5. A life in economics

Edmund S. Phelps*

I was born in Chicago in the summer of 1933 – at the bottom of the Great Depression, as my parents often recalled. Both my father, who was in advertising, and my mother, a nutrionist, ultimately lost their jobs, getting by with help from their parents until 1939 when my father found a job in New York. We settled in a quiet suburb up the Hudson River called Hastings, where I attended the public school until graduation in 1951.

There were some clues in those formative years that I might become an economist. In the evening walks we took when I was four my father taught me to identify the automobile models we saw on the street. Later, at age seven or so, there was my admired survey of all the cats in the complex of apartments where we lived. A few years later I liked to spend the late afternoon by the main road recording the distribution by state of the licence plates of the cars passing by. My kindergarten in Chicago was for gifted children, which my mother only recently mentioned (figuring, I guess, that it would now be safe to tell me). I did very well in school. My parents gathered from all this that I would be some kind of researcher, but it was not clear in what area. No economics was offered in high school (nor sociology or political science in those edgy post-war years). Bored, I spent increasing time with music. Nevertheless I did devour the newspapers my father brought home each night from the city that so excited my imagination. The financial and economic news was a staple of dinner-time conversation. My father had majored in economics and my mother in home economics – also clues, perhaps.

COLLEGE AT AMHERST

Like most Americans entering college, I started at Amherst College without a predetermined course of study and without even a career goal. My tacit assumption was that I would drift into the world of business – of money, doing something terribly smart. In the first year, though, I was awestruck by Plato, Hume and James. I would probably have gone on to major in philosophy were it not that my father cajoled and pleaded with me to try a course in economics, which I did the second year. The lecturer was James Nelson, an economist from Harvard with a trenchant and witty lecturing style, who was to captivate a succession of students – Pollak and Stiglitz, for example. The new textbook in use, by Paul Samuelson, was brilliant. I was hugely impressed to see that it was possible to subject the events in those newspapers I had read about to a formal sort of analysis. The teacher of the next course was Arnold Collery, out of Princeton, whose syllogisms presented a stark contrast. As George Ballanchine believed that a white wine could not be too dry, so Arnold believed that an economics lecture could not be too dry. What launched me into the study of economics, then, was the usual thing: top-drawer instruction.

What drove me on and on in the study of economics was also rather common, it seems. As others have commented about their experience, I kept hoping that if I took just one more course the hidden harmony of economics would be revealed to me and I would be released from its dismal prison. In my own case, however, this gap in understanding was specific. I had a vague sense that the microeconomics taught in one set of courses was not communicating with the macroeconomics in the other courses! You could not read Hansen's or Haberler's surveys of the dissonant views in macroeconomics without becoming aware of this problem. With some hesitation, I decided to go to graduate school. Yale was a new star in the firmament, Lloyd Reynolds having assembled a super-department in just a few years, and it was offering big fellowships. So I went there.

GRADUATE WORK AT YALE

In graduate school, I had close contact and rapport with two of the most brilliant economists at Yale, the Americans James Tobin and Thomas Schelling. I was grateful to them for their lucid teaching, and amazed at their high competence and ability to make everything look easy. In my last year Arthur Okun arrived, himself fresh from graduate school at Columbia. He was a model of braininess and relevance who supported my dissertation and was always a partisan of mine.

I was also drawn to several economists from Europe, especially to the Central Europeans William Fellner and Henry Wallich, the one from Hungary and the other from Eastern Germany. I looked to them for the key to some counterparadigm that might turn macroeconomics around – private knowledge rooted in their background that they must have been too courteous to spread in their lectures and writings. *Mitteleuropa* became a fascination, and I relished the old films and recordings of the interwar period. In fact Fellner and Wallich clearly had the beginnings of such a countertheory. With Central European subjectivity being second nature to them, they emphasized the role of agents' expectations of inflation and, more generally, of prices and wages. It would not be going too far to say that they intuited the idea of a natural rate of unemployment. Yet a substantive conception of how the equilibrium unemployment rate is determined did not occur to them. Fellner produced a model based on a labour union, but its applicability to the American economy seemed to me too narrow to be of much interest.

In the end, my dissertation was based on an idea by Jim Tobin, after my own ideas all came to seem unworkable. (It was another decade or more until I finally assimilated what every mathematician learns early – to work out an example.) The idea, which was hell to work out algebraically, that demand shifts generate an algebraically higher correlation between price change and output change across industries than cost shifts do, and that property provides an indicator of whether costs push inflation, was important in the 1950s. The conceptual framework was awfully problematic, however.[1]

With parchment in hand, I jetted in June 1959 to Los Angeles to begin my first job, at the RAND Corporation in Santa Monica, then devoted mostly to Air Force work. A disproportionate collection of the brightest and deepest of my generation were there – Daniel Ellsberg, Alain Enthoven, John McCall, Richard Nelson, William Niskanen and Harvey Wagner, among those who became well known. Yet it became clear to me that for anyone such as myself who was not intending to throw himself primarily into defence work – whose most meaningful work was to be done in his spare time – the absence of a broad academic stimulus and soundingboard was a serious disadvantage of RAND. I decided to try to get an academic position. Oddly enough, by far the best offer was a research position at Yale's Cowles Foundation combined with reduced teaching at Yale. So in 1960 I was back to thinking full-time about macroeconomics and back to the peculiar New Haven commute between the American Keynesian camp and the Central European crowd.

MASTERING THE TRADE AT THE COWLES FOUNDATION

The five and a half years at the Cowles Foundation formed a distinctive phase of my research, and a necessary stage in my professional development. My best known paper of the period, on the 'golden rule' of national saving, grew out of the industry of research on growth paths started by Solow's famous paper.[2] Other papers on vintage models of investment introduced by Johansen, including one on the technology that I dubbed 'putty-clay', followed. Another line of research extended the Ramsey model to 'risky capital'. I felt these papers were not as deep as I was capable of, however, and compensated by writing a great many of them. An exploration in the monetary area showed more originality: first a paper introducing the concept of 'full liquidity', which could be achieved by sufficient deflation or own-interest on money; and secondly a small book examining the hypothesis that 'fiscal neutrality', which entailed the rule of balancing the budget intertemporally, would, if internalized into people's expectations, deliver optimal growth in some appropriate sense. Working on these problems was often fascinating and usually fun. Coining several terms such as the ones in quotation marks above, some of which caught on, was a particular pleasure. The quality of my interactions with Tjalling Koopmans and, later, David Cass, has stayed in my mind.

One of the rewards in the middle of this period was an invitation to visit MIT in 1962–63, which turned out to be its 'golden year', as Paul Samuelson later called it. There were unforgettable conversations with Paul, some of which I mined for years. I got to teach a course in capital theory with Bob Solow, who clarified the subject as much for me as for the students. Franco Modigliani, who had just arrived, was also hugely stimulating.

