Jim Mirrlees Memorial, Nuffield

Peter Diamond

1. **Outline of 5 topics**

There are five talks today, which we have coordinated by topic. I will talk about my experience of joint research with Jim on taxation and pensions. Then, Nick will talk about Jim's work on development and growth, including their joint papers and the Little-Mirrlees analysis. Also Nick will discuss Jim's role in Oxford. Jim Poterba will discuss the impact of Jim's work on public economics, and provide more on Jim's role in Nuffield. Mike will discuss the impact of Jim's work on the "economic theory of incentives under asymmetric information," for which Jim received the Nobel prize. And Richard will discuss the Mirrlees Review.

2. **Talk**

I first met Jim in 1964 at the Econometric Society meetings in Zurich when Christian Weizsäcker invited both of us to lunch at the home of a cousin of his. 50 years later, while the three of us were at a conference in Lindau, Christian again took us to lunch – at the home of a different cousin. The two lunches mark one of the three ways we got together – my visits to him, his to me, and conferences.

I spent most of 1965-6 at Churchill, and Jim and I became good friends. The intellectual atmosphere at the time had economic growth as pretty much the hottest topic for theorists. We had both written about it, but did not pick up on that topic jointly. Also in the air at the time was increasing interest in uncertainty, particularly how it related to economic equilibrium, that is, to overall outcomes, not only to individual behavior. Throughout his career,

Jim's interest in uncertainty extended widely across the range of topics he worked on.

And we were both interested in public finance. Richard Musgrave's graduate text was recent and a central focus for economists interested in the subject. Central to the book was Musgrave's division of public economics issues among three topics – economic efficiency, income distribution, and stabilization, each analyzed on the basis that the other two were being optimally solved, and so incorporating little, if any, overlap across topics. Also in the air was a very different framing from second-best theory, following the paper by Lipsey and Lancaster.

I returned to Churchill for the summer of 1967, giving a seminar shortly after arriving. I had rediscovered Ramsey's result on optimal taxes in a one-consumer economy and, at the same time, had found that aggregate production efficiency was part of the optimum. Of course, Paul Samuelson had promptly filled in my knowledge on previous writings. What was different in my analysis was that I had used a dual approach; that is, using prices as the control variables for describing the optimum. At the end of my seminar, Jim came up to me to say that since the price vector was the same for every household, the equations I derived could be interpreted as applying to a many-person economy. I proposed that we work on this together. We did, ending with the pair of papers "Optimal Taxation and Public Production, 1 and 2."

That summer, we made great progress on this topic. Much of the math was straightforward, but took time to get right. A few bits were difficult and showed, no surprise, that Jim was a better mathematician than I was. It was a pleasure to work with Jim, we often worked sitting side by side, the

interactions flowed very smoothly and our friendship became closer. As I had married since leaving Churchill the previous year, Kate became part of the friendship of the two families and we spent family time together as well as working on the paper. We made rapid progress and both of us presented the joint paper at our separate 1967 Econometric Society meetings – Jim in Blaricum and me in Washington. And then we submitted the paper to the AER. The referee process took nearly a year, but, as I remember, the acceptance letter merely asked us to consider the two referees' suggestions. Some of them were very helpful.

Coordinating a rewrite was much more difficult in that pre-internet time. After a delay we resubmitted the paper, and I was surprised that the new editor of the AER sent it out for a full fresh refereeing, which took roughly another year. At that point the editor offered to publish the paper if we would cut the length in half. Of course, this was not an option we were willing to consider. By that point, two separate journals had offered to publish the paper without further refereeing, in the next available issue. Jagdish Bhagwati was on the AER editorial board and spoke with the editor, who came back to us with a different offer – divide the paper into two parts and he would publish in successive issues. This was to avoid what he considered was using too much of a single issue on a single paper. It took some time to rewrite in order to fit that goal, and the papers were published in 1971, just over 3 years from the initial submission.

