



Review: Celebrating Ned

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Knowledge, Information, and Expectations in Modern Macroeconomics: In Honor of Edmund S. Phelps by Philippe Aghion; Roman Frydman; Joseph Stiglitz; Michael Woodford

Axel Leijonhufvud

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Celebrating Ned¹

AXEL LEIJONHUFVUD²

1. Introduction

Once upon a time and (conceptually) far away, there was John Maynard Keynes. Keynes revoked Say's Law. In Keynesian economics, supply did *not* create its own demand. Not always, at any rate. In situations where supply failed to create its own effective demand, a benevolent, competent, and solvent government could step in to stabilize aggregate demand. Macropolicy meant stabilization policy and stabilization policy meant aggregate demand management. And so thought most macroeconomists for at least four decades.

Once upon a later time, Edmund Phelps and Milton Friedman postulated the existence of a natural rate of unemployment. Friedman had the most influence at the time, but Phelps was the one to provide a micro-founded model with the natural rate property. Macroeconomics has not been the same since. With the natural rate, Say's Law reentered macroeconomics. Once again, supply created its own demand. So aggregate

demand management lost any theoretical rationale. Macropolicy had to be supply-side policy. But policy theory had also to devise ways to constrain politicians from playing the aggregate demand policies now thought to be pointless. A case can be made that modern macroeconomics stems more from the natural rate doctrine than from rational expectations. That, in any case, is true of the "modern macroeconomics" in this volume.

Thirty years and more after the *General Theory*, much uncertainty remained over the meaning and validity of what Keynes had wrought. Where the micro-based macro pioneered by Phelps would lead was not quickly realized either, and thirty-some years of proliferating natural rate models has yet to produce as strong a consensus in the field as Keynesian economics or later Friedmanian monetarism enjoyed in their time. In October 2001, Columbia University hosted a conference to honor Ned Phelps. The assembly of participants who had come to acknowledge their indebtedness to Phelps was quite extraordinary. One's impression was that it included almost everyone still living who has played a major role in macroeconomics over the last several decades.

The massive conference volume edited by Aghion, Frydman, Stiglitz, and Woodford is the most impressive and consistently interesting Festschrift that I have ever perused. At one level it almost reviews

¹ Review of Philippe Aghion, Roman Frydman, Joseph Stiglitz, and Michael Woodford, eds. *Knowledge, Information, and Expectations in Modern Macroeconomics: In Honor of Edmund S. Phelps*. Princeton, NJ: Princeton University Press, 2004, pp. xi, 579. ISBN 0-691-09485-3.

² University of California, Los Angeles and University of Trento. Once more I thank Daniel Heymann for helpful comments.

itself. The editors' introduction gives an overview of the role of Phelps in the development of modern macroeconomics, grouping his contributions under four headings and the papers into four corresponding parts. There are extensive comments on eight of the sixteen major papers. In addition, Robert Lucas reviews Part I; Robert Solow does Part IV, while Phelps himself provides his own reflections on all four parts. It would be an overstatement to claim that the book surveys the present state of macroeconomics. Real business-cycle theory is not represented, for example. But, with that exception, the coverage is broad and gives a good snapshot of the field.

2. Information, Wage-Price Dynamics, and Business Fluctuations

In general, the papers all have well-defined links to earlier work by Phelps and his various collaborators. Those in Part I hark back to the famous "Phelps volume" (Phelps et al. 1970) whose central concern was to improve our understanding of the short-run dynamics of money wages, prices, and (un)employment. A common ambition among many of those who have worked in this vein ever since has been to provide a microeconomic rationalization of money wage stickiness in the face of shocks to nominal aggregate demand. This is an ambiguous formulation of the problem, which tends to make what we mean by "stickiness" unclear as well. Nominal aggregate demand may increase because of a purely nominal shock, such as an increase in (outside) money, to which the equilibrating adjustment would be a proportional increase in the price level. Or it may increase because of a rise in the prospective rate of return on reproducible assets (as in Phelps's later work), to which the equilibrating adjustment would be a decline in the demand for outside money, an expansion of inside money, a rise in real output and employment, and some rise in the price level. The data produced by the latter process

would show real output and the money stock moving together over the cycle and money wages varying less than proportionally with money. In this case, it would be incorrect to infer that the data show money wages to be "sticky." Moreover, to the extent that they may nonetheless be in fact sticky, this has nothing to do with the causal explanation of the cycle.

