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The New Keynesian Microfoundations

1. Introduction

The central idea of Keynesian economics is that increases in demand stimulate aggregate activity. The contrast with classical models is clearest when an increase in the money supply is considered. In classical models with textbook-style demand curves for money balances, an unexpected once-and-for-all increase in the money simply raises all nominal prices.¹ According to Keynes and his followers prices (or wages) respond slowly and the resulting increase in real money balances raises output.²

This article surveys some recent efforts at deriving this effect of money on output using models in which individual agents maximize their welfare. The models provide microfoundations in that they start from these optimizing agents and construct equilibria in which no individual agent wants to change what he is doing. Two types of models are considered. The first assumes explicitly or implicitly that for each individual firm, there is some cost of changing its price. This assumption, which may appear somewhat ad hoc, has the advantage of being consistent with the

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1. Note that I am ignoring the effect from increases in money balances on real activity that operate through the effect of open-market operations on the distribution of money balances across families. See Grossman and Weiss (1983), Rotemberg (1984), Fusselman and Grossman (1986) and Romer (1986) for an analysis of these effects. Here I am implicitly treating changes in money as being proportionately equal across families.
2. In the *Treatise on Money* Keynes says: "neither economists nor bankers have been quite clear in their minds as to the casual process through which a reduction in the quantity of money leads *eventually* to a new equilibrium with a lower level of money-earnings and of prices" (vol. 1, p. 272, emphasis added). In the *General Theory* (p. 173) this mechanism is outlined in recognizable IS-LM terms.

rather long spells for which individual prices remain constant. In the second category are models with continua of equilibria. In these models, money affects output by moving the economy from one equilibrium to another. While the pressure for output to return to its normal level is weak in the first category of models it is nonexistent in the second. The two types of models also share another characteristic. Several of the multiple equilibrium models lay considerable stress on departures from the Walrasian model which are also important in the literature on costly price adjustment.

I am thus surveying here only a subset of the models whose authors have adopted the "Keynesian" label. Because of space considerations, I only include models in which output depends on nominal variables such as money. These provide the starkest contrast with classical models. It is worth mentioning, however, that the label "Keynesian" has been affixed also to models in which preexisting distortions magnify the effect of government spending on output.³ The mechanisms incorporated in those models may well also magnify the effects of money on output considered here.

While my emphasis is on models in which there is an effect of money on output, it is worth emphasizing that, if prices are rigid, other variables are important as well. In particular, with rigid prices falls in investment demand (due to animal spirits or pessimism about the future) are not accommodated by falls in the prices of these goods. Instead, they directly reduce the output of these sectors and aggregate output. Similar reasoning applies to changes in government spending.

The prototype model of the first type I consider deviates from the Walrasian paradigm in three ways. First, certain agents set prices. This is necessary for it is hard to think of a Walrasian auctioneer, whatever that is, that keeps prices rigid. Usually the agents that set prices are viewed as monopolistic competitors.

Second, since the emphasis is on nominal rigidities some nominal variable must matter. This is accomplished by postulating a textbook-style demand for money. When only these two deviations are operative, unanticipated, once-and-for-all increases in nominal aggregate demand, which are usually modeled as increases in money, do not affect the real allocation of resources. The third and critical ingredient in the prototype model is some reason for prices to be rigid. In Barro (1972), Sheshinski and Weiss (1977), Rotemberg (1982a, b; 1983), Mankiw (1985), Parkin (1986), and Ball and Romer (1986b) there exists an explicit cost of chang-

3. Models of this type include those of Hart (1982), Startz (1984, 1986) and Mankiw (1986b).

ing prices while Blanchard (1983, 1985), Akerlof and Yellen (1985), Calvo (1983), Caplin and Spulber (1986), and Ball and Romer (1986a) restrict directly the frequency of price adjustment.

Given this list of ingredients, one might ask in all seriousness what is new about the current generation of Keynesian models. The major difference between the current and previous generations (Fischer 1977, Taylor 1980a) is an emphasis on the behavior of product markets.⁴ This emphasis has one important theoretical advantage—it is possible to be quite precise about what a monopolistic seller of a good would do if, say, prices are costly to change. This is in contrast to standard models of wage rigidity. In those models it is difficult to model simultaneously the presence of wage rigidity and the behavior of firms and workers in the presence of rigidity. In particular, it is hard to see why, if wages are truly rigid, firms are able to hire as much labor as they desire at the rigid wages.

