

The Structuralist Theory of Employment

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The postwar era showed us that we knew much less about employment determination than we thought. In this country, I remember, economists estimated in the mid-1950's that unemployment could fall to 4.5 percent or less without bringing an inflation problem. Later, it took an unemployment rate around 5 percent to keep inflation stable, as 1964 and 1973 illustrated. By the mid-1980's, it apparently took an unemployment rate of more than 6 percent: witness the turnaround of the inflation rate early in 1987. In Western European economies with few exceptions and in Canada, the rise of unemployment has been much greater, reaching a higher level from a generally lower starting point. Among the OECD countries, the secular increase in joblessness typically exceeds the increase found in the average recession, and it is far more destructive, since its social ill-effects are somewhat cumulative.

I began to try to understand this sea change in unemployment ten years ago. My first effort, on the slump in Western Europe, with Jean-Paul Fitoussi (Fitoussi and Phelps, 1988), invoked wage stickiness nominal or real, to show how the overseas shock to real interest rates that began in 1981 could drive Europe's unemployment rate above the *natural* rate. We did not explore how such an external shock might alter the natural unemployment rate itself. I soon sensed, though, that there was a permanent component to the rise in Europe's unemployment. If I was to rescue the concept of the natural rate from the growing discontent with it, I needed to understand how events had driven up the natural rate in Europe and elsewhere in the West.

To model the natural rate I decided, naturally enough, to return to the road I started

down in the mid-1960's: an incentive-wage theory of the natural rate. In the conception of the natural rate sketched in my 1968 paper (Phelps, 1968), equilibrium in the labor market requires a pool of unemployed workers because of the problem of employee turnover. At too low an unemployment level, firms generally want to pay a wage above their expectation of the going wage in hopes of reducing their employees' quit rates—a situation of expectational disequilibrium, since actual wages then exceed expected wages; for steady-state equilibrium the unemployment rate must rise to that steady level (what we came to call the natural rate) at which joblessness (and hence the quit rate) is low enough that the wage a representative firm sets is just equal to the wage it expects other representative firms to pay. (The natural-rate conception of Milton Friedman [1968] was really the equilibrium rate of labor-force participation; there were only allusions to unemployment.) But this was a partial-equilibrium view of the equilibrium unemployment rate and equilibrium wage. It did not show how prices and quantities in *other* markets (the real interest rate and real exchange rate) might theoretically affect equilibrium in the labor market, particularly the natural rate. Hence, hardly any comparative-static exercises with macro shocks could be performed.

Further modeling, beginning in the late 1970's, simplified and clarified the theory. Steven Salop (1979) stripped money from the turnover model, and Guillermo Calvo (1979) built a similar model of the natural rate based on shirking instead of quitting, an easier case. An explicit treatment of the optimal incentive wage as a function of the unemployment rate was begun by Robert Solow (1979) and further developed in a model of all-shirking or no-shirking by Carl Shapiro and Joseph Stiglitz (1984). Thus the labor-market equilibrium point on the equi-

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librium demand curve, which gives the natural rate, was represented as the intersection of the equilibrium wage curve with the equilibrium demand curve—a pedagogical step forward. These papers too ignored or banished effects from other markets. But the ground was cleared for some general-equilibrium construction. Standing on their shoulders, I was able to fashion some crude general-equilibrium extensions of the incentive-wage theory of the natural rate that I had begun two decades before.

With my recent book *Structural Slumps*, published early last year (Phelps, 1994), I hope to have set out reasonably clearly what seem to me to be the main general-equilibrium linkages: the role of interest rates and exchange rates in the demand for labor (and possibly in the equilibrium wage curve), the role of nonwage incomes (both private wealth and so-called "social capital" provided by government entitlements) in determining the equilibrium wage curve, and the effects of certain kinds of taxes and labor-market interventions on each of the curves. A family of dynamic incentive-wage models, open and closed, are studied, and various thought-experiments performed. The results provide, I think, at least a large part of an explanation for the rise of the natural rate in recent decades.

