth, Meade on trade, Kuznets on patterns of consumption, and summarizing what I'd learned for discussion and criticism, provided a template for learning, thinking, and writing that I have had little reason to revise. I understood that economics was about three things: theory that specified mechanisms and stories about how the world worked, and how things might be linked together; evidence that could be interpreted in terms of the theory, or that seemed to contradict it, or was just puzzling; and writing (whose importance is much understated in economics) that could explain mechanisms in a way that made them compelling, or that could draw out the lessons that were learned from the combination of theory and evidence.

The two Modigliani and Brumberg papers—both on the consumption function, one on time-series, and one on cross-sectional evidence—have always stayed with me. They were written when the topic was in a mess, with dozens of unrelated and incoherent empirical studies. Modigliani and Brumberg provided a rigorous statement of a simple theory of behavior that, with careful statement and manipulation, could provide a unified account of all of the evidence, and that provided a framework that has dominated thinking ever since. In recent years, I have come to think of those mechanisms as incomplete, and in some places even wrong, but the principle of the thing has stayed with me, that a good theoretical account must explain *all* of the evidence that we see, in this case cross-sectional patterns of consumption and income, time-series patterns of consumption and income, and then—albeit some years later—international patterns of income and saving. If it doesn't work everywhere, we have no idea what we are talking about, and all is chaos.

Kuznets' work on consumption, and more broadly on modern economic growth, was another early influence that has lasted. This is much less theoretical, more historical, much more data driven, starting from careful empiricisms, and cautious induction, always with great attention

¹ Peter Diamond, in this volume, refers to the same incident, but notes that he and Jim Mirrlees had not yet begun their collaboration at the time that Mirrlees set the exam. The exam was still close to impossible, if only because reading it took most of the time that was allowed.

to problems of measurement and the quality of the underlying data. Underlying everything is historical measurement, considered and qualified, but leading to generalizations of great scope and subtlety and with significance beyond the topic at hand. Modigliani started from behavior, and used it to interpret the evidence, while Kuznets mostly worked the other way round. To me as an undergraduate, and to me now, the order matters not at all. What is important is a coherent behavioral or institutional account that provides broad insight into understanding the present and the past, and gives us some hope of predicting the future.

I had studied economics only to escape from mathematics, and to complete my degree, but after graduation now needed to work, so I went off to the Bank of England. I think I accepted the job because the interview had been really tough, and because the job offer came on letterhead that was engraved like a high denomination bank note. But the institution was in flux, it had traditionally not employed university graduates, and they had no idea what to do with me or the small cohort of graduates who entered with me. So I went back to Cambridge where I could be with my bride, Mary Ann Burnside, a writer and teacher, born in Topeka and raised in Evanston, who was studying psychology at Cambridge. I was a research assistant on a project measuring national wealth—directed by my college economics tutor, Jack Revell, who felt sorry for me, and wanted to help me live in the same town as my wife. So, just as I had drifted into economics as an undergraduate, I drifted into it as a profession though, at the time, it was just something to do. Revell soon left Cambridge for a Chair in Wales, leaving me funded but without anything much to do. I soon fell in with the Cambridge Growth Project, directed by Richard Stone. The project had begun as what was then known as an “indicative planning model,” centered around input-output analysis, though it was being developed into something more like a large-scale Keynesian macroeconomic model, albeit with a lot of industrial and commodity detail. Like my fellow researchers, I was assigned to work on one of the model’s components, in my case consumption and demand, though the time commitment was not large, and once again, I was left free to work on anything that seemed interesting.

