A Conversation with Murray Kemp

William Coleman*

Abstract: Murray Kemp recalls his decision to study economics, his acquaintance with some founders of Australian academic economics at Melbourne University in the 1940s, his original intellectual influences, his choice of graduate school, his experiences at Cambridge, his difficulties in returning to Australia, his original Keynesian affiliation, the role of Haberler's writings in his 'conversion' to neoclassical economics, his dissatisfaction with the revival of partial equilibrium trade theory, his mathematical style, his research that he judges to be the most successful, and his views on Australian academic life.

Murray Kemp was born in rural Victoria in 1926. After securing a B.Com. and MA at the University of Melbourne, he obtained a Ph.D. from Johns Hopkins University in 1955 (Long and Wan 1998). He was appointed Professor of Economics at the University of New South Wales in 1962, and over the subsequent decades established a formidable reputation as Australia's pre-eminent economic theorist. Over a period of 54 years he has published, by one count, 237 papers and 17 books. By the ISI Web of Science measure of citations, he has been cited more in the post-1980 literature than any other Australian economist. These quantitative dimensions are matched by an interesting diversity of research. Kemp has published on very pure theory, as well as more topical problems. He is well known for his extensive work on neoclassical theory, but he has also made contributions to Post Keynesian theory. He has been credited with mathematising the syllabus of international trade, and yet one of his most discussed papers, and his most republished paper (Kemp and Wan 1976), contains not a single mathematical symbol.

He was interviewed in his Sydney home on 25 September 2003.

William Coleman: *I wonder if I might begin at the beginning with your grandfather's grandfather. Anthony Fenn Kemp: Rum Rebellion rebel, Church elder, republican, monopolist, chronic stirrer, father of 18 children, acquaintance of George Washington, who amazingly lived to the age of 95. How would you summarise the character of this ancestor of yours?*

Murray Kemp: I've always thought he was a bit of a rascal, but probably most members of the Rum Corps were of the same cut. He certainly wasn't an easy man to get along with, if we could believe even half of the anecdotes – and that's all they are – that have been handed down, a rascal.

Coleman: *Could we talk about your immediate family then, your father, your mother – where you were living?*

Kemp: My father and mother grew up in Melbourne. My father was the son of a schoolteacher, two schoolteachers in fact – father and mother. We moved about –

country town to country town, as bank managers do, working their way up. For a year I went to Williamstown High School, then my father moved me into a private school – Wesley College (St Kilda Rd).

Coleman: *A Methodist college. Were you a Methodist family?*

Kemp: No, that didn't count for much at least in those days. I was only there for a year when the school was taken over by the airforce….end of 1941 probably. We were invited by Scotch College to share their playing fields and classrooms.

Coleman: *You speak about sport; was that your orientation, rather than academic?*

Kemp: My father had been an elite footballer, AFL – played with Essendon. He'd been an army boxing champion. He played tennis, cricket and golf. I've taken up most of them, I've added a few – squash in Canada, surfing in Australia. Yes, we were taught at a very early age to play one sport or another.

Coleman: *So in your youth you were a keen sportsman, and your web page tells us that you have remained a 'fierce adversary' on the tennis court. I wonder if you have any thoughts on the connection between creativity and aggression? Apart from being a prolific co-author, a very sociable person (you've never been a solitary), is there some aggression there which helps you in your creativity?*

Kemp: You used the word 'aggression'. Could I substitute 'ambition'? Yes, once I realised I had a minor talent as a theorist I wanted to develop that talent as far as I could take it. I had felt the same way about sport, you're right – I wanted to be a good tennis player.

Coleman: *Not just to have fun or divert yourself – you wanted to become number one, at least you wanted to aim for that.*

Kemp: Not necessarily. I wanted to improve. Sport was never a full-time interest. My father had been fairly good as a sportsman. I enjoyed it. But I play still with colleagues – we even play to win.

Coleman: *Yes, but at school, you came first without bothering much about it.*

Kemp: In terms of academic work?

Coleman: *Yes, that came easily.*

Kemp: It did. I don't know how it happened. I was encouraged by the teachers, something to be gained by going to a private school. Teachers would see a spark. (My English teacher was A.A. Phillips. Ever heard of him? He coined the phrase 'cultural cringe'. That was the way he taught – 'You can do anything that an Englishman can do. Do it!'). At school, the masters continually referred to the time when I would go to 'the shop' [the University of Melbourne]. I was beginning to do well without really making a fuss about it. My brother and I both won the prize called Dux. Somehow we managed to drift to the top. So eventually I drifted off to the university. What should I study? I thought of medicine. I quickly found I wasn't

qualified to study medicine. I thought of law. Same problem – lack of Latin. You wrote prescriptions in Latin in those days and law required Latin, too. Finally I opted for economics, commerce, which I'd studied to some extent at school.