This theoretical advantage also has an empirical counterpart. As Barro (1977) and Hall (1980) have pointed out, the observation of infrequent changes in individuals' wages does not prove the existence of allocative consequences from wage rigidity. The reason is that rigid individual wages are at least consistent with efficient determination of the level of employment. As discussed in section 2, it is much more difficult to argue that the observed rigidity of consumer goods prices has no effect on resource allocation. In that section I also discuss aggregate evidence for the existence of price rigidities.

A second advantage sometimes claimed for the new Keynesian models is that they require only small price rigidities to produce large fluctuations in GNP. This point is due to Akerlof and Yellen (1985), Mankiw (1985) and Parkin (1986) and will be referred to as the PAYM insight. The insight is that the profit function of any agent who sets prices is horizontal at that agent's optimum price. Hence small deviations from the optimal price lead to only second-order losses. Yet they are consistent with large output swings. In section 3, I discuss somewhat more generally the applications of this insight to macroeconomic problems.

The paper then goes on to present in section 4 a static model of monopolistic competition that forms the basis for the later discussion of price rigidity. This model is classical in that, without price rigidities, its unique equilibrium has monetary shocks affecting prices alone. In section 5, a once-and-for-all change in the money stock is analyzed in a simple two-period variant of the static model to which costs of changing

4. This emphasis itself is not altogether novel. See Means (1935), McCallum (1980), Gordon (1981) and Okun (1981).

prices have been added. In this model monetary expansions, at least if they fall within a certain range, affect output. While some issues that arise in models with more periods are masked by the two-period assumption, this model has several interesting features including a multiplicity of equilibria. In particular, for a range of monetary shocks both changed and unchanged prices can be consistent with equilibrium.

The welfare properties of equilibrium price rigidity in response to monetary shocks is explored in section 6. The question, which is addressed also in Ball and Romer (1986b, 1987) is, granted that changing prices is costly, do prices respond too much or too little to monetary shocks. These welfare conclusions prove to be ambiguous precisely because of the multiplicity of equilibria. This multiplicity is also one reason for the apparent difficulty inherent in analyzing the models with infinite horizons and multiple periods. These are covered in section 7.

Section 8 focuses on models in which the multiplicity of equilibria is, by itself, taken to be Keynesian. Here, the different equilibria can be thought of as being due to differences in the beliefs individuals can have about the future or about the current behavior of others. Since the equilibrium that is chosen is a function of expectations, these equilibria can be thought of as depending on animal spirits.

In these models, which include Stiglitz (1979, 1984, 1985), Diamond (1982), Weitzman (1982), Woglom (1982), Bryant (1983), Geanakoplos and Polemacharkis (1986), Roberts (1986), and Woodford (1986), money affects economic activity if it switches the economy from one equilibrium to another. Some of these models exhibit recognizable price rigidity while others do not. In any event they all avoid any explicit cost of changing prices. An issue that is more important here than in the discussion of the models of the first type is why the particular Keynesian sequence of equilibria is chosen. Section 9 concludes with my personal assessment of the new Keynesian microfoundations and of promising directions of future research.

2. *The factual background*

The most discussed evidence for the existence of nominal rigidities of any kind is the correlation of current GNP with past values of the money supply. Since the work of Sims (1972), this correlation has been studied in the context of relatively unconstrained vector autoregressions. The hypothesis that this correlation is absent from U.S. data (or that money fails to Granger cause output) can be rejected using some specifications while it can be accepted using others. Eichenbaum and Singleton (1986) and Bernanke (1986) are two recent examples of opposite results.

Leaving aside statistical significance, if increases in money are correlated with subsequent increases in output because of nominal rigidities they should also be correlated with gradual increases in prices. It is in fact the slow response of prices which is at the center of the much more structured empirical analysis of Rotemberg (1982a) as well as of the evidence on nominal rigidities presented in Gordon (1983). I thus report on the responses of the price level and output to a monetary innovation within a vector autoregression very similar to that of Bernanke (1986). The vector of variables that I consider consists of the logarithms of military spending, the money supply, GNP, the implicit price deflator for GNP, and the nominal rate of return on U.S. Treasury bills.⁵ It also includes constants and deterministic time trends. This autoregression is estimated with quarterly U.S. data from the second quarter of 1953 to the third quarter of 1986. Table 1 gives the responses of money, output and prices to a 1 percent innovation in the money supply. As can be seen from the table, output rises gradually before falling back to its long-run position (this return is oscillatory) while prices rise gradually. This second fact is just as consistent with nominal rigidities as the first. Interestingly, the short-term dynamics do not depend very much on whether the system is estimated in first differences or not.

