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What to Do (Macroeconomically) When OPEC Comes

Robert M. Solow

My assignment was to write a short paper stating the correct macroeconomic policy for 1974–75. This seems a straightforwardly simple task, so trivial, in fact, that one wonders why it has been assigned to two different people. Can it be that Poole and I will produce two mutually contradictory answers to so elementary a question?

In order to find out what should have been done in 1974–75, one needs only three bits of information. The first is a notion of what specifically happened in those years to make them worth thinking about. What (exogenous?) events made 1974–75 different from 1964–65, or from 1954–55? The second requirement is some sort of statement of goals. What objectives is macroeconomic policy supposed to achieve? Presumably, the correct policy is one that comes as close as possible to achieving them. The third and last requirement is a model of the economy. How will the economy respond to this policy or that policy or to no policy, under the circumstances that prevailed in 1974–75?

Once you know those three things, finding the correct macroeconomic policy is just a matter of arithmetic. That is why the task is so simple.

This being so, I shall stick pretty close to the issues of principle just listed, and I shall not try to specify the correct policy in numerical detail: the correct commercial paper rate, the correct full employment surplus, the correct excise on gasoline, and so forth. It would be silly to come all the way to New Hampshire to do arithmetic. The best thing to read on this subject is probably *Brookings Papers on Economic Activity*, number 1 (1975). It contains articles by Poole, Modigliani and Papademos, Okun, and Perry that do indeed consider alternative macroeconomic policies for 1974 in some detail. The same issue also contains an

EDITOR'S NOTE: The discussion for chaps. 8 and 9 appears in chap. 9.

excellent article by R. J. Gordon which does go after the issues of principle in the context of a two-commodity ("farm" and "nonfarm") model, and does it right. My analysis differs from his mainly in trying to place the problem in the framework of a completely aggregated macroeconomic model, just to see if it can be done that way. For general arguments, one can go back to Hicks (1965, chap. 7, and 1974, chaps. 1 and 3). For me and many others, a useful idea is the distinction made by Okun (1975, especially the clear analysis on pp. 376–78) between auction markets and customer markets, and their interplay. E. S. Phelps (1978) has analyzed the response-to-supply shock problem in a way which is more "neoclassical" than mine; his paper complements this one.

Initial Conditions

I turn first to the question, paraphrasing another famous query, what made that year different from other years? I propose to maintain the convenient fiction that 1973 was a year of macroeconomic equilibrium. It wasn't, as Poole points out clearly in his contribution to this book, but most of us have made worse assumptions from time to time. The unemployment rate was 4.9%; and since the benchmark unemployment rate used by the Council of Economic Advisors in calculating potential output for 1973 was 4.8%, the proportional gap between actual and potential GNP was estimated at less than half of 1%. Capacity utilization in manufacturing was fairly high, but generally not at bottleneck levels.

The fly in the ointment was that prices were rising. After two years in which the CPI rose at an annual rate of 3.4%, it jumped 8.8% in the four quarters of 1973. A lot of that reflected the rise in food prices; the nonfood commodities component of the CPI rose by only 5% in 1973, but even that was twice the 1972 rate. The food component of the CPI, which had risen 4.7% in 1972, went up 20.1% in the four quarters of 1973. Nevertheless, I will pretend that 1973 was an equilibrium year. My notion of macroeconomic equilibrium permits steady inflation, as will be seen. More to the point, I can conveniently merge the 1973 increase in the price of food with the other more or less exogenous forces that characterize 1974 and make it an interesting object of study.

To come to the point, the conventional wisdom about 1974 seems perfectly acceptable. The economy was hit by a number of shocks, each of which could be regarded as generating a one-time upward push on the domestic price level. The important shocks included the OPEC-enforced oil price increase, the lagged effects of the depreciation of the dollar in 1971 and 1973, the lapse of price and wage controls in 1974, and the worldwide boom in nonfuel mineral prices which began as the normal effect of an unusually synchronized increase in world demand on an in-

dustry with an inelastic short-run supply and turned into a speculative boom. To these four shocks we can add, as already mentioned, the sharp rise in agricultural prices beginning in 1973 and continuing into 1974, caused in part by crop shortages in the U.S. and in part by strong foreign demand, some of which resulted in turn from crop failures abroad.

It is an abuse of language to describe all of these as supply shocks. That is fair enough for the oil price increase, for the depreciation-induced rise in import prices, and for the crop shortage part of the food price increase. There were clearly demand-side elements in the cases of food and nonfuel raw materials. Even there, however, the supply shock characterization does only minor violence to the facts. Since the demand-side impulses originated outside of the U.S., it is not terribly misleading to classify the result as a rise in the world market supply price to U.S. consumers. It would certainly be a mistake to treat these sectoral impulses as if they were equivalent to bursts of generalized excess demand. Generally speaking, I will argue as if everything that happened was analogous to the oil price increase, so that the policy problem boils down to: What to do when OPEC comes? The real-life problem was more complex, of course, but the important issue of principle is the proper response to adverse supply shocks.

