

# From Flapper to Bluestocking: What Happened to the Young Woman of Wellington Street?

John F. Helliwell\*

- *In 1961, the Royal Commission on Banking and Finance (the Porter Commission) was established to investigate the roles and responsibilities of the Bank of Canada. Based on submissions from the Bank, the Commission favoured a credit-conditions approach as the most appropriate way for conceiving of the structure of monetary policy.*
- *As part of a group of macro and monetary economists from across the country advising the Porter Commission in 1962, John Helliwell participated in the surveys of and interviews within large corporations to determine the effect of monetary policy on them. In this first encounter with the Bank he learned much about the workings of the Canadian economy and met many of those who would influence the Bank's direction.*
- *The Porter Commission promoted the Bank's preparedness to deal with future monetary policy research, encouraging the use of fellowships to attract researchers and publication of the Bank's research and statistical work with the aim of improving Canadian monetary and financial information.*
- *From 1965 on, the Bank began to develop a quantitative research capacity. Helliwell and his colleagues worked on the construction of an econometric model of Canada—RDX1, followed in rapid succession by RDX2.*
- *On its completion in 1971, RDX2 was drawn immediately into the policy arena with the Nixon*

*shocks, the end of the Bretton Woods system, and the oil-price shocks of 1973. Bank researchers were giving papers at meetings of the Econometrics Society around the world, and the Bank of Canada was on its way to operating in the front ranks of the world's evidence-based policy research institutions.*

**W**e first met in 1962. Although we were both still in our 20s, she was, from my vantage point, a serious older person, although not what you would think of as a bluestocking intellectual. When she was approaching the age of 30, her parents saw fit to send her out for career counselling, and thus established, in late 1961, the Royal Commission on Banking and Finance (the Porter Commission) to do the job. Perhaps you are already wondering, in this age when gender is negotiable, how I am able to ascribe femininity where the legislation did not, and when it could still be said that “the primary qualities of good Board members should be intelligence, wisdom and good judgement. We believe these can best be found by choosing highly qualified men from varied backgrounds and experience” (Porter Commission, 548). First, there are her strong family links to the Old Lady of Threadneedle Street, established at the time of the Macmillan Report in 1933, and continued by several of the witnesses called before the Porter Commission. This assumption of femininity was confirmed when I was first employed in the Research Department of the Bank, in the fall of 1965, assigned to build a structural model of the foreign exchange market.<sup>1</sup> To gain better insights into the workings of the market, I spent an afternoon in the interbank foreign exchange trading room in Montréal.

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1. The exchange rate was determined at the intersecting point of separately identified and estimated private and official net excess-demand equations for foreign exchange (Helliwell 1969).

A fairly lively session was ended by “she’s in for 50” (units of 100,000 \$US), and there was no doubt about who “she” was. That settled the gender question for me once and for all, but it is getting ahead of the story.

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For this history,<sup>2</sup> the important parts of the Porter Commission relate to what it heard and what it said about the research base for monetary policy. The basis for the Commission’s approach was provided by the Bank’s own submissions (Bank of Canada 1962). The only echo there of the preceding Coyne Affair is Governor Rasminsky’s communiqué of 1 August 1961 (Appendix to Submission 2, 23–24), which contains his view that “in the ordinary course of events he believed that the Bank of Canada had the responsibility for monetary policy, but that if the government disapproved of that policy it had the right and responsibility to direct the Bank as to the policy which was to be followed” (Porter Commission, 540). He subsequently made clear, as have his successors, that if he were to receive such a directive, he would immediately resign. As one might have guessed, there has never been a directive issued.