While the correlations between money and output have been subject

5. This list also gives the ordering of the variables. Only lagged variables (four lags of each variable) enter in the equation that explains current military spending and so on.

Table 1 RESPONSES TO A MONETARY INNOVATION IN A VECTOR AUTOREGRESSION

| Quarter | Response of: | System in levels | | |
|---------|--------------|-----------------------------|------|----------|
| | | Money | GNP | Deflator |
| 0 | | 1 | 0.46 | 0.19 |
| 1 | | 1.22 | 0.57 | 0.31 |
| 2 | | 1.25 | 1.03 | 0.47 |
| 3 | | 0.98 | 0.81 | 0.66 |
| 4 | | 0.66 | 0.39 | 1.08 |
| Quarter | Response of: | System in first differences | | |
| | | Money | GNP | Deflator |
| 0 | | 1.00 | 0.48 | 0.21 |
| 1 | | 1.31 | 0.63 | 0.38 |
| 2 | | 1.41 | 1.13 | 0.60 |
| 3 | | 1.26 | 1.00 | 0.86 |
| 4 | | 0.90 | 0.74 | 1.09 |

to extensive empirical analysis, their interpretation is somewhat problematic because money is at least to some degree endogenous. On the one hand, King and Plosser (1984) note that the private sector may create more money when its demand is expected to rise. This suggests a positive correlation unrelated to price rigidity. On the other hand, the absence of correlation between money and output is consistent with nominal rigidities if the Federal Reserve is using money purposefully to stabilize output (see Mankiw (1986a)). Neither of these stories accounts for the gradual increase in prices that follows increases in money.

A related set of correlations less subject to this endogeneity is given by the relation between tax reforms and aggregate activity. Poterba, Rotemberg, and Summers (1986) note that in the absence of nominal rigidities increases in indirect taxes accompanied by reductions in direct taxes of the same magnitude should affect neither prices nor output. This can be seen as follows. Suppose the government increases sales taxes on final goods and reduces income taxes leaving government revenue unaffected. Since the burden of taxes is the same and since these two taxes differ only in the side of the market on which money is collected, output should be unaffected. With an unchanged demand for money, prices should be unaffected as well. This means that with flexible wages and prices, pretax wages must fall (so that after-tax nominal wages are unchanged) and pretax prices must fall as well (so prices inclusive of tax are unaffected). With nominal rigidities, pretax prices or wages will not fall, thus reducing real money balances and output. Using both U.S. and U.K. data, Poterba, Rotemberg, and Summers find that these switches do indeed raise prices and lower output.

While this evidence supports the existence of some nominal rigidities it says little about whether wages or prices are more rigid. This extremely important question appears intrinsically difficult to answer because the ratio of wages to prices exhibits very little correlation with GNP.⁶ This means that it appears possible to believe that only prices are subject to nominal rigidities whereas insurance or efficiency wage considerations keep real wages stable. Alternatively one may believe that only wages are subject to nominal rigidities whereas pricing is given by a constant markup over wages.

One notable attempt to gauge the relative rigidity of prices and wages is Blanchard (1986b). He studies how prices respond to innovations in wages and vice versa. To identify these two innovations he restricts the contemporaneous response of each variable to the other. Under these

6. See Geary and Kennan (1982).

identifying assumptions prices appear more rigid as their response to wages is slower than the response of wages to prices.

If rigidities in prices and wages are a central determinant of GNP, then, other things being equal, countries with more movements in prices and wages ought to have fewer output fluctuations. Thus the absence of any evidence that countries with high inflation rates have more stable levels of output might be viewed as discomfiting to nominal rigidities. Yet, as Mankiw discusses in his comments on this article, these countries exhibit smaller responses of output to nominal shocks. Thus the instability in their GNP is perhaps due to the existence of larger nominal disturbances.

