

Benjamin Higgins on Australian Economics and the
History of Economic Thought*

In 1948 Benjamin Higgins was appointed to the Ritchie Chair of Economic Research at Melbourne University. He describes his Australian experience:

Australian economics in the late 1940s are well covered by Heinz Arndt in *A Course Through Life*, and I shall not repeat his story here. Suffice it to say that many of the great figures were still around – Colin Clark, Sir Douglas Copland, Sir Leslie Melville, 'Nugget' Combs, Sir John Crawford, L.F. Giblin – and there was at Melbourne a group of brilliant students who later became famous: Peter Karmel, Murray Kemp, Helen Hughes, Donald Cochrane. I enjoyed my association with all of them.

A number of important things happened to me during my two-year tenure of the Ritchie Chair. First, it was a very productive period for me. Alan Ritchie, son of the Ritchie who endowed the Chair, and who became a close friend, proposed that I devote my inaugural lecture to the question, what do economists know? I decided to tack on to my inaugural five more lectures to make a series of six. These became my book, *What do Economists Know?* It took me back to my first loves, scope and method and history of thought, and I learned some more about history and philosophy of science in writing it. I also wrote a review article on Harrod's book on the trade cycle, which Harrod liked, and when I sent him my article on 'The theory of increasing unemployment' he accepted it for *The Economic Journal*. (The article was yet another elaboration of the stagnation thesis.) I visited Canberra, was horrified by its vast empty spaces, commensurately long commuting times, and lack of human qualities, and wrote an article on 'Canberra: a garden without a city' which was published in a Canadian planning journal and reprinted in an American planning journal. I wrote an article pompously entitled 'Towards a science of community planning' which was published in that holy-of-holies, the *Journal of the American Institute of Planners*. I went back again to scope and method in an article written for the *Australian Journal of Philosophy* on 'Economics and Ethics'.

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One fascinating experience was to appear before the Commonwealth Arbitration Court in the basic wage case of 1949, on behalf of the Australian Council of Trade Unions. The case was a peculiar one in that, in contrast to the 1931 and 1937 basic wage cases, the government presented no expert witnesses and the court called none. I received a visit in my office from the head of the ACTU. After chatting with me for a while to determine my views, he asked me to write a brief and appear for the unions. I went to see Sir John Medley, the Vice-Chancellor, to ascertain how the University would feel about a professor appearing on one side of a basic wage case. Sir John said it would be most improper. 'Of course,' he added, 'if you were subpoenaed, you would have no choice but to appear.' I got the message, and that is how it was arranged.

I was quite unprepared for the court atmosphere, with the judges and the battery of lawyers for the employers all in wigs and gowns. I had expected something like a seminar; instead I was on trial. I was on the stand for three days, I had a sore throat, and as a witness I stood continuously, while the employers' lawyers took turns cross-examining me and sitting down. I must say that the Bench, recognising my naiveté, took pity on me and treated me kindly. My essential argument was that, given the Australian and world economic situation, a ten shilling increase in the basic weekly wage would not cause unemployment or inflation. The court finally awarded a one pound increase, which I would then have opposed as inflationary if asked. This experience led to my article 'Wage fixing by compulsory arbitration' in *Social Research*. But it had a greater impact than that. Like many before me, I came to Australia expecting to find the Workers' Paradise; an advanced, social democratic, welfare state, a model for other societies wishing to avoid the defects of both socialism and capitalism. But the Australia I found in those early post war years was far from affluent (few faculty members could afford a car, for example); it was stagnant, technologically retarded, and it lacked entrepreneurship. The kind of management that Australia's managed economy was getting, wage determination included, did not seem to be the answer to any of these problems. I found myself wishing that I had paid more attention to Schumpeter while I had the chance. Capitalism in a strait jacket just did not seem to work.

Another interesting experience was, together with Dick Downing, assisting the Liberal Party in the preparation of an economic platform for the 1949 elections. We worked with Sir Richard (later Lord) Casey, whom I had met through Alan Ritchie, whose close friend he was. Sir John Medley was abroad, and had lent his country house in Berwick to Dick Downing. The Casey country residence was nearby; sometimes Casey came to us, sometimes we went to him. Both Sir Richard and his wife Mae were people of enormous charm, and it was a delight to be with them. His friend Alan Ritchie had studied economics at King's

College, Cambridge, and had one of the sharpest minds I ever encountered (and kept abreast of economic thought with one of the best private libraries in economics I have ever seen, in his country house at Blackwood. He contributed a good deal to our discussions). Dick Downing's idea, and mine, was that the Labour Party under Chifley was bogged down, too much concerned with dividing the pie and too little with making it grow. In any case, Casey and his colleagues in the Liberal Party applied their economics well enough to stay in power for over two decades. It was my first experience of trying to design an entire economic program for a political party, and a very sobering experience it was.

At the end of 1949 Higgins returned to Canada:

I would have been happy to remain in Australia, but it was impossible. Professorial salaries at the time were not high, and the Australian government taxed me as a single man, because my wife and three children were not resident in Australia. I could not support a family in Canada, and myself in Australia, while paying half my income in taxes. So at the end of 1949, with much reluctance, I returned to McGill.

Once back in Canada I found myself once again bothered by the problem of stagnation in 'capital-scarcity countries' and decided that I should spend some time in underdeveloped countries. The Director of the United Nations Technical Assistance Administration (TAA, as it was then called) was Hugh Keenleyside, a distinguished Canadian economic historian. I wrote to him indicating my availability for an overseas mission. Frank Scott had been appointed Resident Representative to Burma, and it was decided that I should join him at the end of the 1950/51 academic year as his economic adviser. But Frank contracted amoebic dysentery soon after his arrival in Burma, and had to come home. The UN felt that his successor should choose his own economic adviser. Meanwhile the Chief Economist of the UN Commission in Libya had suddenly resigned. There I was with my bags practically packed. It was decided to send me to Libya instead of Burma.

And so, by a series of accidents, I became a development economist, just as I became an economist by accident in the first place. I had evolved from a micro-theorist with an interest in methodology, history of thought, and theory of value into a macro-theorist interested in monetary and fiscal policy, without completely dropping my earlier interests; and then, at the age of thirty-nine, I decided to study the problems of underdeveloped countries, less because of any concern for them than because I felt that learning about them might help me to understand the problem of 'economic maturity', or stagnation in advanced countries, as the other side of the coin.

In broad outline my story is fairly typical of the first generation of 'development economists'. None of us was trained to be a 'development economist', and few of us had any experience with developing countries when we first started thinking and writing about economic development, or even when we first gave advice on policy and planning for such countries. The status, or even the expression 'development economist', did not exist until after World War II. I never took a course in economic development in my life. There were no such courses to take until the 1950s. Schumpeter of course was concerned with economic development, but he called his course and his seminar 'business cycles'; for him the two were the same. Hansen's stagnation thesis was a kind of theory of development, but applied to industrialised economies. There were scarcely any books on the subject. As late as 1953, the only book in the McGill library written by a professional economist who had some experience in developing countries was Jan Boeke's *Economics and Economic Policy of Dual Societies*. When I set out for Libya I had not even read that. So when I arrived in Libya as Chief Economist of the United Nations Commission, I was very wet behind the ears so far as development was concerned.