Mentors and a collaborator

I was soon befriended by Richard Stone, who made it clear to me that I was a kindred spirit or, as he would put it, that “we were on the same side of the movement.” I was not at all sure what the movement was, let alone which side we were both on, but I was enormously complimented by being told so, and I knew at once that Dick Stone’s was the life that I wanted to lead. Dick was married to Giovanna, nee Forli, a glamorous and alluring Italian aristocrat, who had started out as a concert pianist. They lived in a beautiful house with gardens, an extensive library, a Bösendorfer, and spectacularly decorated rooms. Their intellectual and personal lives were inseparable; they worked, they talked, and they entertained. There were many dinner parties, and economists and statisticians from around the world flowed through. And because I was on the same side of the movement, Mary Ann and I were frequently included in the dinner parties, in the heady conversations, and the even headier glasses of claret and burgundy from the cellars of Kings’. I had been admitted into Aladdin’s cave, surrounded by the gems of a good life.

Stone's work inevitably became the model for my own. During the war, he had worked with James Meade who had been hired by Maynard Keynes to construct a double-entry bookkeeping system of national accounts, work for which Stone later received a Nobel Prize. By 1970, he was still heavily involved with the United Nations developing international standards for national accounts, but my own interest in measurement, although certainly triggered by Stone, was not to come to the forefront for some years. Instead, I was immediately involved in Stone's work on demand analysis.

In 1954, Stone had introduced and estimated the linear expenditure system, so that, for the first time, a utility function was not just being used to prove theorems, or to guide thought, but was the direct target of empirical estimation. When I arrived, researchers on the Growth Project were still trying to estimate the model using Stone's original algorithm, and a quick trip to the engineering library provided a much better, up to date set of procedures for estimating nonlinear models, so I set about learning FORTRAN, and soon had some well-converged parameter estimates. (Soon is a relative term: the computer system accepted "jobs" each evening, and returned them, usually with compiler errors, only the next morning.) But as I played with my results, I soon discovered that my new toy had some serious drawbacks. When I used it to calculate income and price elasticities—which were needed for the model—I discovered that the estimated price elasticities from the linear expenditure system were close to being proportional to the income elasticities, a regularity that is supported neither by intuition nor by the theory. It was a terrific idea to use the theory very directly to build an empirical model, but the theory here was doing too much, and the model was not as general as the theory allowed. The solution to these problems was to come through the concept of a *flexible functional form*, proposed by Erwin Diewert in 1973; in my own work, this line of research was to culminate in the "Almost Ideal Demand System" that John Muellbauer and I proposed in 1980 as our own favorite flexible functional form. That model is still very widely used today.

Not long after I joined the Department of Applied Economics, Cambridge University changed its rules so that researchers in the Department could obtain a PhD by submitting the research that they were paid to do, a terrific arrangement that suited me perfectly. By the mid-1970s I had a published book on demand systems and a paper on how to run horse races between various then popular demand systems (published in *Econometrica*, and which was later to win the Econometric Society's first Frisch medal), and my PhD was duly awarded, but not until I had passed a terrifying oral exam. Cambridge required that oral examiners not be supervisors—not that there was much supervision in those days—and PhD theses—including some subsequently famous ones—were not infrequently failed without the possibility of resubmission.

Around this time, I had been befriended by W. M. (Terence) Gorman, then professor at the London School of Economics, who somehow managed to sniff out and make contact with anyone who was using duality methods. Terence was an outstanding theorist who saw the task of theory as providing models and methods that made life easier for applied analysis—I think the mold for that kind of theorist has been lost. He seemed to know more about everything than anyone else, but had a charming if occasionally terrifying way (he was one of my oral

examiners) of assuming that it was *you* who knew everything, and if you couldn't understand him, it was because he had expressed himself with insufficient subtlety and sophistication, setting up a divergent cascade of misunderstanding. I wanted to understand two-stage budgeting, and Terence had worked it all out in a paper in *Econometrica*, but I found this incomprehensible. I was determined to get to the bottom of it, and locked myself away for a week to think and to figure it out. At the end of the week, I understood no more than at the beginning, though I was a good deal more frustrated. Terence had understood very early on that the dual representation of utility—where utility is expressed, not as we had all learned it, as a function of quantities, but as a function of prices and income—allowed an intimate and direct connection between the theory and the data. These methods were spreading quickly at the time, particularly through the work of Dan McFadden. One link with Dan came through John Muellbauer, who had done his PhD with Bob Hall at UC Berkeley, and had learned duality from Bob who, in turn, had learned from Dan.