Here it seems worthwhile to anticipate the story a little. Suppose a monopolist is able to double the price of an important, almost universally used, input, and make it stick. If nothing else happens, or before anything else happens, the relative price of oil will have doubled, and the general price level will rise by a much smaller amount, depending on the weight of oil in the price index and on the particular sort of index being used. But of course other prices will change too. The rise in the price of oil will disarrange relative prices; somehow the prices of oil-intensive commodities will have to rise relative to those of other goods. There will then be further consequences through demand curves, longer-run reverberations, the whole panoply of general equilibrium effects. One can imagine all this working itself out without any further rise in the general price level, or even without any rise in the general price level at all. But one has to imagine it, because it is unlikely that any of us will live to see it actually happen that way.

If, instead, many nominal prices are inflexible downward, then the realignment of relative prices will be accompanied by a substantial rise in the general price level. This "inflation"—if that is the right word for it—will erode part of the original increase in the relative price of oil. Any sensible monopolist will respond by jacking up the nominal price of oil another notch and then the process may continue. What could have been a one-time rise in the relative price of oil can thus be converted into a fairly long process of continuing inflation. (I hope nobody rushes forward to tell me that the process can only happen if it is "vali-

dated” by the monetary authorities. *Das kennt jeder Esel*, as Brahms is supposed to have said to someone who made a similarly vapid remark after a concert.¹⁾

Here, for instance, is the course of the wholesale price index for fuels and related products and power divided by the all-commodity WPI:

Year	1970	1971	1972	1973	1974	1975	1976	1977
WPI	110.4	114.0	119.1	134.7	160.1	174.9	180.0	194.2
Fuels and related products	106.2	115.2	118.6	134.3	208.3	245.1	265.6	302.2
Fuels ÷ WPI	96.2	101.1	99.6	99.6	130.1	140.1	145.1	155.6

Between 1973 and 1974, the nominal price of fuels rose by 55%. The relative price increased by only 30% because the whole WPI rose by some 20%. It took until 1977 for the relative price of fuels to achieve an increase of 55% over 1973, by which time the WPI was up almost 50% over 1973 and the nominal price of fuels was up by a factor of 2.25.

The point of this digression is to remind us all that the policy problem posed by an adverse supply shock is not to try to undo the real effects of the shock. There is no way that macroeconomic policy can replace lost wheat after a crop failure; if OPEC can raise the real price of oil and enforce the necessary restriction of output, then macroeconomic policy cannot make oil cheaper. The secondary effects on the price level and on aggregate output are the macroeconomist’s real concern.

Goals

That brings us to the second element of a correct policy. What are the goals to be achieved or approached? Here a rough shorthand statement is probably almost as good as the optimal controlnik’s objective function, and perhaps better.

I presume that a one-time increase in the price level is not a tragedy. You would hardly seek that outcome, but neither would you be prepared to sacrifice much real output to avoid or reverse it. Severe distributional effects could always be offset in other ways, if the political process were serious about equity. My impression is that it is not. Anyway, I take it that a one-time rise in the price level can easily be tolerated.

Prolonged inflation, however, is painful. I persist in believing that the media and the political process grossly overstate the true social costs of 1970s-style inflation. Whatever esoteric things *we* have in mind when we speak of the real costs of inflation, the public has been allowed to think that a 6% rise in the CPI is a 6% reduction in real income, as if

1. Oder hat er eigentlich “Das weiss doch jeder Esel” gesagt, wie mein hochverehrter Kollege Professor Dr. Dornbusch glaubt?

we were buyers with fixed nominal incomes in a store whose prices have just been written up by six cents on every dollar. Nevertheless, any sensible policy calculation has to take into account the information about the public's sensitivities revealed by opinion polls. In various ways, inflation *is* painful, and the society is prepared to sacrifice real output to reduce it: not in unlimited amounts, but in perceptible amounts.

Output and employment are also valued, needless to say. Right now the pendulum seems to be swinging away from the standard objects of public expenditure toward private consumption and away from direct regulation of economic activity, but that is not likely to concern us in this context.

To sum up, the object of macroeconomic policy is to avoid prolonged inflation of the sort recently experienced, but without generating severe recession. I think 1974–75 qualifies as a severe recession for this purpose. So in fact we had the worst of both worlds.