At the micro level, it is apparent to those with eyesight that individual prices and wages stay constant for long periods of time. In the United States, hourly wages are normally changed yearly even when there are no explicit contracts. The price of candy bars at the corner grocery store also changes very rarely.

The significance of the individual wage data has been widely questioned by noting that most employment relations are relatively long-term (see Barro 1977 and Hall 1980). Thus, a particular paycheck need not represent the payment for the services actually rendered the month before; it can be viewed as that month's installment on what is a rather long stream of payments. These installments could be relatively inflexible with no effect on either hours worked or individual consumption. For this to be true there must of course be some mechanism that ensures that if one worker works one additional hour today, he receives not just the current wage per hour but also a different amount of future payments that is appropriate given his current marginal utility of leisure.

I now turn to data on product price rigidity. The data on which Means (1935) based his assertion that prices in the United States are "administered" are BLS industry price indices. These indices move little—for example, the index for tin remained unchanged from June 1929 to May 1937. This rigidity would be spurious if quotes given to BLS employees move less than quotes given to genuine buyers. Yet using data gathered by Stigler and Kindahl (1970) on actual transactions prices, Carlton (1986) concludes that price rigidity is pervasive.

However, Carlton himself appears unsure of the allocative significance of this price rigidity. The Stigler and Kindahl data are for intermediate goods sold to firms. Many of these transactions are also part of a continuing relation. Thus they too can be viewed, at least in part, as installment payments. In other words, there may again be mechanisms that ensure that when a firm today buys more of some good the stream of

payments from the buyer to the seller is affected not only today but in the future as well. One mechanism capable of ensuring an efficient allocation which Carlton mentions is rationing. Buyers may simply be unable to buy more at the posted price.

We can only be sure that the rigidity of a price affects resource allocation if two conditions are met. First, transactions must be carried out at this price. Second, there must be nothing that prevents an individual from buying one unit more (or less) of the good and thereby having to pay only the current price for the good. Without rethinking the entire fabric of economics we must concede that prices of goods sold in stores to individuals fulfill both requirements.

So the evidence of Cecchetti (1986) that the newsstand price of magazines is very rigid (*Reader's Digest* changed its newsstand price six times between 1950 and 1980) is simply inconsistent with the absence of allocative effects. We can now be sure that at least some monopolistically competitive producers of goods have meaningfully rigid prices. This is one of the main advantages of focusing on such product price rigidity. Yet once one accepts that there are some goods in which price rigidity plays some allocative role, it is difficult to maintain that the price rigidity observed in the context of more long-term relationships is totally devoid of such a role.

One remarkable fact about the rigidity of individual prices is that prices of monopolies tend to stay constant for longer periods of time than do those of duopolies or other tight oligopolies. This fact, which has some bearing on the theories I will survey below, was originally uncovered by Stigler (1947) in his attack on the kinked demand theory of oligopoly. The finding has been confirmed by several subsequent studies. These are surveyed in Rotemberg and Saloner (1986). Unlike *Reader's Digest*, the newsstand prices of *Time* and *Newsweek* each changed nine times between 1950 and 1980.

3. Objective Functions Are Flat at the Top

In this section I discuss somewhat broadly the advantage conferred on Keynesian economics of the PAYM insight that price setters have only second-order costs of being away from their optimal price. Taken as a broad statement about the fact that small deviations from optimal actions can have macroeconomic consequences it offers a great many possibilities.

For instance, consider a Robinson Crusoe economy in which Crusoe must work to eat. There is an optimal amount of work that solves Crusoe's food-leisure choice. Yet at this optimum point, Crusoe is strictly indif-

ferent between working and not working a little bit more. Small changes in work have only a second-order effect on utility.

Now suppose we follow Akerlof and Yellen (1985) and consider “near-rational” strategies. These strategies are defined as leading to at most second-order losses relative to rational strategies. A straightforward near-rational model of macro fluctuations would have Robinson Crusoe simply randomize a little bit over the amount of effort he expends.⁷ Such a model would have the considerable advantage of simplicity. This model could also explain the comovements of other variables with GNP; nothing prevents Crusoe’s randomization from being a little bit systematic.⁸

One objection to this use of PAYM insight is that in the Crusoe model the welfare cost of the economic fluctuations is of second order. Instead, it might be argued that in the presence of monopoly or other distortion, the costs to the individual who leaves his price unchanged can be of second order while simultaneously the cost to society from the price rigidity may be of first order. Later I quarrel with this view; it appears invalid whenever output moves both up and down instead of moving only down.

