Macroeconomic Theory and Policy

The Selected Essays of Richard G. Lipsey

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ECONOMISTS OF THE TWENTIETH CENTURY

Edward Elgar

Cheltenham, UK . Lyme, US

Introduction: An Intellectual Autobiography

All my life I wanted to know. Others wanted to be discoverers; I wanted to know what they had discovered. When I was ten, we were introduced at school to some elementary astronomy and that night lay awake trying to get my mind around the idea of infinity. I resolved to become an astronomer when I grew up. (I meant cosmologist but didn't know the word at the time.) When I survey the great advances in that field during my lifetime, taking us to the very moment of the universe's creation, I often regret that I got seduced by this crazy subject, economics, that purported to have universal laws about the behaviour of people rather than stars. In high school, although I was an indifferent scholar, I read and read and read: natural history, biology, geography, history, astronomy. (I followed H.G. Wells' *History of the World* in a great intellectual odyssey, discovering the beginnings of Western civilization in ancient Mesopotamia and following it up through the First World War.) Darwin was an early intellectual treat, as were adventures with Freud in late high school days - when I was reading the *Interpretation of Dreams,* I became very adept at remembering my own. We used to play a game of 'what would you die for?' My answer was always the same: I would happily' die if for one short hour before my demise I could know the secret of the origin of the universe!

Undergraduate days

My indifferent performance as a student carried on into first year university, where at Victoria College[1]. I continued to get most of my intellectual fodder from outside of the classroom. Added to books, however, was another great stimulation. I entered the college in 1947 with the first wave of Second World War veterans. These men and women were five to 15 years older than we adolescents; they had seen the world and some of the horrors of war; many had suffered through the Great Depression, leaving school in the 1930s for lack of financial support. They set us high standards and they became our mirror onto the world. I became close friends with a veteran who was over ten years my senior, and who I found to be in love with me in a way that I did not know existed and could not reciprocate. From him, I learned at least as much as from my voracious reading. In my second year, I enrolled in three courses that were to change my life: the History of Western Philosophy, Introductory Psychology and Introductory Economics (with a fine text book by John Ise which was really Alfred Marshall for those not yet ready to be turned loose on the master).[2]

Discovering the pagan origins of many Christian dogmas shook the belief in revealed, absolute, religious truth in which I had been raised; reading psychology gave me a more rationalist view of people than I had had before; learning about the complexities of the hidden hand shook my fussy, naive, do-gooding, liberalism (in the American sense of the term). Every day in that fateful autumn with ideas swirling in my head, I walked over the hill from my home in Oak Bay to the college (on the site of what was the old normal school and is now Camosun College). Each day, as I added new knowledge, the ferment swirled faster. Finally one morning in late November, half way to my destination, the whole fabric of my earlier beliefs fell away. I stopped in the middle of the road aghast. Suddenly I believed in nothing that I had inherited from my past; I found everything —factual beliefs, religious explanations, moral precepts — up for re-examination and to be put back in place only if they looked acceptable now. I had had what I subsequently found Descartes had called an intellectual house cleaning. In one short hour, everything, including the religion in which I had been raised and in which I strongly believed, fell away (and, in the case of religion, never to return). It was one of the great experiences of my life — at least on a par with the discovery of the full power of sex.

During that year, I transferred my main intellectual stimulus from outside to inside the classroom. Economics especially was a revelation. I found I could do it intuitively. I always seemed to know one step ahead of the lecturer just what assumption was needed to complete the argument. I ended up explaining the concepts to fellow classmates, many of whom were ten years my senior. I finished with a general equilibrium model in my mind, composed of demand and supply curves made of wire, and all interlocking so that a shock in one market had repercussions on all others, and an intervention that prevented the attainment of equilibrium in one market, set off smoke and sparks in the other markets. I subsequently found out that I had in my mind a rather dramatized, mechanical version of a

Walrasian general equilibrium system.

Suddenly I went from being an indifferent B-level student to a straight A student. After some worry about specialization versus a general education, I decided to enrol in honours economics for my last two years which were taken at UBC in Vancouver. The courses continued to open many doors; I joined the economics club where we read papers to each other and debated openly with our professors in an intellectually challenging atmosphere. In my third year, I took intermediate economic theory, taught by Professor Joseph Crumb and using Boulding's Economic Analysis as a text. Already I was becoming frustrated with the number of theoretical exercises which seemed to end in no new insights into real-world behaviour. Then one of the great moments of intellectual excitement occurred when I read Boulding's exposition of Hotelling's model of duopolists locating on a line. Boulding was extravagant in the range of applications that he suggested for what he dubbed 'the principle of minimum differentiation'. This was mind boggling; this was what I had come to economics to find: theories that explained a wide range of real world observations. So in my third year I formed a major research programme: to find out more about the range of applications of Hotelling's model and to check out some of the more extravagant of Boulding's claims for it. It was decades before I returned to this programme in a long series of papers with Curtis Eaton that is included in the joint volume of collected works by Eaton and myself (*On the Foundations of Monopolistic Competition and Economic Geography*, Cheltenham, UK and Lyme, US: Edward Elgar, 1997).

Sometime in my third year, I made an appointment to see Professor Crumb. I proposed that the economic theory I was studying seemed to have a mathematical form and, since I was learning 'verbal mathematics', wouldn't it be a good idea to learn some formal mathematics? 'No', he advised me, 'economics is based on the three pillars of history, accounting and statistical analysis; learn those as outside courses but do not waste time on mathematics'. The advice was right on some counts, but disastrously wrong on the key one. This was one of the very few influences at UBC that were unhelpful.

William Merrit taught a wonderful honours seminar where we read everything in sight: Sombart, MacKinder, bits of Pareto's *Mind and Society, Mahan's Influence of Seapower on History, James* Burnham, Thorstein Veblen, H.L. Mencken and countless others in a melange of ideas about understanding human behaviour in social and economic settings. I read Hayek's *Road to Serfdom* and was profoundly impressed. The most important book for me, however, was Schumpeter's *The Theory of Economic Development.* It gave me a model of the circular flow of income and output, taking place in real time and disturbed by dramatic innovations which made static welfare maximization more or less irrelevant and perfect competition the wrong norm. At times in the future, this vision became clouded over but it never fully left me and it gave me what I often described as 'an effective inoculation against the excesses of Hicksian comparative statics'. Without clearly realizing it, I had formed another research programme: to evaluate Schumpeter's criticisms of neoclassical, static-equilibrium, maximizing economics.

Professor Robert Clark taught me the History of Economic Thought and I read Smith, Ricardo, Mill and bits of Marx with great interest. Most influential of all the books I read in that course was Lionel Robbins' Essay *on the Nature and Significance of Economic Science.* Coming to economics as a renegade scientist, I was always interested in methodology: how could anyone really establish natural laws about something so complex as human behaviour? Robbins said many wise things from which I profited greatly, but when I came to his chapter on economic statistics, I balked. There I read for the first time the methodology of the Austrian school, which was, as I later learned from Mark Blaug, also the methodology of many of the classical economists. According to this methodology, which is Euclidian in conception, investigators first make assumptions that are intuitively self-evident, then apply the rules of logic to deduce propositions that may not be self-evident. In economics, the trick was to establish assumptions that really were self-evident, standing the test of introspection. Since the assumptions are obviously correct, the deductions must also be correct, no matter how unobvious they may be. If the facts appear to disagree with the deductions of theories, then the facts must be wrong; the deductions cannot be wrong — providing only that they are logically correct deductions — since they are based on assumptions that we know to be correct through introspection. In short, facts are used to illustrate theories but not to test them.

I read and reread the chapter. 'This cannot be right', I said to myself, 'facts based on careful empirical observation must play a more important part in the development of our understanding of the economy than as mere illustrations to be cast aside whenever they disagree with the prevailing theory.' These concerns shaped another of my research programmes: to find out what was wrong with the methodology of *The Nature and Significance* which, as far as I knew, was the prevailing methodology of all economists.

I wrote my honours graduating essay under Robert Clark, doing a major empirical study of the relation between land and building values in commercial property in Vancouver. This involved getting real estate assessments of the value of land and improvements for several thousand properties, visiting each individually to see if the building also included living quarters, and testing for the factors that caused the ratio of improvement values to land values to vary throughout the city. This was a major task worth at least an MA. (I subsequently found that an MA had been given at the Wharton School for a similar study.) Completing all my field work delayed my graduation for a year until *1951*. The study gave me an abiding respect for how important it was to get reliable data, and for how easily observations could upset ideas which seemed intuitively plausible at the outset of any study.

I graduated with straight As' in all my economics courses, with one exception. In Money and Banking I could never understand the relation between stocks and flows in the quantity theory of money that we were taught, and a grade of B + was the result. (Unknown to me at the time, this failure established another research programme in my mind.) I finished my fourth year at UBC in *1950* and left (with an honours essay still to be completed a year later) still innocent of Keynesian economics. Students who took one of the options that I missed, international trade, talked knowingly about multipliers which to me were a mystery.[3]

The civil service

During my third year, I became engaged to a girl who was five years my senior and a veteran with overseas experience. Anticipating the coming obligations of marriage, I had applied for jobs as an economist with several provincial governments. During the period of the final examinations, my engagement broke up. After completing the exams, I travelled to Toronto in the company of one of my professors, Bill Merrit, as a first step to seeing the world. At the time, the first Toronto subway was being built and I intended to get a construction job on it when my money ran out. I was aiming at graduate school but only after a year or two gaining experience outside of the ivory tower.