A Macroeconomic Model: Output and Employment

The last required ingredient is a model of the economy. It would be easy to produce a model in which prices adjust almost instantaneously to shocks, markets clear essentially all the time, and the correct policy is to do nothing. The trouble with such a model is that it fails so transparently to reflect any actual economy. It therefore has to be supplemented by an elaborate pretense that what looks like involuntary unemployment is really voluntary, that what looks like idleness is really investment in human capital, that what looks like excess supply is really an optimal response to some epidemic misperception of the current state of affairs. I suppose it is a step forward to convert transparent failure into opaque failure. Nevertheless, I shall follow a different strategy and analyze the consequences of supply shocks in a model in which prices move only slowly in response to disequilibrium, so that markets do not necessarily clear in a time period long enough for macroeconomic policy to be effective.

The particular model I shall use is a slightly simplified and extended version of the one contained in Solow and Stiglitz (1968). It is simplified by the elimination of some unnecessary and distracting labor market dynamics, and it is extended by the insertion of something to play the role of the oil price. That 1968 model turns out to be almost identical in basic structure to the one analyzed by Malinvaud (1977). As already mentioned, a different but fundamentally consistent approach has been used by Gordon. With all this literature available, I shall be sketchy in outlining the model.

The representative firm produces a single domestic output (y) using inputs of labor (n) and energy (e), according to a well-behaved pro-

duction function. The stock of capital goods is effectively constant in the short run and either user cost is zero or capital requirements are technologically fixed, so that capital can be ignored. The firm is a price-taker in all markets, and maximizes profit given the prices of output (p), labor (w) and energy (q). There arise routine demand functions for energy and labor, and a supply function for output, all functions of $v = w/p$ and $z = q/p$, the real wage and real price of energy. The supply of output is a decreasing function of v and z . It is plotted as a falling curve in the (v,y) plane for given z ; a rise in z shifts the whole curve downward.

The demand for domestic output is a bit trickier, but I will skip all the details here. Assume that real aggregate demand depends on the real wage (positively, but perhaps weakly), the real price of oil (negatively, but perhaps weakly), real autonomous expenditure (a), and the real money supply ($m = M/p$). The rationale for these assumptions is, roughly speaking, that the propensity to spend wages exceeds the propensity to spend other incomes and that energy is "imported" from a sector that spends a large part of its proceeds elsewhere. Autonomous expenditure includes investment and the appropriate "weighted standardized budget surplus" to allow for the fiscal activities of the government. The real money supply enters for the usual reasons. For given z, a, m , the demand function slopes upward in the (v,y) plane, but the slope may be very flat if the various marginal propensities to spend are very similar. The curve shifts up with a rise in a or m , and probably down with a rise in z , at least in the short run, in which the elasticity of substitution between oil and other inputs is very small. (If the role of OPEC were being played by the domestic agricultural sector, one might want to vary these assumptions.)

I shall assume that the supply of labor is given and insensitive to the real wage in the short run; it will be obvious how to relax that assumption. Any reader of Barro and Grossman (1976) or Malinvaud (1977) will realize that one would have to doctor the aggregate demand function whenever there is unemployment. In general, output in the model economy cannot exceed full employment output, defined as the level of output producible with full employment of the labor force and with the level of energy imports that makes its marginal product equal to z , given full employment. With labor inelastically supplied, full employment output graphs as a horizontal line in the (v,y) plane and shifts down (slightly) for higher values of z . As this story makes plain, I am assuming that domestic firms can always satisfy their demand for energy at the going price.²

2. A complete analysis of this two-factor model, with labor and "energy" treated symmetrically can be found in Robert E. Marks 1978. Marks does all the "Clowerization" of demand and supply functions that I treat cavalierly here.

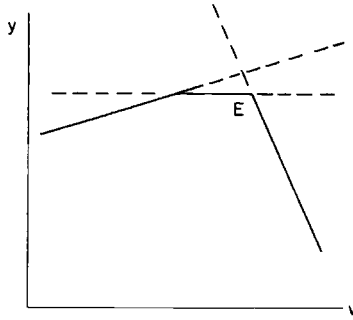


Fig. 8.2 An increase in real money supply or real autonomous expenditure causing an upward shift in the demand function

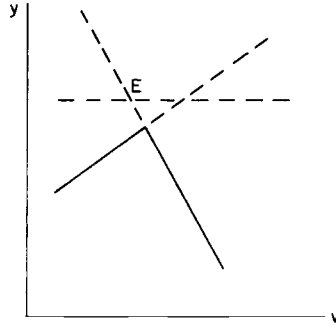


Fig. 8.3 A decrease in real money supply or real autonomous expenditure causing a downward shift in the demand function

over, the current state of the economy helps to drive w and p (and therefore v and z).