Within a few short years I became an internationally known economist. Yet I came to feel that I was simply winning (or losing) footraces by a few steps. I saw that if I was to do anything of unusual depth and distinctiveness I would have to think much harder than I had generally done – to raise the level of my game. There is, as I was to appreciate better, a big difference between scanning existing models for their unnoticed implications, on the one hand, and, on the other, acquiring an independent empirical sense of how in some overlooked or misunderstood way the economy works.

One might have thought that this success at playing the game would be rewarded with promotion to tenure. However, Yale had squandered one tenure slot after another in those years, building up what must have been the largest Economics Department in the country, until the President was finally resistant to creating yet another. But it has to be added that few among the professoriate showed willingness to fight the Administration on the matter. Although I had spent long enough at Yale not to mourn leaving, I worried that the episode would be a setback for my reputation and that the next location might be markedly less convenient.

Tenure offers at full rank arrived from the University of Pennsylvania and Northwestern University in 1965. Remembering the wind off Lake Michigan when I was a child, and preferring to be near New York, I chose Penn. My departure from Yale in January 1966, which actually began with an accumulated semester of leave and an SSRC fellowship, marked a kind of passage into my years of high creativity, which were nearly continuous for a decade.

YEARS OF DISCOVERY AT PENN

My efforts at a theoretical understanding of the Phillips curve began in earnest over the summer of 1966 in the Sidgwick Avenue building at Cambridge and my first few months at the University of Pennsylvania in the autumn. In the preceding winter I had written a paper on optimal inflation/unemployment control, published the next year, in which an expectations-augmented quasi-Phillips curve was written down:[3]

$$p - p_{-1} = \phi(u) + p^e - p_{-1}, \tag{1}$$

equivalently:

$$p = \phi(u) + p^e, \tag{1'}$$

where p is the money price level being set, p^e is what it is expected to be, and u is the unemployment rate. But there was nothing about the microeconomics of the function ϕ. Furthermore, the money wage level was implicitly the passive partner of the price level rather than the other way around, as Phillips and most practitioners supposed. A microeconomic understanding of the relationship between inflation and unemployment did not yet exist.

With the benefit of hindsight the puzzles I was struggling with can be reduced to a few basic problems: how can there be involuntary unemployment, particularly in conditions of equilibrium in the expectational sense? How could the unemployment rate remain, however briefly, below its natural level? In such an infra-natural state, what is the process by which nominal wages go on spiralling upward? How might one introduce into this model the Lerner–Fellner acceleration hypothesis that as long as monetary policy, say, kept the unemployment rate below its natural level, the rate of increase of the average wage would steadily increase? I had only a foggy notion at best of the answers to any of these questions. However, I did have the sense that the way to the answers was somehow to lay out a model – not a complete system of differential equations, but a serviceable description of a highly stylized hypothetical economy nonetheless.

There were bits of labour economics that I started from with each new attempt at a model. I had read a little of Dunlop and Slichter, the Harvard labour economists; Paish, the LSE economist; and Wallich, my colleague over several years at Yale. From them I took away the impression that when the economy is pressured, at least for a time, into operation at a level in excess of its equilibrium steady-state level, the low unemployment rate poses various inconveniences for firms, which try in turn to cope by setting higher wage rates. I also had a more recent memory of the dynamics of employment arising from employee turnover behaviour as it was modelled by Richard Lipsey in an otherwise econometric paper of his on wage inflation and employment. Yet these insights, however necessary, were missing something fundamental, it seemed to me. They did not put us into the mind of the firm, or its personnel manager. Man is a thinking, expectant being! What was needed was a model of a sequence: the firm's expectations, its subsequent actions and those of the others, the discovery of the others' actions, the formation of new expectations, and so forth.

I had also read the paper on wages and employment, replete with econometric estimates, by Sargan of the LSE. This paper postulated a required nominal wage level that is an increasing function of the employment rate (hence decreasing in the unemployment rate), given expectations of the price level. I took from this paper the rather important point that the rate of increase of nominal wages is a function not just of the level of unemployment, but also the change of employment. It also encouraged my impression that when firms plan to increase employment they offer an increased wage simultaneously; the wage is not completely described as a feedback response to discovery of changes in the market wage and the total unemployment rate. On the other hand, at the embryonic stage of my thinking then, this paper was a distraction and an unnecessary complication. For weeks, I focused exclusively upon expectations of the price level by the personnel manager and his employees rather than their expectations of what the general money wage level is going to be.

By the time I was settled into the University of Pennsylvania I had a 'story' about labour-market equilibrium and wage dynamics – to use the two phrases that made up the title of the paper I was attempting to write.[4] The unemployment rate might move to so low a level that, to moderate the associated quit rate, every firm wants to offer its employees a *higher real wage* as an inducement not to quit with such readiness; but as all firms pass along the resulting money wage increase, the price level increases in proportion (beyond what it was going to do anyway), an increase that is unexpected. To keep the unemployment rate down, there must be a succession of such wage increases and hence continually unexpected inflation – hence an inflation faster than whatever rate was expected. In this scenario, the unemployment rate is below equilibrium; the steady-state equilibrium must be one with a larger rate.

One day, though, it struck me that something was amiss with this story. Suppose that each wage increase in accompanied by a proportional increase of productivity, so that the price level remains unchanged and the real wage is increased. Then it would be implied by the original story that the reduced unemployment rate was consistent with equilibrium. Each advance of the real wage would generate another reduction in the equilibrium volume of unemployment, causing equilibrium unemployment to vanish in the limit. No satisfactory theory could have this implication. The mistake in the first model was that it made the employees' quit rate at a firm respond to an increased real wage independently of whether the same increase in real-wage rates occurred at all the other firms.

The model was then reconstructed: the quit rate is a decreasing function of the firm's relative wage. For simplicity, only the relative wage and the unemployment rate determine the quit rate, not the real wage. In the revised version, if the unemployment rate is driven to a sufficiently low level, every firm raises its wage in the expectation of achieving an increase in its relative wage in order to induce a moderation of its quit rate; but as all firms try to outpay one another the result can only be disappointment – a disequilibrium in which expectations of the money wage at other firms are found to be too low. Equilibrium in the labour market thus requires a large unemployment rate – large enough to dissuade the representative firms from attempting an unrepresentative outcome. The resulting Phillips curve was:

$$w - w_{-1} = \phi(u) + w^e - w_{-1}, \tag{2}$$

where w denotes the money wage level. The equilibrium steady-state unemployment rate, which makes $\phi(.)$ equal to zero, is a positive number. If monetary policy keeps on yanking up firms' nominal demand prices in order to induce firms to go on employing beyond the steady-state rate, firms will pass along each round of wage increase in proportionally higher prices; money wages will continue to go up, round after round, always in excess of what firms expect them to go up by.

