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 philoso-

phy were it not that my father cajoled and
economics, which I did the second year. T
economist from Harvard with a trenchant a
to captivate a succession of students - Pol
new textbook in use, by Paul Samuels
impressed to see that it was possible to subj
I had read about to a formal sort of analys
was Arnold Collery, out of Princeton, wh
contrast. As George Ballanchine believed t
dry, so Arnold believed that an economics
launched me into the study of econom
top-drawer instruction.

What drove me on and on in the stu
common, it seems. As others have comme
hoping that if I took just one more course
would be revealed to me and I would be r
my own case, however, this gap in underst
sense that the microeconomics taught
communicating with the macroeconomics i
read Hansen's or Haberler's surveys
macroeconomics without becoming awa
hesitation, I decided to go to graduate sc
firmament, Lloyd Reynolds having assem
few years, and it was offering big fellowshi

GRADUATE WORK AT YALE

In graduate school, I had close contact a
brilliant economists at Yale, the Amer
Schelling. I was grateful to them for their
high competence and ability to make eve
Arthur Okun arrived, himself fresh from gr
a model of braininess and relevance who
always a partisan of mine.

I was also drawn to several economis
Central Europeans William Fellner and He
and the other from Eastern Germany. I lo
counterparadigm that might turn macroeco
rooted in their background that they must h
their lectures and writings. *Mitteleuropa* b
the old films and recordings of the interwa

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. An-> other line of research extended the Ramsey model to 'risky capital'. I felt

these papers were not as deep as I was capa
by writing a great many of them. An explor
more originality: first a paper introducing t
could be achieved by sufficient deflation or
secondly a small book examining the hypot
entailed the rule of balancing the budget in
into people's expectations, deliver optimal
Working on these problems was often fasci
several terms such as the ones in quotation
caught on, was a particular pleasure. The q
Tjalling Koopmans and, later, David Cass,

One of the rewards in the middle of th
MIT in 1962-63, which turned out to be i
later called it. There were unforgettable
which I mined for years. I got to teach a
Solow, who clarified the subject as much
Modigliani, who had just arrived, was also

Within a few short years I became an in
I came to feel that I was simply winning (
saw that if I was to do anything of unusu
have to think much harder than I had gene
game. There is, as I was to appreciate
scanning existing models for their unnoti
and, on the other, acquiring an independe
overlooked or misunderstood way the econ

One might have thought that this succes
rewarded with promotion to tenure. Howev
slot after another in those years, building u
Economics Department in the country, unti
to creating yet another. But it has to be add
showed willingness to fight the Administra
spent long enough at Yale not to mourn lea
would be a setback for my reputation and t
markedly less convenient.

Tenure offers at full rank arrived from
Northwestern University in 1965. Rememb
when I was a child, and preferring to be
departure from Yale in January 1966,
accumulated semester of leave and an SS
passage into my years of high creativity, v
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 expexpectations-augmented quasi-Phillips curve was written down:[3]

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

equivalently:

$$p = \phi(u) + p^e, \qquad (1')$$

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 behaviou
Lipsey in an otherwise econometric pap
employment. Yet these insights, however
fundamental, it seemed to me. They did no
its personnel manager. Man is a thinking,
was a model of a sequence: the firm's expe
those of the others, the discovery of the o
expectations, and so forth.

I had also read the paper on wage
econometric estimates, by Sargan of the LS
nominal wage level that is an increasing
(hence decreasing in the unemployment ra
level. I took from this paper the rather imp
of nominal wages is a function not just of t
the change of employment. It also encoura
plan to increase employment they offer an
wage is not completely described as a f
changes in the market wage and the total
hand, at the embryonic stage of my thinkin
and an unnecessary complication. For w
expectations of the price level by the per
rather than their expectations of what the g
be.

By the time I was settled into the Unive
about labour-market equilibrium and wage
that made up the title of the paper I
unemployment rate might move to so l
associated quit rate, every firm wants to of
as an inducement not to quit with such rea
the resulting money wage increase, the p
(beyond what it was going to do anyway),
keep the unemployment rate down, there i
increases and hence continually unexpec
faster than whatever rate was expected. I
rate is below equilibrium; the steady-stat
larger rate.