In one of those quirks of fate that have so often influenced my life, I got a telephone call in Toronto offering me a job with the BC provincial government in what was then the Bureau of Economics and Statistics of the Department of Trade and Industry. Had I not been planning to marry, I never would have applied for the job and now that I was off to see the world, there was even less reason to take a civil service job. But I was flattered at being picked from apparently a large field of applicants so, mumbling about being caught by respectability and responsibility (I was deep into George Bernard Shaw at the time), I took the train back to Victoria and reported to the Bureau in September.

I worked there for a year and then was given a leave of absence to do a two year MA programme at Toronto. I returned to work at the Bureau after each of my academic years in Toronto, working mid-May to mid-September, getting the statutory raises and full annual holidays (with pay). My period there was not without interest and I learned many useful lessons — not the least important of which was that there are ivory towers outside of academia and that government research often means providing reasons to justify decisions already taken on political grounds. My final job before leaving the Bureau was to help a senior economist write a paper advising on the extension of the government-owned railroad into the interior of the province and on into the Peace River district of Alberta. It rapidly became apparent that this would be a big money loser. But advice to this effect was unwelcome, while Chamber of Commerce material on why the railroad would build an empire was what we were being asked to provide. Rather than write what I didn't believe, I asked to be relieved from the job and departed the government a few weeks later a sadder and wiser person.

MA Years

Two years as an MA student at the University of Toronto, where I went on the strong urging of Bill Merrit, taught me much. We had an excellent course in micro-economic theory, and I was introduced to the mysteries of

Keynesian economics. But Toronto was most famous for its economic history and I got all of it that I could. I was particularly privileged to take Harold Innis's course on Empire and Communications given in the last year of his life. I will always remember his last lecture which he, and some of us, knew was his farewell to the academic world. I shared an office with a left wing political science student, Ted Goldberg, who later went on to become a senior negotiator with the Union of Automobile Workers and even later, to return to the University of Toronto as head of the Department of Hospital Administration. Ted, his wife, Grace and I became life-long friends. I remember being profoundly influenced by an argument we had on the future of socialism. I said that planned economies would stand or fall on their ability to allocate resources efficiently. Ted poured scorn on the idea 'as if, with all the important issues facing society today, efficiency of resource allocation mattered much, let alone enough to determine the success or failure of the great socialist experiment'. I was impressed by his argument and it took decades before I saw how wrong he was.

At Toronto, I continued to maintain my A results on all economics courses. In retrospect, the biggest missed opportunity was the course which Bill Hood (fresh from Hood and Koopmans on the Identification Problem) taught in mathematical economics. Several of us wanted him to take us through Hick's appendix to *Value and Capital* and teach us the required mathematics of which I was totally innocent. Unfortunately, on a vote, this plan lost to the majority who wanted to learn about the latest fad, linear programming. So I lost an opportunity to learn at least some relevant mathematics.

Between my first and second years at Toronto I had married and begun a tumultuous seven-year relation which finally ended in *1959*.[4] Partly because my wife looked fondly on earlier years in England during her first marriage, and partly because we students still looked to England as the intellectual leader in economics, I asked my professors about study in the UK. On advice from Professor Nat Wolf, I applied to, and was accepted at, Wadham College, Oxford, as a research student. As an insurance policy, I had also applied to Chicago and the London School of Economics (LSE). When I was offered a prestigious Frank Knight scholarship, I decided, after much soul searching, to accept Chicago and say no to Wadham. (LSE had also offered me a place but did not require an early decision.) Shortly before hearing of my success at Chicago, I saw a small notice advertising the Sir Arthur Sims Fellowship to study in the UK. Since it was open to anyone in the social sciences and humanities, I thought it a long shot but applied in any case. I was interviewed in Ottawa and, rather to my surprise, was offered the fellowship a few days after I had accepted the Chicago scholarship. Much agonizing produced no obvious reasons for deciding between the LSE and Chicago. Finally, I made the decision to go to the LSE because they had dropped French as a PhD requirement while Chicago still maintained the old language-competence requirement. Being a total dunce at foreign languages, and having taken seven years to get through five years of required French in high school and college, I opted for the easy route: avoid any more foreign language requirements. So I made one of the most momentous decisions of my life, to go to the UK rather than the US, on the basis of avoiding a minor language requirement — and once again made, for the wrong reasons, what in retrospect turned out to be the right decision!

PhD at LSE

The LSE in the fall of *1953* was an exciting place. Students from all over the world crammed into the graduate common room. I wondered if I was up to the challenge intellectually. When asked about my prospective research on the fellowship application form, I had chosen, rather by default, an empirical study of the changing pattern of Canadian trade with the US and the UK. The LSE at the time followed a fully *laissez-faire* policy with respect to research students. I was assigned to Helen Makower in International Trade and on our first interview, she told me to go to whatever lectures attracted me, read as I wished, and come back when I had something written to show her. I attended James Meade's seminar on international trade, and Lionel Robbins's great Wednesday afternoon general economics seminar in which the whole of economics was grist for our mill. Outside of that, I spent my first term playing bridge and chess. I had played some duplicate with my father who went on to become a life master and I wanted to learn the ACOL system used by the championship-winning British bridge players. I was innocent of chess but soon learned the rudiments and then dug myself into several books. Euwe's *Judgment and Planning in Chess* was particularly valuable. In between the two regular seminars, chess and bridge, I had time to attend maybe six or eight lectures in the whole term. I tried Professor Meade's lectures in which he was expounding what was later published as *Trade and Welfare.* My fellow graduate student, Max Corden, attended religiously, but one dose of apples and blankets was too much for me and I stupidly gave up the opportunity to hear it all from the master. I also listened to Lionel Robbins lecture once or twice on the history of economic analysis.

In that first term, I dropped in on one lecture by my supervisor, Helen Makower, on advanced economic analysis given to the undergraduate economics specialists. By chance, she happened to be expounding Viner's new book on customs unions. I listened to her exposition of trade creation and trade diversion and said to myself, 'this is all supply side, which no doubt is important, but there must also be a demand component'. I went home in a fit of intellectual excitement and scratched out numerical examples which got me nowhere and then went to indifference curves. About 2 a.m., after finishing a bottle of sherry, I had the outlines of what eventually became my article 'Trade Diversion and Welfare'. This was the first time I had ever done a piece of economic analysis on my own. I showed it to my supervisor who wasn't quite sure what to make of it. Nonetheless, I decided to do my thesis on customs union theory (not then knowing that Professor Meade was working on the same problems) but I put my article aside until I could fit it into a wider analysis — and could persuade my supervisor that it was interesting. Sometime later, when I met Harry Johnson for the first time, I told him about my proof that a country could gain from joining a purely trade diverting customs union with no trade creation. He was excited.

Work on my thesis was hampered by my lack of technique. I had been taught only two techniques for analysing problems in international trade. The first was numerical examples. I had read Frank Graham's *Theory of International Values* in which he solves by pencil and paper calculations ten-country, ten-commodity numerical models! The second was geometry which was the dominant tool of analysis and which was well expounded in Meade's *Geometry of International Trade,* and at which Harry Johnson was a master.

But customs unions problems as I saw them required three countries and three commodities. I could no longer avoid the conclusion that I had been discouraged from reaching as an undergraduate: mathematics was indispensable for analysing many economic problems. So, like many economists before me, I set out to read R.G.D. Allen's *Mathematical Analysis for Economists* and as a PhD student at the LSE I began to teach myself how to differentiate x^2!

Early in my second year at the LSE, I accepted an unlikely job. The movement that was to end in the European Economic Community was just getting up to full steam and it held, as part of its evolution, a meeting in London called the Council of Westminster with delegations from most of the European states and those countries with close relations to them. The Canadian government wanted to be represented, but not thinking it worthwhile to send a delegation from Canada, they cast around for Canadians already in the UK to become delegates. They settled on three of us. My old teacher Robert Clark, who was spending a sabbatical year at Manchester, Harry Johnson, already famous as an *enfant terrible* among UK economists, and myself, a research student on a prestigious fellowship at the LSE. I knew some trade theory but little about trade policy and nothing about diplomacy or the formation of the EEC, so this was pretty heady stuff.

We were briefed by the Canadian High Commissioner in London and this was when I met Harry Johnson for the first time. Spaak, Monet and several other great Europeans attended the conference. Urged on by Bob Clark, I even made an intervention arguing that to make permanent exceptions from the free trade obligations for former colonies on the grounds that they were LDCs was dangerous.

I argued that the rules should, sooner or later, apply with equal force to all. (A theme I returned to decades later with a lot more knowledge to back it up than I had then.) After my intervention, I met Bill Phillips for the first time. He was a New Zealand delegate on the same terms as we Canadians were. He greeted me with 'a little bit of religion got mixed up with the economics in your speech'. I was impressed, and depressed: I had not thought of the principle of equality in a customs union as being religious. It was a long time before I returned to this theme of trade policy. It was an even longer time before experience with the GATT exemptions granted to LDCs convinced me that I was right in my naive *1954* intervention. I now realize that Bill's accusation of religion was a characteristic put down that has to be endured by anyone who feels that free market principles are worth establishing generally.