A number of features of this model stood out. As already noted, the invariance of labour-market equilibrium to whatever inflation rate was expected was a sensational aspect. This was not because there was intense substantive interest on the part of economists in whether a steady inflation of, say, 6 per cent per year, might make for tighter labour markets than 5 per cent. As I suggested at the start of this paper, the fascination lay in the implication that Keynesian aggregate demand management – through monetary policy, at least – could not achieve an arbitrarily chosen unemployment rate within some admissible and reasonable range. Keynesian forces could only make transient departures from the gravitational pull of the natural rate.

A second feature was that the unemployment existing at the natural rate, and indeed virtually everywhere on any equilibrium path, was involuntary – not just in Keynes's sense of the term, but in the everyday sense that the unemployed could not get a job by offering their labour for less than the going wage. As far as I can recall, I was not fully aware of this implication at the time of writing, nor for some time after! But eventually it became clear to me why the model implied that an unemployed worker could not obtain a job that way: if the firm were to accept such a worker at a lower wage though that worker did not apparently differ from employed workers with regard to the likelihood of quitting, the firm would have to assume that the worker's quit rate would be higher as a result; but that tradeoff would be inoptimal for the firm to accept since it had already calculated the optimum on the wage-quitting opportunity locus.

Another feature – an 'optional extra' – of the model was the property that, starting from unemployment in excess of the natural level, the equilibrium path would approach the natural rate only gradually. The argument was simply that firms will not jump their employment rolls to the natural level since they face rising marginal cost of imparting firm-specific training, or indoctrination, to new recruits. Whether a specified unemployment rate today will generate unexpected inflation thus depends on the rate yesterday. The augmented Phillips curve became:

$$w - w_{-1} = \phi(u, u_{-1}) + w^e - w_{-1}. \qquad \text{(2rev)}$$

Hence there was an equilibrium path of the unemployment – a path along which the expected wage is always matched by the actual wage, hence a path given by $\phi(u, u_{-1}) = 0$ – that approaches the natural rate only asymptotically. This was the notion of persistence. (In contrast, the idea of 'hysteresis', as used in my 1972 book, at any rate, referred to the effect of unemployment history on the natural rate, either a permanent effect or a long-lasting one.[5])

This exploration would have been a heady experience even on a desert island, but it became even more exciting when I saw that several other theorists, most of them too young to be known in the profession, were beginning to chart the same waters, usually with a broadly similar perspective – the angle of incomplete or imperfect information. There was a unique opportunity to convene an informal conference to talk about our venturings into this new area. I called Jim Blackman at the NSF, who had strongly backed me before, and Donald Lamm at Norton, with whom I worked on a classroom paperback series, and got the financing and the book contract I needed to go ahead. The conference was held at Penn over a weekend in January 1969. We learned quite a lot from each other at that time, it seems to be agreed, but the conference volume turned out to be the more significant result. This collection of papers with my introduction – which came (with flagrant inaccuracy!) to be called the 'Phelps volume' – seems to have become something of a watershed event in the history of macroeconomics. It had an impact on the macroeconomics fraternity far beyond the sum of the impact that the papers would have had coming out separately and in a trickle.[6]

It would be an omission of some of my more novel and free-wheeling work at Penn not to mention several excursions, many of them in collaboration with colleagues, into related or only distantly related areas: game-equilibrium growth with Robert Pollak, the effects of public debt on capital deepening with Karl Shell, the effects of monetary and fiscal policies on inflation with Edwin Burmeister, customer markets with Sidney Winter (then at Berkeley), and optimal population growth – the Mozart effect, as Nordhaus dubbed it, which was taken up by Julian Simon. It was also important to have as colleagues two outstanding authorities in macroeconomics, Lawrence Klein and Sidney Weintraub, even if at that time we did not have interests that precipitated any active collaboration. I finally saw that I had been fortunate to spend this most seminal period with economists who were interesting, ambitious and uncommonly open to new ideas.

Once or twice in those first professional years I remember feeling like a vessel for the outpouring of ideas and I wondered whether it would go on and, if so, for how long. It did not go on, nothing of that richness, at any rate. There was, in fact, a bit of a slump following the end of my first marriage and the fitful reconstruction of my personal life. Settling in New York City while still teaching at Penn over 1970–71 was hard (I began to hope for a satisfactory appointment in New York), and the city still offered the excitement and distraction for which it was known. However, a second period of serious originality turned up.

THE 1970s IN NEW YORK

This new phase in my work began with my joining the Economics Department at Columbia in autumn 1971 and ran about eight years. It was the third time that changing jobs helped me to turn the page and tackle new problems. With Kel Lancaster and Ron Findlay, I participated in the rebuilding of the Department – Pheobus Dhrymes and Robert Mundell at the senior level, and Guillermo Calvo and John Taylor (about whom more later) at the junior level. Prospects for a good run were pretty bright, and they were to be realized.

My personal life also entered a new phase. At Columbia I met Viviana Montdor, who had come from Buenos Aires (via Paris). When we married in 1974, I also gained a stepdaughter, Monica, and my parents a granddaughter. As anyone who knew us then will recall, we later added our remarkable dog Shaggy, a warm and just pal to each of us. Thus settled into family life, I soon began producing papers – and ideas, I think – at a fairly high rate.

The seeds of most of the research that I did *outside* macroeconomics in those years were planted in the academic year 1969–70 spent at the Center for Advanced Study in Behavioral Science at Stanford. Before leaving for the Center, I met with Amartya Sen, who showed me his new work on social welfare and conveyed to me the importance of John Rawls, the great philosopher, who was also going to be at the Center. I had considerable interchange with Rawls, as it fortunately turned out. There were also some meetings with Kenneth Arrow, one of our greatest economists, with whom I had become acquainted at RAND through Bob Summers, then a Yale teacher of mine and the husband of Ken's sister Anita. Through Jack I became acquainted with a philosopher set, some of whom had similar or kindred interests, including Tom Nagel and Tim Scanlon. A whole new field had sprung up in front of my eyes.