A Macroeconomic Model: Prices and Wages

I confess to a good deal of uncertainty about the correct way to model macroeconomically the determination of wages and prices. I have a lot of company. Anyone who professes certainty is pretty certainly wrong. In order to get on with it, I am going to make simple assumptions; I would be the first to admit that they could profitably be refined.

In particular, I shall adopt the formulation of Solow and Stiglitz (1968). The nominal wage is driven by a modified Phillips curve:

$$w'/w = h(n/n^s) + kp'/p,$$

where n^s is the (fixed) supply of labor, $h(\cdot)$ is an increasing function that crosses zero somewhere, k is between zero and 1, and the prime denotes a time derivative. It is easy to think of plausible modifications of this equation and I could easily go along with most of them. There is debate about whether the price change term on the right reflects backward-looking catch-up effects or forward-looking expectations and about whether it should be represented in some more complex way. Some researchers prefer long lags (too long, in my opinion³) and some prefer rational expectations (too rational, in my opinion⁴). Within limits, one could allow for minor complications informally in using the model; but the simple formulation is easy to represent in a plane diagram. Even rational expectations would not make a drastic difference given wage and price stickiness.

For the price level,

$$p'/p = g(y^d/y^s) + iq'/q + jw'/w.$$

Here $g(\cdot)$ is an increasing function, y^d and y^s represent aggregate demand and supply, and the two remaining terms allow for cost-side impulses. Presumably i is less than j , and j is between zero and 1. In any model in which markets do not always clear, a question arises about the proper definition of (unrealized) supply and demand and about the communication of excess supply/demand to the market. I think the broad issues to be discussed in this paper would come out the same no matter which reasonable position were taken on such matters.

I want to make one further drastic simplification. Each of the curves in figures 8.1–8.3 is parametrized by z , the real price of energy. If z were changing all the time, then all of those curves would be shifting all the time. To keep track of the model, one would need a third z -axis. To avoid that complication, I shall assume that the story begins with a one-

3. I have in mind the fact that the long lags often come from equations in which the dependent variable is a short-term interest rate. It is hard to believe that rates of inflation several years old contain information about the next quarter's rate of inflation (all that is relevant for a 90-day bill, say) not contained in more recent observations.

4. I regard rationality of expectations in the Lucas sense as an empirical hypothesis way over at one end of the range of possibilities. The other end of the range is occupied by simple rules of thumb. The a priori plausibility of rational expectations does not seem high; the empirical evidence in its favor that I have seen is very weak and very indirect, certainly no better than that for rules of thumb. Moreover, the hypothesis of rational expectations has not been able to account, so far as I know, for the wide dispersion of actually reported expectations at any instant of time, except by the undocumented assumption that information sets differ. But the differences in information would have to be incredibly large to account for the observed dispersion of expectations.

time jump in the nominal energy price q imposed by the external monopolist. There may be a corresponding jump in the domestic price level p , but it is incomplete, so that the net result is a jump in $z (= q/p)$. After that, I will take it that the monopolist is able to keep z constant, and does so. It is as if the original nominal increase had been calculated to be large enough so that, even after the initial jump in p , the real price of energy had settled right where the monopolist had wanted it in the first place. Earlier on, I pointed out that this was not the way things actually worked after 1973: the real price of oil kept rising for several years and became part of the stagflation problem of the 1970s. But it will be easier if we try to handle that sort of thing informally in discussing the model, rather than formally in the model itself.

With that simplification, the q'/q term disappears from the price equation, because it can be set equal to p'/p . The new coefficient j is really $j/(1-i)$ and the new function g is really the old one multiplied by $1/(1-i)$. I continue to assume that (the new) j is less than one.

Now the wage and price equations appear as two linear equations in w'/w and p'/p . They can be solved for w'/w and p'/p (provided $jk \neq 1$) and thus for $v'/v = w'/w - p'/p$. The determinants of v'/v are y^d/y^s and n/n^s (or perhaps better n^d/n^s if the labor constraint is actually binding so that $n^d > n = n^s$). These right-hand-side variables are themselves functions of v alone in this version of the model. So the end result is an equation of the form $v'/v = f(v)$, where f is a decreasing function. This gives the dynamics of the real wage. There is a critical real wage \bar{v} , the root of $f(\bar{v}) = 0$, which will be maintained if ever achieved. The key property of this stationary real wage is that pressures in the goods and labor markets are balanced; w and p may be inflating together or deflating together, but both at the same rate. If v is below \bar{v} , $f(v) > 0$ and v is increasing; if v is above \bar{v} , $f(v) < 0$ and v is decreasing.

The model is now essentially complete. The point (v, y) is always on the lower envelope curve of figures 8.1–8.3. At any such point we can calculate the direction and speed of change of v , and therefore the motion of the economy along the envelope curve: to the right if $v < \bar{v}$, to the left if $v > \bar{v}$. In the absence of shocks, eventually v tends to \bar{v} , and the economy to the corresponding state on the envelope.