So when John came back to England, we discovered that we had much in common, and knew a lot of things that seemed both tremendously useful and not widely understood. So we wrote *Economics and Consumer Behavior* to explain it all. John and I were ideal complements; he was careful, sometimes even fussy, and with the stronger theoretical bent: he had been publishing rapidly since coming back from California, and he had lots of important, unpublished material on which we could draw. I was less careful, impatient to get on, had a good sense of what was important and what was not, but regularly needed to be pulled back and made to think harder. The book was published in 1980, and 30 years later still sells a remarkable number of copies. I think of it as a synthesis of Dan McFadden, Terence Gorman, and Richard Stone. It tried to lay out a vision of how theory could be taken directly to the data, and modified or refuted depending on the results, with the whole thing leading up to an integrated view of policy and of welfare economics. Looking back, I realize how naive we were, but I see no reason to modify my view that this is what we would like to achieve, even if the goal is a good deal more elusive than it seemed to be with the confidence of youth.

Moving westward, in stages

The early 70s were a time of university expansion in Britain, and a great time to be a young economist. In Cambridge, I often played tennis with Mervyn King--currently Governor of the Bank of England and a member of the Wimbledon Lawn Tennis and Croquet Club--and we would relax on the lawn afterwards and work ourselves into a lather over the fact that no one was offering us professorships, professorships that only a few years before, were grudgingly handed out to (sometimes long deserving) aspirants in their late-50s and early 60s. (Most British departments then had only one or two professors.) In the event, neither of us had long to wait, and I accepted the Chair of Econometrics at Bristol before my 30th birthday--rather old in those years. I loved Cambridge but, except for the Bank of England, I had been there since undergraduate days, and a Chair meant much more money, which I badly needed. Mary Ann had died of breast cancer in 1975, and I had two children under five: it was time to move on.

It was during my time at Bristol that John Muellbauer and I worked together on our book. The computer facilities at Bristol were terrible—the computer was a mile away, on top of a hill, so that boxes of punched cards had to be lugged up and down. I was told to get a research assistant, which was sensible advice, but I have never really figured out how to use research assistance: for me, the process of data gathering—at first with paper and pencil from books and abstracts—programming, and calculation has always been part of the creative process, and without doing it all, I am unlikely to have the flash of insight that tells me that something doesn't fit, that not only this model doesn't work, but that all such models cannot work. Of course, this process has become much easier over time. Not only are data and computing power constantly and easily at one's fingertips, but it is easy to explore data graphically. The delights and possibilities can only be fully appreciated by someone who spent his or her youth with graph paper, pencils, and erasers.

Given how far it was up the computer hill, I substituted theory for data for a while, and wrote papers on optimal taxation, the structure of preferences, and on quantity and price index numbers, but I never entirely gave up on applied work. Martin Browning had come to Bristol for his first job, and we worked together on life-cycle labor supply and consumption. This led to some good ideas for combining time-series of cross-sectional surveys to generate true panel data, and this remains some of my most cited methodological work.

While still at Cambridge, I had met Orley Ashenfelter at a conference in Urbino, and he invited me to visit Princeton for a year, which I did in 1979 and 1980. A year later, he came to Bristol as a visiting professor, bringing a young Canadian graduate student from Princeton, (and subsequent John Bates Clark winner) David Card. Faced with Bristol's hill top computer, and its limitations when you got there, Dave didn't last long, and fled back to the US, only to be denied admission at the border, and deported to Canada. The Bristol department was outstanding in those days, with a bevy of future stars but there were difficulties beyond the computing facilities, especially when Mrs. Thatcher cut the University's budget. This prompted an understandably bitter discussion to decide which "tenured" faculty members were to lose their jobs. The endless penny-pinching began to make it very difficult to work.