The years around 1970 were inflationary times, however, and wage-price dynamics was usually analyzed presuming purely nominal shocks. Various models, among which Robert Lucas (1972) was the most influential, were built showing that gradual adjustment of prices or wages could be rationalized using incomplete information assumptions and reconciling in this way the short-run Phillips-curve with a natural rate of unemployment invariant to changes in a neutral money. But the incomplete information models could not provide a persuasive account of the persistence of the real effects of the postulated shocks. Phelps, who had initiated this literature, chose eventually to rely instead on the staggering of individual price revisions to explain the gradual adaptation seen in the aggregate data.

The four essays that make up Part I of this volume are in this tradition, but the focus now is on inertia of the inflation rate rather than of the price level. Guillermo Calvo, Oya Celasun, and Michael Kumhof extend earlier work by Calvo (1983) in which firms revise prices at random but finite intervals to make them update their price policies (now comprising both the level and the rate of change of prices) in a similar manner. Gregory Mankiw and Ricardo Reis achieve much the same results by assuming that firms update their information at random, finite intervals. Michael Woodford relies on the idea of "higher-order expectations," which Phelps and his younger colleagues used as an argument against the rapid convergence to rational expectations equilibrium assumed in many models at the time (Frydman and Phelps 1983). A price-setter

must ask himself what prices his competitors expect, and what the competitors expect others to expect, ..., etc. This “imperfect common knowledge” slows down price adjustments to nominal shocks and, in Woodford’s model, accounts for the stylized fact that inflation peaks after the peak in output. In his comment, Lars Svensson shows, however, that the model is less than robust but sensitive to the particular assumptions made.

One information imperfection that is not quickly or easily resolved has been rather neglected in this literature (and persistently so): How are agents to distinguish reliably between a permanent increase in outside money and a temporary increase in inside money? One will be neutral in equilibrium, the other not.

Several high or hyper-inflations have been ended without serious output recessions (Thomas Sargent 1983; Daniel Heymann and Leijonhufvud 1995). Calvo et al. stress that their model is therefore applicable only to *moderate* inflations. Lucas, in his comment on these papers, also points to this finding as a general problem for inflation models of adaptive pricing. It is not merely a matter of central-bank credibility, and further modeling of rational price strategies are not by themselves likely to resolve this difficulty. The financial structure of the economy from which the disinflation starts has to be taken into consideration. Whether prices respond flexibly or not, an abrupt change in regime is apt to disrupt preexisting agreements. In the case of moderate inflations, rapid disinflation is likely to create widespread liquidity problems, as the real burden of outstanding debts becomes heavier than anticipated, and output will fall as firms scramble to restore their balance sheets. Disinflation of a hyper does not run into these problems because it starts from a situation where next to no financial structure remains.

Bruce Greenwald and Joseph Stiglitz take a different tack to short-run price inflexibility, taking as their point of departure a

famous paper by Phelps and Winter from the 1970 volume. In the Phelps-Winter model of monopolistic competition, current pricing decisions affect not only current sales but also the evolution of the firm’s customer base. Greenwald and Stiglitz show that in models of this type, higher interest rates, greater uncertainty, and negative demand shocks all lead to higher price mark-ups and thus tend to impart some cyclical stickiness to prices.

None of these papers provides a model that will explain Bewley’s findings (Truman Bewley 1999) and imperfect information would seem of little relevance to them. However, those findings must surely be regarded as setting the priorities for further work on price stickiness in macro from now on.

3. Imperfect Knowledge, Expectations, and Rationality

The papers in Part II move from imperfect information to imperfect knowledge and, in the last chapter, imperfect or at least doubtful rationality. Roman Frydman and Michael Goldberg take up the empirical failures of the rational expectations version of the monetary model of exchange rates. Their paper shows that by replacing rational expectations with “theory consistent expectations” (as in Frydman and Phelps 1983), one can do far better in accounting for the data. Theory-consistent expectations accord expectations a “degree of autonomy” which is denied them by the rational expectations hypothesis. This means, however, that it introduces “free parameters” in the model in defiance of what is today widely considered best econometric practice. One may wonder, however, whether insisting on no free parameters in this context may not leave the econometrician fishing in bottomless waters for the parameters supposedly down there in the “deep.”