My work on economic justice grew out of this stimulus. After missing the point at first, I finally argued for a concept of economic justice that could be understood as the perspective of Rawls's book *A Theory of Justice*: it means justice in the society's design of the reward structure used to motivate the contributors to production and exchange and to allocate the resulting gains.[7] This apparently innocent definition leads to the view that no economic justice can be owed to those in the society who are not contributors, nor to those in other societies with whom there is no trade or other co-operation – though there exist just terms under which international co-operation could take place. (This view was not developed in the Penguin paperback I edited in 1974 called *Economic Justice*[8] but it is put forward in my 1985 introductory text *Political Economy*[9] and in my essay on economic justice in the *New Palgrave Encyclopedia.*[10])

A related series of papers explored the implications of the Rawlsian 'maximin' criterion for the structure of tax rates in a market economy. An unexpected result was the finding that the marginal tax rate on the top income from labour must be zero, for if it is positive, the state is missing an opportunity to make a mutually advantageous deal with the highest earners that lowers their marginal rates and thus encourages them to earn more and pay a larger tax bill. Three papers on the optimal balance of taxes on labour and capital, two of them co-authored with Janusz Ordover, a former student, produced rather less in the way of quotable results.[11] A paper on maximin-optimal economic growth in an overlapping generations context, this one co-authored with John Riley, yielded some results which lived on in the work of Alan Auerbach and Laurence Kotlikoff.[12]

Also growing out of my year at the Center was a short paper on what I called statistical discrimination, the idea that people use the societal group in which a person belongs to type him. Finally, a paper of mine appeared on the theory of the inflation tax which provided a much-needed general-equilibrium perspective,[13] though it was misleading in suggesting that necessarily the optimal tax on cash balances was positive – a question on which my 1972 book managed to be more nearly correct.

My most important work of this decade, however, was probably the co-operative programme that I began with Guillermo Calvo and John Taylor at

Columbia to reconstruct the Keynesian paradigm on the foundation of rational expectations cum non-synchronous wage-setting. The latter idea, which goes back at least to Fellner, was explored in the final pages of my 1968 paper on money wage dynamics and labour-market equilibrium and in an appendix to the 1970 version; but it was not properly worked out.[14]

Non-synchronous wage- or price-setting became an escape route from the new classical paradigm of Robert Lucas and Thomas Sargent. Implicitly, their paradigm stood as a criticism of the models in the *Microfoundations* volume in which expectations were not postulated to be rational in the sense of Richard Muth. One curious feature of the models in that volume was the property that, leaving aside the esoteric wealth effect emphasized by Metzler, a change in the money supply, if immediately declared or certainly if preannounced the day before the change, would cause expectations of the price level and the nominal wage level to change equiproportionately and thus cause actual prices and wages to do the same, leaving real balances, the real rate of interest and the rate of unemployment unchanged – provided that people's expectations showed an understanding of the underlying homogeneity property of our models. It was only changes in the velocity of money stemming from poorly understood or unnoticed causes that would have a non-neutral and generally a disequilibrating effect.

The second limitation of the seminal models was less obvious and more interesting. In the event that an unanticipated war broke out, say, the employment rate (if starting close to the natural rate, at any rate) would move to a level above the natural level. In part this would be because wage rates would not have risen in anticipation of the war or because the typical firm underestimated the rise in demand experienced by other firms, and thus also underpredicted the general rise of wage rates – which would operate to hold down its own wage increase. But, as a sort of after-shock, there would tend to be a continuing elevation of employment above the natural level, corresponding to a continuing deficiency in the level of money wage rates in relation to the war-swollen level of demand, as long as the war went on unabated. The reason is that firms would not increase their wages by the whole amount necessary to accommodate fully the increased demand as long as they considered the chances that the war would end with as little warning as it began; the firms would hedge against this risk. This seemed all quite wonderful to some of us, but the advocates of rational expectations brought a new insight to bear that changed the thrust of the model. If the wage was right on the average, because firms had the probabilities of war and price right, then, disregarding any non-linearities, we may conclude that the *expected value* of employment is equal to the natural level – a boom if the war continues, a recession if not. Thus the model, when supplemented by rational expectations, failed to deliver the possibility of a boom or slump for the duration of the underlying disturbance in terms of the *expected value* of the employment rate. All of this was nicely formalized in the rational-expectations models.

This work at Columbia began with a paper by Taylor and me in which, to make the point as simply as possible, we supposed that all prices are set each period with a lead-time of *two* whole periods.[15] So if a shock comes in the present period, it will affect not only current output but also output in the next period as well, since it is too late to adjust prices in time to neutralize those foreseeable consequences for output. Taylor's later papers, particularly those on wage-setting, most nearly resembled the sketch of overlapping money-wage commitments such as I had discussed in my 1968/1970 paper. A little later Calvo worked out his continuous-time model of overlapping price commitments, which was great fun and a wonderful pedagogical tool.

The other interactive work I cherish from that period is my investigation with Calvo of implicit contracts under modern postulates of what came to be called, in something of a misnomer, asymmetric information – A does not know everything that B knows and maybe (symmetrically) vice versa.[16] Costas Azariadis, working in the classical tradition of insurance analysis, in which all states of the world are fully observable and there can be no unshared or concealed information, had developed a model of the optimal wage-employment contract between a risk-averse worker and a risk-neutral employer. The setting had the feature that there is some sort of transaction cost to be paid by the worker – say, an airfare or a voyage-long commitment – so the worker wants to have an understanding of what will happen to his earnings and leisure under each contingency, every one of them observable to the worker. The implications for the optimal contract included a kind of real-wage rigidity while, independently of that feature, the employment/layoff level reflected the marginal utility of leisure and the worker's risk-aversion in an optimal way. The setting and the conclusions were wildly at odds with the doctrine I had been trying to develop, so I was eager to figure out what, if anything, was wrong with it. Calvo and I worked out, to a degree, the optimal contract under conditions in which the worker cannot observe the state of business prospects – for all he knows, the employer would pretend that business conditions dictated his services every day; the employer is not trusted to do anything but equal marginal revenue product to the wage, which is precisely what the employer proceeds to do. We showed that the optimal real wage might be lower the more depressed the employment level in the firm (or in the industry) and the more elevated the general price level. We also concluded that when times were bad there might be underemployment. This brief work was probably the high point of our collaborations, and it is a pity that some confusions on my part remained to mar the final text. The line of approach was taken up by Sanford Grossman and Oliver Hart a few years later, and by Matthew Canzoneri. I think, though, that the implications of the modern contract-theoretic approach to wages and employment has not been adequately developed and its implications not adequately tested.

All three of us, I am sure, took tremendous pleasure in our interaction at Columbia, which ran for a decade until Taylor left, then Calvo. As Robert Lucas once exclaimed to me, I had an entire school there at Columbia. It is not given to many to have that experience. But precisely because the others were so brilliant it was remarkable that the group held together as long as it did.

During this period of the 1970s some papers of mine on disinflation were a clue to one of the directions I would later take.[17] These papers showed that, if rational expectations were assumed, the winding down of inflation could be accomplished without a recession; indeed, a transient boom could be a by-product, as one of them pointed out. I was as uncertain as readers must have been over what to make of this finding. Later, Laurence Ball, now at Johns Hopkins, was to pick up this theme.