Compared to Britain, Princeton seemed like a paradise awash in resources, so when I was invited to return on a permanent basis, I gratefully accepted. In spite of an increase in bureaucratization over the years, Princeton remains a wonderful environment in which to work, even after the financial crash of 2008; never in 30 years have I felt that my work was hampered by a shortage of funds. Princeton is close enough to Washington and New York so as not to be isolated from finance and from policy, but sufficiently withdrawn to have an element of the ivory tower, and to be insulated from the waves of fashion that sweep all before them in hothouses like Cambridge, Mass. And it is a terrific university, both for undergraduates and graduates. Both of my children went to Princeton, one as a math major, and one as an English major (like many of their cohort, both now work in finance), and the breadth and depth of their experience was much superior to what I had in Cambridge. By the time they graduated, they were immeasurably better educated than I had been at the same age.

Princeton was everything I had hoped for. I taught the first course in econometrics to the incoming PhD students, a class that in my first years had a stellar bunch of young economists, including several superstars in the making, Princeton also attracted outstanding new PhDs as Assistant Professors, one of whom, John Campbell from Yale, shared an interest in consumption and saving. I remember the two of us happily wandering off to the Engineering Library to try to find out about the spectral density at zero and how to estimate it.

Alan Blinder and I wrote a Brookings Paper on saving, and as a result of that, I began to think about the time series properties of consumption and income. I realized, after a lot of agonizing and checking my imperfect understanding of time series analysis, that one common version of the representative agent permanent income model made no sense. The permanent income hypothesis says that consumption is equal to permanent income defined as the annuity flow on the discounted present value of current and future earnings. The relative smoothness of consumption—the pro-cyclical behavior of the saving ratio—then follows from the fact that permanent income is smoother than actual income. But the time-series people had done a pretty good job of showing that aggregate per capita income was stationary only in differences, and that the differenced process was positively autocorrelated. This implies that permanent income is *less smooth* than measured income; growth shocks, far from being cancelled out later, are actually signals of even more growth to come. Of course, it is only the representative agent version that has this disturbing property, and one of my students, Steve Pischke, figured out that with a proper micro model, in which there is a plausible model of what consumers can actually know, something like a standard view can be restored. But this work taught me something important, that representative agent models are as dangerous and misleading as they are unrealistic.

Developing interests

Before coming to Princeton, I had started thinking about economic development, and had spent a summer at the World Bank helping them think about their Living Standards Measurement Surveys, which were just getting under way in the early 1980s. Senior Bank researchers had become concerned about how little was known about poverty and inequality in the poorer countries of the world, and felt that a household survey program was the answer to a better system of measurement. Arthur Lewis had just retired from Princeton when I arrived, but was still around, and he was supportive of my first steps in economic development, even though my approach was so different from his own (for reasons I never quite understood, he always referred to me as “chief.”) To the end of his life, he remained bitterly disappointed that the economics profession was so little interested in why most of the people in the world remained so desperately poor, and what might be done about it. He felt that his own work had failed to set in motion the professional effort that global poverty required. Another Princeton economist, Mark Gersovitz, who was a great admirer of Arthur’s, also became a mentor to me; he generously shared his knowledge of the economics of poor countries, on almost all aspects of which Mark had made important contributions.

My new interest in household surveys turned out to be a lasting one, eventually leading to my 1997 book on *The Analysis of Household Surveys*, which focuses on developing countries, and

has lots of examples of useful and interesting things that can be done with such data. It also covers the basics of household survey and design, which had long dropped out of courses in econometrics. Students in economics are rarely taught about how the design of a household survey might be relevant when they come to analyze it, and one of the aims of my book was to fill this gap, as well as to discuss some of the practical issues that arise when standard econometric methods are applied to household surveys, especially from poor countries. I was fortunate that this book coincided with a revival of interest in development economics, especially microeconomic development, as well as with a rapid expansion in the availability of household data from around the world, so that it has been widely used.