Coleman: *What was your impression of standards at the Faculty of Commerce?*

Kemp: Well, at the time, a young schoolboy had no notion of what standards might be. Most of the faculty had been taken away by war duties. Douglas Copland and L.F. Giblin were in Canberra.

Coleman: *And what's your impression of Copland?*

Kemp: Well I knew him better in Canada, much later. A very impressive man, a big man physically with a considerable presence. He just had to enter a room and conversation stopped and those present waited for him to say something. Yes, he was that kind of person. (You must remember that in those days there was one professor to a department. A 'God' professor; he was used to bossing people around. I'm speaking of any professor, in those days. They were strong people. Partly, I think, chosen for their personalities).

Coleman: *Giblin?*

Kemp: Quieter man, similarly impressive.

Coleman: *He would give you lectures, or just an occasional address?*

Kemp: Just an occasional address when he was in town; he came up to the campus to see his friends and they immediately collared him and dragged him into class.

Coleman: *Brigden also spoke to you.*

Kemp: He came along, one day, I think on a separate occasion. We knew him as one of the authors of the tariff report (Brigden *et al*. 1929), which for us was virtually a textbook in those days, a volume Australian economists can be very proud of, the greatest thing published in this country. He was a very quiet man, but subdued I thought because he wasn't well.

Coleman: *Can you remember what were the formal texts at Melbourne University?*

Kemp: In the principles course we used a textbook by Frederick Benham, an Englishman who had been in Australia in the 1920s and contributed to the tariff debate in the *Record*.

Coleman: *Did you read any 'classics' in your studies?*

Kemp: Oh yes, the curriculum was a very severe one. We read Marshall (*Principles*), Chamberlin, Joan Robinson, Triffin's book on imperfect competition (*Monopolistic Competition and General Equilibrium Theory*, 1940). We read the *General Theory* of course, which Copland had been teaching within a month of its appearance.

Coleman: *What was your impression of reading the* General Theory*? You're an undergraduate.*

Kemp: Yes, that didn't matter – you read the treatises on the subject, whatever they were. We read Adam Smith, Ricardo, Mill, Wicksell. Well, it wasn't a course in the history of thought. It was part of a micro course, but we didn't make these distinctions in those days. There wasn't such thing as a trade course. But every lecturer used an open economy model; that's how economics was taught in a country that depended so severely on trade.

Coleman: *Pigou?* Economics of Welfare*?*

Kemp: Certainly. Knew it well. [Laughter]

Coleman: *Did you know Jim Cairns?*

Kemp: Oh, I knew him quite well then, haven't seen him since.

Coleman: *Can we talk about your inspirations at the University, if I can put it that way. Both personal and intellectual. Perhaps we can begin with the intellectual ones. Cairns, several times in his memoirs has spoken of what an inspiration reading the* General Theory *was to him. What was the inspirational book (or books) to you in this period of time?*

Kemp: Well, I was influenced of course by all those great treatises that I've mentioned, including the *General Theory*, which we were taken through page by page, paragraph by paragraph, by Joe Isaac who later became a professor at Melbourne and Monash, member of the Arbitration Commission, and still going strong. But, looking back, I was influenced also by a book that wasn't on the reading list, a book entitled *The Clash of Progress and Security*, by A.G.B. Fisher, a New Zealander.

Coleman: *What appealed about this book?*

Kemp: By then I'd read Schumpeter's *Capitalism, Socialism and Democracy* and I recognised that Fisher had much the same ideas, ten or twelve years earlier. The notion that progress depends on incentives to invest, to spend on R and D, and that inevitably resulted in inequality and this is the dilemma that any society, capitalist or socialist has to face up to.

Coleman: *I'd like to ask you something about your Arts degree. What subjects did you do in your Arts degree?*

Kemp: Well, many wanted to do philosophy.

Coleman: *You did philosophy?*

Kemp: Philosophy was regarded as the queen of all disciplines in those days, it's a sad change that's taken place. I did extremely well; I was encouraged to go on.

Coleman: *Sydney Sparkes Orr was the Professor of Philosophy?*

Kemp: Not yet, he was just a young, terrible lecturer.

Coleman: *What about your mathematics? What did Melbourne give you with respect to your mathematics?*

Kemp: Well, Melbourne – the Commerce Faculty – gave nothing. But I realised that I couldn't go far in economics without getting at least what passed for mathematics in those days, some algebra and some calculus. I went across to the maths department and took the first-year maths course. During the war years we took national service, we chopped wood, harvested wheat, picked fruit. Anything to help out the 'war effort', and one year we went down to Anglesea on the south coast to chop wood. In charge of that camp was Major Potts, a teacher of mathematics. Yes, the mathematics master gathered us together one night and said, 'Well, we're going to have some long nights here in the forest. Would any of you like to learn some calculus?' I put up my hand, so did one or two others and for ten weeks or so, it was really quite an intensive course in calculus.