The significance of those latter papers finally became clear. They served to demonstrate the possible abuse of the idea of rational expectations. It is one thing to portray an economy guided by beliefs based on its well-studied past that are the subject of an understood *consensus* as possessing the stochastic equivalent of rational expectations. In this special situation, equilibrium analysis may give an acceptable approximation. It is quite another thing, however, to analyse an economy 'as if' rational expectations were an inherent property – as if the agent's guess was as good as any, so the analyst may as well treat it as the theoretically correct expectation. There are situations in which an agent cannot have a clear idea of the expectations of the other agents and thus a theoretically based expectation of what actions the other agents are going to take. An agent cannot use the analyst's model to form his expectations since he has little or no idea of how, quantitatively, the other agents are using that model or even if they have not switched to some quite different model. This is the thrust, as I recall it, of my paper in early 1980 on the 'trouble with rational expectations' in the context of disinflation analysis.[18]

It was a special pleasure to discover that a former Columbia student, who I had gotten to know better during a year at NYU some time earlier, Roman Frydman, had been working on expectations formation from the same perspective. Roman was to go much further than I, showing that the expectations-of-expectations problem may prevent agents from converging to the rational-expectations equilibrium. The scepticism and hostility that research so admirably basic as this met in the profession was sad to see, even for a near 50-year-old veteran such as myself who had seen the tactics of scorn and derision, in Harry Johnson's memorable phrase, used before. I felt bound to counterweigh this reaction with as much encouragement to Roman as I could provide and to do what I could to see that this work was given a fair hearing.

(This experience and our extensive collaborations have presented me with a rich friendship.)

A subsequent paper by Frydman and my manuscript were published in a 1983 volume of papers from a conference we organized in 1981.[19] This volume also contained a paper by a former Columbia student, Juan Carlos di Tata, in which he independently discovered the same problem. It has been something of an uphill battle for this volume. In the 1980s it was not taken up by a significant segment of the profession, and some of its message, it has been remarked to me, seeped into professional consciousness without recognition (or even knowledge) of where it had come from. Now, in the 1990s, some kindred work is being done by David Canning, lately of Columbia and now of the University of Belfast; by Bart Moore, a doctoral student of mine now at Rutgers University; and by Marcus Miller of the University of Warwick who, by coincidence, did some research assistance for me at Cowles three decades ago. Further, Frydman and I ourselves began a new paper a couple of years ago, and Frydman together with a former student of his, Michael Goldberg, have done some theoretical and empirical work based on these and subsequent ideas.

It took me a while to understand the rather low receptivity to the volume. For some time I could see only that a science develops momentum in a certain line of analysis, that something like an industry develops with its accumulated conventions and standards. (Recently the argument occurred to me that a researcher can normally expect to maximize citations by correcting or building upon an established and ongoing research programme – since there will exist so many citations that may be diverted from others to one's self – not by venturing into an area where there are few or no citations to begin with.) It may be, too, that scientists feel driven to know the outcome of research along one line before shifting to another line. But another problem was that readers of these models going beyond rational expectations could not easily foresee what second generation of results could be hoped to derive from them. (Some readers might think to say this is obvious because it is well known that economists only like constructive work, not Teutonic essays in critical political economy. But that would be counterfactual, as evidenced by Milton Friedman's critique of the Phillips curve, in which nothing useful was put in its place, or Lucas's critique of econometric policy-making, which does not tell us, if I am not mistaken, what to do instead.)

A PERIOD OF SYNTHESIS: THE FIRST HALF OF THE 1980s

Somewhere, several years ago, I saw an analysis of the typical profile of scientists: the period of apprenticeship and subsequent mastery, the years of creativity, and finally the period of synthesis – if I remember the word used – in which the individual attempts to integrate the research from that hermetic past with the society to which he belongs. 'Let me tell you about my past couple of decades,' the scientist writes, 'and why I think what I learned is applicable to a wider range of things.'

This phase, which I had kept putting off, began at the end of the 1970s, two decades after my doctorate. Whether by then I had run out of ideas to explore or had merely stopped trying to produce them, the fact was that I wanted at last to attempt to set down what I thought was important in economics in the form of an introductory textbook and I had reached the point where I thought I might be able to do it. Following long discussions with Donald Lamm at Norton, the New York publisher, I had signed a contract to do just that ten years earlier, so I already had a publisher. Work began in earnest in January of 1980, and a first draft was completed in December 1983. (My wife and I celebrated with a trip to Patagonia.) Nearly another year was spent adding some appendices on the open economy and repairing the worst chapters and pages.

The book – my *Political Economy* – came out in the spring of 1985.[20] Seeing that book out, in bound copies with a beautiful jacket, was a thrill – far and away the biggest thrill I ever got from seeing any work of mine in print. I knew, however, that it was too sophisticated for classroom use at most places. In the end it got few adoptions – the Stockholm School of Business, the London School of Economics, Cambridge (thanks to Partha Dasgupta) and Columbia (thanks to Brendan O'Flaherty), to mention some but not all of the most notable. Larry Summers championed my text at Harvard but to no avail. I saw that for the security of students, whom the department wants to recruit as majors, and for the convenience of the instructors, whom the department is producing for the PhD market, what is paramount is that each chapter be reducible to a rather simple exercise. In a sense this stylistic consideration is prior to the content. Books probably no longer provide the optimum medium for this purpose. There are some pretty serious costs from bringing up students on this kind of diet, however.

EUROPEAN YEARS: FROM THE MID-1980s TO THE PRESENT

Even before the 1980s I began to spend the summer in a European university where I could work with little interruption and give a few lectures. The first years were usually at the University of Mannheim, where Juergen Schroeder and his wife Marlies were wonderful pals to us in those days. Increasingly, though, our eyes strayed to the south, and with a considerable effort I broke into the circle of scholars at the European University Institute, where I was invited for a month in 1983. It was a great pleasure, Viviana and I having fallen in love with Italy, to spend most of my 1985–86 sabbatical in Italy – first Rome, finally Florence, with a hiatus in Paris.

Our visit to Rome was warmly encouraged by Luigi Spaventa, who laid the ground for an invitation from the Banca d'Italia to spend some months there in the post of Visiting Scholar – a new venture for them. There had grown up a large literature in international macroeconomics with which I was largely unfamiliar, and so for a couple of months I read and worked on an interesting exercise, the extension of Tobin's dynamic aggregative model to the open economy. It was a great pleasure some time later to give that paper at the conference in Jim's honour and to see it published later in the *Festschrift*.[21]

Increasingly it seemed to me, however, that having yet another macroeconomic model focused on disturbances of the economy away from a fixed natural rate and the economy's subsequent adjustment process, could not advance us very far in understanding the strange and apparently new problem gripping Europe: the largest volume of unemployment since the Depression with rather little accompanying disinflation. I began to mull over the other curiosity about this episode, the remarkable heights to which real interest rates had risen earlier in the decade without any abatement. While still at the Bank I wrote a paper with a new slant in which I argued that the rise of real interest rates was imported, not home-made, as a great many of my Italian friends seemed to believe – a product of the fiscal stimulus instituted in the United States.[22] This external development was sapping Europe of its capital stock and having an adverse effect on its economic welfare. But at that time I had not worked at trying to connect the unemployment problem to the real-interest problem.