The book was published by the World Bank, with whom I have continued to work over the years. One of the dangers of being an academic economist is that it is easy to wander off on a trail that becomes narrower and narrower, intellectually exciting perhaps, but of interest to very few. For me, the World Bank has been a constant source of interesting topics that are of substantive importance, at least to some people. Of course, most of the problems that come up are too hard to expect real progress, but occasionally a problem comes up where I feel like I can do something, even if it is just clarifying a misunderstanding. In this way, talking to people at the Bank has helped keep me grounded as an applied economist.

Confirmations and refutations

One of my most fruitful collaborations at Princeton was with Christina Paxson. She had done her PhD at Columbia in labor economics, out of frustration at being unable to study development. So we became development economists together, and collaborated on a wide range of topics. We looked at life-cycle saving, showing that it is impossible to argue that the cross-country correlation between saving rates and growth rates comes from the life-cycle story according to which the young, who are saving, are lifetime richer than the old, who are dissaving. There is just not enough life-cycle saving to account for the size of the relationship.

We also argued that, if individuals are independent permanent income consumers, the accumulation of lifetime shocks will cause people's consumption levels to drift apart with age, whether or not their earnings do so. If a high school class reassembles for its 25th class reunion, the inequality in their standards of living will be much larger than was the case when they graduated. This was one of those nice but too rare cases where a prediction that comes out of the theory, whose empirical validity is unknown in advance, turned out to be confirmed in the data. Of course, there are other possible explanations, for example that consumption is more closely tied to income than the permanent income theory supposes, and that the spread of cohort earnings increases as the cohort ages, because people get different opportunities over life, because they make different use of them, and because these advantages and disadvantages accumulate over time. Yet the key insight is still the same, that outcomes depend (at least in part) on the accumulation of luck, which drives ever expanding inequality in living standards within a fixed group of members as they age. Inequality in wealth is driven by a process that accumulates an accumulating process, and grows even more rapidly, another prediction that turns out to be correct.

Chris and I also wrote about household economies of scale and their effect on the consumption of food. Economists have long used per capita income as a measure of welfare—for example in calculating poverty or inequality—but this can't be quite right. For one thing, the needs of adults and children are not the same. But even when there are no children, household economies of scale imply that larger households are better off than smaller households at the same level of per capita income. Some goods are public goods within the household—housing itself, heat, cooking of meals—and the need for them expands less than proportionately to the number of household members. With the same per capita income, larger households can substitute away from such goods towards the more private goods, such as food, especially in poor countries, where food needs are often very far from being met. Yet Chris and I found something very odd, which is that, holding per capita income constant, larger households spend less per member on food. And we see the largest *reduction* in food consumption precisely where we would expect to see the largest *increase*, among households in the poorest countries for whom a large share of additional resources go to food.

The food and household size puzzle remains largely unresolved yet it is perhaps linked to another paradox that I have recently investigated with my friend Jean Drèze. (Jean is responsible for almost everything I know about India. He is a scholar and social activist, living without apparent means of support, whose work and writings have had an unparalleled effect on policy.) In India today, which has been and is experiencing historically high rates of economic growth, we see another very strange food-related fact, which is that per capita calorie consumption has been *falling* for the last two decades. This is happening in spite of rising per capita incomes, even among the poor, and in spite of the fact that Indian men, women, and children suffer one of the highest rates of physical malnutrition in the world. Indian adults are among the shortest in the world, and Indian children display higher levels of stunting and wasting than in much poorer places in sub-Saharan Africa. Jean and I suspect, though it is far from proven, that the reduction in calories is a consequence of a reduction in hard physical labor, which is largely fueled by cereal consumption. This contention turns out to be politically sensitive in India, where some argue that the fall in calories is an indication of unmeasured immiseration, driven by the supposed horrors of globalization.