Coleman: *In the camp?*

Kemp: In the camp, no electric light.

Coleman: *Melbourne University only introduced a Ph.D. degree in 1946, so the research environment there was not very well developed.*

Kemp: Dick Downing told me: don't bother writing papers. That was the attitude at Melbourne.

Coleman: *Extraordinary. We are just a provincial receptacle for what is being produced overseas. But you went on to do a masters degree, and you were encouraged to pursue further your studies, by Ben Higgins, is that right? What did he say to you?*

Kemp: Ben Higgins arrived from Canada in 1948 as the new Ritchie Professor, taking over from Giblin – who never came back. I told him I'd been up to Canberra and had been interviewed by the Treasury, and been offered a job. He was quite angry and said, 'You're not going to Canberra, you're going to North America to do a doctorate'. Not England, certainly not England. (We admired England – it was great to travel about the country. We were always brought up on British history. But when I finally went to England, in 1956-57, it was clear that England was no longer one of the leaders).

Coleman: *The ANU was opening up at this time, of course under Copland's leadership. Was there any thought of going to the ANU?*

Kemp: Well, at the critical time there was no ANU. I was looking in 1948 to get out and to go to the States, and ANU wasn't more than an idea. Friends of mine, a year or two later, were given scholarships. There was no physical presence, except for the old University College.

Coleman: *What was it about Johns Hopkins that made it the place for you to go?*

Kemp: It was not huge and Ben thought that was a great advantage, being able to knock on your professor's door anytime and have a chat.

Coleman: *Who was your supervisor?*

Kemp: In the end, Evsey Domar. I felt a bit out of place at first at Hopkins as did John Chipman, my close friend now. We'd been through all the theory that we were being subjected to at Hopkins, we'd been through it without mastering it really, and it was good for both of us to do it again, and with more sophisticated teachers than Melbourne could provide during the war years.

Coleman: *You arrived at Johns Hopkins with calculus and linear algebra, which is the usual equipment of 98 per cent of economists.*

Kemp: I would not overemphasise my maths training. It consisted of a first-year course, and a bit of night work in the forestry camp! But there was no one on the Johns Hopkins economics department to teach these things, so I went across with John Chipman and [Carl] Christ and [Arnold] Harberger and we showed the maths people Samuelson's book which had just come out, *Foundations*. And they said, 'Look: the best thing is to take a course in algebra', and that's what we did. We all sat there and learned about rank and we soon realised we could put it to good use in economics. That was the extent of the maths training, except that T.C. Liu came across from the IMF on Saturday morning and taught us mathematical economics. No textbooks in economics; they were electrical engineering texts, had chapters on difference equations, differential equations, which he thought we should know something about.

Coleman: *When did you first come across Samuelson's* Foundations*? Was it known about in Melbourne, to students?*

Kemp: No, it only came out in 1949. No, I was at Hopkins when it came out, it wasn't taught – there was very little time to read it. I think it had to wait until I went to McGill, as with Arrow's book, which came out a few months later.

Coleman: *Robert Lucas has said that he was an economic historian – like Jim Cairns – but – unlike Jim Cairns – he said that reading the* Foundations *changed his life. Would you say anything half so dramatic, or was it purely another worthwhile book?*

Kemp: I think that, as I became more mature, the *Foundations* played the same role in my life. But I would add that Arrow's book also greatly influenced me. After completing my two years at Hopkins, I rejoined Ben Higgins at McGill. (He was by then chairman of the faculty at McGill). He called me up one night and said, 'You better get a ticket and come up here'. Instead I bought a car and drove up slowly. I realised – and other Associate Professors realised – that we had to do something to acquire mathematical logic, which was the tool that Arrow used in his book. So we went to the maths department and met a young assistant professor of mathematics who was worried that he wouldn't have enough students in his class

on mathematical logic. We were able to help him out, and learned a great deal in the process.

Coleman: *How many years did you stay at McGill?*

Kemp: I went up there in the middle of 1951, stayed till the middle of 1959. With a year off in England, as a Nuffield Fellow.

Coleman: *Where did you stay?*

Kemp: Nominally at Cambridge. Cambridge was a great disappointment, as there was no provision for graduate work and I was postgraduate. There were six Australian graduate students, Ph.D. students. I think it was the experience of each of them that they'd made a huge mistake in coming to England. They all had nominal supervisors but had no real guidance and no course work.