This occasion is a welcome chance to talk about that book. The middle section will set out its main ideas. The last section will take up some criticisms of it to date. It will be useful to begin, though, with a brief review of the preexisting theories, as they seem to enjoy a great deal of momentum, and I am not sure that their unsuitability or inadequacy is widely appreciated

I. Preexisting Theories

Keynesian economics, old or new, has been used in explaining the secular elevation of unemployment, particularly the cumulative rise since the mid-1970's. In the Keynesian interpretation, much of this increased joblessness is laid to monetary policy in the United States and Germany, which is seen as maintaining a deficiency of aggregate effective demand in order to reduce

inflation (Robert Eisner, 1994; Franco Modigliani, 1994).

Neo-Keynesian theory—the theory that if aggregate demand is kept deficient, causing the inflation rate to fall steadily ahead of the expectations on which firms based their money wages and prices, unemployment will seek a level above the natural rate—is often an essential tool. But the premise in the neo-Keynesian interpretation, that monetary policy in the West has been predominantly disinflationary since the late 1970's here and earlier in Europe, shrinking the expected inflation rate by keeping actual inflation under it, is false. Central banks attacked inflation in the early 1980's and again in the early 1990's. But they were only reacting to the rising inflation of the 1970's and the late 1980's, not creating a brave new world of near-zero inflation. In the United States, there is *no* cumulative decline of inflation since the mid-1960's: In 1986 and again in 1994 the annual inflation rate subsided merely to its level in 1967, about 2.5 percent. In Germany too, inflation returned last year only to its 1960's average, also 2.5 percent.

This long-run stability of the inflation norm also refutes the old-Keynesian hypothesis that the *equilibrium* unemployment rate has been pushed up by policymakers' achievement of reduced inflation—seen as a move down a negatively sloped long-run Phillips curve. Besides, the Phillips curve remains dead. If one asks whether the residuals of the reduced-form cross-country time-series regression equation for the unemployment rate of 17 OECD countries in my book (Phelps, 1994 Ch.17) are correlated over time with the level of the inflation rate, the answer is basically no. The correlation has the wrong sign for most countries. (It is significantly negative only for Denmark, while significantly positive for Belgium, Spain, and the United States)

The neoclassical approach to employment determination, also known as real-business cycle theory, has also failed to illuminate the increased joblessness (see e.g., Robert King et al., 1988). This approach does not encompass involuntary unemployment. It does address total employment, usually as a

ratio to the working-age population. Unfortunately, from the market-clearing and highly aggregative perspective of RBC theory, the strong upward trend of this employment variable over recent decades is apt to be seen as the expansion phase of a long cycle. Some economists of neoclassical persuasion have expressed satisfaction at this trend, which they regard as a sign of the economy's good health.

For me, it is the unemployment view that rings true. The United States is besieged by the social problems arising from the reduced availability of jobs (longer waiting times, etc.) and the reduced gains they offer (relative to alternatives) at the low end of the labor market. For the least-educated males, there has in fact been a decrease in labor-force participation, not an increase.

A new entrant to the field is the hypothesis of hysteresis, or strong persistence, which grew to prominence in the 1980's as high unemployment continued in Europe. Out of this came the theory that last year's unemployment rate gives this year's level in the absence of any fresh shock (see Olivier Blanchard and Lawrence Summers, 1986; Assar Lindbeck and Dennis Snower, 1986). A long-noted source of hysteresis is the dole. Where unemployment benefits are high and open-ended, job losers may prefer to go on being unemployed even if the old opportunities return. (Jacques Rueff, 1931). Another source of hysteresis arises when the length of workers' joblessness costs them their morale, skill, and capacity to work; no longer employable, they may still report themselves unemployed (see Phelps, 1972). Now it is also argued that the existing work force at firms, whether acting through a union or merely informally, admits "outsiders" only to replace departing insiders or when a permanent increase of demand appears to have occurred. A corollary of this hysteresis theory is that a stimulus to aggregate demand, even if one-time in nature, will permanently lift employment, contrary to natural-rate doctrine.