At this point, I was eager to continue working with Jim and we continued the earlier pattern of visiting each other's universities. Jim was at MIT in the spring of 68. I was in Oxford in the summer of 69, including a memorable month with the two families on the Mull of Kintyre. Jim at MIT in the fall of

70. I was in Oxford for 1973-4 and Jim at MIT for the spring of 76. Also in this period, we co-taught a Jahnsson course in Finland on public economics. This involved planning a curriculum for a year-long course and having two two-week intensive lecture periods, with us being together in Finland. For the spring session our families were there as well. At one point, we briefly considered writing a text. We went as far as constructing a table of contents and deciding who would do the first draft of each chapter – as, when apart in that pre-internet time, we normally passed manuscripts back and forth in full, rather than bouncing smaller pieces off each other. We looked at the magnitude and nature of the task of a text and both promptly concluded that we did not want to do it.

During the 70s, we finished 5 more papers that appeared in journals, and submitted a sixth. After this intense period, the interactions slowed down, reflecting family reasons limiting travel and our additional separate interests – Jim's in principal-agent models with asymmetric information and mine in search theory. But we continued to have ongoing work, in slow motion and primarily used for conferences. So, the period of intense collaboration ended after roughly 15 years, without stopping working on something together for the next 35. Indeed, there are still two papers part way through the refereeing process and a manuscript by Jim from 1979, that I did not get started on.

I want to mention our writing process on two more topics. I don't remember the occasion, but I was in Oxford for a visit, and Jim gave me the draft of a paper he had written analyzing optimal shadow prices, prices that could be used to plan optimal government production. Shadow prices played a central role in Jim's development work with Ian Little. Jim's analysis was in a specific special model. I had also written a paper on this subject, also in a

special model, but a noticeably different one. Our both finding exactly the same result in two very different models suggested that there was something more general. The result was that using the optimal shadow prices to calculate the profits of a constant returns to scale industry should show zero shadow profits. This result gives a shortcut for calculating some shadow prices and a way to check the consistency of others. I recall going up to Jim's rooms in Nuffield with this comparison of our two papers. And leaving about two hours later, with our having essentially finished the paper "Private Constant Returns and Public Shadow Prices." What struck me that afternoon was a sense that the functioning of our minds showed an optimal distance between them. If we thought the same, we would merely be dividing up the work by collaborating. Thinking too differently would require sorting out how to communicate with each other and how to frame what was the research topic.

I showed a draft of today's talk to Bob Solow. He suggested addressing how we dealt with differences in opinion. That triggered one fond memory, although I do not remember what paper was involved. We differed on the strength of inference that should be drawn from some finding. We satisfied both of us by using a sentence with somewhat different meanings between American and British English. Despite probing my memory repeatedly, I have not come up with any other example of a difference of opinion arising in our work.

Starting in 1974, I became involved in analyzing US Social Security policy. An issue was how much to increase the benefits of a retiree who delayed the start of benefits. The actuaries told me it should be actuarially fair. I wondered what an optimal tax approach would show and suggested to Jim we work on this together. The question fit well with Jim's work on principal-agent models

with asymmetric information. We wrote four papers on this, the first published in 1978. And he discussed the topic in his European Economic Association Marshall Lecture, published in 1995.

Our opening attack on the problem had all workers the same ex ante, but subject to a stochastic termination of the ability to earn. The government did not try to measure the ability to work. This could be assumed to be a consequence of an inability to measure due to asymmetric information or simply a government choice, given that attempts to measure ability to work would have both type one and type two errors. Such measurements, with errors, are common with disability programs, while not being made with retirement programs. And sometimes, the ages for the two programs overlap.

The optimum provides insurance against an early loss of the ability to work, financed by lowering compensation below the marginal product. The analysis was simplified by the assumption of a fixed labor supply by those working; that is, only an extensive margin. The optimum kept workers just willing to continue work through rising compensation with continuing work. The model had government control of consumption through observability (and taxation) of savings.

A second paper assumed that the government could not observe individual private savings. While the optimal compensation path was different from that with observable savings, worker indifference to continued work remained a property of the optimum. And Jim did calculations of the levels of optimized social welfare with and without observability of private savings, in order to see the impact on optimized social welfare of an inability to control savings. I recall the difference being smaller than I expected. While we were focused on pensions, these papers can be considered as an early part of the dynamic

public finance literature. This second paper was rejected by Econometrica in 1982, after four years of refereeing. It is still unpublished, despite having been accepted by the Journal of Public Economics, because of still needing updating for how the literature has advanced. I have kept returning to it, encouraged by Jim, but without quite finishing. I still haven't given up. Another published paper in this line of work considered only a payroll tax of earnings. In 1979, Jim drafted a paper with heterogeneous disutility of work. In contrast to this line of work, none of the other issues we worked on for conference volumes led to our sustained attention.