One of my fellow students at the LSE was Jacques Parizeau, subsequently to gain fame as a Quebec separatist and leader of the Parti Quebecois. He had money and sophistication, whereas I was poor and provincial. Jacques took my wife and I to my first French meal and taught me a bit about French wine, and cheese-that-was-not-cheddar. Jacques used to visit our fourth floor garret in Goodge Street where my wife and I lived in poverty in one room with the lavatory three floors below which we shared with the employees of the ground floor shops. Since Assia was invariably unready when he arrived, he would wait at the working man's pub across the street. When we came to fetch him, we would find him, pint of beer in hand, dressed immaculately in a pinstriped suit with a bowler hat and furled umbrella reading *The Times,* and surrounded by beer drinking, Andy-Capp-style, British working men. We always welcomed Jacques's visits because not only did we enjoy his company, but he invariably brought bottles of beer with him. After he left, we would return the bottles to the pub and spend the refunded deposit money on the two staple items of our diet: potatoes and bacon pieces (the small scraps of bacon left over after all possible slices had been taken off a hunk of bacon which sold for sixpence a pound).

An LSE staff member

After two years as a research student, I was asked by Helen Makower to apply for one of the three assistant lectureships that the LSE was advertising that year.[5] I was amazed, not thinking myself worthy of being considered for such an august appointment. My interview consisted of a long debate with the senior liberal professors. They asked sceptically how I could contend that reducing any tariff, by means of either a customs union, a GAIT agreement or unilaterally, could possibly be anything but unambiguously beneficial?

After our interview, we were taken to lunch in the senior common room. I sat next to a junior staff member with whom I had played bridge, George Morton. I told him of a great discovery that I had just made: if a country has two imports subject to tariff, removing one tariff may not raise welfare; indeed there would generally be a non-zero rate that would be best to select for this variable tariff. George expressed disbelief, surprise and then great interest in that order. He invited me to his home and together we worked out the mathematical proof that appears in section VI of the 'General Theory of Second Best': given a tariff on one import, the second best optimum tariff on the second import is greater than zero but lower than the given tariff if the two imports are substitutes. This was the first mathematical proof that I had ever developed and I doubt that I could have done it without a great deal of help from George Morton.

In due course, Kelvin Lancaster, Hans Leisner and I were appointed to the LSE staff as assistant lecturers. The next three years were an intellectual feast, with a degree of freedom from administration and onerous teaching duties that I was never again to experience. Duties consisted of taking five discussion classes per week which backed up the first year microeconomic lectures given by Ralph Turvey and the second year lectures on macroeconomics given by Kurt Klappholz. The format at the LSE was for each student to attend a weekly lecture given to the entire years' group of several hundred students, and one discussion class with 15 - 20 students. Assistant lecturers gave the discussion classes.

We all attended Lionel Robbins's great Wednesday afternoon seminar and we had time to read, talk and write. Kelvin Lancaster, Chris Archibald, Ed Mishan, Kurt Klappholz, Lucien Foldes, Maurice Peston, Bob Gould and many others were stimulating company. We all read everything that each other wrote, made detailed notes on, and had long discussions about each article. In a very real sense, everything that any of us published was a joint piece by all of us. Ed Mishan kept us up to date with developments elsewhere and we held discussion groups on the latest works such as Friedman's new book on the Permanent Income Hypothesis.

During my first year on the staff, two sequences of events stand out. The first began one morning when I was in my room reading an article by Andrew Ozga on customs unions. Kelvin Lancaster came in for the usual morning gossip. I said to him in high excitement, 'You know these guys are all discovering the same theorem in all sorts of different guises.' Kelvin replied that ever since he had read Samuelson's *Foundations,* he had wondered why people put such stress on fulfilling selected optimum conditions when all of them could never be fulfilled. I went to the common room for coffee and, as luck would have it, I bumped into Harry Johnson and explained our great insight to him. 'Publish immediately' was his advice. Kelvin worked out the general proof and I worked on my customs union example and the literature survey. Harry played an important part in arguing that if the article was to have the impact it deserved, we should do an exhaustive survey of the literature. He suggested several articles of which we were unaware and helped us write an article which had world-wide impact rather than going unnoticed as it might have done if less care had been spent on it.

The second sequence began when I ran into Harry Johnson in the senior common room and he reminded me of the point I had made to him about trade diversion and welfare some 12 months previously. He asked, ‘Had I done anything about publishing it?’ ‘No,’ said I. ‘Well’ said Harry, ‘I have had a similar article submitted to the *Review of Economic Studies* and I advise you to publish your idea quickly.’ I wrote it up in a few days and submitted it to *Economica,* the LSE house journal. It was published about the same time as the *RES* published the same point in an article by Hans Gehrels. In my version, however, I had warned that my two-commodity geometrical demonstration could be misleading because customs unions raised three-commodity problems in which some, but not all, tariffs were removed. Gehrels seemed to miss this point and concluded that the consumption effect invariably worked to raise welfare — so I had a second publication pointing out Gehrels’s omission and making the second-best point in more detail. So once again accident played a key role in my professional life. If I had chosen any other lecture of Helen Makower’s to attend in my first term as an LSE graduate student, the odds are that I would have remained innocent of customs unions long after I had chosen another thesis topic; and if I had not told Harry Johnson of my idea, I might never have published it.[6]

Harry also got me on the programme of the Association of University Teachers of Economics (AUTE) to talk about my thesis on customs unions a year or two later. I never thought to publish my talk but I met my LSE colleague Professor Frank Paish in the staff lavatory not long after and he said he thoroughly enjoyed my talk and that I really should write it up. So one of my most widely read articles, ‘The Theory of Customs Unions: A General Survey’ had a rather accidental birth.

Then in my second year on the staff, we all read Patinkin’s *Money, Interest and Prices.* Our first reaction was that it was a beautiful piece of work and Lionel decided to spend the year going through it in his seminar, chapter by chapter. Controversy quickly arose about just what the real balance effect amounted to. This was in many ways the most intense three months of my life. My marriage had reached its most hectic state and my mind was shifting back and forward between personal and professional matters. I often lay awake at nights with visions of stocks and flows swirling in my head. When I had some of my ideas more or less sorted out, I gave a paper on the real balance effect in Robbins’s seminar. In it, I unconsciously followed Patinkin in only dealing with instantaneous (what Hicks called weekly) equilibrium. Andrew Ozga asked me what would happen if I let the model evolve for n periods and the penny dropped: Patinkin only analysed weekly equilibrium and that was why he needed such very restrictive assumptions to get quantity theory results. For example, any increase in the money supply had to be distributed to each agent in proportion to the amount of money he or she already held. It had seemed to us that if the quantity theory depended on such strong, unrealistic assumptions, it was not worth much. I worked out the results for full equilibrium (which became Part 1 of my subsequent paper with Chris Archibald on Monetary and Value Theory) and then joined forces with Chris to work on the question of the classical dichotomy (Part 2 of our paper). Robert Clower later said that our paper got stocks and flows correctly sorted out for the first time in the history of monetary economics. We got the whole of the quantity theory and the real balance effect right in ten pages, which I still think were the most elegant ten pages I was ever involved in writing. Now, at last, I understood why the undergraduate course on the quantity theory of money had not made sense to me; it, the instructor, and I, were hopelessly confused about stocks and flows and temporary and full equilibrium.

At the end of my third year on the staff, I came up for tenure which came with promotion to full lecturer. I was urged to complete my thesis which I had put aside on being appointed to the staff in order to write articles on customs unions, monetary theory and second best. I spent a full year working out the various analyses in the second half of the work. Then I got ill and the decision to give me tenure came before my thesis was finished. When I finally defended it, James Meade, now moved to Cambridge, was my external examiner. After I had successfully completed that hurdle, Helen Makower asked me about publication. I replied, without a lot of consideration, that I thought the work too crude to publish. Nothing more was said until 12 years later when people were still working on problems I had studied and it was recommended that the thesis be included in the LSE dissertation series being published by Weidenfeld and Nicolson. So, in 1970, it finally saw the light of day, over 12 years after I had finished it. I am sorry that I did not publish it in *1958* since it contains many ideas still not fully exploited in the literature.

Many of them were couched in a numerical example first used by Makower and Morton and closed by me with a balance of payments equilibrium condition. Once the general equilibrium condition was included, all sorts of surprising things happened. Many of these could have been investigated in more general terms than I had used but they have not been taken up. (See Chapter 9 of my thesis reprinted in these collected works.)

The next year, our group of young economists read, in manuscript, Phillips's article on unemployment and wage rates. I was fascinated and brought it back to Bill after a weekend's study bestrewn with comments only to discover to my surprise that the article was already in page proof without, as far as I could tell, having been criticized and improved by comments from his LSE colleagues in the economics department. Bill, however, encouraged me to follow up on my comments and possibly turn them into an article. This I did and spent a year working out regressions (at least my research assistant, June Wickens, did so on a mechanical calculator at the rate of about two a day) and trying to understand the curve in terms of microeconomic theory. My attempt has been judged defective by the profession but it did help to set the agenda of trying to understand such macro relations in terms of micro behavioural relations.

Methodology and textbooks

All through my first years on the staff, I continued to worry about methodology. The prevailing methodology was that described by Robbins in *The Nature and Significance of Economic Science*. That methodology has been so totally swept away that it is hard for today's economists to imagine a world in which *the* accepted method of criticizing a theory was to ask if its assumptions were 'reasonable'. Nonetheless, we all spent countless hours arguing about the reasonableness of the assumptions of various theories. As theories became more complex, grappling with more involved forms of behaviour, this method became less and less satisfactory.

My dissatisfaction was shared by several of the other young Turks at the LSE and, in the mid- 1950s, we began to meet to discuss methodology. From Agassiz we learned about Popper and, at some point towards the end of our first year of discussions, the resolution to my undergraduate worries became apparent:

- a theory has empirical content in so far as it rules out some states of the universe;
- the more it rules out, the more content it has;
- a theory that is consistent with all states of the universe has no empirical content;
- a theory is tested by confronting its predictions with evidence to see if the states that it rules out actually occur.