In an installment on a much bigger effort (Mordecai Kurz 1997), Kurz with Hehui Jin

and Maurizio Motolese also seek to loosen the constraints that rational expectations impose on macromodels. But they strive to do so while keeping expectations still anchored in the model structure. Kurz et al. abandon the common knowledge assumption of representative agent models to allow agents acting on a multiplicity of individual beliefs at any one time. The result is a model that shows much greater volatility than a corresponding rational expectations one and which, therefore, admits the possibility of socially useful monetary policy. Yet, individual beliefs have to be rational in the sense that the statistical properties of the actual macroeconomic time series are common knowledge.

David Laibson, Andrea Repetto, and Jeremy Tobacman tackle a puzzle that is common knowledge to us all and, as such, quite an embarrassment to standard economic theory, namely, the fact that a great many people make long-term investments at low rates of return and at the same time borrow at high rates of interest. Laibson et al. seek to resolve this “debt puzzle” by invoking quasi-hyperbolic or hyperbolic preferences, following—again—in the footsteps of Ned Phelps (Phelps and Pollak 1968). Hyperbolic intertemporal preferences will lead to dynamic inconsistency in behavior of the sort first discussed by Strotz fifty years ago. “Consumers appear to be in two minds,” as the authors put it. The two minds or selves are of different ages and find themselves playing a game against one another. In this game, the consumer’s accumulation of long-term, low-yield assets is a commitment strategy by the prospective old self, which tends to constrain the liquidity of the younger self in the short term, thereby giving rise to a high marginal propensity to consume in the present and a willingness by the feckless youngster to borrow at high rates.

This is an admirably well-crafted paper that keeps the reader’s interest as the authors move systematically from the statement of the puzzle, to the stylized facts, to

the hypothesis, through simulation of the model and its calibration to the stylized facts. In the end, the resolution of the puzzle is not complete. As Pollak points out in his comment, some households hold long-term assets at low yield, and borrow on credit cards at high rates, even though they also hold liquid assets and their behavior is not accounted for by the hyperbolic model.

But there is an issue that goes beyond the question of how much may be “explained” by hyperbolic discounting. The strategy of dealing with anomalies of choice theory by searching for a topological transformation of preferences that will “save” the data goes back at least to Friedman and Savage (1948). It has become rather widespread in today’s behavioral economics. It has the inestimable benefit of allowing the theorist to use all the accustomed tools of his trade and describe the agent as if he or she were “rationally” optimizing the suitably transformed preferences subject to the usual constraints. But the neoclassical appearances are misleading, for the fact remains, of course, that people who borrow at high rates and invest at low rates are not maximizing wealth. That the separation theorem fails to hold is not inconsistent with “rationality” as usually understood but the underlying reason for it failing (“being in two minds”) is hardly consistent with it. One expects, therefore, that this strategy will prove to be at best a temporary one in the development of behavioral economics.

4. Determinants of Equilibrium Unemployment

Although Phelps distinguishes himself as a “structuralist” rather than a “New Keynesian” (cf., Phelps 1990), he has played an important role in the development of New Keynesianism and remains close to this school. What he has in common with the New Keynesians above all is the view that labor markets will settle down to equilibria where jobs are rationed at equilibrium wage

rates.³ To New Keynesians, and to Phelps, therefore, the natural rate of unemployment is not an efficient state but one that might potentially be improved by policy, albeit not by just inflating nominal aggregate demand.

Part III of this volume brings us five papers on labor market equilibria with unemployment. Dale Mortensen has the distinction here of being the sole author returning from the 1970 Phelps volume. There is good reason, for his paper more than anyone else's shows the maturation of the ideas that were mostly promises 34 years ago. Mortensen's problem is wage dispersion in equilibrium—observably identical workers are paid differently. He brings to this question that rarity—a fully articulated model of how the market works, of the adaptive dynamics behind the stock-flow equilibrium in which the flows of separations and new hires are equal. Two hypotheses about wage-dispersion are confronted using a very large and detailed Danish data set. One hypothesis would attribute dispersion to monopsonistic market power by heterogeneous firms; the other to local monopoly power of unions. In the Danish case, only the latter turns out to be consistent with the data.

The other four papers in this part all advance structuralist (institutional) hypotheses about the determinants of the natural rate of unemployment. Christopher Pissarides sketches a model explaining how regulatory obstacles to the establishment of new firms may affect aggregate employment. When entrepreneurship is discouraged by regulations and bureaucratic impediments, job creation suffers. In addition, the discouraged entrepreneurs will show a higher incidence of unemployment when remaining in the labor force. A highly preliminary check of some data shows a strong negative correlation across seventeen countries between

the employment-to-population ratio and a cost of start-up index. This is pretty ambiguous, however. One notes that the countries with high start-up costs in Pissarides's sample are Catholic countries in the south of Europe where female labor-force participation tends to be lower than in the north. May it be that the Protestant Ethic works in wondrous ways sometimes?