Coleman: *Do you remember any of the names of these Australians?*

Kemp: Yes, Geoff Harcourt. I think Geoff Harcourt was the only one to successfully complete his degree, and it took him years, had to go through at least two supervisors.

Coleman: *Did you go to the seminars at Cambridge?*

Kemp: There were almost none. I went to what there was.

Coleman: *And what was there?*

Kemp: Joan Robinson's book had just come out.

Coleman: Accumulation of Capital*?*

Kemp: Yes, a group of us met to go through it, without Joan's presence. And we were greatly disappointed with the book. If only she had been able to use some mathematics! It was obvious to all of us that in a couple of days we could write down the mathematical core of it, and that would make it much easier for everybody.

Coleman: *Do you have any memories or impressions of the other people at Cambridge at the time? Who were the other intellectual presences which made any impression on you in Cambridge, apart from Robinson?*

Kemp: [Richard] Stone, whom I had met earlier at Hopkins, was there, as head of the Department of Applied Economics. But his wife had died, and the year I was there he never came in. Once I waved to him from a distance; I waved, he waved back. That's as close as I got to him. Brian Reddaway, who succeeded Stone later, and who'd been in Australia in the 1930s, I got to know a little. But he wasn't a theorist so it was just a social relationship. Joan Robinson was there, and E.A.G. Robinson. But they weren't visible to visitors.

Coleman: *What was your College?*

Kemp: I didn't have one, I wasn't a student. I lived with an elderly pair of sisters. I left Cambridge in June, came to Melbourne until August, and then went back to McGill.

Coleman: *What was the occasion of your moving to MIT?*

Kemp: Evsey Domar had moved from Hopkins to MIT. He wrote and said, 'We can find a job for you here, if you'd like to come'. By then I was married, my wife was an archaeologist (she had graduated from Paris). She was not unhappy about going from Canada down to Harvard.

Coleman: *If Cambridge was very disappointing, was MIT everything you could have hoped for – or still not quite?*

Kemp: It was everything I needed at that time. I knew that there was a magic cupboard to which one of the secretaries had a key. And in that cupboard there was a collection of Paul Samuelson's published papers. So I asked the secretary if there were multiple copies and she said, 'Oh yes, lots of copies; would you like a set?'. I said, 'By all means'. So I read every article that Paul had published. First, the Foundations, then the articles. That was when I grew up and became a man, as an economist. I don't think of myself as being in any way mature or developed until sometime in the 1960s. The years at MIT made me aware of what I might be capable of. And even though I'd published in all the leading journals by then, I began to see that those papers weren't anything to be particularly proud of. They were Keynesian many of them, easy-to-write Keynesian papers.

Coleman: *Right, so was it at MIT you feel you realised you had a talent?*

Kemp: Yes, I knew that I had colleagues who were highly regarded in the profession, whose work I could easily understand – Samuelson, Solow…

Coleman: *Can we now talk about your transition to the University of New South Wales, and I'd like to quote Henry Wan. He says: 'Murray came from Australia and received training in both America and Britain. After a period of teaching at McGill and MIT he decided, 35 years ago, to return to the shores of the Pacific, and to set up at the University of New South Wales. It was a decision not easily made and not without difficulties for himself and his family'. Is that an accurate statement? Would you care to comment on it?*

Kemp: It was difficult in the early years. We were a new University – we were not welcomed, in some quarters.

Coleman: *Economics was not welcomed, or you personally were not welcomed?*

Kemp: No, the University was not welcomed. The University had been established in 1949, but economics had been created only in 1958.

Coleman: *So the department wasn't welcomed.*

Kemp: It was not. And we had trouble recruiting, because there were no graduate schools churning out doctorates. The University was a subject of great public comment in those days, in the *Bulletin* and other magazines. Nothing kind was said about this new University, which had grown out of the technical college, Sydney Technical College.

Coleman: *But was it difficult to make the decision to come out? 'It was a decision not easily made'.*

Kemp: Well, it was professionally difficult. But I'd been away for ten years and my parents were growing old so I felt I should come back, at least for a time. My wife had acquired an interest in the Tasmanian Aborigines, and she wanted to do some digging there. So we thought, 'For a couple of years, yes'. But it grew on both of us.

Coleman: *Why don't we push on from these institutional things to more intellectual things. I'd like to begin with your first publication, which I read with great interest. I say that quite sincerely, for reasons which I will explain. 'The Interest Rate and the Money Supply in Keynes' Economics', in the* Economic Record *of 1949. Now this piece deserves a very prominent place in Post Keynesian theorising because your paper is the first to press that the endogeneity of the money supply is absolutely critical for understanding Keynesian economics. Today that is very common; it is standard among the Post Keynesians, and has been so since 1970. But until then almost everybody matter-of-factly took the exogeneity of the* General Theory*'s money supply as the way it was. But you in 1949 very clearly state the critical nature of endogeneity.*

Kemp: William, I'm delighted and surprised to hear you speak that way, because the complete truth is I have not looked at that paper since it came out. I don't remember anything about it.