In the mid-1980s, my friend Guy Laroque was spending some time in Princeton, working with my colleague Sanford Grossman. Guy and I had known each other for a decade, and we had jointly organized an Econometric Society meeting in Athens in the 1979, I as the econometrician, and he as the theorist. Sandy Grossman was always short of time, so that Guy had a lot of free time on his hands during his visits to Princeton—where he would usually stay at my house—so we got to talking about an issue in which I had become interested, which is why primary commodity prices behave as they do. I had been thinking about the economies of sub-Saharan Africa, many of whose macroeconomic policies are dominated by enormous fluctuations in commodity prices. In the mid 19th century, Egypt had become fantastically rich from the high prices of cotton that resulted from the American civil war, and had then gone into receivership with Britain during the subsequent collapse, a story that was repeated (with variations) many times subsequently. Nor were outside authorities very good at advising countries how to deal with the problem. During the 1970s, as the world price of copper

collapsed, the World Bank kept increasing its forecasts of future prices, driving countries like Zambia deeper and deeper into difficulty.

Guy and I wrote a number of papers on our findings, which in the end, were remarkably slim. There is a theory of speculative commodity demand and storage, first developed by Ronald Gustafson in Chicago in the 1950s, and later developed by Joe Stiglitz and David Newbery in the 1970s, and Guy and I turned this into something that could be taken to the data, only to discover that it could explain very little of what we could see. This is another of these irritating but frequent puzzles. We have a long-established theory—whose insights are deep enough that *some* part of them *must* be correct—which is wildly at odds with the evidence, and where it is far from obvious what is wrong, or how the theory might be amended to give us a better handle on the mechanisms at work.

One thing I try to do is to find new implications of old theories, and more specifically some implication that permits a relatively straightforward confrontation between theory and evidence. Ideally, such a prediction can be tested by something very simple, like a cross-tabulation of one variable against another, or a straightforward graph, if only one knows what to tabulate or what to graph. This method makes the investigation and manipulation of the theory do the work that is often assigned to econometric method, and it avoids at least some of the econometric controversies that abound when inadequately developed questions are taken to the data. Whenever I have managed to do something like this, I have been better at getting refutations, or generating puzzles, than in getting interesting confirmations. Indeed, I can remember only two clear cases of the latter. One is the consumption inequality story. The other was in the early 1970s when Britain and other countries were experiencing a burst of high inflation, and I argued that consumers, who are buying goods one at a time, not an index of all goods, have no immediate way of distinguishing unanticipated inflation from relative price increases of the goods they happen to be buying. In consequence, unanticipated inflation will cause a short-run increase in the saving ratio. This was quite contrary to what most people thought would happen, yet was quickly confirmed, not only in Britain, but in a range of other countries.

There is something very exciting about making a theory-based prediction that is not at all obvious—especially if it seems obviously wrong—but which turns out to be true in the data. Yet what happens after that is by no means assured, depending among other things on whether other explanations—even if developed *ex post*—are judged to be as or more convincing. Even refutations, although less elating at first, are usually productive, because they lay the platform for subsequent emendation and redevelopment of the theory, so that there is at least a possibility of progress. Indeed, if the theory is one that is heavily used in our normal thinking about the world, refutations and emendations may be more productive than the confirmation of a new theory that is less deeply embedded in our understanding.

One of my standard ways of finding good research topics—though one that is not easily taught or passed on—is to “play” with models and data until I find something that I don’t understand. It is nearly always the case that this lack of understanding, or the sense of a paradox, is only apparent. That two ideas, both of which seem correct, are mutually inconsistent is nearly

always because I don't understand one of them. Or if some data don't seem to support the earlier results, it is usually because I have misunderstood the earlier findings, or because I have made errors in the calculation (something that is much more frequent in applied work than is commonly recognized.) But one time in a hundred, the misunderstanding or paradox is not just mine, but is more widespread, and that is gold that is worth the prospecting. I have also learned to trust my instincts about empirical findings that seem to me to be absurd. Either the supporting work is wrong, or there is something I don't understand. One example is my work on the Wilkinson hypothesis, which claims that income inequality acts as pollution in the social atmosphere and undermines the health of all who live there. The evidence in favor of this proposition turned out to be a web of bad data, selective reporting, and wishful thinking, but in showing that, I came to understand much about the insidious effects of inequality more generally, especially of the extreme (and today expanding) inequalities that separate the very rich from the community in which they live.