About that time, I read Friedman's famous essay on positive economics. Although I agreed with much of it, I found its arguments much cruder than Popper's subtle reasoning, and in one of our meetings I registered fundamental disagreement with his position that only predictions should be tested. It seemed to me that everything empirical that is contained in a theory, assumptions and predictions, should be confronted with facts wherever possible.[7] For example, if a theory with an obviously incorrect factual assumption makes predictions that consistently pass test, we would like to know why. Possibly the incorrect assumption is not necessary for the predictions being tested (in which case it might as well be stripped away) or the theory may contain a second, offsetting, incorrect assumption. Surely then we will learn by identifying these.

No doubt we were naive in thinking it would be easy to test and to refute theories. But we did find an effective way around the methodological impasse created by the Euclidian-Austrian-Robbinsian methodology. Although many methodological positions have been advocated in post-Popperian methodology, few have advocated going back to that earlier methodology in which the only test of a theory was the intuitive plausibility of its assumptions, and the only use of facts was to illustrate theories. I believe that our efforts played some small part in making that methodology unacceptable.

After a year, we formalized our meetings into the *ISE Staff Seminar on Methodology, Measurement and Testing in Economics* (the M^2T seminar). This was heady stuff. Just ten years after the young, first-generation university student in a small provincial Canadian university had read the book by the great Lionel Robbins and said 'There must be something wrong with its methodology', I was on Robbins's staff and helping to identify what we all came to believe was wrong and what had to be done about it.[8]

By 1960 we were pretty clear about our new methodology. (At least it was new for us, although people such as Terrance Hutchinson had been on to it much earlier.) At that time, I conceived the idea of writing a first-year textbook that would convey the new methodology. The idea came to me in Easter 1960 while walking across a field in Hertfordshire and, in a rush of intellectual excitement, I began to write. The chapter on methodology came pretty quickly as did most of the micro chapters.[9] After some indecision, I decided to call the book *An Introduction to Positive Economics,* and for years it was known as *IPE,* until it followed the fate of virtually all other successful textbooks and became known by its author's name, *Lipsey.*

In writing the book, I had at least two main objectives.'[10] The first was to sell the new methodology; the second was to make microeconomics interesting and relevant. At the time, the prevailing US textbook was Samuelson. I had taught my first introductory course — a summer school session at UBC in 1956 - out of its third edition. The macro was great, but the micro was not. When one had finished the macro, the micro came as an afterthought with the kind of attitude 'Well kiddies, the examiners will want you to know some of these curves so here they are, boring though they may be'. I had been raised a microeconomist and firmly felt that all macro relations had to be derived from micro foundations and I set out to make micro as interesting and as relevant as it had been under Marshall. To some extent I think I succeeded in the early editions of *Positive Economics* with both of these objectives. Certainly, the Robbinsian methodology which dominated our discussions in 1955 was totally unused by 1970.[11]

In the first edition, I followed most of my colleagues in the M^2T seminar in being a naive falsificationist. But I was always uneasy with this position and had many arguments with colleagues who were more convinced than I. Finally, my experience with trying to test some of the theories of the 'Phillips loops', which, sadly, I never published, convinced me that there was no such thing as a categorical refutation of a theory. As a result, I moved to the position that neither refutation nor confirmation could ever be final because they were both subject to error and revision. The result was an alteration in the second edition of *Positive Economics* that became famous and the subject of much debate. In the collected works (Volume One, Chapter 24) 1 quote the relevant four pages from the 4th edition where I had finally settled on a form with which I was happy.

I had trouble with the macro part of the book because that was neither my strong point nor my major interest. I had had to attend Kurt Klapphoiz's lectures on basic macro in order to learn enough to run the discussion classes on that part of economics during my early years on the staff. As I wrote the early macro chapters, I quickly ran into the prevailing theory that the national income identities could be used to restrict the universe of possible outcomes. (I remember a visiting US economist telling us that Keynes had 'discovered' some of the economy's most important identities which allowed us to deduce all sorts of things.) This, of course, worried a methodologist: if a definitional identity is consistent with all possible states of the universe, how can it be used to restrict possible outcomes, i.e., to give content to theories?

I had learned much of my macro from Swedish process analysis and from the Phillips water-flow model and so saw macroeconomics as describing behaviour in real time in disequilibrium situations (no Walrasian auctioneer). As a result, I tried to put down a circular flow model with the values of the flows of S, I and C as they would be given by recorders on a Phillips water-flow model of the circular flow of income taking place in real time. But I could not get the numbers and the identities to come out. This set me worrying about what was meant by the Keynesian identities and by the prevailing interpretation, found in virtually all of the textbooks of the time, that out of equilibrium actual S was made equal to actual I by unintended changes in inventories. But this seemed to imply that we could deduce an empirical prediction, inventories must be changing out of equilibrium, from a definitional identity. Furthermore, the prediction seemed to be contradicted by the evidence: inventories do not always rise when income is below potential and fall when income is above potential.

It took me a year to resolve this problem. When I had finished the macro part of *IPE* in mid-1962, I wrote up the results of my thoughts on identities in an article which *Economica* rejected and which I subsequently used for my contribution to the festschrift honouring Lionel Robbins's 70th birthday. I was later sorry, since the article went largely unnoticed. I did, however, have two private satisfactions. First, I was right about identities when virtually all other economists were wrong.'[12] Second, I saw my interpretation that S = I must be understood as an equilibrium condition, while the accounting identity of S ≡ Y plays no part in the operation of the behavioural model, slowly adopted in the textbooks without anyone noticing the change.[13]

It took me several editions to get it exactly right in the textbook, and by that time, the new interpretation of S = I as an equilibrium condition was accepted. I dropped the circular flow resolution with measured S not equal to measured I which is reproduced in the macro volume of my readings. *(Macroeconomic Theory and Policy,* Cheltenham, UK and Lyme, US. Edward Elgar, 1997).

Positive Economics began my career as a textbook writer. The book was an instant success. The first printing sold out in a few months and the first edition went through six reprints, each larger than the one before.

No sooner was the book in print than Peter Steiner and I set out to write what we thought would be an American adaptation, entitled *Economics*. It soon became clear, however, that *IPE* was too austere and too sophisticated for the typical first-year US undergraduate. (At the time some seven per cent of UK teenagers went on to university while the number was closer to 40 per cent in the US.) Slowly, over the editions, three-quarters of all the things that made *IPE* distinctive were eliminated from *Economics*. One of the important casualties was most of the questioning at the end of every Part suggesting that the theory being taught might be wrong, or at least in need of serious amendment. This slowly gave way under enormous US market pressure to teach theory as something closer to (but not exactly) revealed truth, particularly in micro. It was a very painful process and, although a good but more orthodox book emerged, I felt at every stage that I was taking part in the dismemberment of my own baby. It was not my co-author's doing, it was the relentless pressure of the US market. Fortunately, the British book, *Positive Economics,* remained my own and I did to it what I wanted, including keeping at the end of each part a full chapter on measurement, testing, criticism and, where appropriate, alternative theories.

My next textbook grew out of a course I invented at the LSE where any lecturer was allowed to advertise and teach his own course. After learning some maths, I became worried about the strongly anti-maths attitude in the core sub-department of LSE economics called Economics A&D.[14] People with a high degree of mathematical competence could be found in the statistics department and a few other places, but there were almost none in A&D. One exception was Bill Phillips, but he was not a part of our group and conversed mainly with the statisticians. So to help remedy the deficiency of mathematics in A&D, I invented a course in which I took the economics the students knew and taught them the maths that was implicit in it. Several staff members attended, and when I went on leave to Berkeley in 1963-4, Chris Archibald took it over and worked it up into a much more coherent course. We then wrote it up into a book entitled *An Introduction to a Mathematical Treatment of Economics.* Its distinctiveness was that it was not just a maths cookbook. Rather it alternated a chapter of maths with a chapter that applied that maths to economics the readers already knew. The book was highly successful in the UK and went through three editions over 20 years. Its US version was not a success - although it did get a few highly enthusiastic users. I often wonder if the US version failed because the publishers insisted on promoting it just like any other mathematics cookbook.

In the early 1980s, I joined forces with Professor Colin Harbury of City University in London to rewrite a short book of his called *Descriptive Economics,* which had already gone through six editions. We worked it into a longer applied book designed to compliment *Positive Economics* and entitled *An Introduction to the UK Economy* (first published in 1983). In my earlier experience, UK students had often studied two books, a basic theory book such as *IPE* and an institutional-descriptive book which covered the structure of the UK economy. It has always seemed to me to be a shame that North American courses did not do the same. In North America, we typically turn out students who have been exposed to some quite complex economic theory but who know virtually nothing about the economy to which that theory is meant to apply.[15]

Two years later, Colin and I began to write a textbook called *First Principles of Economics* directed at the UK sixth form which is the UK equivalent of grades 12 and 13 (last year of high school and first year of university in many places). I was concerned about what seemed to me to be the low quality of textbooks available at that level and about the fact that, by introducing high school algebra into the macro part of the 6th edition of *IPE,* published in 1983, I had written it out of sixth form use.[16] Our job turned out to be much more difficult than we had anticipated and it occupied too much of my available time over three long years. In the end, it was only a modest success and, although I enjoyed and profited from my collaboration with Colin Harbury, it remains the one textbook I suspect I would not write if I had it all to do over again.