The other three papers in this part all deal with the question of why European unemployment, which was lower than in the United States in the 1950s, '60s, and '70s, has been considerably higher on average than the American rate in the 1980s and '90s. In the last fifteen years or so, the claim has become commonplace that the higher European unemployment is due to various European institutions. But these same institutions produced lower unemployment than in the United States before the early 1980s. At present, therefore, the tack taken on this is that European institutions worked fine way back when not much was happening (although that is not quite the way some of us remember the world of our youth) but are too inflexible to allow Europe to adapt in the turbulent nineties.

Lars Ljungqvist and Tom Sargent take as their point of departure the observation that it is the *duration* of unemployment spells that distinguishes the recent European and American experiences. Separation rates are similar, but the European worker who is laid off typically stays unemployed far longer than his American counterpart. They build a model of the individual worker's behavior based on John McCall's (1970) search model, with the principal addition of a stochastic loss of skill in case of job separation. The stochastic dynamic program is then used to simulate "life histories" of identical workers subject to the same shocks but eligible for different unemployment benefit programs. Two examples of workers with a lot of seniority are highlighted. Both lose a large part of their human capital on being laid off. One receives no unemployment compensation,

³ Such equilibria are termed "involuntary unemployment" states in the New Keynesian literature. This usage has nothing to do with Keynes's concept (cf., Leijonhufvud 1997).

does get a new job after awhile, and then starts rebuilding his applicable skill level. The other is entitled to substantial unemployment compensation for a prolonged period which keeps his reservation wage up above the job opportunities that come his way. His reservation wage gradually drifts down, as in McCall's model, but in this case never catches up with the deterioration in the market value of his skills.

The life stories are eminently plausible, and even readers averse to thinking of macroeconomics as the stochastic dynamic program of a representative agent will find this a splendid use of the technique. This is such good economic theory, in fact, that one has to remind oneself that the extent to which it explains European unemployment remains an open question.

James Heckman's "Lessons for Germany" is not much beset by such doubts. The incentives of the welfare state are such as to "portend a second-rate German economy in the future." This is because the "world economy is more variable and less predictable" than in the past. "We live in an era of creative destruction" and the modern welfare state stands in the way of necessary adaptation. (Is it really so self-evident that in a more variable and less predictable world *less* social insurance is the right thing to do?) The "new economy" requires a "new economics," focused, not (as the old one) on stable technologies and relationships between broad homogeneous aggregates, but instead on "the gains from trade among idiosyncratic individuals" and "the benefit of exploiting local knowledge about particular possibilities and circumstances that are not widely known." (This new economics sounds rather like old Hayek).

Heckman does take note of the problem of the integration of East Germany. Was it not for that, he says, Germany might arguably have one of the lower European unemployment rates. (The lower European unemployment rates do *not* compare that unfavorably with the United States.) Although he concedes that much of the East

German unemployment is cohort-specific and concentrated in age groups hard to retrain, this does not reduce the severity of his judgment of the German welfare state and of German prospects. "Far from providing social justice at the price of efficiency, [the current system] provides security for some at the cost of exclusion for others." A European reader is likely to see Heckman's own measures of income inequality as of some relevance to social justice as well. The ratio of top decile income to bottom decile income given is 3.01 for Germany (in 1984) and 5.78 for the United States (in 1991).

Stephen Nickel, Luca Nunziata, Wolfgang Ochel, and Glenda Quintini draw on data for twenty OECD countries. They divide the problem into two parts. First, they look at shifts in the Beveridge (vacancies-unemployment) curve and inquire into the institutional factors that might explain changes in the ability of an economy to match the unemployed to available vacancies. A decrease in this ability—a rightward shift of the Beveridge curve—should indicate an increase in the natural rate of unemployment. Secondly, they search for institutional factors that might directly increase the real cost of labor relative to the demand for labor. The list of institutional factors to be considered is impressively long and several of them require the construction of some index since they have no natural metric. A number of regressions are run with these variables and the results used to simulate what unemployment would have been in 1990–95 had institutions in the various countries stayed unchanged since the 1960s. On this basis, Nickel et al. find that their institutional variables account for 55 percent of the 6.8 percent rise in unemployment of the countries in their sample, 63 percent if they exclude Germany, where institutional changes explain nothing (pace Heckman?!).