One day, though, it struck me that som
Suppose that each wage increase in accom
productivity, so that the price level remai
increased. Then it would be implied by
unemployment rate was consistent with eq
wage would generate another reductio
unemployment, causing equilibrium unemp

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:

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

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 su
that worker did not apparently differ fro
the likelihood of quitting, the firm would
rate would be higher as a result; but that
firm to accept since it had already
wage-quitting opportunity locus.

Another feature - an 'optional extra' - c
starting from unemployment in excess of
path would approach the natural rate only
that firms will not jump their employment
face rising marginal cost of imparting firm
to new recruits. Whether a specified unem
unexpected inflation thus depends on the

$$w - w_{-1} = \phi(u, u_{-1})$$

Hence there was an equilibrium path of
which the expected wage is always match
given by $\phi(u, u_{-1}) = 0$ - that approaches
This was the notion of persistence. (In con
in my 1972 book, at any rate, referred to th
the natural rate, either a permanent effect c

This exploration would have been a
island, but it became even more exciti
theorists, most of them too young to
beginning to chart the same waters, usuall
-the angle of incomplete or imperfect
opportunity to convene an informal conf
into this new area. I called Jim Blackman
me before, and Donald Lamm at Norton,
paperback series, and got the financing a
ahead. The conference was held at Penn c
learned quite a lot from each other at that
conference volume turned out to be the m
of papers with my introduction - which ca
called the 'Phelps volume' - seems to hav
event in the history of macroeconor
macroeconomics fraternity far beyond th
would have had coming out separately and

It would be an omission of some of my
at Penn not to mention several excursions,

tion 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 impo
philosopher, who was also going to be
interchange with Rawls, as it fortunately
meetings with Kenneth Arrow, one of our
had become acquainted at RAND through
of mine and the husband of Ken's siste
acquainted with a philosopher set, some
interests, including Tom Nagel and Tim
sprung up in front of my eyes.

My work on economic justice grew out
point at first, I finally argued for a concep
understood as the perspective of Rawls's
justice in the society's design of the rew
contributors to production and exchange a
This apparently innocent definition leads t
can be owed to those in the society who a
other societies with whom there is no tr
there exist just terms under which internat
(This view was not developed in the Pengu
Economic Justice[8] but it is put forward in
Economy[9] and in my essay on econom
Encyclopedia.[10])

A related series of papers explored
'maximin' criterion for the structure of ta
unexpected result was the finding that the
from labour must be zero, for if it is
opportunity to make a mutually advantag
that lowers their marginal rates and thus
pay a larger tax bill. Three papers on the
and capital, two of them co-authored with
produced rather less in the way of
maximin-optimal economic growth in an o
one coauthored with John Riley, yielded
work of Alan Auerbach and Laurence Kotl

Also growing out of my year at the C
called statistical discrimination, the idea th
which a person belongs to type him. Final
theory of the inflation tax which provided
perspective,[13] though it was misleading
optimal tax on cash balances was positive -
managed to be more nearly correct.

My most important work of this de
cooperative programme that I began with C

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
rate. All of this was nicely formalized in the

This work at Columbia began with a pa
make the point as simply as possible, we su
period with a lead-time of *two* whole peri
present period, it will affect not only curren
period as well, since it is too late to adjust
foreseeable consequences for output. Taylo
on wage-setting, most nearly resembl
money-wage commitments such as I had di
little later Calvo worked out his continuou
commitments, which was great fun and a wo

The other interactive work I cherish fro
with Calvo of implicit contracts under mod
called, in something of a misnomer, asym
know everything that B knows and maybe (
Azariadis, working in the classical tradition
states of the world are fully observable a
concealed information, had developed
wage-employment contract between a risk
employer. The setting had the feature that th
to be paid by the worker - say, an airfare o
the worker wants to have an understanding o
and leisure under each contingency, ever
worker. The implications for the optimal co
rigidity while, independently of that feat
reflected the marginal utility of leisure an
optimal way. The setting and the conclusi
doctrine I had been trying to develop, so
anything, was wrong with it. Calvo and I w
contract under conditions in which the w
business prospects - for all he knows, t
business conditions dictated his services eve
to do anything but equal marginal revenu
precisely what the employer proceeds to do
wage might be lower the more depressed th
in the industry) and the more elevated t
concluded that when times were bad there
brief work was probably the high point of
that some confusions on my part remained
approach was taken up by Sanford Grossm
later, and by Matthew Canzoneri. I think, th

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." 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 byproduct, 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 SO-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 collabo
rich friendship.)