In the parent paper of 1968, the model allowed for some very short-run inertia in employment and output, so that the current state need not always be exactly on the envelope. In that version of the model, the dynamics of the real wage can be described by an equation $v'/v = f(v, y)$, because v does not determine y instantaneously. The function $f(v, y)$ can be quite complicated in general, although it remains unambiguously a decreasing function of v . In Solow and Stiglitz (1968) it is argued that the locus $f(v, y) = 0$ is likely to be upward-sloping, with v increasing

to the left of the curve and decreasing to its right. This more general version of the dynamics of v is represented in figures 8.4–8.6.⁵

Figure 8.4 reproduces figure 8.2, and superimposes four possible versions of the curve $f(v,y) = 0$. Any intersection like A,B,C, or D represents a stationary point for the real economy. Output, employment, and the real wage are constant. The nominal prices p,w , and q may be inflating or deflating, depending on the price and wage equations, but they are all inflating or deflating at the same proportional rate, so nothing real is happening. (Remember, since m and a are assumed to be constant in the background, this means that fiscal and monetary policy are passively accommodating the ongoing inflation or deflation.) A little further reflection shows that B,C, and D are stable points, while A is unstable. If we stick to configurations like B,C, and D then, after any shock the economy will gradually move toward such a (quasi) equilibrium.

A Stylized Story

Once upon a time, there was an economy in macroeconomic (quasi) equilibrium, at point E in figure 8.5. The market for goods and the market for labor both cleared. The real wage was constant, although the nominal wage and the price level may have been inflating (or even deflating) at the same proportional rate. If that were in fact so, then the central bank was keeping the real money supply constant and the fiscal authorities were keeping their net contribution to real demand (the real weighted standardized budget surplus) constant. And far across the sea, OPEC was maintaining the price of energy in fixed proportion to the price of domestic output. Time passed.

One fine day, OPEC announced a big increase in the price of energy. Always responsive to costs, the domestic price level jumped too, but not as much as the energy price, so there remained a very large rise in the real price of energy. What else is likely to happen?

1. The horizontal line (whose height measures net output at full employment) will drop because more gross output has to be paid to OPEC in exchange for energy and because the profit-maximizing input of energy falls. The size of the drop will probably be larger in the short run than in the long run as substitution effects strengthen with time.

2. The aggregate supply curve shifts down. With the stock of capital frozen in the short run, and labor and energy the only variable inputs, there is a rising marginal cost curve for domestic output. The supply

5. In simplifying the parent model for the purpose of this paper, I inadvertently overlooked the implied change from $f(v,y)$ to $f(v)$. Professor John B. Burbidge of McMaster University caught the slip and called it to my attention. I thank him for that and apologize to the reader. Luckily, the message of this paper is not affected at all. The reader can simply imagine the stationary- v locus to be vertical.

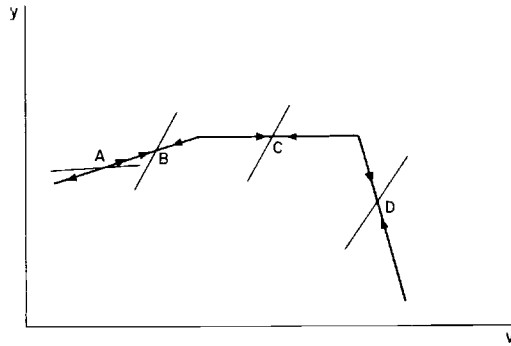


Fig. 8.4 Four possible versions of the curve $f(v,y) = 0$.

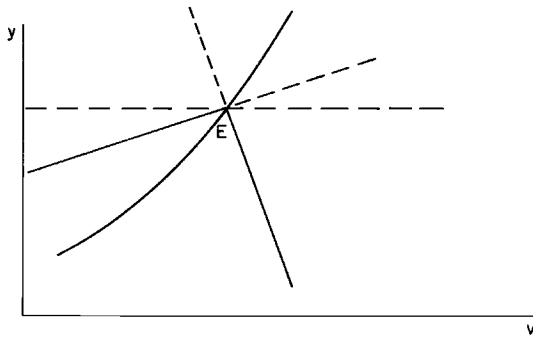


Fig. 8.5 An economy in macroeconomic (quasi) equilibrium, at point E

curve in figure 8.5 may be regarded as showing, for given z and each v , the output at which marginal cost equals price. The new higher value of q implies higher marginal cost at each level of output. Since the initial rise in p does not match the rise in q , marginal cost catches up with price at a lower level of output for each value of the real wage. This could be a very important element in the whole picture.