This raises the more general question: would I become a textbook writer if I had my life as an economist to live over again? The big negative is the amount of time and the unrelenting demands of publication schedules. Over the 35 years since I first began to write *IPE* I have been the only author, or the equal co-author, of 30 editions of six distinct textbooks.[17] In most of them, we rewrote a third to half of the material each time. The way we worked was for all of us to be responsible for all the material, and on successive editions, to alternate the chapters on which each author did the first draft of the revisions. This meant, as my co-authors will attest, that having more than one author, increased rather than reduced the workload because there were more critics to suggest new additions and revisions of old material. Also, the publisher's schedules are unrelenting. Miss a deadline by weeks, and you miss selling for the whole teaching year. I estimate that something like half of my research time, and something like one third of the time I would have otherwise devoted to my personal life has gone into the textbooks. Had I not written all these, I would have written probably twice the number of articles in learned journals; I would have written a book with Curtis Eaton on the work that we did in the 1970s on the foundations of imperfect competition and spatial economics; and I would have written a book on methodology.

On the plus side are four considerations. First, *Positive Economics* fulfilled my research programme of finding out what was wrong with the Robbinsian methodology which I had been taught. I think it did something — largely unnoticed by the profession — to end the old methodology in which the test of a theory was the reasonableness of its assumptions. Second, I think I did something to restore student interest in microeconomics, particularly in the UK and in the many foreign countries in which *IPE* has been sold. *(IPE* has been translated into 15 foreign languages and sold in a UK subsidized, English-language (the ELBS) version to the former British territories in Asia and Africa.) Travelling about the world, I meet my students everywhere and get immense satisfaction from their personal testimonies. Once, while passing through a remote checkpoint in Kashmir, the official inspecting my passport looked up and said, 'You are not the man who wrote *the* book?' When I said, 'If you mean *Positive Economics,* the answer is yes', he grasped my hand and said, 'Thank you'. That kind of satisfaction of meeting students for whom my books have been a real learning experience is massive compensation for learned articles not written. Third, all of the textbooks have helped to keep me the generalist that I wanted to be. I estimate that you need to know a minimum of three times as much as you write down in a chapter if you are to do it right. That means that, on every revision, you have to do an enormous amount of reading on all those areas in which you are not actively keeping up in the course of your own research. This is something which I know I would not have done if it were not for the relentless discipline of the textbooks. Finally, it would be less than honest not to mention money. I think I am one of the last writers of first-year textbooks not to have known that there was real money at the end of all the effort. I remember being in Lionel Robbins's office sometime after I had finished the micro half of the book. Lionel said he had heard that I was writing a textbook and did I know that John Hicks still made £500 a year on royalties from *Value and Capital*. The floor opened up and nearly swallowed me: 'Five hundred pounds a year' said I. I went home with dreams of real money to spend — and fortunately not knowing that £500 was what the book would make for me in its first weeks not its first year.

Public policy and the REStuds

In 1962, the conservative government under Harold Macmillan set up the National Economic Development Council (nicknamed NEDY) to investigate the conditions causing slow growth in the UK and advise on how the growth performance could be improved. Sir Donald MacDougal was appointed its director of economics under Sir Robert Shone. Donald asked me to join the staff. The LSE said I was needed for the first-year lectures so I merely spent all my spare time as a consultant. My co-worker and I botched a job of testing Frank Paish's views that operating the economy a little below potential was more conducive to growth than operating it a little above it.[18] When we finally got it right, a brief report appeared in the first NEDY report and a long paper that I wrote on it was never published (although I gave it at many US universities during my sabbatical year in the US in 1963-4).[19]

Early in the 1960s, I had been invited to join the XYZ club which met over dinner in the House of Commons and was meant to expose Labour politicians and sympathetic academics to each other's thoughts. I greatly enjoyed these dinners and, walking over Westminster Bridge from NEDY, where I was helping to advise the Conservative government, to attend dinner with Labour MPs, I was often struck with how far I had come from my beginnings in Victoria.

One memorable dinner came a day or two after Labour's surprising election victory in October 1964. The weekend after the election the new prime minister, Harold Wilson, had retired to the country for the weekend to decide on his policy with respect to the balance of payments which was showing a large deficit at the time. He decided against devaluing sterling and thereby hampered his entire first administration with the overriding need to support the overvalued pound. At dinner, a senior LSE colleague who was also a member of the club gave a defence of Wilson's decision. I was appalled. I was sure that devaluation at the outset of the term, which could in any case be blamed on the outgoing Tories, was the right thing to do. It was an early object lesson to me in the power of exchange rates to mesmerize politicians and academics, leading them down counter-productive alleys. This experience was much in my mind when I wrote 'The Balance of Payments and the Common Market', which is reproduced in these essays.
In the late 1950s, I was appointed to the board of the *Review of Economic Studies* still edited by its first editor, Ursula Hicks, but run to a great extent by its assistant editor Harry Johnson. After Harry had gone to Manchester, the Hickses and the Kaldors had a falling out. One of them had written an unfavourable review of a book written by another of them (I forget which). Nicky Kaldor and I had become good friends and to get back at the Hickses, Nicky hatched the plot of having me replace Ursula as the journal's editor. He was correct in arguing that, although a good applied economist, she was not in touch with the kind of theory her journal was then publishing so that she really should be replaced. But his real motives were less honourable. His plan was that the young economists on the board would lead a revolution and install me as editor with Frank Hahn, John Black and John Parry Lewis as assistant editors. The young board members met and agreed that a change was in order but, disagreeing with Nicky's motivation, we decided to ask both Nicky and Ursula to go. So the plot backfired, when on the appointed day the revolutionaries called for a replacement for both senior editors and we four youngsters were installed as the new team.

Not long after being installed, we began to get signals that all was not well with the administration. Letters asking about unfilled subscriptions, unanswered letters and uncashed cheques crossed our desk. After a few discrete enquiries, the person in Cambridge who had been handling the administrative side of the journal suddenly departed. We visited her office and there we found literally hundreds of unanswered letters, unopened telegrams and uncashed cheques. More than half of the correspondence of the last year had been unanswered. It took Frank Hahn and I many weeks to sort out the mess and write to the many confused, and often irate, subscribers.

New frontiers at Essex

Sometime in early 1963, I received a phone call from one Albert Sloman wanting to talk to me about a new university. I was very busy with NEDY and did not reply. He pursued me relentlessly until, more to get rid of him than to oblige him, I agreed to lunch at the French Club. Earlier, Chris Archibald, Bernard Corry and I had submitted a brief to the Robbins committee on higher education in which we argued that the proposed new universities were too small — a maximum enrolment of 3,000 was targeted for each. Sloman said he was stuck with this size, at least as the first phase, but intended to meet our concerns by concentrating on only a few departments so that each could reach optimal size, even though the whole university was still small. He also saw the university as a research institution which would give professional training to British students on the model of MIT rather than a dilettant's education on the model then prevailing in the Oxford PPE degree. I was swept away with the idea of a new frontier in which we would give the UK an educational institution suited to the professional needs of the last half of the 20th century. I came home from lunch and announced to my wife 'I am going to Essex!'.

I think I was the only person to leave a chair to go to a new university. (In 1961, I had been appointed an LSE Professor at the then-early age of 33.) Most of the others who were appointed to the new universities as founding professors were bright young lecturers who were offered the carrot of early promotion to a chair.

I will never forget the day when the five founding professors, the vice chancellor and the registrar sat down in the Senate House of the University of London and the VC said, 'Well gentlemen, let us proceed to build a university'.

For five years, I inhabited our new frontier. We all loved Albert Sloman and would have followed him to the ends of the earth.

I became Dean of the School of Social Studies, Chairman of the Department of Economics, and filled a host of other offices, such as chairman of the disciplinary and the catering committees (!) and member of the VC's unofficial inner advisory group which consisted of four or five key deans and department chairmen meeting once a week in the VC's room.

My experiences in those five years deserve an essay on their own. Suffice it to say that they were exhilarating, maddening, rewarding and frustrating in more or less equal amounts. At first, we had an idealized version of a Greek city state. Every person with tenure was a member of Senate and our debates were informed, constructive and witty. The staff was so small that we all lunched together in the local pub. Then as numbers increased, the scientists began lunching together, and the social scientists and arts people lunched as a second group. Then, as numbers rose further, the members of each discipline began lunching by themselves and the barriers that old universities attribute to some archaic practices and strive to break down asserted themselves quite naturally.

The student revolutions hit us hard and early. We mismanaged the disciplinary actions following a student breakup of a seminar given by a scientist from the government's chemical warfare research laboratory. From that time on, I was in the thick of many staff-student battles, student takeovers, strikes and who knows what else. Although the events poisoned many relationships, I was proud of the staff (both academic and support) and the graduate students in the economics department who acted as a moderating force and a liaison between staff and student leaders through our many battles. Our greatest success at Essex was to help to establish postgraduate training in the social sciences as the norm for the UK and to build large, research-oriented departments within a small university. At the time, it was usual for a person to get a good bachelor's degree and then take employment as a professional economist in the academic, private or public sectors. It was apparent to the younger economists that this was no longer appropriate. More training was needed. When I was promoted to professor at the LSE, I had tried to get a taught MA instituted, but to my surprise, my colleagues voted it down with many arguments, including the assertion that teaching beyond the bachelor's degree was too American in outlook. So I went to Essex precisely because Sloman saw the need for professional graduate training which went hand in hand with large, research-oriented departments (not at all the UK norm at the time). We succeeded beyond our wildest dreams. It turned out that the students were more attuned to the needs of the times than their professors. What UK academics everywhere had rejected, the taught MA in the social sciences was voted for by students with their feet. Graduate students came in large numbers to the University of Essex and within four years we were one of the largest graduate schools of social science in the UK. My memory says that we were second largest after the LSE but I cannot verify that. What is certain is that we had many students and the academic world is peppered with Essex economic graduates. Of our first graduating BA class of ten economists, at least four went on to become academic economists.