The reader comes away with some questions unanswered. For example, among the countries showing the most dramatic Beveridge shifts in the 1990s were Japan,

Sweden, Finland, and Norway. But these occurred in the aftermath of financial crashes and banking crises—which are not among the institutional change variables that Nickel et al. consider.

The shocks-and-institutions stories of “old Europe” doing badly in an increasingly volatile world that will not tolerate inflexibility have great intuitive appeal to the generation of macroeconomists represented in this volume. Some cautionary notes are sounded though. On the increased turbulence, Olivier Blanchard comments: “There is a catch: We may all know it, *but the data just do not show it....*” (italics added). And Jean-Paul Fitoussi finds these institutional stories banded about only too freely:

If we had followed conventional wisdom in each decade we would have recommended that every country in the world adopt the French institutional model in the 1960s, the Japanese one in the 1970s, the German one in the 1980s and the U.S. one in the 1990s. The nationality of the winning model of the present decade (the 2000s) is still unknown.

Why then do they hold that these stories have over our intuition? Because natural rate theories leave us no alternative. One or more of them *have to be true*—at least if your intuition is firmly shackled to the natural rate of unemployment.

5. Education, Technical Change, and Growth

The fourth part of the Festschrift harks back to Phelps before the Phelps volume, that is, to the work on growth in the 1960s that first gave him a widespread reputation.

Philippe Aghion, Peter Howitt, and Gianluca Violante focus on three aspects of the wage-distribution changes since the early 1980s, namely, that inequality has increased both between and within educational groups, but that the latter reflects changes in temporary income whereas the former is due to changes in permanent income. They develop a model of skill-biased technical

change in which the personal adaptability of older workers plays a critical role. Those who do adapt to novel technology and cooperate with new capital do very well; those who do not adapt are left working with old and depreciating capital and do badly.

Skill-biased technical change is central also in the paper by Daron Acemoglu, who provides a modern reformulation of the induced innovations literature of forty years ago. His model of “directed technical change” incorporates the market-size effect of Romer. With a larger stock of human capital, the skill bias of technical change will also be larger. This suggests an explanation for the larger apparent skill bias of recent decades and a rather pessimistic prognosis for the evolution of income differences between the highly developed and the less-developed economies.

Doubts about the marginal productivity foundations of income distribution theory keep cropping up among much praise for these papers from the commentators. Thus, Nancy Stokey reminds us that the aggregate production function “is simply an artificial construct invented by economists” while Robert Hall notes that if skill bias is to account for the change in relative wages over the last twenty years, one would have to postulate “*negative* rates of growth of the efficiency of less skilled labor.” Can we be any more confident that the marginal productivity theory rules the upper tail where dwell our corporate leaders, investment bankers, and (a bit further down) the largely non-teaching stars of academia? Robert Solow notes the virtual demise of collective bargaining in the United States but is skeptical that it has been replaced by atomistic competition. He misses “any serious discussion of the mechanism or process of distribution.”

The United States has shown basically trendless growth for some 125 years. This motivates the quest of Charles Jones for a model of steady state per-capita income growth. A successful theory, he notes, should have a “compelling and intuitive justification”

for the required linearity property. This he supplies by showing that the utility-maximizing fertility choice of dynastic family heads will result in constant population growth! Next, Jones assumes that the larger the population, the more productive ideas it will generate, and since ideas are nonrivalrous this results in higher per-capita income.⁴ Thus is the ghost of the good Reverend Malthus put to rest!

The proposition that sheer numbers will augment technical progress seems less than intuitively compelling. If and when larger population size leads to a more highly articulated division of labor—which it has not always done—it might well induce more innovations. But the production theory of these neoclassical growth models draws all its inspiration from Ricardo's factor proportions and none from Smith's division of labor.

Jess Benhabib and Bart Hobijn revisit Phelps (1962), in which he demonstrated that the elasticity of steady-state output with respect to the savings rate was independent of whether technical change was embodied or disembodied. They show that in an intertemporal general equilibrium model where both saving and labor supply are endogenously determined, this independence no longer holds. Instead, the distinction matters both for the long-run steady state and for the transition dynamics.