For good measure

Measurement is not much of a focus in economics today. Even the obligatory course on national income accounts that used to be the first thing encountered in macroeconomics courses is no longer much taught, nor are students exposed to the construction of index numbers. Academic economists spend a lot less time with the creators and producers of data than once was the case, to the detriment of both groups; economists often do not understand the data they work with, and the evolution of national income accounting practice has taken place without much input from academic users. Yet much of what we think we know about the world is dependent on data that may not mean what we think they mean, or that are contradicted by other data to which, for no very well-developed reason except habit, we give less weight. One example that has much concerned me is the inconsistency between national accounts data and household survey data that is encountered in many countries. Only some of the differences are attributable to differences in definition, others are to an under-researched *mélange* of errors in the surveys—misreporting, coverage, or something else—as well as weaknesses in the national accounts data. There is no basis that I can see for the usual view that the national accounts are correct, and the survey data are wrong. For example, there is little doubt in my mind that the Indian national accounts overstate Indian growth rates, not through conscious manipulation in any sinister way, but because the whole apparatus is shaky and outdated, and certainly not built to work well in a rapidly growing and changing economy.

India has been a continuous source of fascination. For anyone of my age who grew up in Britain, India was the magic tropical kingdom, and (along with Robert Louis Stevenson's South Seas) the perfect imaginary contrast to the dreary grey and cold of Edinburgh. And like all schoolboys of the age, we were brought up on (one-sided) stories of Empire. Years later, India has become the exemplar, not of imperial glory, but of global poverty, and of the hope that economic growth might one day do away with it. My work there has focused on price indexes, and on how they affect measures of poverty, and I have worked with the Government of India to improve their own poverty measures. Much of this work has been with Jean Drèze; it is a

constant challenge to keep up with his skepticism, ground level knowledge, and technical knowledge of economics.

I have also been involved with the International Comparison Program (ICP), which originated at the University of Pennsylvania in the 1970s, and which is now run by a global consortium headed by the World Bank. Almost everything that we know about the empirics of economic growth, global poverty, and global inequality depends on estimates from the ICP, which is essentially a giant price collection enterprise, gathering millions of price quotes on closely comparable goods from almost all the countries of the world. These prices are turned into a system of price indexes (purchasing power parity indexes) that can be used to convert each country's national accounts into an internationally comparable currency. The technical advisory process for the ICP involves an uncommonly diverse and interesting group of national income accountants, statisticians, and economists, as well as numerous subject specialists—construction, housing, etc.—who try to solve an infinitude of practical and theoretical problems that underlie the definition of prices, as well as the calculation of index numbers. Alan Heston, who worked with Irving Kravis and Bob Summers on the first ICP in 1978—covering less than a dozen countries—remains active in this process, bringing to it more than 40 years of experience, as well as the world's deepest understanding of national accounts and price measurement. Working with him has been an education in itself.

Looking back, looking forward

One is only asked to write an account of this kind once a certain age has been obtained, and while it is certainly good to be asked, there is something of an obituary quality about the enterprise. This makes it seem somehow inappropriate to write about future work, or even about currently unfinished work. Yet, I don't feel any differently about my current work than about my past work. In particular, I have had the good fortune in recent years to have the office next to Danny Kahneman, whose knowledge, curiosity, and interest in learning and changing his mind are models for how to prolong a long and distinguished career. Over the last decade or so, he has been working with the Gallup Organization to collect data, in the United States and around the world, on how people evaluate and experience their lives. There are wide open and important questions of what the various measures of "happiness" really mean, and the extent that they can and should be used for policy and welfare economics. I do not know whether a new welfare economics can be built around such measures, but working with Danny on these issues has taught me much, and we are making progress on the difference between life evaluation and hedonic experience and on the distinct ways that each responds to income. The old question of whether money buys happiness turns out to have a complicated answer. As always, working with someone from a different tribe can be immensely frustrating—we can spend an enormous amount of time on what subsequently turn out to be non-issues—as well as immensely rewarding, as when I realize that there are completely different ways of thinking about phenomena about which I had thought my views were long settled.