So the PAYM insight does not, by itself, justify rigid prices. It does offer some relief from the notion that models with price rigidity are ipso facto implausible because keeping prices flexible is cheap. This relief is only partial. The insight also justifies keeping output fixed in response to changes that make output changes profitable. So why are the near-rational firms in the world opting to keep their prices fixed while their quantities vary? Perhaps this is a coincidence. More likely, one needs nontrivial costs of making prices flexible.

Before closing this section it is worth speculating on the additional richness that the PAYM insight would give more traditional dynamic models. In such models, the effects of temporary increases in government spending depend on the marginal propensity to consume. Yet an optimizing individual is essentially indifferent as to when he spends an additional dollar of income. This means that the marginal propensity to consume can be any finite number (positive or negative), and individuals

7. Suppose Robinson Crusoe’s utility function is Cobb-Douglas over consumption and leisure with an exponent on consumption equal to $\frac{1}{3}$. Suppose that there are 24 hours of leisure per period and that there is a linear technology that converts hours of work into consumption. Then optimal labor supply is 8 hours. Raising labor supply by 10 percent lowers utility by 0.2 percent.
8. Near-rationality can justify also the polar opposite of excessive movements in output. It is for instance near-rational for Robinson Crusoe to maintain constant his leisure in response to small variation in his productivity.

would still only suffer second-order losses from their lack of perfect optimization.

4. *A Static Model with Monopoly*

In this section I consider a simple static general equilibrium model. This model borrows heavily from Rotemberg (1982b), Mankiw (1985), Blanchard and Kiyotaki (1986), and Ball and Romer (1986b). The representative consumer at t has a utility function given by:⁹

$$U_t = [(1/J) \sum_{i=1}^J (C_{it})^\theta]^{1/\theta} / \beta - L_t, \quad (1)$$

where there are J goods, C_{it} is the consumption of good i at time t while L_t is labor supplied at t . The parameter β must be smaller than one to guarantee concavity. Consumers maximize at t the expected present discounted value of U_t which can be written as

$$V_t = E_t \sum r^{t-t'} U_{t'}. \quad (2)$$

In equation (2), ρ is a discount factor while E_t takes expectations conditional on information known at t . In the simplest version of the model opportunities for intertemporal trade are limited so maximizing (2) is equivalent to maximizing (1). Throughout, I impose a cash-in-advance constraint which requires that

$$\sum P_{it} C_{it} \leq M_t. \quad (3)$$

where M_t is the level of money balances and P_{it} is the price of good i at t . The next question is how money is acquired. The simplest approach is to assume that money is the only asset and that money at t is equal to labor and nonlabor income at t .¹⁰ In principle, it is possible to extend the

9. The model can thus be interpreted as either having a single consumer or all variables can be interpreted on a per capita basis.

10. This approach creates some delicate issues of timing. First the money supply is announced. Then, nominal wages are set to clear the labor market. This is followed by an opportunity for firms to set (or change) their prices. Banks then must extend credit to workers and equity holders. This credit is in the form of money that is spent on goods. At the end of the period the firms then give the money to the banks in the name of the workers and capitalists thereby canceling their debts.

analysis so that it applies when there are other assets as well. Suppose that there is an asset whose nominal rate of return from t to $t + 1$ is i_t . Then the usual intertemporal budget constraint would require that

$$\sum_{\tau \geq t} M_\tau / R_\tau = A_t + \sum_{\tau \geq t} W_\tau L_\tau / R_\tau \quad (4)$$

$$R_\tau = \prod_{0 \leq j < \tau} (1 + i_j) \quad (5)$$

where W_t is the nominal wage at t . Obviously, when (5) is the appropriate constraint, the path of interest rates must adjust for (3) to hold. Since (3) holds, one can obtain the demand for goods by maximizing (1) subject to (3). This gives

$$C_{it} = (P_{it}/P_t)^{-r} (M_t/P_t)/J \quad (6)$$

where r equals $1/(1 - \theta)$ and

$$P_t = [1/J \sum_i (P_{it})^{1-r}]^{1/(1-r)},$$

so that P_t is a price index for period t . Constant elasticity demand functions like (6), or the very slight generalization in which real money balances are raised to a power as well, are a virtual constant in this literature. If, in addition, it is assumed that no saving takes place, one can obtain a static labor supply schedule, which is given by

$$L_t = (W_t/P_t)^{\beta/1 - \beta} \quad (7)$$

which means that labor supply is upward-sloping as long as the utility function is concave in consumption.