Coleman: *You present the IS-LM model and you say that the really critical equation is the money supply equation. If the interest rate is 'pegged' at some given rate, then price flexibility will not get you full employment.*

Kemp: I don't think I ever received the offprints, but I am glad to hear you say something nice about my first paper.

Coleman: *One thing which I'd like to take you up on is that, in this period, you could be described (if we have to use summary labels) as a Keynesian. Was just about everybody a Keynesian in this period?*

Kemp: Yes, I'd say so, at least at the universities I know anything about. Of course, there were places like Minnesota where there were well-established people who never really accepted the *General Theory*. And even in England in the early days there were people who had their reservations. But at Melbourne, yes: I can't remember anyone who spoke against the *General Theory*.

Coleman: *Now this Keynesian orientation was not quickly passing. Can I read from your review of James Meade's* Theory of International Economic Policy,

which came out some time in the later 1950s (Kemp 1957)? In your review you criticised the Theory *for not paying heed to, and I'm quoting here now, 'the controls which are essential for the purposes of maintaining full employment'. You say 'this problem was sidetracked by the assumption that price flexibility takes care of unemployment'. So you were a Keynesian.*

Kemp: The Keynesianism, I think, disappeared very soon after that review. The early papers which attracted attention were in the *American Economic Review*, the *Quarterly Journal of Economics* and the *Economic Journal* – they were all strictly Keynesian. All accepted with a minimum of fuss, no rewriting. Yes, I was still a Keynesian.

Coleman: *Why did you change?*

Kemp: I began to understand that the IS-LM system isn't enough. It's just such a simplified economy with so many things missing.

Coleman: *What things?*

Kemp: Well, the Keynesian system was subject to the same criticism as the later multi-sectoral fixed-price versions of the 1970s. An essential part is the fixed wage. Surely a macroeconomic theory should explain how the wage rate is determined?

Coleman: *You had learned to appreciate Gottfried Haberler's work on the Depression?*

Kemp: He'd written a book, not long after the *General Theory*, it was the first major work which cites the *General Theory*, a book he must have finished in Switzerland. It was called *Prosperity and Depression*. It was somewhat submerged, or overwhelmed, by the *General Theory*, and didn't get the attention I think it deserved. A pity. I think Haberler is the great neglected economist.

Coleman: *What did you get out of Haberler?*

Kemp: I think he's written the best book on the trade theory, to start with. *The Theory of International Trade, with its Applications to Commercial Policy* (1936).

Coleman: *You're very impressed by this book. What are the attributes of it that impressed you?*

Kemp: Well, he's got far beyond the Ricardian model, at a time when people like Taussig and Viner had not. They were still pretending that the Ricardian theory was worth studying.

Coleman: *But in the 1930s we had Heckscher and Ohlin, didn't we?*

Kemp: Yes, but at the time when Haberler's textbook came out in English they weren't well known.

Coleman: *So, in other words, to English speakers it was Haberler's book which was the fresh wind in trade theory.*

Kemp: Haberler had gone beyond the Ricardian model way back in the 1920s with his non-linear production frontier. Haberler had developed the specific factors model, that to this day young Americans think is an American invention – in fact it was the dominant trade model between the world wars. Haberler has a chapter in his book which goes almost as far as Samuelson, Jones, Ruffin and others went – much later.

Coleman: *I can see Haberler's contribution to trade, but we first mentioned his name with respect to your fading interest in Keynesian economics. Did it just give a better picture of prosperity and depression than Keynesian economics?*

Kemp: A much more complete model; it wasn't just IS-LM. It tackles the questions that hadn't occurred to Keynesians. The fact that Meade and Ohlin were awarded the Nobel Prize, and not Haberler – that's a great travesty, I think.

Coleman: *Your account of your fading interest in Keynesianism is what I would describe as 'internalist' – you're thinking about it, you're looking at the model and you find it's incomplete. It's not an 'externalist' account of the decline of Keynesianism, which would commonly begin with 1960s inflation. Is it true to think that, in your intellectual career, the principal dynamics have been internalist ones of logic, consistency, proof – rather than the more externalist?*

Kemp: Well, I became interested in welfare economics almost as soon as I went to McGill.

Coleman: *You have always been interested, it seems to me, in conflict within trade issues. In a review of the* Principles of Economic Policy *by Kenneth Boulding …*

Kemp: I reviewed that?