The main substance of the Bank’s submissions relate to what they and the Porter Commission described as the “credit conditions approach to monetary policy.” The Porter Commission was preceded by the Radcliffe Report in the United Kingdom and the Commission on Money and Credit in the United States, and perhaps partly for that reason was able to tell a more complete and coherent story about the objectives, structure, and constraints of monetary policy. Credit conditions are “reflected in the availability of credit as well as in the

effective yields obtainable on financial assets of various kinds” (Bank of Canada 1962, 11). The Bank submissions were clear that credit conditions were to be thought of as endogenous variables influenced by the structure of financial markets, by changes in the demand for goods and services, and of course, by Bank policies, operating principally through variations in cash reserves (Bank of Canada 1962, 28). Monetary policy was seen as part of an overall mix of fiscal, monetary, and debt-management policies appropriate to “the degree of utilization of the nation’s productive capacity and labour supply, the degree of pressure on price levels, and the state of its balance of payments” (Bank of Canada 1962, 9).

The credit-conditions approach was in turn adopted by the Porter Commission, both as a basis for its survey and other empirical work on the effects of monetary policy on expenditures (Porter Commission, Chapter 21, plus Appendix volume), and as the most appropriate way for conceiving of the structure of monetary policy. The cost and availability of credit both mattered, as did the term structure of interest rates. It should be no surprise that debt management merited both a separate submission (Submission 4), and a Porter Commission research study by Jacques Parizeau so soon after the Conversion Loan of 1958, which was one of the few debt-maturity increases large enough to have had macroeconomic consequences.<sup>3</sup>

The Porter Commission assembled in 1962 an impressive fraction of the country’s macro and monetary economists. By one of those lucky breaks that optimists assume in the same way that economists often assume perfect information, I was invited along as a young spear carrier in the wake of the University of British Columbia’s John Young, one of the assistant directors of research (with Don Daly and the Bank’s Bob Johnstone) under Research Director Bill Hood, then at the University of Toronto. Harry Johnson, oscillating between Chicago and the London School of Economics, spent the summer in the Porter Commission’s offices (in Toronto at Yonge and St. Clair) producing with John Winder an early econometric analysis of monetary policy lags. Grant Reuber of the University of Western Ontario was there doing his pioneering estimation of monetary policy reaction functions (Reuber 1964), later followed up in the Bank’s own RDX models, many years before they became known as Taylor rules.

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2. In revising the paper for publication, I have been much aided by confirmations, amendments, and corrections kindly provided by Fred Gorbet, George Post, and Ian Stewart.

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3. As subsequently revealed by simulations of RDX2 (Helliwell, Christofides, and Lester 1976).

Ron Shearer worked on the current account, learning in the process that the gnomes of Zurich were in fact the treasurers of importing and exporting firms changing their foreign exchange purchase and sales patterns in fundamental ways when times were uncertain (Porter Commission 298–99; Young and Helliwell 1964, Chapter 11). And times they were indeed uncertain, as on 2 May 1962, Canada ended its 11-year period of floating exchange rates much as it had begun, as a means of dealing with uncertainty. Canada had opted for a flexible exchange rate because officials were unable to find a fixed rate that would settle the markets. At the end, in early 1962, at least as I heard the story, the Minister of Finance wanted assurance that, if the rate was left to float freely (reserves were flowing out fast in April as the government tried informally to hold the rate at 95 cents U.S. per Canadian dollar), it would not drop below 90 cents. Officials could not provide that assurance, even though they believed the equilibrium was well above that rate, so it was decided to peg the rate. And 92.5 cents was apparently chosen as the simple average of the 95 cents they had been defending and the 90 cents no one wanted to breach.

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The main empirical work of the Porter Commission, under the direction of John Young, was a large mail and interview survey of the effects of monetary policy on corporations. By including interviews with presidents and treasurers of the largest corporations in the country, it was possible to cover directly about half of the country's business-capital spending, and the Dominion Bureau of Statistics managed stratified sampling of the rest. There were critics of the interview approach to untangling complex decisions, but texture gained thereby was important. The prevalence of rules of thumb for capital-spending decisions prepared us for the subsequently perennial finding that a highly smoothed cost of funds always dominates more

immediate measures of interest rates in econometric estimates of business fixed-capital expenditures. And it was also easy for us to see, especially from the experiences of the smaller firms, how tightness in bank lending fed through pretty quickly to inventory squeezes. Most helpful of all, getting back to the uncertain times, was the fact that there were two periods of tight money in recent management memory, 1956 to 1957 and 1959, and even as the Porter Commission questionnaire was being prepared there came the foreign exchange crisis of May 1962, accompanied by a striking, if short-lived, bout of tight money.