Firms are assumed to be monopolistic competitors who maximize the expected present discounted value of profits. These firms are assumed to take the wage as given and to have access to a linear technology so that

$$Q_{it} = L_{it}, \quad (8)$$

where Q_{it} is output of good i at t while L_{it} is labor input into good i at t .

In the absence of price rigidities the firm's problem is thus the usual monopoly problem with constant marginal cost W_t and constant elasticity of demand. For the monopolist's problem to be well defined, this

elasticity r must exceed one, so that θ must be between zero and one. Prices then equal W_i/θ . Under the assumption that the labor market clears, equilibrium wages can be obtained from (7) and (6). Using these wages, the optimal price from the point of the view of a single firm, P_i^* is approximately¹¹

$$P_i^* = P_i(M_i/P_i)^{(1-\beta)/\beta/\theta}$$

When all firms charge this optimal price, equilibrium aggregate output, real money balances, and employment equal $\theta^{\beta/(1-\beta)}$.

Under perfect competition, which obtains here if each good is supplied by two firms competing in Bertrand fashion, the real wage is one and employment is one. So, since θ is less than one and $1/(1-\beta)$ is greater than one, output is lower under monopoly than under perfect competition. The tendency for monopoly to raise price translates in general equilibrium into low real wages which discourage labor supply.

5. *An Increase in the Money Supply in a Two-Period Model*

The simplest dynamic model in which the effects of increases in money in the presence of rigid prices can be studied has two periods. In the first period money is given by some initial value M_0 and is expected to remain at this level forever. Prices are therefore given by (9) with money equal to M_0 . In the second period, money increases unexpectedly by k percent. In the absence of rigidities, prices would rise by k percent, output and real balances would be unaffected. Suppose however that changing prices is costly. Mankiw (1985), Akerlof and Yellen (1985), and Blanchard and Kiyotaki (1986) compute the size of this cost that leaves each firm indifferent between maintaining the first-period price and raising its price.

The way this computation is carried out in Mankiw (1985) and Blanchard and Kiyotaki (1986) is to assume that all firms hold their price fixed and to compute the gain to a single firm from optimally changing its price. Denote this gain $f(k)$ where, for small k , $f(k)$ is proportional to k^2 . For costs of changing prices equal to $f(k)$, not changing prices is an equilibrium.

As discussed in Rotemberg and Saloner (1986) this equilibrium is not unique. This can be seen as follows. Suppose the firm that sells good j raises its price. This will raise the demand of firms producing substitutes for good j . With the demand curves given by (6), the demand for all

11. This approximation is exact whenever, as in the flexible price equilibrium, firms all charge the same price.

other firms increases as long as the elasticity of demand, r , exceeds one. This additional demand provides an additional incentive to bring price closer to P^* . Moreover, if β exceeds one-half, or more generally, if real wages do not fall much in recession, P^* rises when firm j raises its price. Thus, in general, if costs of changing prices equal $f(k)$ and one firm raises its price, it becomes optimal for all the others to change their price. Such a collective price increase would of course also make it optimal for the original firm to raise its price. There is thus also equilibrium in which all the firms raise their price. Of course, for costs of changing prices sufficiently large (that is, larger than some critical level $f^*(k)$) a single firm would not change its price even if all other firms changed theirs. Only for costs larger than $f^*(k)$ is not changing prices the unique equilibrium. For costs of changing prices between $f(k)$ and $f^*(k)$ there are two additional equilibria. In one of these, all the firms change their price. In the other, a fraction α between zero and one of the firms change their price. This fraction α is increasing in the cost of changing prices. Given that this fraction is changing its price, firms are indifferent between changing their prices or not.