The connection began to emerge in conversations with Jean-Paul Fitoussi at the OFCE in Paris to which, then as Research Director (now President) he had invited me for the fall months. We ended up with three kinds of model having the property that a fiscal stimulus to investment demand (and less generally to consumption demand) in one country could, and most likely would, be contractionary for the rest of the world – the more so the larger the country and the larger the percentage stimulus, of course. These models were, first, one with a customer market mechanism, a second based on considerations of the economics of labour-hiring and labour-hoarding, and a third involving the sort of two-sector technology introduced by Hirofumi Uzawa. The argument was always that, for the home country, the foreign real-interest shock operates to drive down real-asset prices, which contracts the supply of jobs offered by domestic firms. Our models were diametrically opposed to the Mundell–Fleming model in the flexible exchange-rate case, which had fuelled the notion that aggregate demand stimulus anywhere in the world could serve as the 'locomotive' to pull up employment everywhere. The little monograph we finally brought out, *The Slump in Europe*, while not setting the world afire, aroused sufficient interest to make me want to develop it further.[23] Some thoughtful comments by Kenneth Rogoff, then on his way to Princeton from Berkeley, may have provided the little bit of reinforcement that nearly every investigator needs to embark on a very long and risky study.

As I thought more about the European unemployment experience of the 1970s, and more especially the 1980s, I began to believe that the problem was not simply a disturbance of the unemployment rate away from the natural rate, which was the main (though not the sole) view taken in the Fitoussi–Phelps models, but a structural shift of some kind pushing up the natural rate itself. The new Keynesian models, for which I bore no small share of responsibility, were hopelessly inadequate for explaining the high and sustained elevation of the unemployment rate in Europe; their function was only to explain deviations from the natural rate and their persistence.

What I have attempted in the past several years, since the latter book, is to build up a theory serving to *endogenize* the natural rate of unemployment – not by making it unnatural, in the sense of bringing inflation and monetary factors back into the picture, but rather by dropping the makeshift assumption that it is a constant in the sense of a fixed or moving parameter, immune even to non-monetary forces. The aim is to show the natural rate to be a determinable function of the state variables and shift parameters of general-equilibrium-type non-monetary models. As the vehicles for this analysis I worked with de-monetized versions of the trio of models sketched in the Fitoussi–Phelps volume: the first based on firms' assets in the form of employees having firm-specific training (having roots in my 1968 model of labour turnover as the source of a positive natural rate); another based on the customer as the asset in which a firm invests; and the third a version of the two-sector technology used by Hirofumi Uzawa and others – the latter two models invoking shirking rather than quitting (turnover) as the source of the natural rate. Among the non-monetary variables on which these models focus are the real rate of interest, which is seen as a powerful influence on the demand price for labour, and non-wage income per worker, which is portrayed as a vital influence on the supply price of labour in reasonably generalized incentive-wage-type models. The unanticipated fruit of this theoretical work was the discovery that if *all* countries engage in fiscal stimulus in the form of increases in public debt or increased public expenditure, the effect is a contract of employment rates as a result of the real-interest effect. The book came out in January 1994.[24]

Alas, the microeconomic foundations of the theory are not fully worked out in the book. This is a calculated risk I decided to take in order to get the ideas and the empirical support for them before the profession rather than to wait for as many months or years as it might take me (with help from collaborators) to fill in the gap. The hope is that the profession will still tolerate unfinished investigations such as this one in view of their probable heuristic value.

An enjoyable aspect of this long research is the opportunity it has provided to collaborate with three of my doctoral students over the period of the project. I had, of course, taken great interest in the work of several of my doctoral students in the past. Mention must be made of Mordecai Kurz, my first dissertation advisee, who taught me a good deal more than I taught him – but he was older than I, so I was comfortable with the situation. There was Koichi Hamada, whose dissertation on net foreign investment in international growth models was a first-class piece of work. There was, also from my period at Yale, the excellent thesis by Seong Yawng Park on putty-clay models. He also began a promising career in economics, but was soon diverted into his father's faltering business in Seoul, which he turned around into the successful empire it is today – the Kumho group, including Asiana Airlines. (Let me remark that he did not forget. He donated to Yale the Tobin–Okun–Phelps TOP grants, he gave us royal treatment over a long visit in 1989, and I saw him again, with Arrow and Stiglitz in tow, for a storybook evening in a medieval retreat.) At Columbia, too, there were dissertations central to my own work: those by Janusz Ordover, Juan Carlos di Tata and Bart Moore, all of whom are mentioned above. But I was unusually lucky more recently to find so many students willing to contribute so much when there was such an enormous amount of work to be done. Three students arrived in time to contribute importantly in the development of the natural rate theory. Gylfi Zoega joined me in designing econometric tests, using a OECD cross-section of national time series, and in doing simulation analyses of one of the models.[25] Hian Teck Hoon co-authored most of the papers based on the Phelps–Stiglitz–Salop model of the employee turnover problem as the source of the natural rate.[26] George Kanaginis took on the strenuous work of analysing a neoclassical two-sector model with many of the same questions in mind as those behind the modern models.[27] I can only hope that their efforts will finally be seen to be worthwhile for them.

It is much too soon to say whether this research will be judged to be as successful and important as my early work seems to be regarded. Whatever the outcome, there seems to be no alternative but to keep on working and hoping that the results will have been worth the effort. Besides, it is not as if our efforts were some terrible sacrifice. Those of us who have been well treated in the economics profession are extraordinarily fortunate to be faced with questions whose intellectual challenge and importance for society are so satisfying to work on.

The European experience has had other effects on my career and life. The connection with Jean-Paul Fitoussi, already mentioned, led to a continuing association with the OFCE and the Institut d'Etudes Politiques in Paris. A similar association developed with Luigi Paganetto in Rome who was the architect of the Economics Department (and more) in the new branch of the University of Rome, called *Tor Vergata.* Ultimately Luigi and I became co-directors of an annual conference on generally international questions, typically of some interest to Italy, at the huge Villa Mondragone, an outpost of the University between Frascati and Grottaferrata. The summer life of my wife and I has increasingly revolved around this annual event, the preparations for it and the celebrations afterward. The latter generally take place in Spoleto, during the music festival, with Luigi, his wife Stefania, and Angelo Airaghi, who has been a key force at Finmeccanica behind the Mondragone conference as well as the Spoleto festival, and his wife Alma. (A moving *Meistersinger* directed by Menotti himself and some clangorous American avant-garde music for a Fourth of July concert were special favourites of mine.) So, though to the despair of fun-loving friends, having opted for a monkish existence whenever I had the choice, I stumbled into the beautiful life in spite of myself.