6. *Modern and Not-So-Modern Macro*

The 1970 Phelps volume was exciting in its day. One remembers it with affection. The Phelps *Festschrift* is a remarkably good collection and shows how far the field has advanced over thirty years. But it is "normal science" by now. And for all of the references to a more rapidly changing and unpredictable world, it is pretty tame macro. That is not just this book, of course. It is the state

of the field of which these contributions make such a good sample.

Is the reality with which macro has to deal equally tame? That is at bottom a question about whether the system we are studying is stable or, in by now archaic language, about whether the economy is "self-adjusting." In modern general equilibrium macro, stability is an article of faith. In Keynesian days, it was an article of heresy. The relevance of the issue to unemployment theory may be shown by comparing Phelps and Keynes.

For a number of years, Ned Phelps concentrated his work on the problem of understanding the persistence of high rates of unemployment in Western Europe. Such a long-lasting situation, Phelps judged, could not be due to monetary shocks and/or to the failure of nominal wages to adjust. It had to be a result of a system in equilibrium. The theoretical problem, therefore, was to explain why the natural rate of unemployment was high. The reasons for such a persistent slump had to be "structural."

In natural-rate models, employment and output are determined in the labor market. Phelpsian firms pay efficiency wages to elicit the best effort relative to the wage from their employees. These efficient wage rates are higher than the corresponding reservation wages of labor. Flexible real wages will bring this labor market into an equilibrium where the demand price of firms equals the efficient wage. It will be an inefficient equilibrium, however, which leaves some people unemployed who are willing and qualified to work at prevailing wage rates.

This natural-rate equilibrium will shift as a function of the level of asset prices. Higher expected net revenues, lower taxes on profits, or lower real interest rates will raise asset prices and reduce unemployment. This sounds Keynesian, but isn't. In this theory, asset prices cannot work through aggregate demand but have to work through supply-side mechanisms. In the "customer markets" version of the theory, for example, lower real interest rates raise the present value of

⁴ Somewhat oddly in a *Festschrift*, Jones misses the opportunity of giving priority to Phelps for this idea; Phelps (1968).

retaining customers. Firms, therefore, will reduce mark-ups. Lower mark-up means higher demand prices for labor and consequently increased employment and output.⁵ Conversely, higher interest rates or reduced revenue expectations will make firms increase their mark-ups and thereby reduce employment.

Keynes's *General Theory*, like that of Phelps, was a *real* theory but not an equilibrium theory (as that term is used in general equilibrium theory). From an initial equilibrium, a reduction in investment expectations would cause saving to exceed investment at full employment output. If real interest rates did not immediately respond so as to coordinate intertemporal plans, reduced aggregate demand would cause real output and employment to fall. With the real market rate exceeding the natural rate of interest, unemployment would be higher than the natural rate of unemployment. As long as the intertemporal disequilibrium persisted, flexibility of money wages would not restore the system to the natural rate of unemployment but instead threaten a Wicksellian cumulative deflation and thereby a collapse of the financial system. It was fortunate, in Keynes's view, that wages were sticky enough to save us from that eventuality.

Implicitly, the Keynesian full-employment equilibrium is assumed to be unique. If saving out of NAIRU income equals investment, flexible wages will bring the system to full employment. Conversely, if the system were somehow to maintain itself at full employment, the real interest rate would come to coordinate the intertemporal plans of households and producers. Phelps is not committed to rational expectations. The system is not necessarily on an intertemporal equilibrium path. Expectations may turn out to be wrong. But the kind of intertemporal inconsistency between savers and investors that Keynes worried about is not a factor in his theory. So

with saving equalling investment, flexible wages will bring the economy to NAIRU.

Keynes theorized about an adaptive dynamical economy. The stability of such a system depends crucially on what variables effectively govern the adaptation of prices. To Keynes, present saving is not an effective demand for future goods nor is the offer of labor by itself an effective demand for present consumer goods. It is the combination of these two effective demand failures that prevents the Keynesian economy from converging on the two "natural" values of unemployment and the real rate. The market values of the effective excess demands do not sum to zero. It is in this sense that Say's Law fails to hold. When it fails to hold, there is a case for aggregate demand policies.