Still, for some level of costs which, for k small, is proportional to k^2 , price fixity is the unique equilibrium and, given (6), output rises. At this point, it is worth raising the question of what precisely makes price changes costly. There are undoubtedly some small administrative costs of changing prices. These are sometimes referred to as "menu" costs although more than the printing of menus must be involved since the newsstand price of magazines is printed with the rest of the magazine. Another possible cost, which is stressed by Okun (1981, p. 141–53)¹² and Rotemberg (1982b), is the cost of customer dissatisfaction with firms whose pricing appears erratic.¹³

Ideally such customer dissatisfaction would be modeled explicitly and much remains to be done on this. For the moment imagine that any change in price costs goodwill which affects future purchases. At least to some extent, the loss in goodwill by one's competitor is a gain to oneself. It is even possible that the loss in goodwill from changing one's own price is reduced if others are changing their prices as well. This would exacerbate the multiplicity of equilibria mentioned above, since it would mean

12. Okun's analysis differs from that in the text because he feels that customers are upset only if firms change prices in response to demand changes while they accept changes based on movements in costs.
13. Whichever of these costs is deemed important, it is obviously minimized when prices are quoted in the unit of the means of payment. McCallum (1986) also argues that fixing prices in different units (that is, in terms of an index of prices) provides only small benefits.

equilibria in which all firms change their prices exist even when k is quite small.

The increased output produced when money rises requires that workers work more. It is important to discuss the mechanism that brings forth this additional effort. In the simple model outlined above, W_t is flexible so the real wage adjusts to make workers willing to work more. For the case of static budget constraints, the required change in the real wage can be obtained from (7). In the case in which there is an intertemporal budget constraint as in (4), the required change in the real wage is generally more difficult to compute but conceptually presents no new problems. With utility strictly linear in labor supply, we know that the equilibrium nominal wage discounted at the nominal interest rate is independent of time.

An alternative approach is adopted in Blanchard and Kiyotaki (1986). They postulate that workers each possess unique attributes so that they too act as monopolistic competitors when they set their wages. Then their loss in utility from keeping their wages constant and agreeing to work more in response to increases in M can also be suitably bounded. Both of these approaches have the very un-Keynesian implication that in recessions workers are close to indifferent between working and not working.

Suppose then that one believes that intertemporal substitution of leisure cannot explain the movements in employment.¹⁴ One can then obviously not believe that the constancy of goods' prices is the only departure from the classical model. But is one then required to believe that there are nominal wage rigidities as well? The paper by Akerlof and Yellen (1985) suggests that this is unnecessary and that it is sufficient for there to be a "real" imperfection in the labor market.

Akerlof and Yellen (1985) postulate an "efficiency" wage that employers pay to increase worker diligence. In their model, effort by workers is increasing in the wage they receive. This implies that the optimal real wage depends only on the responsiveness of effort to the real wage.¹⁵ It is independent of employment and can easily be above the marginal rate of substitution between consumption and leisure. The decline in output that results from reductions in the money supply in the presence of rigid prices then leaves the real wage unaffected, while reducing labor demand and increasing unemployment.

14. See Mankiw, Rotemberg, and Summers (1985), Eichenbaum, Hansen, and Singleton (1985) for aggregate evidence on this sort of intertemporal substitution.

15. For a general discussion on the strengths and weaknesses of efficiency wage models see Katz (1986) and Stiglitz (1986).

6. Are Macroeconomic Fluctuations Excessive?: Welfare Issues

In this section I discuss the welfare effects of changes in the money supply. Suppose, as is true in any equilibrium, that all goods are supplied in equal quantities. Then using (8), one can write individual utility as

$$L_t^\beta / \beta - L_t. \quad (10)$$

Since the level of output supplied by monopolistic competitors is lower than one (the competitive level), the derivative of the representative utility (10) with respect to employment is positive at the monopolistic equilibrium. Expansions in output are good, contractions are bad. As Mankiw (1985) points out, this accords well with popular accounts of business fluctuations. Moreover, suppose that output is sometimes high and other times low. Then, since utility is concave in consumption the existence of aggregate fluctuations itself also reduces welfare. Stabilizing output using the money supply is a good idea.

A different question is whether stabilizing output by forcing firms to always change their prices is a good idea. This question of whether there is excessive price rigidity (or excessive output variability) given the costs of changing prices is addressed in Ball and Romer (1986b, 1987). The answer is not obvious because the losses from the *fluctuations* in output are of second order,¹⁶ the same order as the cost of changing prices.