Then came a slap in the face. We were visited by the University Grants Committee which received the entire amalgamated university finance as a lump sum from the government every five years which it allocated among the universities. The committee was dominated by oldline professors who had rejected expansion of their universities, more professional graduate training and large research-oriented departments — all of the things that Essex stood for and was making work. They paid us back by criticizing us strongly in front of our lay council at the end of their visit and then determining that we got one of the lowest operating grants (my memory says it was *the* lowest but again I may be wrong on details) of all the 44 British universities. It was so low that we would not have been able to accept enough students (2,000) to fill the buildings that were already built. There was a question in Parliament and our grant was raised somewhat.[20]

This episode made me recall a conversation that I had with a well-connected LSE colleague before I left for Essex. He told me that the UK establishment did not want new universities to challenge the existing great ones. They wanted only second rate and obviously inferior copies. 'if you do succeed in what you are hoping to do in Essex, they will find a way of killing you' was his parting shot. Well, they did not kill us but they certainly found a way of wounding us.

I said to myself, 'I have devoted five years to building a university for the late 20th century and the country does not seem to want it'. I went home and phoned some friends in Canada and quizzed them about the academic scene there. Prospects looked good so I decided to take a year's leave to visit Canada and sound things out.

This was not my only reason for leaving Essex. A second reason concerned my own professional career. After five years of intense work in the administration I had done little reading in economics and was aware of failing behind a rapidly advancing frontier. I had to decide whether my future lay in administration or in remaining a professional economist. After much soul searching, I opted for the economist. It then became apparent that I must get out of all administration and that it would be very difficult to do so inside the university where I was so much a part of the fabric. With the best will in the world, the VC and I would find it difficult to keep me out of administration, especially in times of crisis.

Leaving Essex was like leaving one's first love. It was our creation and we loved it and were proud of it. I am still proud of what we accomplished and pleased that the School of Social Studies has gone on from strength to strength and currently rates among the top UK universities in the UGC's assessment of research accomplishments.

Return to Canada

Nonetheless, my decision to return to economics required leaving Essex, and the attitude of the UK establishment made it easier to leave the UK. Still, this was not an easy decision, as I had come to regard myself as British and I spoke of 'emigrating back home'. Furthermore, my second wife was English and all of my four children had been raised in England. For them, leaving England was in no sense of the term 'going home'.

After a year in 1969-70 as visiting professor at the University of British Columbia, I accepted the post of Sir Edward Peacock Professor at Queens University in Kingston, Ontario.

While visiting the University of British Columbia, I returned to my undergraduate interest in Hotelling's model. I also met Curtis Eaton and discovered that we were both working on the same model but from different perspectives. We decided to join forces and began a highly productive cooperation which lasted ten years and produced about a dozen papers on what we came to call 'address models of value theory'. Another research programme laid down when I was an undergraduate was now being fulfilled. These papers are published in the volume of our joint collected works, *On the Foundations of Monopolistic Competition and Economic Geography* (1997).

At Queens, I began by teaching welfare economics and stabilization policy, which was applied macro. I quickly reinforced what I knew already, that I had fallen way behind the frontier. I sat up late into the night reading just ahead of my students and it was three years before I felt I was back near the frontier of macroeconomics (which, contrary to popular belief, had never been my major interest).

Ever since my early work on the Phillips curve, I had been interested in incomes policies. A piece that Michael Parkin and I published in the late 1970s became quite influential, in spite of its flawed econometrics and is included in the collected works (Volume Two, Chapter 12). In the autumn of 1975, I was visiting the UK when the news broke of the Trudeau government's resort to a wage control policy in Canada. I immediately fired off a telegram whose approximate wording was 'STRONGLY PROTEST THE INTRODUCTION OF WAGE-PRICE CONTROLS IN CANADA STOP THEIR DIRECT EFFECTS ARE DUBIOUS AND THEIR LONG-TERM EFFECTS ARE HARMFUL'.

When I returned to Canada, I was asked to go to Ottawa with a number of economists to advise the lawyers who were considering the possibility of a Canadian Labour Council (CLC)-financed, court challenge. Since Canadian provinces have jurisdiction over labour matters, the Federal government needed to infringe on provincial powers to set up wage controls. The Feds found their justification in the 'Peace Order and Good Government (POGG) Clause' in the British North American Act which gives the Federal government almost unlimited powers in times of a national emergency'. The issue was whether or not ten per cent inflation, which had persisted for a year or two and had not yet been attacked by any of the traditional tools of fiscal and monetary policy, constituted a 'national emergency'. This was a pretty weak case.

Shortly thereafter, I was asked if I would prepare the expert evidence to accompany the CLC's constitutional challenge. The Federal government then referred the case directly to the Supreme Court rather than fighting the challenge through the lower courts. They must have known that this tactic made it harder for experts to attack their case since the Supreme Court is supposed to rule only on matters of law, not fact.

The question arose whether or not expert evidence could be submitted at all. There was no precedent for it. Yet the case turned on the question of fact 'is there or is there not a national emergency?' We decided that I would prepare the evidence and the lawyers would submit it hoping that the Supreme Court would have to read it in order to decide whether or not it was admissible.

I had two weeks to do the job. The first week was spent planning and the second week in writing. I holed up in my office at Queens, chain smoking cigars, and working my very good secretary day and night. For the last 72 hours I never closed my eyes. It is the longest I have ever stayed awake. When I was done, I drove the nearly 200 page manuscript up to the Kingston bus station — there were no courier firms in those days - and put it on the bus to Ottawa where an employee of the law firm collected it and delivered it to the Supreme Court. On the way home, I saw flying saucers — a phenomenon I later learned was common with people suffering extreme sleep deprivation. Considering the haste with which it was written, I think it was a pretty good document. It was later translated into French and published in a French Canadian journal but its only English record is in the Supreme Court of Canada's papers.

The outcome was that the Supreme Court, for the first time in its history, took notice of expert evidence by explicitly responding to its arguments. In doing so, the justices split three ways. The minority said the government could do more or less anything under POGG, while the majority said a rare emergency was required r action under POGG. That majority, however, split into two groups, one of which said there *was* such a rare emergency while the other said there was not! The net result was in favour of the government's current actions by the judgement of the first two groups, but simultaneously, by the judgement of the second and the third, to find that the government must not use this power lightly, implying that next time the result might be different. That seemed a good result to me. We cannot have the Supreme Court routinely second guessing the government on matters of national emergency but the courts can say in effect 'Be careful, do not abuse this power (as you may well have done this time but we do not want to say so)'.

In 1979—80, I spent six months in the UK as visiting professor at City University and then a year at Yale as Irving Fisher Professor.

I returned to Queens in the fall of 1980 and continued to teach graduate courses in macro theory and in stabilization policy. About this time, rational expectations and the new classical approach to macro theory, and soon real business cycle theory, began to become established. I sat up nights learning the new theories in order to teach them to my students and then add 'the hypothesis of rational expectations is a potentially fruitful one but I think the new-classical, market-clearing model is a dead-end line of enquiry; I think, however, that the profession will have to explore that alley all the way before the advocates accept that it has a dead end'. It seemed to me that I did not want to spend my own scarce time as part of that investigation so I decided to leave macroeconomics as a teaching field. By that time, I had, in any case, become increasingly interested in microeconomic policy. I had written several policy papers and was writing a regular monthly column in the *Financial Times.* First, I wrote the column alone but later I joined forces with my good friend and Queens professor, Douglas Purvis. Our efforts were honoured in 1982 when we received the National Business Writing Award 'for distinguished financial writing by Canadians who are not primarily journalists'.

While spending the summer of 1982 at our house in Ireland, I received a phone call from New York asking me to contribute a chapter on economic issues to a volume commissioned by the American Assembly entitled *Canada and the United States: Enduring Friendship, Persistent Stress.*[21] This was a new version of a volume first published in the 1950s and widely used since then. I knew nothing about Canada—US trade relations and did not even know a countervailing from an anti-dumping duty and certainly had not heard of 'escape clause actions'. So, under normal circumstances, I would have said ‘No’. However, in another one of those chances which have been so influential in my life, I had just learned of a serious personal tragedy and, to get the speaker off the line so that I could worry about my personal affairs, I said yes.

Over the next six months, I gave myself a crash course in trade policy and institutions. By the time I had finished my review of Canada-US trade policy and multilateralism under the GATT (having first learned more about the GATT than its name), I had become alarmed about the outlook for Canada. Protectionist sentiment seemed to be growing everywhere, particularly in the US. As a small trading nation, Canada seemed particularly vulnerable. On the positive side, when I studied Canadian industry’s adjustment to the large tariff cuts instituted by the Kennedy and Tokyo rounds of GATT negotiations, I concluded that the infant industry stage was over and large sections of Canadian industry could stand on their own feet. It was high time, it seemed to me, that we removed the rest of our still-comparatively-high tariffs letting those firms that could compete prosper and those who could not compete disappear.[22]

The C.D. Howe Institute

While I was working on the Canada-US manuscript, and becoming depressed with the state of academic macroeconomics, I was visited by Wendy Dobson, president of the prestigeful C.D. Howe Institute, a sort of Canadian counterpart of Brookings and the Institute for International Economics combined. She had heard that I was unhappy with academia and offered me good terms to move **to** Toronto and become Senior Economic Advisor to the Institute. I agreed and began five fruitful years at the Institute. Wendy has told much of the story of my time at the Institute in her article in the festschrift in my honour being published at the same time as these essays. (Eaton, B. Curtis and Richard G. Harris (eds), *Trade, Technology and Economics,* Cheltenham, UK and Lyme, US: Edward Elgar, 1997).