Coleman: *Yes, you did (Kemp 1960). You say it is 'a most satisfying text'; 'it sets economic policy clearly in the context of political decision making'. What interested me particularly about this comment was that you were interested in conflict or politics, which I consider to be synonymous – but your work itself has generally been quite pure. I mean, the political economy of tariffs and trade restrictions has not been your focus.*

Kemp: How does a democratic society make policy choices, when almost any policy is going to hurt somebody – how do you resolve this dilemma? I became interested in normative economics, and my introduction was reading through Arrow's book. Later there was Jan de V. Graaf's book – his doctoral thesis from Cambridge (still the best introduction to welfare economics). I began writing when I was still a Keynesian. The first articles came out in the early to mid-fifties. I was really a slightly twisted character at that time, doing things that were not quite consistent with each other. I was still a Keynesian, still writing Keynesian articles, without any welfare emphasis and, of course, employment obviously is part of

welfare. At the same time, working in welfare economics with a full employment assumption.

Coleman: *Why were you happy to take that assumption, the full employment assumption?*

Kemp: I was young. I was commenting on great economists' work, and they'd made the assumption. You tend to take over assumptions that other people have made. It was the Keynesian assumption that eventually bothered me. Well, I could think of unemployment as dependent on distortions of the economy, one kind or another, including inflexible wages. I was happy to continue with full employment assumptions, and deal with the externalities and distorting taxes, in a context of an economy that could be made fully employed, if you could just get rid of these distortions. That means getting rid of trade unions, and a lot of other things that are also regarded as part of the democratic society.

Coleman: *Is there a dilemma there, if trade unions are part of the free society?*

Kemp: It's the same dilemma that I encountered in the *Clash of Progress and Security*. How are we to look upon strong trade unions? We are brought up in Australia to think of them as an essential part of democracy. Their way of dealing with the clash that Fisher talked about. I've never been able to quite accept trade unions, just as Schumpeter never was. There must be a better way of doing it.

Coleman: *When I read some of your own retrospectives I get a strong impression of your impression of progress over the past 50 years. I wonder if you could do a stock-take of the progress that you have made, or been a participant in, or a close witness of?*

Kemp: I think most of my colleagues accept the proof given by Henry Wan and myself in 1972 that trade under Arrow-Debreu assumptions is potentially gainful, that is, after compensation. (In the same year, and from the same campus, Dan McFadden and Michel Grandemont (1972) provided a very similar proposition).

Coleman: *So that was the pinnacle of progress?*

Kemp: Oh, it was in 1972. But already the general equilibrium theorists were becoming disillusioned by the Arrow-Debreu assumption of a complete set of markets. It doesn't take much reflection to realise that there are more markets that don't exist than markets that do exist. All the forward markets that don't exist. Even wheat. What happens to the gains from trade? Well, much later, I teamed with a young Chinese, Kar-Yiu Wong, who was just completing his doctorate. We corresponded. Eventually he said, 'I'm coming out to spend a year with you'. He came out to Sydney, and we wrote a paper proving that trade is still potentially gainful, after compensation, no matter how many markets are missing. That is, we discovered that missing markets could be coped with. We also found that we didn't have to deal with a finite economy, a finite number of individuals; we could deal with overlapping generations so long as there were one-parent families, and no bequests. Uncertainty is not a problem. I also wrote a paper on chaotic economies, with Koji Shimomura, showing that chaotic behaviour doesn't affect things either.

Coleman: *But once we get two-parent families with bequests?*

Kemp: Then there are two pairs of grandparents, each aware of the existence of the other, and each pair knowing both pairs are planning to make bequests. So it becomes a game. They are both engaging in strategic behaviour with their eye on the other player. The other player being the other set of parents. I have talked to all the leading specialists in overlapping generations, and they say, 'You will never prove existence'. I agree.

Coleman: *Presumably the problem of being 'aware of the existence of the other' occurs in contexts outside of the family?*

Kemp: Consider the Heckscher-Ohlin assumption that in each country all agents are identical – preferences, endowments, information. This assumption is accompanied by the assumption that the agents are unaware that they are identical. However, alert and intelligent agents will soon be aware that they are identical. But if intelligent agents know that they are identical then they will know also that they will choose the same strategy. Hence each agent will choose the strategy which, if all other agents choose the same strategy, is socially optimal. In effect, the agents will play a cooperative game. This argument strikes any part of economic theory that relies on a representative agent ignorant of his own identity. Several illustrations, especially from the theory of international trade, have been provided by myself and my co-authors. The Ramsey-Pigou-Samuelson general-equilibrium theory of tax incidence and the modern theory of endogenous growth are also vulnerable (see Kemp and Long 1992; Kemp and Shimomura 1995; Kemp and Shimomura 2002; Kemp 2002).