3. The aggregate demand curve will shift down if OPEC is not a big buyer of domestic output. There is obviously a lot more to be said on the demand side than this model is capable of saying, especially when one takes account of effects through capital markets.

4. Unless fiscal and monetary policy accommodate the initial jump in p , there is a further, and perhaps more important, force depressing the aggregate demand curve. The real money supply will fall unless M jumps along with p ; and slow adaptation of nominal government spending, com-

bined with the normal progressivity of the tax system, will generate real fiscal drag.

5. If p jumps when q jumps, but w doesn't, or jumps even less than p , then v falls suddenly. The economy is displaced to the left along its (new) lower envelope curve. There is nothing in the model that requires the real wage to fall suddenly, but there is nothing against it either, and that seems to be what actually happened.

6. I am not happy with the part of the story that limits the initial response of the price level to a single jump that offsets part of the OPEC price increase, after which z remains constant and the price equation reverts to form. I have the feeling that there ought to be some residual cost push in there somewhere, but I do not see how to do it nicely without losing simplicity. Perhaps it is adequate to allow for a (temporary?) worsening of the price-Phillips curve by a NW shift of the stationary- v locus in figure 8.5. I am not too happy with this ad hoc device either, but it may be better than nothing. (Here is where Okun's model helps a lot.)

The net result of all this is figure 8.6. The old equilibrium point at E is drawn for reference. The new equilibrium is at G and, if nothing else happens, the economy will slide along the envelope curve toward G. I have drawn G on the falling part of the envelope to be compatible with the suggestion that the shift in the supply curve is the dominant geographic feature of the diagram. That could be a mistake if the contraction of the aggregate demand (described under 3 and 4 above) is strong enough.

Instead of a single initial point, I have drawn three possibilities, labelled F_1 , F_2 , and F_3 . In the case of F_1 , the initial reduction in v and

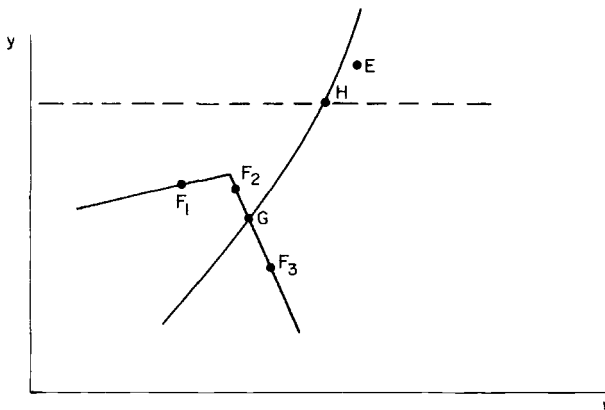


Fig. 8.6

The old equilibrium at point E, the new at point G, and a target equilibrium at point H

contraction of aggregate demand are enough to convert the supply shock into a state of Keynesian unemployment, with excess supply in both the goods market and the labor market. That seems a little unlikely in general, but perhaps the shocks of 1973–74 were extraordinary enough to have this effect. F_2 and F_3 are both, like G , situations of what Malinvaud (1977) calls classical unemployment: more goods could be sold at current prices, but price would exceed marginal cost at any higher output. In the case of F_2 , even though there is excess supply of labor, the wage-Phillips curve generates enough wage increase and the excess demand for goods is slight enough that the real wage will rise and recoup some of its initial loss before stabilizing at G . In the case of F_3 , there is enough excess demand for goods so that the price level will outstrip the nominal wage and the real wage will fall further toward G . Starting from F_2 , output and employment fall along the supply curve as the real wage rises; starting from F_3 , output and employment rise.

Evidently the model can generate a few different immediate outcomes, depending on quantitative details. But they have some important features in common. In the immediate aftermath of an adverse supply shock, output and employment are down, the real wage falls, and inflation accelerates. Only the last of these statements requires further comment. The initial cost-induced jump in the price level is certainly a contribution to faster inflation. At a point like F_2 or F_3 there is excess demand for goods (compared with balance at E) and that is a further contribution. At F_1 , however, there is excess supply of goods, and always there is excess supply of labor. These work to reduce the rate of inflation. Finally, I have staged at least a temporary worsening of the short-run price equation, represented in figure 8.6 by the fact that the stationary- v locus passes to the left of E . It would take a very steep wage-Phillips curve to overturn the presumption that inflation accelerates initially.

Policy

At last I come to the question; but by now the answers are obvious.