The survey and interviews took us everywhere, and thereby I got to meet John Young's Ottawa friends: Gerry Bouey, then Chief of the Bank's Research Department of 72 persons (including library and clerical support staff) lodged in the wooden temporary building on Sparks Street, and Simon Reisman, then in the Department of Finance. It was by this route that I was first introduced to the young woman of Wellington Street, as well as indirectly through the Bank's Bob Johnstone, and later Al McKay, working for the Porter Commission.

I have described the Bank's credit-conditions approach to monetary policy as having a big impact on the Porter Commission approach and conclusions. This approach has stood the test of time remarkably well, being fully consonant with what I take to be the forefront of current research on the effects of monetary policy. In return, the Porter Commission asked how the Bank might be better prepared for dealing with future monetary policy and research. They took the position that the growth in research training and tools would require expansion of the Bank's small-scale use of outsiders on summer projects and temporary assignments. They "might create occasional fellowships to encourage studies of financial markets and policy of value to the Bank" (Porter Commission, 552). That part is now in welcome operation, although with a 40-year lag that would have looked long even to Harry Johnson and John Winder. The Porter Commission also approved the Governor's suggestion that the Bank might undertake more econometric and other work on the impact of monetary policy on spending decisions and hoped (552) "that the Bank will not hesitate to publish more of its own research and statistical work as part of a continuing program designed to improve Canadian monetary and financial information."

So when she reached 30 in the mid-60s, when the young were being advised "never to trust anyone over 30," the Bank started to develop a quantitative research capacity. George Post had already been brought in

with his newly minted PhD in the econometrics of investment, and I turned up in the fall of 1965 on a back-and-forth sharing with Nuffield College, Oxford.<sup>4</sup> In early 1966, Ian Stewart was brought in from Dartmouth to be the full-time Bank leader of modelling, and Larry Officer and Harold Shapiro were brought in as academic consultants. All three of them had constructed econometric models of Canada for their PhD theses; George Post was spearheading the development of a computerized data bank; and Mike McCracken was developing econometric software at the Economic Council of Canada. The first Bank modelling was started in the summer of 1966, and by the following summer, boxes of computer cards were sent off every night by bus to the Université de Montréal computer centre, to be returned the following morning with the first mis-punched card turned upright in the box. All of this was taking place in the building seen immediately to the left of the Bank, shown in the photo taken from the far side of Wellington Street. Modelling teams were sometimes sent off to spend the night at the Université de Montréal computing centre to increase the number of daily turnarounds. It was a time when it really paid to follow the old carpenter's adage, so often ignored when computing is too easy, to "measure twice and cut once."

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Progress really speeded up when the Bank acquired a terminal connected by long-distance modem to a university computer in Salt Lake City. It was pretty hard even then to send a full deck of cards without a line failure, and the late-night teams needed to be fuelled by large tins of cookies. But well-being is evaluated, I have learned since, by how one's current

4. George Post reminds me that the stage was well set for us by the quantitative research already being done at the Bank by Peter Cornell, Bernie Drabble, Dave McQueen, and others.

circumstances compare with where one started, and with one's expectations of what is feasible. We were happy with our lot, and with our progress.