Coleman: *I wonder if I can ask you what is your least favourite fad in trade theory over your lifetime? Are there some new fashions which you've never cared for?*

Kemp: Yes. There's been a return to partial equilibrium trade theory. People became captivated by the possibility of dealing with trade problems in terms of strategic behaviour. But trade theorists did not know how to deal with it in a general equilibrium context. Brander and Spencer (1981) wrote their paper very partially and it's remained that way ever since.

Coleman: *You cannot be interested in the partial?*

Kemp: It's just the beginning of serious study. I did see the need to tackle some of the problems in a general equilibrium context. In very recent joint work with Shimomura (which is already attracting attention) we show that Arrow-Debreu assumptions imply that price-taking behaviour entails at least one household that is incompletely informed and/or irrational (Kemp and Shimomura 2004). In another paper I wrote with Shimomura (Kemp and Shimomura 2001) we extend the Arrow-Debreu model by allowing each household to have its own (possibly non-convex) production set, and allow them also to engage in strategic behaviour. Then we are able to show that (given the existence of equilibrium) trade is gainful.

Coleman: *But the issue for Brander and Spencer is presumably free trade versus 'rigged' trade, isn't it?*

Kemp: No. They had governments who were subsidising exports, which trade theorists had always thought was just giving money away. There couldn't be gains from that. They showed that in a very special case (so special that we have lost interest) that subsidising exports could make the country better off. But what we've shown is that free trade – that is no tariffs, but with each household setting its price given its market power – is gainful.

Coleman: *Let me ask you a question, which has been asked quite literally thousands of times in dozens of countries over the last 20 years. The question is, 'Are tariffs and quotas destructive of national economic welfare?' This question has been asked in a survey of economists thousands of times. So what would you say if this question was put to you? 'Are tariffs and quotas damaging to national economic welfare?'*

Kemp: They can be, yes. I have spent a good part of my working career demonstrating that trade is gainful under conditions that I've laid out. Those conditions that aren't all particularly realistic. We have always recognised the Infant Industry Argument, which is one variety of the learning argument. There may be a case for restricting trade, giving industries time to learn the trade.

Coleman: *What about optimal tariff arguments?*

Kemp: That is a Beggar-Thy-Neighbour argument. You harm other countries**.**

Coleman: *You do, but who cares about them?*

Kemp: Perhaps some people still love their neighbours.

Coleman: *And Keynesian arguments for tariffs are not acceptable to you because you'd rather write to the editor, and solve unemployment in other ways.*

Kemp: I realise we are living in a world with trade unions, especially in Australia. It would be much better if you got rid of those tariffs, so long as we could put something in its place, the idea of lump-sum compensations – making sure there are no desperately poor people.

Coleman: *You have been obviously very interested in real-world issues. But you have lived your life as a scholar, a university man, not as a public advocate the way some of your academic peers have. May I ask: have you ever written a newspaper article on economic issues?*

Kemp: Yes. [Laughter]. I have to think a long way back, and it was by invitation. I'm not sure whether it still exists but the London *Times* used to put out a periodical supplement, and when I was at Cambridge they asked me to write on the Canadian economy. So I did.

Coleman: *So that was 195….*

Kemp: 1956.

Coleman: *OK. Can we now talk about your books?*

Kemp: While I was at MIT I took over from Charles Kindleberger the graduate seminar in trade theory. For me that was a first dip of the oars into graduate teaching of trade. And I accumulated quite a few notes and when I came out here I wrote them up, and they became the first textbook: *Pure Theory of International Trade*. (That was the book that Henry Wan probably had in mind when he talked about mathematisation of trade theory). That was a very influential book. Not in terms of copies sold, but it was the first time anyone had tried to set it up mathematically, and not take hundreds of pages saying things that could be said in a few paragraphs.

Coleman: *Is this possibly a difficulty with your books: your concision?*

Kemp: Yes. I agree it's a difficulty.

Coleman: *Might I just quote some reviews of various of your books? 'It is frequently very terse', 'writes in a terse style'; 'much terser than the alternative'; 'terse to the point of obscurity', 'Too short' (see Johnson 1965; Kruger 1970; Petith 1973; Herberg 1977). Is that a fair criticism? Or carping?*

Kemp: I was breaking new ground. The ideas were developed at MIT where I had a good bunch of students in the class. So it wasn't expected that it would be suitable for every teacher of trade theory, and it wasn't suitable. It was developed by other people into more readable books.

Coleman: *What did mathematics give you as an economist?*

Kemp: It gave me the language, the ability at least to understand what people like Walras, Pareto, Hicks and now the French were writing about. Without the mathematics it makes no sense at all. People have tried to write non-mathematical general equilibrium theory; but it can only give you the flavour, most of it is two by two.