I take it that the aim of macroeconomic policy is to steer the economy to a point like H in figure 8.6. The level of income at E is no longer attainable. In accepting H as a reasonable target, I am taking it for granted that nothing can be done about the location of the stationary- v curve, or about wage and price behavior in general. It is possible, of course, that when the extra cost-push effects of the energy price rise have worn off, the curve will drift back to its old position through E and the eventual target equilibrium will be a bit to the right of H . But anything that could have been done by way of policy to modify wage and price behavior post-OPEC could have been done pre-OPEC. This is a separate matter. I do not discuss the possibility of settling for some lower

point on the stationary- v curve with a smaller rate of inflation because I want to avoid the tiresome argument about the existence or nonexistence of a long-run trade-off, or about the length of time before the trade-off evaporates.

I have already argued that it would be a mistake to try to reverse the initial one-shot increase in the domestic price level stemming from the representative firm's attempt to evade the cost increase imposed by OPEC. It is not so damaging as to justify the loss of real income that would be necessary to roll it back from the demand side. Indeed, if the initial point were like F_1 , there would be a presumption in favor of accommodative fiscal and monetary policy to cancel the secondary reduction of aggregate demand caused by the fiscal drag and the reduction of the real money supply induced by the immediate price increase. (Between the end of 1973 and the end of 1975, real M2 fell by 3 or 4 percent; if potential output rose by a routine 6 or 7 percent during those two years, then the equivalent reduction of the real money supply was more like 10%. It is equally commonplace that progression in tax rates added some unintended fiscal drag.) If the initial point were like F_2 or F_3 , then immediate replacement of lost aggregate demand would add little or nothing to output, but would, by adding to excess demand for goods, worsen inflation. Eventually, however, on the way to equilibrium at H, most of the dissipated aggregate demand would need to be replaced via expansionary fiscal and monetary policy: most, but not all, because the target is H, not E.

I am inclined to believe that there was a large component of Keynesian unemployment in 1974, so that expansionary fiscal and monetary policy would have been effective in raising output and employment at least temporarily. If that were all, however, the economy could only have inflated its way toward G.

If the economy is to be steered toward equilibrium at H, then somehow both the aggregate demand and aggregate supply curves have to be made to pass through H. It should come as no surprise that the correct adaptation to an adverse supply shock requires a component of supply management. The obvious tools would have been reductions in payroll taxes and in excise taxes. In the absence of any broad-based federal excise tax, the natural recourse would have been to federally financed reductions in state and local excises. I presume that a negative federal excise tax is too unorthodox to live. (If OPEC and the other disturbances catalogued earlier are the wave of the future, then there may be some case for the creation of a broad-based federal indirect tax, just so that it can be used routinely as an instrument of stabilization policy.) Of course, such policy moves have demand effects too, and these would have to be reckoned into the overall calculation and supplemented or offset depending on the rest of the fiscal-monetary package.

Aggregate supply-oriented policies have the additional advantage of shifting the balance in the goods market so that even if output is temporarily demand-limited, there might be some reduction of inflationary pressure.

In the event, of course, we got mindless demand contraction, as if the inflation of 1974 were a reflection of excess demand, followed by only slightly less mindless demand expansion, with results that are obvious to everyone. Well, almost everyone.

Comment Neil Wallace

Several times in the course of this conference, it has been noted that the conference is less about rational expectations than it is about the macroeconomic implications of non-price-rationing—so-called sticky prices, or, as I would prefer to put it, queue or first-come, first-served rationing. Yet, the discussion has not gone much beyond that remark.

I prefer to speak of there being queues rather than sticky prices because sticky prices have various implications, not all of which still need explaining. Thus, sticky prices give rise to the Phillips curve. This correlation contradicts the pre-signal-extraction full employment models, according to which any change in aggregate demand is felt entirely in prices and not at all in real output. But business cycle correlations are explained by models built along the lines of Lucas (1972). It is sticky prices which result in queues or first-come first-serve allocations that we have not explained. And queues are what many of us mean by involuntary unemployment.

I will comment in a general way about the modeling of queues. This is relevant for Solow's paper, because he would, I think, defend the macroeconomic model he uses on the grounds that it or something like it—say, some version of the model described by Malinvaud (1977)—adequately accounts for non-price-rationing or queues.

The general problem posed by a phenomenon like queues can be put as follows. The only economic model that we know a fair amount about is the Arrow-Debreu general competitive equilibrium model. But many important phenomena are not accounted for by that model. Examples are queues, money, and limited liability. I want to approach the discussion of how to model queues by considering ways of modifying the Arrow-Debreu model.

In modern expositions of the Arrow-Debreu model, we distinguish between the *physical environment* of the model and the *competitive scheme*. The physical environment consists of the technology, the total resources, and the preferences of the agents. A specification of the

physical environment implies a set of feasible allocations and the subset of those that are Pareto optimal. The competitive scheme consists of a description of who owns what, of price-taking behavior, and of market clearing. When competitive equilibrium exists, the theorems of welfare economics connect up the allocations implied by the competitive scheme with the set of Pareto optimal allocations implied by the physical environment.