Other European activities developed around this time in parallel to the Rome activity. Axel Leijonhufvud and I go way back – to August 1967 when he was the discussant at the Montauk Point conference at which I had the opportunity to present my natural rate paper (the proceedings of which came out in the 1968 *JPE*). So it was a great pleasure when he invited me to join him in organizing the summer school in economics at the University of Siena. (Not long ago Alesandro Vercelli brought out a 'hits of Siena' volume at Macmillan.) It is also surprising to see how even the best and the busiest will travel for tens of hours in order to show up and present their latest work. Axel and I played impressario for four seasons until the fuel tanks began to signal empty.

At the time I first met Jean-Paul in Florence I met Kumaraswamy Velupillai, a man of many parts out of Sri Lanka via Tokyo, Cambridge and Lund, who is mathematician, engineer and economist – and now brain theorist and Japanist – all somehow in one mind. Having read so widely he can refer to your latest work and some related idea of, say, Niels Bohr in the next breath, which has the therapeutic effect of making you feel terrifically good. In recent years he has made a speciality of hosting conferences at the edge of the world – Aalborg being a leading example, and Montevideo another. I was the beneficiary of a particularly attractive invitation when he and Bjorn Thalberg asked me to give the first in the Arne Ryde memorial lecture series at the University of Lund. This was a very special experience. The book that came out put on record my accumulated views on the state and progress of macroeconomics in the years I had been at work on it.[28]

My European story got a new chapter in the past couple of years. Fitoussi was asked by Jacques Attali, President Designate of the embryonic European Bank for Reconstruction and Development, to field a team for a week-long mission to Moscow in September 1990 to represent the Bank in a four-agency study of the (then still extant) Soviet Union requested by the G-7. I joined as one of the economists, along with Jean-Paul, Ken Arrow, Philippe Aghion (a young economist from France being recruited to the Bank) and Jacques LeCacheux, an economist at 'Science Po' and OFCE (who did the beautiful translation of my textbook into French). This was a wonderful experience. Our signature, virtually, was the assortment of beat-up taxis from which six or seven of us would spill out in front of the ministry we were visiting, in the style of the old circus *shtick*, while the more venerable international agencies favoured their traditional black limousines. (Later, of course, the EBRD was severely criticized for the tasteful and attractive outfitting of its London headquarters – but that is another story.) Meals and the hotel were pretty dismal. The general impression of drabness and stagnation could not altogether be shaken off. However, the first view of St Basil's church at Red Square, softly aglow in the dark of late evening, was stunning. We managed a night off to see a *Boris Gudenov*. When a city's *third* opera company is that good it is clear that the city is a world-class cultural centre.

Above all it was stirring to feel the energy and zeal of the new group of reformers coming into powerful positions. After you have met some of them you cannot help but feel confident – maybe unreasonably – that the drive for individual liberty and free markets is quite strong in Russia. Ken and I were given the assignment of thinking about the reform talk then starting – the plans of Shatalin and Yavlinski – and writing it up. Our paper finally appeared a year later in the proceedings of a Villa Mondragone conference.[29] These charter members of the EBRD became the nucleus of the Economic Advisory Council of the Bank, which was inaugurated in April 1991.

That might have been the end of my Eastern European/Russian foray, were it not that Roman Frydman, who had branched out in that direction too, was constantly providing new stimulation and information about the area. With an invitation from John Flemming, who had become Chief Economist of the

Bank, I decided to spend my 1992–93 sabbatical year at the EBRD. In the summer, before arriving, I had some remedial training through a paper written in collaboration with Frydman, Andrzej Rapaczynski and Andrei Shleifer on corporate governance and finance problems looming up in Eastern Europe.[30] But there was so much to learn. It was months before I stopped dreading that my ignorance of so much that was important in the region would prove a problem, or at least an occasional embarrassment. More important, there were so many conceptual questions to think about, and not very many months, really, in which to think about them.

What emboldened me to take this assignment was the conviction that I had an important message to send. Since the future of the economy – and especially the typical Eastern European economy – is subject to a great many uncertainties, we want decisions to invest and to start up enterprises to be undertaken by those who think they have an inkling of what future demands and supplies are going to be, and of what goods will be demanded or supplied. So we want resource allocation to be under the substantial control of entrepreneurs, with their various visions, not under the state with its monistic viewpoint. Furthermore, we want a system in which, after the entrepreneurs with their diverse ideas have placed their individual bets, there is learning from this decentralized experimentation and there is competition – free entry and no soft budgets from the government – in order that bad ideas are abandoned and the lessons learned can inform the next round of entrepreneurial bets. As this view of the essence of capitalism had to a large extent derived from my earlier work on departures from rational expectations, it was inevitable that this thinking was very often done in interaction with Roman Frydman, than whom no one has thought more deeply on this subject. Much of my work that year was devoted to making sure that all the other things that need to be said about private versus state ownership were also stated and, preferably, assembled into a coherent exposition. It was, in fact, an arduous year since in evening and weekends, when I might have looked forward to rest and recreation, I had instead to polish up the manuscript and later the galleys of my book on unemployment. (As Viviana was tied up in New York for months by two weddings to organize, the friendship of Judith and Dennis Snower and of Beatriz and Philippe Aghion were a godsend.)

The written product resulting is largely contained in the 1993 *Annual Economic Outlook* – the maiden issue in the series.[31] I drafted a chapter on the grounds for favouring private ownership and control of most enterprises – the justification for capitalism, in effect – and a chapter on the main obstacles to entrepreneurial control now faced in Eastern Europe. A rather valuable survey of progress (or the lack of it) on several reform fronts was also prepared under my direction, with some sensational calculations on effective tax rates which I invited Pentti Kouri to do. I also chipped in some material for the chapter on output and employment trends. It was tremendously gratifying to see the lengthy and highly appreciative review of this publication by Samuel Brittain in the *Financial Times* (30 September 1993).

The other excitement at the Bank revolved around personnel. There was the prospect for a while of staying on to work with Attali once Flemming, who had announced he would be returning to his roots in Oxford, had left. But in the negotiation we were miles apart, for a variety of reasons. And it would not have worked out well. For one thing, Attali was later forced out of the Presidency. For another, I found that returning to New York would not be a bar to doing much of the research on Eastern Europe that I would have liked to undertake at the Bank. As had happened before in my career, what seemed like a mixed or even unfavourable outcome had turned out for the best.

So I returned home in September 1993, richer for a fascinating experience on the fringes of insiderdom, yet very content to be again an independent scholar. I plan work on a project long on my agenda – a scheme to subsidize employment of low-wage labour in order to pull up the wage rates and employment rates of the working poor. My long research on the micro–macroanalysis of unemployment, which at first seemed to be unrelated, has made it possible to view this scheme in a setting of unemployment – with differential unemployment rates alongside differential wage rates. The Russell Sage Foundation has provided the needed grant and facilities.