Soon after moving to the Howe Institute, I finished my essay on Canada—US economic relations. I immediately suggested to Wendy that I write a book assessing Canada’s trade options and testing my growing conviction that a free trade agreement with the US was the best route to preserving and expanding the international trade that was Canada’s life blood. I soon joined forces with Murray Smith who knew much more than I did about current trade policy and institutions. We thought we would be two small voices crying in the wilderness. By the time we were finished, however, we were two early voices in a mounting chorus suggesting that the free-trade option be seriously considered — in spite of the Canadian conventional wisdom that it was political suicide even to suggest free trade with the US.

Under Wendy’s tutelage, I learned to master economic policy, not just its analysis but how to influence it. Wendy taught me that to have influence, one had to appreciate the constraints under which policy makers were acting and advocate feasible policies, not just first-best policies. She also taught me the futility of the typical academic, over-the-transom approach: publish an article advocating some policy then move on to another issue. When Murray Smith and I had finished our book on free trade, I thought we were done. But to Wendy that was just the beginning. She made dozens of appointments in Ottawa for us. We expounded our analysis to all the relevant politicians and civil servants. At first it was intimidating, but after a while it was exhilarating. I particularly remember a meeting at the Department of External Affairs when about a dozen senior mandarins marched into the room all holding copies of our book, well thumbed and full of markers. For two hours they proceeded to grill us in detail on its contents and suggestions. With Wendy’s assistance, we helped to make it acceptable for Canada-US free trade to be taken seriously in Ottawa.

After our book came out, followed closely by the MacDonald commission report which made big news for its advocacy of free trade with the US, the government of Canada made the fateful decision to open negotiations. Fortunately, the opposition did not take the possibility seriously until too late. Once the government had made the decision to negotiate, there was little the mounting opposition could do to reverse the decision.[23] Nonetheless, the debate got hard and dirty. I made speeches all over the country; I wrote about a dozen pamphlets, and close to ten chapters in books; I appeared on numerous radio and TV talk shows and debates. One of the early debates was with Mel Hurtig, publisher and prominent opponent of free trade with the US. The televised debate was in the Grand

Theatre in Kingston, Ontario, a town associated with the name of Sir John A. Macdonald, the father of Canadian Confederation and the architect of the 'National Policy of 1878' which first introduced high tariffs to protect Canadian manufacturing industry.

When the agreement was finally signed, the opposition-dominated Senate blocked the enabling legislation and an election was fought on the issue of free trade. I had just finished my second book on Canada—US free trade, *A Guided Tour through the Canada US Free Trade Deal,* with Robert York. I took to debating platforms once again and, with a small band of engaged Canadian economists such as John Crispo, Murray Smith and Ron Wonnacott, we fought the detractors with the spoken and the printed word. From August to November, 1988 1 never took a single full day off while I was in Toronto. I did get away from Canada from time to time but never while in Toronto was there a day free from the battle.

On election night, Robert York and I watched the returns in my Toronto apartment. I had bought a bottle of Dom Perignon and said that, before the night was over, it would either be thrown unopened down the rubbish shute or opened in celebration of a victory for free trade. By midnight, the bottle was open and a celebration well under way. After the election, the let down was enormous and in early December I suffered what used to be called a nervous breakdown. When I told my doctor how I had behaved over the last six months, he said I was lucky to be alive.[24]
I was involved in several other important policy issues while at C.D. Howe. We tried our best to make the newly elected Conservative government take the budget deficit seriously in 1984-5. We failed. Had they done so, the pain would have been over before they came up for re-election and the country would not have been in the fiscal shambles it was in ten years later. I also wrote an 'Inflation Monitor' that tracked the anti-inflation policies of the 1980s and I commissioned work on the Bank of Canada's policy of zero inflation. I knew this policy was going to cause one of the great disputes of the decade as soon as I heard the Governor announce it in a speech. As a result of my early commissioning of articles, we had a volume ready for distribution just as the debate broke out in earnest.[25]

The CIAR and return to BC
In 1988, 1 was asked to become a member of the academic advisory board of the Canadian Institute for Advanced Research (CIAR). Led by a great Canadian, Fraser Mustard, the Institute was breaking new paths in encouraging advanced research. They had begun in the natural sciences, had branched out into population health and were now ready to get into economics with concern over economic growth. Fraser Mustard and I never agree on who seduced who but we were both willing partners in my becoming leader of the project entitled 'Economic Growth: Science and Technology and Institutional Change in a Global Economy'. Fraser Mustard, when he read an earlier draft of this piece had the following to offer:

> Ruth Macdonald (the wife of the former finance minister Donald Macdonald) was with the Institute and knew about our frustrations that there seemed to be little appreciation in Canada about the role of science and technology in economic growth. Ruth was a great friend of Wendy Dobson, President of the C.D. Howe Institute where Lipsey was acting as senior economic advisor, and in their discussions they suggested that the Institute should consider a program in economic growth and that Dick Lipsey should head it. Ruth and Wendy arranged for Dick and Fraser to have lunch with them at the University club. From Fraser's perspective ... he thought he was going to meet a fairly traditional economist. I [Fraser] was, of course, thrilled to find you [RGL] making the point that understanding technological change and economic growth was one of the largest if not the largest challenge facing economists. There was no doubt in my mind that you had to create the program.

So it appears that I am like the grown man who returns to the high school reunion to find that he was seduced by the school charmer when, at the time, he was fully convinced that he had seduced her!

Once I accepted the offer to lead the project, I needed a university base. Simon Fraser University made me, and the CIAR, offers we could not refuse. So in the summer of 1989, my wife and I went to British Columbia. For me it was a return to God's Country where I had been born; for Diana it was a move to the best climate and most exciting part of Canada. My daughter, Joanna, became the project's administrator and moved out to BC with her partner at the same time.

I led the programme for three years. (I should say that Fraser and I led it for he was very much a hands-on administrator, who took a strong interest in all of the projects he was funding.) Early in 1994, 1 passed the leadership over to Elhanan Helpman but I remain a Fellow of the Institute and a member of the Growth and Policy Project.

Rarely does a person get a chance to change fields so drastically in his 60s. For me, however, it was a golden opportunity to fulfil the last of my undergraduate research programmes: to understand and further develop the concepts that Schumpeter advocated and that so impressed me in my now-distant undergraduate days. Because I was launching myself into a field in which I was not well read, I embarked on two years of intensive reading into conventional macro growth theory and, more importantly, into the vast amount of empirical research on the innovative process and on the theories that are designed to have contact with that knowledge and that stem from the seminal work of Richard Nelson and Sidney Winter. It was also a chance to return to teaching, which I had given up in 1983. Since coming to SFU in 1990, I have given each year a course on 'Technological Change and Economic Growth's emphasizing historical and theoretical understanding and the need to have a deep knowledge of technology in order to understand the processes driving economic growth. A selection of my early articles on this subject is included in the micro volume of these collected works.

The great discovery for me after my return to this subject was the profound implications of accepting that technological change is endogenous to the economic system. As John Rae understood in the 19th century, much of classical and neoclassical economics is turned on its head when the implications of endogenous technological change are understood. I refer to the micro understanding of endogenous technological change that grew up in the 1950s and 1960s, decades before macro theorists discovered the concept. In 'Markets, Technological Change and Economic Growth' reprinted here (Volume One, Chapter 2), I take up the Schumpeterian theme of the irrelevance of perfect competition as a standard of efficiency and show how the stylized facts of endogenous technological change buttress the Schumpeterian position. In 'A Structuralist View of Economic Growth' by Lipsey and Bekar also reprinted here (Volume One, Chapter 3), I return to the grand themes of long-term growth: major changes in what we call 'enabling technologies' that cause occasional periods of deep structural adjustment, starting with the neolithic agricultural revolution and culminating in the ICT revolution that is today transforming our society with changes as deep as any in this millennium. My book on growth and change is scheduled for completion at the beginning of 1999. In line with my original motivation, to know rather than to discover, it will be a synthesis of what is already known. I hope, however, that it will add new understanding to the process of long-term growth and change by drawing together knowledge developed in many different strands of empirical and theoretical research.
It is too early to say where all this current work will end, but the journey that has taken me to the Schumpeterian ideas about long-term growth has been a fulfilling one, and I would not have had it differently. Theoretical research, university administration, applied research, policy advocacy and broad historical analysis, coming in that order have been a varied and satisfying diet.

A postscript on political views

My political views have often been a subject of public speculation. I have never supported a political party. Many of my Mountpelerin Society friends find me a wishy-washy liberal (in the US sense of the word), while many of my social democratic friends regard me as a reactionary conservative. While an undergraduate and an MA student in Canada, I began as a follower of Hayek and von Mises but then, as I saw more of market failures and human suffering, I moved closer to the position of J .S. Mill in accepting the organizing power of the market economy but believing in a strong social policy to alleviate some of the harmful effects created by free markets.

While I lived in the UK, I termed myself a fellow traveller of the Labour Party, in sympathy with their social policies but not willing to join them because I could not accept their non-market economic policies. During that time, I was in any case more interested in economic theory and methodology than in politics. I also fell victim to an error that I have repeatedly warned against in my later writings. I was raised on the formal defence of the price system that perfect competition produced an optimum allocation of resources. When my early experiences in Europe showed me how far that defence was from any economic reality, I erroneously concluded that there was no adequate defence of the price system. Only much later did I come to appreciate the enormous power of the informal defence, particularly in the context of economic growth driven by endogenous technological change.