Age brings mental and physical deterioration, but if the former can be temporarily held at bay, it also brings a perspective from having seen the roundabout go past so many times, and

from having seen and thought about the earlier incarnations of current enthusiasms. I have been recently writing about the current wave of randomized controlled trials in social science. These are often useful devices, but they are currently being used in what seems to me a non-scientific way, not as a complement to theory, enabling its empirical investigation, but as a substitute for it. I think that this is a problem, not only in economics, but also in medicine, where the randomized trial often rules as the only provider of acceptable evidence, in spite of many thoughtful and sometimes devastating critiques over the years by statisticians, physicians, and philosophers. Indeed, to my way of thinking, the turn to randomized controlled trials, or substitutes like instrumental variables or regression discontinuity designs, is a symptom of a deeper malaise, which is the seemingly ever widening gulf between applied work and theory. It seems a long time since the early 1980s when econometric and economic theorists, as well as applied econometricians saw themselves as working on different parts of what was clearly the same enterprise.

When I was starting out in Cambridge, the Econometric Society played an enormously helpful part in my career, and in those of my contemporaries, not just those of us who were econometricians or theorists, but for anyone who was doing quantitative applied work. *Econometrica* published a good number of applied papers that were worth emulating, and the society held summer meetings in different European cities every year. Those meetings catered to a “broad church” of economists, and gave us a chance to meet and to be met, and to present papers before a wide audience of European and American economists. In those days—the early 70s—there was almost none of the networks or networking that is so important today, and of course no internet, so that getting working papers was very much a hit and miss business. So the Econometric Society played a vital role in building European economics. In both North America and Europe, an Econometric Society fellowship was a mark of having become a full member of the profession, usually coming at around the same time as tenure in a good department.

I believe that the Econometric Society still plays something of this role in Europe, but it otherwise seems to have become much less important, at least for applied work. In large part, the Society’s earlier European role has been taken by the National Bureau of Economic Research, which after Marty Feldstein became President, became a central focus for networking in applied economics, with similar organizations appearing later in Europe. Like many in the profession, I owe a great deal to Marty and to the NBER, which has frequently been the venue for trying out new work and new ideas. It was through the NBER, through David Wise’s aging program and the entrepreneurship of Richard Suzman at the National Institute on Aging, that I owe my interest in health. In recent years, I have also had a fruitful relationship with the American Economic Association. I had the honor to act as its President in 2009, and over many years of meetings, I was part of the movement to expand the Association’s role in publication. This eventually came to fruition with the publication of four new Association journals, all of which have terrific editorial boards, and all of which are publishing excellent papers.

It has been a good time to spend a life in economics. Compared with many others, the profession is remarkably open to talent, and remarkably free of the nepotism and patronage

that is common in professions in which jobs are scarce. It is also a profession that, deservedly or undeservedly, is very well-rewarded. The best gifts of a profession are the people it brings, to talk to, to work with, to be mentored by, and to make friends with. I have been truly fortunate in this respect. Princeton has provided me with extraordinary students, not only in economics, but in the Woodrow Wilson School, which brings masters' students with multiple gifts, interests, and experiences; working with them is a constant joy and inspiration. Many of my oldest and best friends, many of them also mentors, have come to me through economics. Through economics too, I met my wife, Anne Case, and our personal and professional lives are almost entirely integrated; Anne is my critic, my colleague and coauthor, and my friend. In many, if not all respects (our lives are faster, more interconnected, and it is much harder to have dinner parties every night without servants) we try to lead the ideal academic lives that I had first glimpsed and admired in Cambridge forty years ago.