Wellington Street (c. 1963). (Photo courtesy of the Bank of Canada Archives, PC 300.5-236)

The first model was a Meade-Mundell-Fleming open-economy affair with a supply side based on an expectations-adjusted Phillips curve, detailed modelling of the housing and mortgage markets, and a monetary policy reaction function for the short-term interest rate.<sup>5</sup> Even George Freeman, the management enthusiast for our modelling efforts, thought that we might in this case be going too far: "How could it be possible to reduce the complex art of Bank decision-making to a simple equation?" We researchers argued that such decisions were probably systematic, in which case an equation might establish the key historical determinants. Or perhaps they were just random, in which case nothing would turn up, so why not let the chips fall where they may? We compromised by carrying on as planned, and reducing the possibility of potentially embarrassing commentary by simply including the reaction function in the model under the generic labeling of "short-term interest rate equation." If the Bank had been more adventurous, might the results have been Rasminsky instead of Taylor rules? With prices quasi-fixed in the short run and flexible in the longer term, the model offered short-term policy trade-offs that disappeared in the longer term (as depicted in Helliwell, Officer, Shapiro, and Stewart 1969).

5. We did not even consider using the perfectly mobile capital version of the model so often used in later theoretical work, since it was then, as now, importantly at odds with the data.

The model was initially named HOSS, after the initials of the main contributors, with attendant horseplay about what was needed to make HOSS run. Something more formal was needed if the model was to be a flagship for the increasingly sophisticated 30-something woman of Wellington Street. Calling the model RD for Research Department sounded like too much of a commitment to Senior Deputy Governor Robert Beattie, who thought RDX would better reflect the experimental nature of this research. So it was called RDX1, before we could be reminded by Paul Bradley, originally a chemical engineer, that RDX was already the name of an explosive. When it came time to publish a series of model papers in 1969, fulfilling the commitment to make Bank research available to researchers outside the Bank, the Deputy Governor dropped the other shoe: "Isn't it premature to publish a model which is still in the experimental phase?" But George Freeman prevailed, and the Bank of Canada Staff Research Studies series was born.<sup>6</sup>

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RDX1 was not yet in print by the time RDX2 was under construction. The original conception had been to move from the aggregate model RDX1 to a sectoral RDX2, and much industrial-level modelling had been put in train. However, industrial disaggregation would have starkly limited the development of a fuller and tighter integration of the supply side of the economy, of mutually consistent short- and long-term dynamic properties, of an integrated financial system, and of fuller and more integrated linkages with other economies. Seen in those terms, the choice was easy. The aggregate business sector became the core of the pri-

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6. The first seven Bank of Canada Staff Research Studies described the RDX models and their supporting research. The Staff Research Studies series, and the contemporaneous creation of the *Bank of Canada Review*, put into effect the Bank's commitment to enlarging and opening the national capacity for quantitative macroeconomic research.

vate sector in RDX2, with factor-demand equations consistently derived from hierarchical modelling of cost-minimizing behaviour. The same aggregate firm was used as the basis for a congruent set of equations modelling price-setting, short-term adjustment of employment and average hours, and factor-utilization (or, equivalently, inventory-accumulation/decumulation) responses to unanticipated changes in demand or profitability. RDX2 was the first, and remains perhaps the only, model to have an integrated hierarchy of factor demands (including hours and intensity of use) based on their relative speeds and costs of adjustment.

Both RDX1 and RDX2 differed from previous models in their detailed modelling of the government sector, with separate treatment of the main expenditure and revenue components for both federal and provincial (and municipal) governments. The explicit treatment of the demand-side and policy forces governing the evolution of fiscal balances, spearheaded by Fred Gorbet's research for his PhD thesis, permitted the behaviour of automatic stabilizers to be studied more realistically than previously, contingent on the model's inherent dynamic structure, always the hardest part to pin down (Helliwell and Gorbet 1971).

While the real side was based on the emerging literature on consistent modelling of output supply and factor demands, the links between the financial and real sectors were inspired by Tobin's (1969) general-equilibrium approach to monetary theory. Fuelled by the innovative portfolio modelling of Gordon Sparks (including measures of portfolio disequilibrium used for modelling the effects of credit availability on investment spending), RDX2 was the first and probably only full-scale embodiment of Tobin's  $q$  theory of investment and of the supply price of capital (both described in Tobin 1969), with the latter driven by prices in both bond and equity markets. The supply price of capital was defined in both nominal and real terms, differing by the endogenous expected rate of change of the consumer price index, with the expectations process derived from modelling the relative demands for debt and equity.