The competitive scheme can be criticized for not being an explicit noncooperative game. Put differently, it can be criticized for requiring the services of an outside agent, the Walrasian auctioneer. But recent work shows that there are explicit noncooperative games whose equilibrium allocations approach those of the competitive scheme as the number of agents approaches infinity (see, for example, Townsend 1978). This justifies studying the sort of model Solow uses by posing the question: Does it depart from the physical environment of Arrow-Debreu, from the kinds of noncooperative games implicit in Arrow-Debreu, or from both? Admittedly, to ask this question is, in effect, to require that outcomes implied by models which are described as being disequilibrium models should nonetheless be equilibria for *some* game in *some* physical environment. But without such a requirement for what constitutes a model, one cannot argue that the structure of the model is invariant in the face of alternative policies and one cannot appraise alternative policies in terms of the Pareto criterion.

Solow's model and ones like it have the following structures: starting with arbitrary prices, one assumes that agents optimally choose quantities taking those prices as given and, perhaps, taking into account that they may be rationed. The resulting quantities imply excess demands, which, in turn, imply new prices via assumed price adjustment rules. Since the physical environment of such models is not described as being different from that of the Arrow-Debreu model, the claim must be that there is some game whose equilibrium in the physical environment of Arrow-Debreu yields the above structure and, in particular, the price adjustment rules and the rationing schemes.

While we cannot rule out the possibility that the sort of model Solow describes is the equilibrium for some game, there are reasons for doubt. Although many different games can be formulated for a given physical environment, there have to be some standards for what constitutes an equilibrium concept for a game. Any equilibrium concept would seem to involve optimization on the part of agents and some notion of consistency between the environment agents think they are in and the environment implied by the outcome of the game. (By the way, to impose rational expectations is to impose some degree of such consistency, but it would be a great mistake to think that one achieves overall consistency by taking some so-called macroeconomic model and altering it along the

lines of Sargent and Wallace 1975.) Thus, if there are price setters, it has to be shown that the environment is such that the price adjustment rules in Solow's model are optimal courses of action for them. But as Gordon and Hynes (1970) argued, that seems doubtful. In any case, the efficient way to proceed is to start with the description of the game and to see what it implies.

More fundamentally, though, there is reason to believe that phenomena like queues will not be adequately modeled without departing from the physical environment of Arrow-Debreu. Everyone concedes the need for models that assign a smaller allocative role to markets than does Arrow-Debreu. Such models would, in a sense, explain which markets exist and which do not and would likely assign some allocative role to devices like queues. It seems obvious that such models will have to depart from the costless multilateral communication implicit in Arrow-Debreu. When one examines how much information is costlessly exchanged in the explicit noncooperative games whose outcomes mimic those of the competitive scheme, one begins to appreciate how much costless communication is implicit in Arrow-Debreu (again, see, for example, Townsend 1978).

While analogies are dangerous and this one particularly so since Solow is from MIT, it is as if Arrow-Debreu is economics without friction in the same sense as there is physics without friction. And as there are many physical phenomena unexplainable in terms of physics without friction, so there are many social phenomena unexplainable in terms of economics without friction. While not much is implied by saying that the modeling of queues calls for an economics with friction, the notion that we must depart from the physical environment of Arrow-Debreu in order to model queues does suggest that we not make policy recommendations based on a vision of what is feasible and optimal in the Arrow-Debreu environment. A model with friction will not imply the same set of feasible allocations as a model that is in all other respects similar but has no friction.

What seems to divide those at this conference are our guesses about the macroeconomic implications of models that will successfully confront phenomena like queues. Some of us believe such models will have macroeconomic implications much like those of Solow's model. Others believe such models will have macroeconomic implications much like those of Lucas (1972). Since I belong to the latter group, let me say why.

So far I have discussed the modeling of queues per se. In an important sense, though, it is not queues per se that constitute the problem for macroeconomics. Rather, it is the way the lengths of queues vary over time and in a way related to aggregative variables like the price level and the government deficit. A model of queues per se need not be able

to address this cyclical variability. But a combination of the elements that give rise to queues per se and the informational constraint elements in Lucas (1972) will, I think, explain cyclical variation in the lengths of queues. My guess that the macroeconomic implications of models of queues will be much like those of Lucas (1972) is based on my belief that the informational constraint ideas in Lucas will be fruitful in explaining the cyclical variability of queue lengths.

In general, I would have liked to see much more of this conference devoted to discussions of why we differ so much in our guesses about the macroeconomic implications of models of queues. We seem, instead, to have spent quite a bit of time both in the papers and in the discussion simply announcing our guesses.

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