A subsequent paper by Frydman and r
1983 volume of papers from a conferer
volume also contained a paper by a forme
Tata, in which he independently discover
something of an uphill battle for this volun
by a significant segment of the professior
been remarked to me, seeped into pr
recognition (or even knowledge) of wher
1990s, some kindred work is being do
Columbia and now of the University of I
student of mine now at Rutgers Univers
University of Warwick who, by coincidenc
me at Cowles three decades ago. Further,
new paper a couple of years ago, and Fryd
of his, Michael Goldberg, have done sor
based on these and subsequent ideas.

It took me a while to understand the ra
For some time I could see only that a scien
line of analysis, that something like an ind
conventions and standards. (Recently the
researcher can normally expect to max
building upon an established and ongoing
will exist so many citations that may be div
by venturing into an area where there are f
may be, too, that scientists feel driven to k
one line before shifting to another line. Bu
of these models going beyond rational ex
what second generation of results could be
readers might think to say this is obvio
economists only like constructive work
political economy. But that would be cour
Friedman's critique of the Phillips curve,
its place, or Lucas's critique of economet
tell us, if I am not mistaken, what to do ins

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 MI
PRESENT

Even before the 1980s I began to spend th
where I could work with little interruption
years were usually at the University of Ma
and his wife Marlies were wonderful pals
though, our eyes strayed to the south, and
into the circle of scholars at the European
invited for a month in 1983. It was a gre
fallen in love with Italy, to spend most of
Rome, finally Florence, with a hiatus in Par

Our visit to Rome was warmly encourag
ground for an invitation from the Banca d'It
the post of Visiting Scholar - a new ventur
large literature in international macroeco
unfamiliar, and so for a couple of months I
exercise, the extension of Tobin's dynam
economy. It was a great pleasure some ti
conference in Jim's honour and to see it pub

Increasingly it seemed to me, how
macroeconomic model focused on disturba
fixed natural rate and the economy's subseq
advance us very far in understanding the st
gripping Europe: the largest volume of un
with rather little accompanying disinflatio
curiosity about this episode, the remarkable
had risen earlier in the decade without any
wrote a paper with a new slant in which I
rates was imported, not home-made, as a
seemed to believe - a product of the fisca
States.[22] This external development was s
and having an adverse effect on its econo
not worked at trying to connect the unempl
problem.

The connection began to emerge in co
at the OFCE in Paris to which, then as Re
had invited me for the fall months. We e
having the property that a fiscal stimulu
generally to consumption demand) in on
would, be contractionary for the rest of the
country and the larger the percentage stimu

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 generalequilibrium-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 general-

ized incentive-wage-type models. The una
work was the discovery that if *all* countri
form of increases in public debt or increase
contract of employment rates as a result o
came out in January 1994.[24]

Alas, the Microeconomic foundations of
in the book. This is a calculated risk I deci
and the empirical support for them before th
as many months or years as it might take m
fill in the gap. The hope is that the profe
investigations such as this one in view of th

An enjoyable aspect of this long researc
to collaborate with three of my doctoral stu
I had, of course, taken great interest in th
students in the past. Mention must be m
dissertation advisee, who taught me a good
he was older than I, so I was comfortable w
Hamada, whose dissertation on net foreign
models was a first-class piece of work. T
Yale, the excellent thesis by Seong Yawng
began a promising career in economics, but
faltering business in Seoul, which he turned
it is today - the Kumho group, including A
he did not forget. He donated to Yale the T
gave us royal treatment over a long visit i
Arrow and Stiglitz in tow, for a storybook
Columbia, too, there were dissertations c
Janusz Ordover, Juan Carlos di Tata an
mentioned above. But I was unusually luc
students willing to contribute so much w
amount of work to be done. Three stude
importantly in the development of the natu
me in designing econometric tests, using
time series, and in doing simulation analy
Teck Hoon co-authored most of the papers
model of the employee turnover problem
George Kanaginis took on the strenuous
two-sector model with many of the same qu
modern models.[27] I can only hope that the
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 codirectors 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, s
which has the therapeutic effect of making y
years he has made a speciality of hosting co
-Aalborg being a leading example, and
beneficiary of a particularly attractive invita
asked me to give the first in the Arne Ryc
University of Lund. This was a very special
out put on record my accumulated view
macroeconomics in the years I had been at w