Development of matching book and market values of the business-capital stock took a lot of work, as did consistent measurement and modelling of the domestic and foreign-ownership ratios that were needed for the explanation of international flows of capital services. We should probably have automated these data-assembly processes to a greater extent in the first instance, as they proved difficult to maintain in later years as staff turned over.

RDX2 was designed for international linkage on four major fronts: trade in goods and services, direct and portfolio capital movements, migration, and exchange rate determination. By the time RDX2 was released to the world in 1971, Canada was back on a flexible exchange rate system, and we were scrambling to make RDX2 equally usable under fixed and flexible exchange rates (Helliwell and Maxwell 1972). Recognizing the large share of trade and capital market linkages with the United States, and taking advantage of a somewhat similar (but much more closed) MPS model of the United States then under construction,<sup>7</sup> all Canadian flows of goods, services, capital, and people were split between US and the rest of the world (ROW). The US flows were then linked to variables endogenous to the MPS model of the United States, and the ROW flows linked to relevant aggregates of ROW variables. When I was at the Bank in 2003–2004, I politely (I hope) bemoaned the fact that the Bank now runs, in different departments, separately conceived U.S. and Canadian models without explicit linkage. Some things are easier when research groups are smaller.

With the completion of RDX2 in 1971, the Bank's bluestocking conversion was complete, and papers were being given at the world and regional congresses of the Econometric Society in Australia, New Zealand, the United Kingdom, and the United States. The RDX team relied heavily on effective combination of full-time researchers, part-timers, and student assistants, some of them economists whose later careers included a Nobel Prize, presidencies of major universities, and many federal appointments at the deputy minister level, as well as, of course, senior Bank of Canada management.<sup>8</sup> Collaborations with other central bank modelling teams were developing apace, and Project

Link was just getting started. The RDX2–MPS bilateral linkage was not part of the forecasting structure of Project Link,<sup>9</sup> but was included because it was the only international modelling project that extended linkage beyond goods into services, direct investment, portfolio flows, and exchange rate determination, directions that others were keen to travel when data and modelling resources permitted.

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The Bank thus went in a very few years from flapper to serious bluestocking. Especially because this was done with the active involvement of university-based researchers, this took the Bank and its research into the public arena in ways that even now seem pretty adventurous. I spent 1970 full time at the Bank, paid for by a Killam Fellowship from the Canada Council, taking days off to be an expert adviser to the House of Commons Finance Committee, then studying tax reform. We even developed within the Bank, and subsequently published, a stochastic general-equilibrium model (Helliwell 1968) of the macroeconomic implications of the highly contentious tax-reform proposals of the Royal Commission on Taxation. Gerry Bouey and George Freeman took the progressive view that it should be possible to keep the Bank's reputation and independence unsullied by the other involvements of its part-time researchers, and their optimism seems to have been justified.

Once RDX2 became operational, it was drawn into the policy arena pretty quickly. Less than a month after the Nixon shock of 15 August 1971, the Federal Reserve Bank of Boston held its annual conference, this one fortuitously focused on financial relationships between Canada and the United States. Governor Brimmer of the Fed was given the unenviable job of defending the application to Canada of import surcharges designed to convince other countries to revalue their exchange

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7. The model was known under different names, e.g., MPS for Michigan, Penn, and the U.S. Social Sciences Research Council; and MIT-Fed, for MIT and the Federal Reserve Board. It became the Federal Reserve Board's macro-econometric workhorse.