Throughout most of my life I found myself more at home with the humanity and sympathy of my social democratic companions than with the attitudes of my friends on the more extreme right. Over the years, however, I have slowly come to accept that good intentions often produce measures with unambiguously bad results. I have also come to believe that the big state is not the best defender of the rights of the less powerful. Instead, it too often becomes the tool of the powerful, operating through such power groupings as unions and special interest groups. I believe that, as the 20th century was the century of the big government, the 21st century will become the century of smaller government and increasing private-sector initiatives, even in the social welfare sphere. I have also been forced to admit that my right wing friends were correct in claiming that many social welfare measures would end up adversely affecting the recipients as they responded rationally to the incentives we gave them. The result is dependent groups, and sometimes even dependent societies, as sections of the Canadian Atlantic provinces come close to being.

So I have ended up where I began, a follower of Hayek and a Gladstonian liberal (i.e., a liberal in the 19th-century European sense of the term). I have always accepted the value of the price system as a means of coordinating decentralized decisions on economic matters. Although I still believe that it should not be beyond human wit to alleviate some of the more socially undesirable results that the price system produces, I have also learned through bitter experience that this is much easier to say than to do. We must go on trying, but we must also realize that every social welfare experiment needs to be reversed as soon as it is clearly seen to have failed in its purpose.

Endnotes

1. That small precursor of the University of Victoria was then a two-year, arts-college extension of the University of British Columbia (UBC).
2. At that time, a Canadian university course lasted an entire academic year and one took five or six courses each year.
3. In private correspondence, Colin Harbury has offered the following observations: as an LSE undergraduate 1947—50 1 did not end up innocent of Keynes but, arguably worse, had to struggle to try to understand the cryptic series of papers in the *EJ* between Robertson and JMK which it seemed at the time were not fully appreciated by my teachers! I was, however, luckier than you on grasping the stock/flow distinction because I had the great advantage of watching James Meade operate the Phillips' water machine and reading the *Economica* article on it.'
4. I will not say more about my first marriage except to note that the name of my first wife will convey to many that the word 'tumultuous' is not an over dramatization: she was the Assia Gutman (whom I met as Pam Steele) whose association with Sylvia Plath and Ted Hughes was later to achieve international notoriety.
5. UK ranks then were Assistant Lecturer (non-tenured), Lecturer (tenured), Senior Lecturer (the top grade for those who did not have a distinguished research career) and Reader (the typical top grade for those with a distinguished research career). A Professor (who was said to hold a chair) was rare. In many smaller departments, there was only one professor who was also the head of the department. LSE was unusual in having about ten professors of economics.
6. I have several times been the subject of historical reconstructions that were wide of reality — so much so as to make me mistrust all such historical accounts written by third persons. One obvious error was in Harry Johnson's review of my thesis whose belated publication is explained in the text (Johnson, *Economic Journal,* 82, 728—30). According to Harry, I was a 'soldier' trained for active combat' and sent into battle 'by the [LSE] high command'. Anyone who knew the LSE at the time would know that my account of LSE's totally *laissez-faire* approach to supervision was the correct one rather than Harry's assumption that the LSE in the 1950s was like the Chicago he knew in the 1960s, in which PhD students were directed to particular subjects by their supervisors. (Naturally I prefer Max Cordon's review of my dissertation.) Two other reconstructions are Neil de Marchi's version of the days of the M^2T seminar and Nancy Wulwick's interpretation of Phillips's and Lipsey's motive in writing the famous Phillips curve papers. Giving my side of these matters would require more than the space available in this far-ranging essay — although Mark Blaug has several times urged me to 'correct' de

Marchi's version. Fortunately, Robert Leeson is doing his best to cope with the myths that Nancy Wulwick is trying to create. See articles by Leeson in *The History of Economics Review* (22, 70—82), a forthcoming reply to Wulwick in the same journal and another forthcoming in *The History of Political Economy,* as well as articles by Wulwick in *The Southern Economic Journal (53,* 834—57), the *Oxford Economic Papers* **(41,** 170-88) and *The History of Economics Review* (22, 83-94).

7. We should not worry that some assumptions are untestable, nor should we insist, dogmatically, that only testable statements should be admitted into a theory. My only point is that everything in a theory that has empirical content, whether in its assumptions or its predictions, is a valid subject for testing because we want to know if anything we say that has factual content is actually consistent with the known facts.
8. Nancy Wulwick has me '... itching to get into combat against Robbins and other old-line liberals such as Plant and Paish'. To me this seems nonsense, I was a first-generation university student from a provincial corner of a provincial ex-colony. I was awed to be in this intellectual company. I was itching to figure out my methodological concerns and when I did, I wrote my text *Positive Economics* to proselytize them but I always had the highest respect for Lionel Robbins. When I went to Essex, I sought to recreate the atmosphere of his Wednesday afternoon seminar, There is no doubt that I was then to the left of the old-line liberals, but I did not have a great interest in politics and was concerned mainly to understand and evaluate the basis for the claim that economics was, or could become, a genuine science.
9. In the preface to early editions of *An Introduction* to *Positive Economics (IPE),* I incorrectly asserted that the book grew out of my first-year lectures. (I am not sure why I told this fib, but it might have been because I thought it would help to sell the book.) The facts were the reverse. I volunteered to give the first year lectures in the revised degree introduced in 1961 because I was already well on with my book.
10. Since there has been some controversy about my reasons for writing IPE and about what I thought was important in it, it is fortunate that I committed my views to print at the time. See 'Positive Economics in Relation to Some Current Trends', reprinted in these collected works (Volume t, Chapter 23).
11. Colin Harbury has pointed out in private correspondence that, compared with the average UK text in use at the time, Samuelson contained much empirical material. I agree, but still maintain that this was almost all on the macro side. In contrast, he neglected microeconomics in the way that I describe.
12. Two others who got it right, and made some related criticisms of the use of identities in economics. were Kurt Klappholz and Ed Mishan, both members of the M^2T seminar. See their 'Identities in Economic Models', *Economica,* May 1962.
13. In the old model, when *ex ante* S did not equal *ex ante* I. unintended investment occurred in the form of inventory accumulation so that the identity $S \equiv I$ could be fulfilled.
14. The LSE first degree was three years. After a common Part I, students specialized in any of the many social sciences taught at the school. Those who chose economics could specialize in International Economics, Money and Banking or Economics Analytical and Descriptive (A&D) where most of the theorists and many of the applied economists were located. In the old degree, Part I took two years; in the new degree instituted in 1961, Part I was only one year. Until I left to go to Essex, I taught the basic micro-macro theory course in the new degree.
15. In private correspondence, Professor Harbury has challenged this view saying that UK students typically studied only one book that mainly covered pure theory. Cohn certainly has more experience than I, so possibly my sample was a biased one. In any case, what I advocate still seems a good idea to me, even if it only existed in a fictional UK Utopia of my imagination.
16. Readers of the manuscript have suggested other reasons why the use of *Positive Economics* declined in the early 1980s. Be those as they may, the sales of the first year of the 6th edition set a one-year record but then fell off significantly. This is consistent with my view that the elementary algebra that I introduced into the macroeconomics section of that edition does much to account for the book's rapid disappearance from use in the UK sixth form.
17. I did the full revision of seven editions of *IPE* and half of the eighth (having brought in a co-author, Professor Alec Chrystal of City University, in London, England for the 8th edition published in 1995); I shared equally in revisions of ten editions of *Economics* and took a small part in the 11th published in 1995 (and have now passed that book over fully to Paul Courant for the 12th and subsequent editions). I did all three editions of *An Introduction to a Mathematical Treatment of Economics* equally with Chris Archibald; I took over half responsibility for the Canadian edition of *Economics* with Doug Purvis from the third edition and did the eighth

myself after Doug's untimely death, and I have now recruited a co-author, Chris Ragan of McGill University, to share the ninth with me and eventually to take over the whole of the Canadian edition; I did both editions of *First Principles* equally with Cohn Harbury and two of *An Introduction to the UK Economy*. This makes altogether 30 editions of major textbooks in which I was either the only author or an equal co-author.

18. Sir Donald tells this story in some detail in his memoirs.
19. If Nancy Wulwick's allegation that I was a card-carrying member of the Labour Party, or even a prominent supporter, was correct, I would never have been asked, or agreed, to serve on NEDY.
20. No doubt there were many other reasons for the UGC's rejection of Essex. One additional important reason was the student unrest, another was the narrow base of subjects which resulted from our decision to concentrate on a few subjects so as to gain economies of scale in these.
21. Chapter 3. 'Canada and the United States: The Economic Dimension' in *Canada and the United States: Enduring Friendship, Persistent Stress,* C.F. Doran and J.H. Stigler (eds), (Englewood Cliffs:Prentice Hall) 1985.
22. There has not been room to include in the present volumes any of my writings on trade policy, which would in any case fill a volume on their own.
23. I have told this story in 'Will there be a Canadian—American Free Trade Association?', in *The World Economy,* September 1986, 9, 217—38.
24. I also played a not insignificant part in Canada's decision to invite itself to the US-Mexican free trade talks and so turn a hub-and-spoke model of bilateral agreements for hemispheric trade liberalization into a NAFTA which embraced all contracting parties within one agreement. This was done while I was a member of the government's International Trade Advisory Committee (ITAC) and chairman of the working party overseeing the working of the US—Canada FTA, so the story must be told at a much later date.
25. *Zero Inflation,* Lipsey, Richard G. and Robert York (eds), (Toronto: C.D. Howe Institute, 1990).