My European story got a new chapter in
was asked by Jacques Attali, President Desi
Bank for Reconstruction and Development,
mission to Moscow in September 1990 to re
study of the (then still extant) Soviet Union
one of the economists, along with Jean-Paul
young economist from France being recru
LeCacheux, an economist at 'Science Po' a
translation of my textbook into French). This
signature, virtually, was the assortment of
seven of us would spill out in front of the
style of the old circus *shtick,* while the more
favoured their traditional black limousines.
severely criticized for the tasteful and at
headquarters -but that is another story.) M
dismal. The general impression of drabness a
be shaken off. However, the first view of
softly aglow in the dark of late evening, was
to see a *Boris Gudenov*. When a city's *third*
clear that the city is a world-class cultural ce

Above all it was stirring to feel the ener
reformers coming into powerful positions.
you cannot help but feel confident - maybe
individual liberty and free markets is quite
given the assignment of thinking about the re
of Shatalin and Yavlinski - and writing it up.
later in the proceedings of a Villa Mondra
members of the EBRD became the nucleus o
of the Bank, which was inaugurated in April

That might have been the end of my East
it not that Roman Frydman, who had branc
constantly providing new stimulation and in
invitation from John Flemming, who had bec

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 employme
gratifying to see the lengthy and highly appre
by Samuel Brittain in the *Financial Times* (30

The other excitement at the Bank revolved
prospect for a while of staying on to work wit
announced he would be returning to his roo
negotiation we were miles apart, for a variety
worked out well. For one thing, Attali was l
For another, I found that returning to New Y
much of the research on Eastern Europe that I
the Bank. As had happened before in my car
even unfavourable outcome had turned out fo

So I returned home in September 19
experience on the fringes of insiderdom, y
independent scholar. I plan work on a project
subsidize employment of low-wage labour i
and employment rates of the working p
micromacroanalysis of unemployment, whic
has made it possible to view this scheme in
differential unemployment rates alongside di
Sage Foundation has provided the needed gra

TAKING STOCK

Having recently reached 60, I have been tak
of my accomplishments is a source of pleasu
on others is a large part of the satisfaction. I f
driven any more to try to rack up achieveme
worried that the grim reaper would take me
mark.)

I am relieved that I managed a long span
out there was a feeling that I often worked
was not awfully serious about the subject. A
fit very well into the everyday life of the p
(the conferences, referreeing and so forth). I
my 40s, I concentrated in businesslike fashio
areas for me and made some progress in t
have gone too far, in fact. If no one has bee
seems to be your irresponsibility, you sh
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.
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6. Phelps *et al.* (1970), *Microeconomic Foundations of Employment and Inflation Theory*, New York: W.W. Norton and Co.
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11. See, for example, 'On the Concept of Optimal Taxat of Economic Growth', *Journal of Public Economic*
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25. 'Foreign and Domestic Determinants of Unemplo Real-Exchange Rate Channels' in *Taux d'lnterêt e* Policy Evaluation Group of OFCE, Presses d Politiques, 1993.
26. See 'Macroeconomic Shocks in a Dynamized Mode *American Economic Review,* September 1992.
27. 'Fiscal Policy and Economic Activity in the Neoclass *Finanz Archiv*, 1994.
28. *Seven Schools of Macroeconomic Thought,* Oxford:
29. Kenneth J. Arrow and Edmund S. Phelps (1991), 'Pr of Information and Decision in the USSR: Com *Economica,* **81**, November.
30. 'Needed Mechanisms of Corporate Governance *Economics of Transition*, April 1993.
31. *EBRD Economic Review: Annual Economic* Reconstruction and Development, September 1993