These are all relevant features of the labor market. But the evidence that they have the quantitative importance for unem-

ployment determination claimed for them is tenuous. It is quite true that, after fluctuations with little trend in the 1960's, there was a seeming ratcheting-up of the unemployment rate to a higher plateau in the period from 1973 through 1981, and seemingly another ratchet from 1982 to the present (see Marco Bianchi and Gylfi Zoega, 1994). But this impression may be due entirely to the fact that the first elevation of the real oil price, which started in 1973, was itself very slow to decay and was soon replaced by the second oil price shock in 1979. Then, 1982 and subsequent years showed a steep elevation of the world real long-term interest rate that has persisted with little fluctuation to this day.

One may also consider the residuals from the regression equation in my book for their correlation with the unemployment rate *five years* earlier. (Of course, the regression equation included a one-year lag of the unemployment rate, which is not part of the hysteresis hypothesis.) Not surprisingly, a significantly positive coefficient on the earlier unemployment rate was found in a few countries: Spain, Norway, Denmark, and the Netherlands. The coefficients were not very large (about one-third), though, and the average R^2 much smaller than that. At the other extreme, there is the United States with significantly negative hysteresis, and France and Belgium, which also show negative hysteresis, though somewhat short of significance. I would have to say that, even if there are instances in which hysteresis was of quantitative importance for aggregate unemployment, the evidence does not suggest that this importance is at all widespread.

I conclude that the natural rate is alive and well in all or at any rate the overwhelming majority of advanced economies. The great problem with it has been the elusiveness of how it is determined.

II. A Theory of the Natural Rate

I come finally to what I hope my recent book has contributed. For me, there are three theses that are the centerpieces of the

theory, and the two tested so far have both come out very well. One of these is the thesis that a permanent external upward shock to the world real interest rate drives up immediately and permanently the natural unemployment rate of any country that is integrated with the world capital market. The theory is that this external parameter shift lowers the equilibrium labor demand curve (in the employment-rate-wage plane) against an equilibrium wage curve that is lowered less, if at all, with the result that the employment rate (1 minus the unemployment rate) is decreased. The increased overseas interest rate operates through the channel of the domestic rate, which it pushes up, of course; the contractionary effect of higher interest costs on the real demand price of labor is an ancient theme of capital theory and hence not at all surprising. Insofar as the channel to the domestic interest rate is weak, there results a depreciation of the real exchange rate (well defined even in the nonmonetary models used here), which has a similar contractionary effect.

The results from the reduced-form crosscountry time-series panel-type study, which I described earlier, turned out well: the estimated effect of the external real interest rate on a country's unemployment is of the right sign, of reasonable size, and statistically quite significant. This is just one empirical study. In recent times hardly a year has gone by without another statistical study, generally using long historical time series, showing the negative effect of the real interest rate on economic activity.

The profession rightly reacts cautiously to these studies, mine and the others. Many economists fear that the result on the effect of the real interest rate is simply measuring the effect of tight money, which they see as driving up real interest rates as it drives up unemployment. This counterinterpretation rests on a misunderstanding, however. First, Keynesian models do not all have the feature that tight money leaves the real interest rate elevated as long as employment remains depressed; the "IS curve" could be positively sloped, reflecting the poorer prospective returns to investment at low

output, in which case the real interest rate will tend ultimately to a lower level after output has been pulled down by demand management. (Thus it is questionable whether monetary policy can engineer a permanently elevated real interest rate.) Second, since the typical interest-rate increase has been one "imported" from abroad, the relevant Keynesian analysis would describe the immediate impact as an upward movement along the country's "LM curve," not an inward shift of that curve, and thus an increase in the velocity of money; so the consequence should be an expansion of employment, not a contraction. (It is only under the Bretton Woods system of fixed exchange rates that the foreign interest-rate rise would entail an inward shift of the LM curve, which is needed to hold up the currency; but that system folded in 1971, before the sample period was half over.) Finally, and independently of the first point, the regression equation controls for the influence of "LM factors" by including the first difference of the inflation rate on the right-hand side of the equation. A contractionary LM development will presumably cause this variable to turn negative so that the decrease of employment will be credited to it and not to the real interest rate—as long as there are episodes in which the latter is driven up or down for non-LM reasons, as indeed there have been.