Coleman: *So mathematics gives you the generality?*

Kemp: The generality, yes. If you stick to two by two you don't know if it's true only for two by two. In fact there are very few comparative static propositions that are valid in complete generality. None, in fact. In the 1970s, what we'd always suspected was proved to be true, by Hugo Sonnenschein, Gerard Debreu and Rolf Mantel, who wrote a succession of papers in which they showed that using general Arrow-Debreu assumptions there can be no easily interpretable comparative-static propositions.

Coleman: *I would like to take up this issue of generality, which is such an important intellectual value to you. What if I was to put to you that general equilibrium serves this quest for generality quite poorly, because equilibrium is surely the special case, isn't it?*

Kemp: Even when we were writing our paper in 1972, using the Arrow-Debreu model, already there was unrest: not among the everyday economists, but among the élite. What were they worried about? They were worried about the fact that the Arrow-Debreu model is equivalent to a model in which the markets open only once. One day they open, and, with perfect foresight, establish prices that will clear the markets for as long as the model going to run. But already people like Roy Radner were dealing with quite different types of models in which the market opens every morning with new information, knowing full well the information you had yesterday and the day before is false – sequential markets – and proving the existence for that model.

Coleman: *So we can't prove the gains from trade under a fixed-price model?*

Kemp: I've never tried to.

Coleman: *You've never tried to. It's a dead end?*

Kemp: It's a dead end because there isn't market-clearing.

Coleman: *Yes, I know, but that's the point isn't it? We're saying, 'this is the way the world is: markets do not clear'.*

Kemp: I'd rather continue to assume full employment, and write letters to the editor about the damage the trade unions are doing.

Coleman: *Can I ask you the favourite colour question, asked of pop stars. Not what is you favourite colour, but what papers are you most fond of?*

Kemp: I was recently asked that question by a Japanese University, giving me a limit of four published papers. (I realised they'd probably prefer four linked papers, so these are not necessarily the ones I'm most proud of). Two papers on the 'gains from trade' (Kemp 1962 and 1972). The papers on custom unions: Kemp and Wan (1972 and 1976) and Kemp and Shimomura (allowing the excluded countries to retaliate). Now, outside trade theory, I'd say some of the pure welfare economics papers: the one in which Yew-Kwang Ng and I (1976) found that Paul Samuelson was wrong in claiming that the Bergson-Samuelson social welfare function requires only ordinal information. It was believed by two generations of economic theorists and we showed it's not right. You can't expect to derive a Bergson-Samuelson social welfare function without cardinal information.

Coleman: *Do you have a sense, when you look over your vast number of papers, of false starts and dead ends, and wasted attempts in anything?*

Kemp: Yes. There were not many theorists to talk to when I arrived in Sydney in 1961. Some of those papers written in the early 1960s I wish I hadn't written.

Coleman: *You really feel that way?*

Kemp: I was trying to set an example to the younger people at NSW; to get published.

Coleman: *You are not a solitary author. On the contrary, co-authorship has been something we've been interested in. Your co-authors include Henry Wan, Ngo Van Long, Ronald Jones, Nissan Livatian, Takashi Negishi, Ed Tower, Albert Schweinberger, Yew-Kwang Ng, John Fleming, Steven Turnovsky , Winston Chang, Wilfred Ethier.... and many others. But when I look through these names, I would venture – I'm probably wrong – there's almost not a single person who is an Australian resident.*

Kemp: Wan, Long, Negishi, Ng, Turnovsky, Schweinberg all lived in Australia at one time.

Coleman: *What do you think of the general quality of Australian academic life?*

Kemp: I think it's too much influenced by the business world. The Canadians never had this problem.

Coleman: *I believe it's your opinion that in economic theory the pinnacle belongs to France.*

Kemp: At the present time there are probably as many members of the top twenty theorists in France as there are anywhere else. That doesn't mean that the general standard of economics in universities is high in France. Far from it. There are now seven or eight – perhaps as many as ten – departments staffed by very well-trained economists. But there are still many universities controlled by people who call themselves Marxists, but they're really economic historians. For a long time economics was very much a sub-discipline.

Coleman: *But now it's different, you're saying?*

Kemp: It's becoming different.

Coleman: *What is becoming different in economic theory?*

Kemp: We have made so much progress: missing markets, uncertainty and overlapping generations. But bring in strategic behaviour by pairs of grandparents with bequests, and you will never prove existence with overlapping generations models. So, the two theorems of welfare economics – Arrow-Debreu. It's all up in the air. I wish I was a graduate student!

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* William Coleman, School of Economics, Australian National University, Canberra ACT 0200, Australia. Email: william.coleman@anu.edu.au.

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