8. Fred Gorbett adds: "My major comment is that you are missing what to my mind is one of the most significant contributions the Bank made through its pioneering modelling efforts, and that was serving as a centre for the creation of intellectual capital that later became deployed in public policy-making through the transfer through the Bank and into government of people like George, Ian, myself, and a host of bright young researchers from Quebec, particularly, who were attracted by the intellectual rigour of the research being done at the Bank and who have gone back to very senior positions in the Quebec public service. Michel Caron, Gilles Godbout, Jean-Guy Turcotte, Jean St. Gelais, and Henri Paul Rousseau are a few of the names that come to mind immediately. This is a very important public good that the Bank's pioneering efforts created." I agree with him. The Bank was very farsighted in hiring people like me, who had not yet completed their PhD dissertations, and giving them time, space, and the support (intellectually as well as otherwise) to do so.

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9. The first major world meeting of Project Link, under the direction of Lawrence Klein, was held in Hakone, Japan, in 1969. Stephen Goldfeld (1974, 279), reported, I am sure on good information from Ian Stewart, that RDX2 alone had more computer code than all of the national models of Project Link put together. The national models used in the first years of Project Link are described in Ball (1973), which also includes a chapter on the RDX2–MPS bilateral linkage.

rates (Brimmer 1971). Since Canada had already been back on a floating exchange rate for a year by then, and the Canadian dollar had already been revalued by the market, the Canadians present, including Deputy Governor Bill Lawson and George Post, were inclined to cry foul. Round-the-clock work by Ian Stewart had generated RDX2 simulation results (Helliwell 1971) showing that Governor Brimmer was wrong to have expected the 1970 revaluation of the Canadian dollar to have produced more immediate effects on the bilateral current account. The quality of the Canadian evidence in response far exceeded that underlying the original policy and its defence. Participants from both countries were more convinced by the solidly established research base for the Canadian case, and the ability to produce the simulated bilateral current and capital account consequences on demand. RDX2 appeared at two subsequent Boston Fed conferences. The first, on international aspects of stabilization policies, in 1974, was a natural place to show how the bilateral transmission of monetary and fiscal policies was influenced by alternative ways of modelling trade, capital movements, exchange rates, and migration. While the qualitative results were regarded as useful, and endogenous migration proved a more important channel than many would have expected, Stephen Goldfeld noted that much depended on model dynamics, especially in the MPS model, that were difficult to pin down with either firm theory or strong evidence. This, in my view, was the weakest link of the large quarterly models, with or without endogenous expectations; relatively small changes in specification could lead to quite large changes in dynamic responses, even if the longer-term equilibrium properties are pinned down by the appropriate restrictions. This inability to discriminate conclusively among alternative short-term adjustment paths is part of what led many subsequent modellers, both inside and outside the Bank, to rely more heavily on annual data, and to impose more explicitly forward-looking expectations structures when and where these are also consistent with the data.

Almost from the time of RDX2's birth, and of the Nixon shock that marked the beginning of the end of the Bretton Woods system, commodity prices, including most importantly, the price of oil, had begun their upward spiral that would soon lead, spurred by the Arab-Israeli War, to a trebling of world oil prices in 1973–1974. The challenge for modellers, and for monetary authorities, was whether to treat this as a price-level shock to be accommodated or something that needed to be offset by drops in other nominal prices

so as to keep the overall price level stable. On average, the central banks of the world treated the shock as a one-off level shock and were prepared to provide monetary accommodation to partially cushion the immediate effects on aggregate output and employment, hoping that wages would not start an upward spiral. It was not as common then as it is now to make use of price indexes with the more volatile components removed and to stake out a middle ground that had some potential for accommodating the oil-price-level effects while assuring those setting wages and prices that non-energy inflation would not accelerate.

There was always bound to be some stagflation in the train of the oil-price increases; the issue related to how to manage monetary policies, and, most importantly, expectations of monetary policy. In retrospect, the first oil shocks should have been accommodated less fully. Once inflation rates started to approach double digits, people forming expectations started to change gears (to follow the terminology introduced by John Flemming in 1976), altering their emphasis first from the price level to the rate of inflation, and then to the rate of change of the rate of inflation.