TAKING STOCK

Having recently reached 60, I have been taking stock. Looking back at some of my accomplishments is a source of pleasure. Often the influence they had on others is a large part of the satisfaction. I feel at peace about my career, not driven any more to try to rack up achievements quickly. (When I was young I worried that the grim reaper would take me before I had a chance to make a mark.)

I am relieved that I managed a long span of serious work. When I started out there was a feeling that I often worked with talent and enthusiasm, but I was not awfully serious about the subject. And in a way it is true that I never fit very well into the everyday life of the practicing professional economist (the conferences, referreeing and so forth). But I am proud that, as I got into my 40s, I concentrated in businesslike fashion on what I thought were the best areas for me and made some progress in those chosen directions. Maybe I have gone too far, in fact. If no one has been annoyed for some time by what seems to be your irresponsibility, you should consider whether you are holding your imagination too much in check.

There has also been criticism from the other side – that I was afraid to strip away the realistic trappings from my most important models and devote the needed months and years digging into the rigorous utility foundations for the stripped-down models. But I thought that style was not my comparative advantage. There were other pressing questions that I thought were at least as urgent. Not to have started the public-finance approach to optimal inflation, not to have discovered the optimality of a zero marginal tax rate at the top (under certain conditions!), and not to have shown the theoretical possibility of disinflation without recession (even *with* a boom) – to take the examples from the 1970s that come first to mind – would have been a loss for me.

While I have enjoyed looking back of late, mostly I look forward to my future work. Being 60 is a nice juncture. There is the luxury of choosing projects knowing that career impact cannot be a large part of the equation; the other rewards, especially those from the work itself, are the sole criteria. This is very liberating. Moreover, I can still work about as hard as ever. There seems to be little reason why a person's 'creativity' should diminish in later decades.

In the next decade, I want to work more on the Eastern European transition, possibly the most interesting event of my adult life; more on the situation of the working poor in my country, possibly the most important subject on which I can contribute; and more on the determination of unemployment, which continues to be (if I am right) poorly understood. If this agenda comes to feel oppressively serious, a diversion or two may occur to me – maybe something on the stock market or perhaps politics. I am looking forward to these and other – unforseeable! – projects in the future.

NOTES

* This paper was largely written in June 1993, while the author was visiting at the University of Rome *Tor Vergata*, and revised in September and October 1993 while a Visiting Scholar at the Russell Sage Foundation.

1. A short essay that grew out of the dissertation can be found in my two-volume collected papers, *Studies in Macroeconomic Theory*, New York: Academic Press, 1979 and 1980.
2. The golden rule paper and the other papers referred to here up to 1980 or thereabouts are in the collection cited in the previous note.
3. 'Phillips Curves, Expectations of Inflation and Optimal Unemployment over Time,' reprinted in the collection previously cited.
4. 'Money-Wage Dynamics and Labor-Market Equilibrium', originally appearing in the *Journal of Political Economy*, August 1968, Part II, and reprinted in the aforementioned collection.
5. *Inflation Policy and Unemployment Theory*, New York: W.W. Norton and Co., 1972.
6. Phelps *et al.* (1970), *Microeconomic Foundations of Employment and Inflation Theory*, New York: W.W. Norton and Co.
7. John Rawls (1971), *A Theory of Justice*, Cambridge, Belknap Press.
8. *Economic Justice*, Penguin, 1974.

9. *Political Economy*, Norton, 1985.
10. 'Distributive Justice', *The New Palgrave Dictionary of Economics*, Macmillan, 1987.
11. See, for example, 'On the Concept of Optimal Taxation in the Overlapping Generations Model of Economic Growth', *Journal of Public Economics*, 1979.
12. 'Rawlsian Growth', *Review of Economic Studies*, February 1978.
13. 'Inflation in the Theory of Public Finance', *Swedish Journal of Economics*, January–March 1973.
14. 'Money-Wage Dynamics and Labor-Market Equilibrium', previously cited.
15. 'Stabilizing Powers of Monetary Policy under Rational Expectations', *Journal of Political Economy*, February 1977.
16. 'Employment–Contingent Wage Contracts', Appendix to Phelps, 'Indexation Issues', *Journal of Monetary Economics*, Supplement: Stabilization of the Domestic and International Economy, 1977, 149–67.
17. 'Disinflation without Recession', *Weltwirtschaftliches Archiv*, December 1978, and 'Obstacles to Curtailing Inflation' in J.H. Gapinski and C.E. Rockwood (eds), *Essays in Post-Keynesian Inflation*, Ballinger Publishing Co., Cambridge, Mass., 1979.
18. 'The Trouble with Rational Expectations and the Problem of Inflation Stabilization' in R. Frydman and E.S. Phelps (eds), *Individual Forecasting and Aggregate Outcomes*, Cambridge: Cambridge University Press, 1983.
19. Roman Frydman and Edmund Phelps (eds) (1983), *Individual Forecasts and Aggregate Outcomes*, Cambridge: Cambridge University Press.
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21. 'The Effectiveness of Macropolicies in a Small Open-Economy Dynamic Aggregative Model' in *Money, Macroeconomics and Economic Policy*, MIT Press, 1991.
22. 'Appraising the American Fiscal Stance', Banca d'Italia, November 1985. Published in M.J. Boskin, J.S. Fleming and S. Gorini (eds), *Private Saving and Public Debt*, Oxford: Basil Blackwell, 1987.
23. Jean-Paul Fitoussi and Edmund S. Phelps (1988), *The Slump in Europe: Open-Economy Macroeconomics Reconstructed*, Oxford: Basil Blackwell.
24. *Structural Slumps: The Modern Equilibrium Theory of Unemployment, Interest and Assets*, Cambridge, Mass.: Harvard University Press, 1994.
25. 'Foreign and Domestic Determinants of Unemployment Rates through Real-Interest and Real-Exchange Rate Channels' in *Taux d'Interêt et Chômage*, Second Annual Report of the Policy Evaluation Group of OFCE, Presses de la Fondation National des Sciences Politiques, 1993.
26. See 'Macroeconomic Shocks in a Dynamized Model of the Natural Rate of Unemployment', *American Economic Review*, September 1992.
27. 'Fiscal Policy and Economic Activity in the Neoclassical Theory – with and without Bequests', *Finanz Archiv*, 1994.
28. *Seven Schools of Macroeconomic Thought*, Oxford: Oxford University Press, 1990.
29. Kenneth J. Arrow and Edmund S. Phelps (1991), 'Proposed Reforms of the Economic System of Information and Decision in the USSR: Commentary and Advice', *Rivista di Politica Economica*, **81**, November.
30. 'Needed Mechanisms of Corporate Governance and Finance in Eastern Europe', *The Economics of Transition*, April 1993.
31. *EBRD Economic Review: Annual Economic Outlook*, London: European Bank for Reconstruction and Development, September 1993.