As far as I am aware, with one exception, Jim's interest in pensions did not move from developing theory in order to inform policy to detailed policy recommendations. The exception followed from Jim's role in the China Economic Research and Advisory Programme, which organized a panel in 2004 to consider reform of Chinese pensions. Jim was to be part of the panel, which I happily joined. We presented our report to Wen Jiabao, the Chinese Premier, and various ministers. At the time, the government was partial to the approach focused on funded individual accounts, along the lines of the Chilean reform and recommended and strongly pushed by the World Bank. Eventually the Chinese government came around to the Panel's view that a "notional account" approach was better. This approach, pioneered by Italy and Sweden, uses a defined-contribution vocabulary to design and present a partially-funded defined-benefit plan. Individuals have accounts with a stated value. The value is increased by contributions and by an annual rate of return with a rule set by law, reflecting wage growth. At retirement, the account value and a measure of life expectancy are used to set an annuitized retirement benefit in a quasi-actuarial way.

Sightseeing with Jim was always fun. When Jim and I and other participants in this panel went sightseeing together, we were led by Jim's wife Patricia, with her extensive knowledge of Beijing. This was the occasion when I first met Patricia.

I want to say a few words about Jim from the perspective of friendship. It was always a pleasure to be with Jim, whether we were working, sightseeing, hiking, or just hanging out. He was always calm, cheerful, and engaging. He had a wide span of interests, particularly including music and literature. He was very observant and insightful as to the intent of others.

On this, I want to read a very short anecdote written by my wife Kate, who was originally Priscilla. She asked me to read it, because it is so perfectly Jim. Here is her anecdote in her words:

> **It was in July 1972 and it was Jim's birthday. Gill and the girls had planned a picnic on the grass in some garden and had invited us. We were living at the time in Oxford in Aubrey Silberston's apartment and to get to the picnic, we had to take a bus and walk a bit. Although it was an informal event, I chose to wear a long dress, to celebrate Jim. It was a simple dress, jersey fabric, no décolletage or anything startling. It was just down to my feet. From the time we left the house, walked to the bus, rode the bus to the destination, I was an object of curiosity in my odd garb – it was odd then, though it wouldn't be now. Gill and Katie and Fiona welcomed me in some surprise, although, being wonderfully tactful, made no comment. Jim greeted us, got us comfortably seated on the ground, then turned to me and said quietly "Thank you for the dress."**

He understood and appreciated that it was to celebrate him. …He was a gentleman to cherish.

In that summer of 1972, while Kate and I were visiting Oxford, we were waiting to get to the top of a list to adopt. We had informed the agency of our travel and location, but they had not informed the social worker we were connected to. He called our house in Cambridge, was quite cross and informed our tenant that we had 24 hours to show up in Boston or the baby would go elsewhere. Fortunately, our tenant was TN Srinivasan, who rushed to MIT, where he learned of Jim's phone number from Nick Stern. While Jim could have simply telephoned, around ten that night, he knocked on our door, to deliver the news in person. We reached the social worker and negotiated an additional 24 hours, scrapping our plan to join up with Jim in France. Jim was going there to work with Ian Little, who expressed relief that I did not appear and possibly get in the way of their work. The connection between Jim (and Nick and TN) and our son Matt has always been part of our family lore.

Jim and I spent some, but not a great deal of, time together in recent years. We were together at Jim's 80th birthday party, at my younger son's wedding in Bali, in conferences in Lindau and Singapore, and in my few stops in Hong Kong. I cherished the time we did have together. I miss him, a real friend and a force for good.

I want to close with a small item on Jim's role in the profession, knowing you will hear much more from those who saw him in action in Oxford. Jim's reputation for good judgment was outstanding. Back when transatlantic travel was not part of the job market, the MIT department heard from Jim of a student we should hire. Fortunately, I was heading for Oxford for a brief visit with Jim. To confirm Jim's judgment, my task was to interview Kevin

Roberts. Halfway through our scheduled chat, I reached into my pocket and pulled out a letter from the department head, E. Cary Brown, offering Kevin a job. Happily, he came to MIT, although he did not choose to stay around as long as we would have liked.[1]

[1] Kevin moderated the session where this was presented.