Another central theme is the thesis that assorted interventions in the labor market operate to raise the natural unemployment rate, not just to lower the wage. The taxation of employment is the leading example. (Barriers to firing a worker once hired are another.) More generally, the burden of taxation on wages relative to that on nonwage incomes (returns from existing bonds and equities, services of consumer durables, and entitlements) matters for employment. The theory says that a value-added tax is fairly benign for the natural rate, since it is a tax falling more or less proportionally on wage and nonwage income alike; the payroll tax (and the personal income tax too if nonwage incomes tend to escape or evade it) bears disproportionately on the reward to

not quitting and not shirking, with the result that the natural unemployment rate is pushed up (as any fall of the wage curve is less than the fall of the demand curve). These properties of the theory trace back to the feature that the propensity to quit or to shirk is always a function of wage rates (the employer's own wage offer and the going wage) relative to the employees' nonwage incomes; without that feature, a one-time Harrod-neutral technical advance would be nonneutral for the natural rate (and much else), contrary to what is suggested by the absence of any systematic trend rate of change in the unemployment rate for more than a century.

The third big theme of the theory is the role of wealth and social capital—or the flow of benefits therefrom (see also Zoega, 1993.) If I own my house and a car and whatnot, I am more likely to be an independent sort with a high propensity to quit or shirk or strike or be absent from my job. If the welfare state will provide me free or nearly free of charge with my apartment, hospitalization, education, and so forth, my dependence on steady employment in my job or in future ones is considerably reduced. Of course, if in addition there is a means test, so that with little or no earnings I will receive unemployment benefits, food stamps, and other assistance not otherwise made available, I have even less reason to preserve my employment. And if my employer now must make large severance payments to retire me, I again become freer of my boss. The impact of this economic independence is a rise of the equilibrium wage curve, as employers drive up the going wage in an effort to elicit better employee performance; and insofar as employers do not restore performance, the impact is also a drop of the labor demand curve (reflecting the reduced marginal productivity of employees). The ultimate effect, then, is a swollen natural rate, whatever the effect on the equilibrium wage.

The task of generating fixed-price indexes of private wealth (to be normalized by the average wage, presumably) for most of the OECD countries—not just the capital stock in private enterprises but also consumer

durables, net holdings of foreign assets, land, and so forth—is something for the future, however, as it is likely to be a demanding project. Yet there are indications that the results may support the theory.

III. The Agenda

Several theoretical questions will need to be answered for the theory to become thoroughly accepted. Edmond Malinvaud (1995) in a recent review article has raised the point that a decision of households to consume less need not bring the drop of real interest rates and consequent expansion of employment that is my equilibrium solution (in a closed-economy model, say, for simplicity). It may spin the economy into an expectational disequilibrium, and the postulated nonmonetary nature of the model's economy is no guard against that. To me this is a problem, since I have never regarded the attainment of equilibrium as axiomatic. But ultimately it is an empirical issue, and I am increasingly convinced that most of the time the world economy is reasonably well modeled as if it were in inter-temporal equilibrium, thus following rational expectations.

As Michael Woodford (1994) has complained in his review article, at places I fall back on the hypothesis of static expectations when rational expectations appear to be too complicated. The worst instance, perhaps, is that I always treat the current going wage as if it were expected to continue for the whole future. Thus workers are not estimating their human capital correctly except in steady state. Another instance is the assumption that a customer regards his supplier's real price as permanent, as if unaware that in the model real prices ultimately converge.

Another issue on the purely theoretical side is that some of the models, particularly the open-economy and two-country models, are so complex that any hope of understanding how they behave requires one to downplay some effects and emphasize others. Thus some of these models are simply too rich to deliver determinate answers to many of the questions one might want to

put to them. The next step, then, may be to simplify. But some things of importance may be lost in the process.

Finally, there is always the possibility that some factor I have omitted, such as the emerging market economies or some change in the factor-saving bias of technological advances, is now the driving force behind the natural rate—or soon will be. My expectation, though, is that the framework I have developed will be able to incorporate such added factors.

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