The stagflation of the 1970s inspired the Boston Fed to hold their 1978 Edgartown conference, entitled "After the Phillips Curve: Persistence of High Inflation and High Unemployment." This was the conference where Lucas and Sargent (1978, 50) made their assertion "that modern macroeconomic models are of *no* value in guiding policy, and that this condition will not be remedied by modifications along any line which is currently being pursued." Challenged by Ben Friedman to state exactly where the predictions of macroeconomic models had been "wildly incorrect," they replied that, in 1970, leading models had suggested that 4 per cent growth could be accompanied by 4 per cent inflation. To an evidence-based researcher like me, it was an eye-opener that comparing *ceteris paribus* model properties to a shock-ridden period of history would constitute grounds for dismembering an entire line of empirically based research. My assignment for the conference, as the discussant for Lawrence Klein's paper, was to assess the extent to which the Project Link national models had in fact been able to forecast, starting in 1973, the stagflation between 1974 and 1976. They had all predicted stagflation, even if less than that which had actually occurred. All of the models were capable of capturing the broad stagflationary consequences of a supply-side shock, although at that time there was still little empirical basis to permit the modelling of gear-changing inflationary expectations. I concluded that the models all needed work on the

modelling of inflation expectations, and many also needed a supply-side better equipped to deal with oil-price changes. But I had certainly found no evidence to support what Lucas and Sargent had been saying in the previous session.

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What was even more surprising to me was that the equilibrium real-business-cycle approach Lucas and Sargent offered as their preferred alternative was to be based not on estimation and formal testing of one structure against another, but on whether a calibrated version of their theoretical model could be used to derive distributions of endogenous variables that looked something like the actual distributions. This seemed a very unsatisfactory way for evidence-based social science to operate. When I eventually realized that the production core of the real business-cycle-model was nested within the RDX2 supply-side framework, it was then easy to do nested hypothesis testing, and to show at remarkably high levels of significance that the so-called “Solow residuals” taken to represent exogenous changes in technology in fact contained the largest part of the variance of output, easily explicable within the RDX2 factor-utilization framework (Helliwell 1986).

The Bank was approaching 40 during the aftermath of the first oil-price shock. Canada was partially sheltered by a flexible exchange rate, but faced the excise-tax features of oil-price increases. The federal government responded with a temporary price freeze on domestically produced oil, a process that took several years to

unravel. Canadian oil and gas exports were taxed and restricted, and the National Energy Board, provincial and federal governments, and the oil and gas industry were united in the view that, despite the price increases, the production curves for oil and gas were on a downward slope that made gas from the Mackenzie Valley Pipeline essential to meet domestic needs by 1980. Driven by student interest in modelling the macroeconomic effects of such a large project, a pipeline sector was developed at the University of British Columbia for RDX2, and the results entered the public policy arena pretty quickly. The Bank then really needed to rely on its policy, adopted earlier by Gerry Bouey, of a clear separation between Bank and non-Bank uses of Bank research and models. Just as well, because parallel cost-benefit modelling soon showed that the presumed immediate need for frontier energy resources was a fiction. This pitted a few academic scribblers (Pearse 1974) with RDX2 in their toolkits against the National Energy Board and the vast weight of government and industry opinion.

I marvelled then, and often since, that the trust and mutual respect among Bank and non-Bank collaborators was such that the modelling co-operation proceeded unimpeded for the whole decade of the 1970s. I have since learned, in the course of research into the determinants of well-being, that such trust not only fosters good research and policy-making, but also increases the happiness of all concerned. This makes it that much easier to build and maintain the intellectual capital required to keep the Bank at the forefront of macroeconomic research. The growing size and maturity of the Bank’s internal research teams meant that there was less need for active outside leadership. The woman of Wellington Street was by now a freestanding bluestocking able to recruit with the best of universities, to offer challenging research careers, and to operate in the front ranks of the world’s evidence-based policy research institutions.



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