As the Great Depression has receded from us in time, it has become increasingly clear that Keynes's theory exaggerated the prevalence and magnitude of these effective demand failures. Certainly, his conviction that household saving had a permanent tendency to run ahead of private-sector capital accumulation cannot have been shared by any American economist for several decades at least. (And the Keynesian literature provides few clues about what to expect when the problem is a permanent tendency of too little saving). Moreover, Phelps's judgment that European unemployment is an equilibrium phenomenon in the explanation of which deficient aggregate demand plays no part may well be roughly right. Still, one cannot just dismiss the hypothesis that deficient aggregate demand had a lot to do with creating the high European unemployment to begin with, even if its persistence may be better explained along the lines, for example, of Ljungquist and Sargent in this volume.⁶

⁵ See the excellent summary and appraisal in Woodford (1994), pp. 1800–806.

⁶ Blanchard (p. 351) handsomely concedes to Phelps: "As a combatant initially on the opposite side, and one involved in many (intellectual) skirmishes with Ned over the years, I would submit that he has won the war" but maintains nonetheless that "movements in aggregate demand surely played a role in affecting the timing of the increase in unemployment ...".

In the long run-up to the European Monetary Union, the macropolicies of European countries were generally quite conservative by the standards of earlier post-war decades.

In general equilibrium macroeconomics, Say's Law is taken to hold simply as a consequence of the aggregation of individual budget-constraints spanning all markets (Robert Clower and Leijonhufvud 1981). Much of the oft-praised discipline of general equilibrium theory stems simply from the consistency that the aggregation of binding budget-constraints imposes on the system. IS-LM, in contrast, evaded this discipline and allowed aggregate demand to constrain supply rather than supply creating its own demand. IS-LM may sometimes prove too loose and undisciplined a construction—but by the same token general equilibrium will on occasion prove too tight and too tidy.

Keynes's point was that some of the market excess demands obtained by the aggregation of *ex ante* budget constraints would be irrelevant to the adaptive stability of the system. But there is another, related issue of equal importance, namely the *ex post* consequences of *violations* of budget constraints. The extremes of macro instability arise from violations of the equal value condition which is the foundation of the theory of exchange—and thus of general equilibrium theory.

These violations of budget constraints are of two kinds. One occurs when a government runs a deficit financed by outside money creation, the other when private-sector promises to pay are not fulfilled. Modern economies are fairly robust systems. Moderate inflations or moderate rates of default will not impair their functioning very much. If some agents are found to violate the equal value in exchange condition, others have to bear a loss. In relatively normal circumstances, the incidence of these losses is determined quickly and easily. In moderate inflations, all pay the inflation tax. In isolated defaults, the immediate creditor takes the loss. But the overall system equilibrium

is not much affected. In high inflations or great depressions, things are not that simple. The image of a general equilibrium somewhat “tweaked” by the inflation tax is totally inadequate in the case of high inflations. The entire structure of the economy changes. Intertemporal markets disappear and spot-markets fragment. Stock markets become inactive because reliable accounting of real earnings becomes impossible, as does holding managers accountable. Markets for bonds and all but the very shortest nominally denominated contracts disappear. Bank intermediation largely dries up because of the decline in the demand for real balances. So: no stocks, no bonds, no intermediation. A high-inflation economy cannot grow because growth cannot be financed.

Finance can be crippled by credit crises as well as by inflation. When an economy reaches a state where the ability to pay of a large proportion of agents is conditional on being able to collect from others whose ability to pay is itself conditional ... and so on, the bursting of a speculative bubble or the failure of a large intermediary will trigger a crisis. The enforcement process that is then triggered does not have much to do with optimal calculation and the associated equilibria. It becomes rather a matter of the system mindlessly grinding away, ruining some and saving others in an often highly arbitrary manner. If market processes are just left to run their course, the eventual outcomes will then not conform to those notions of justice and fairness that have previously made people willing and accepting participants in the system.

Consequently, when default occurs on a large scale, the rules themselves end up in the political arena. In this process, the effective rights and obligations of agents become still more uncertain and ultimate outcomes very opaque. The resumption of full employment and of growth may then be long delayed.

It is important that we learn to understand better these “untidy” processes which, when

they occur, threaten the social order. Much as I admire the papers in this volume, I am also left with the overall impression that modern macroeconomics leaves too little room for the extremes of instability.

7. Conclusion

The remarkable thing about this *Festschrift* is that all of its papers have strong links to contributions of Ned Phelps, some recent, others up to forty years old. The debts to Phelps that are recognized here are quite genuine, not just pro forma. The volume as a whole should be an eye-opener to many readers who may not have been aware of the full range and long-lasting influence of his contributions.

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