I now present a simplified version of the arguments in Ball and Romer (1986b, 1987). Consider k percent changes in the money supply which are equally likely to be positive or negative. If prices are unchanged, employment changes by k percent as well so the second-order effects on utility are equal to

$$(\beta - 1)L^\beta k^2/2. \quad (11)$$

Since utility is linear in leisure, this can be interpreted as the leisure cost of the fluctuations. Now consider the costs of changing prices that lead to the fluctuations. Here, I compute the costs $f^*(k)$ that ensure that the unique equilibrium has constant prices. These costs are such that even if all firms but one change their price optimally, that is, by k percent, the

16. To first order, the gain from an expansion equals the size of the expansion times the marginal utility of employment. Thus if contractions are as likely as expansions of the same size the first-order effects of both cancel.

remaining firm would prefer to keep its price fixed. This cost is thus equal to the cost from having output differ from the optimal output by rk percent at the equilibrium levels of real wages and real money balances. This cost is easily computed to be

$$(r - 1)QPk^2/2$$

in nominal terms. At the equilibrium real wage this equals

$$(r - 1)Q\beta k^2/2 \quad (12)$$

units of leisure. Equation (12) gives the costs to a firm from changing its prices while (11) gives the costs to individuals from price rigidity. Price rigidity is thus excessive, in that individuals lose more than the costs of changing prices if

$$I(1 - \beta)/[J(r - 1)] > 1 \quad (13)$$

where I is the number of individuals. A low value of β renders price rigidity excessive because utility is very concave and individuals lose much when output fluctuates. Similarly, a high value for the perceived elasticity of demand, r , means that firms must let output fluctuate a great deal if they alone keep their prices fixed. Such large fluctuations are costly to firms, so a high value of r reduces price rigidity. If I equals J and firms face an elasticity of demand of at least two the ratio in (13) is less than one. Prices move too much.

Ball and Romer (1986b, 1987) also conclude that ratios of the form of (13) are not unambiguously greater than one. It is worth noting that I have analyzed the equilibria with least rigidity since I have assumed that a cost of $f^*(k)$ is necessary to keep prices constant. If instead, as in Ball and Romer (1986b) a cost of only $f(k)$ is required, excess rigidity becomes more likely. As noted in Ball and Romer (1987) the welfare properties of the model depend on the choice of equilibrium.

This dependence might be even more severe if one felt the cost of changing prices represents mainly a loss in customer goodwill. Then the loss in goodwill to any single firm could be much larger if it changes its prices alone than if all firms change theirs as well. In other words, the cost from having all firms change their prices could be much lower than the sum of individual costs of changing prices. In this case equilibria with rigid prices would tend to be more inefficient than equilibria with more flexible prices. As we will see in the next section, once dynamics

are explicitly recognized the choice of equilibrium determines not only welfare but also the qualitative features of the model.

Before closing this section, it is worth noting that the expression (11) for the welfare cost of fluctuations is valid both under monopoly and competition. One interesting feature of (11) is that the cost of k percent fluctuations is bigger under competition (when L is one) than under monopoly. Because the relevant part of the utility function has constant relative risk aversion, individuals are willing to give up a certain fraction of their consumption to eliminate k percent fluctuations. Such a constant fraction represents more consumption when output is high, as under competition.

7. Dynamic Models of Costly Price Adjustment

Ss Rules The two-period model obviously fails to capture an important feature of economies with rigid prices. In the presence of costs of changing prices the price that is inherited from the past is not necessarily the price that was optimal just before the latest change in the money supply. This has the important consequence that when prices start far out of line, even a small change in the money supply can get firms over the brink and induce them to change prices. To take this into account, a model is needed in which the money supply evolves over time and firms adopt optimal dynamic strategies. For the case of fixed costs of changing prices, a general model of this type has yet to be developed. Moreover the multiplicity of equilibria that plagues even the simple model of section 4 makes it likely that such a model will be badly behaved.¹⁷ One case for which equilibria have been studied (in Rotemberg 1983 and Parkin 1986) is the case of constant rate of monetary growth.

Sheshinski and Weiss (1977) consider a firm for whom the optimal price in the absence of costs of changing prices P_i^* grows at a constant rate. They prove that, in the presence of fixed costs of changing prices, the firm will follow an *Ss* pricing policy. Each price change leads to a price equal to S times P^* . The price is then kept constant until it equals s times P^* . At this point the price is changed again. Since P_i^* grows at a constant rate, the time it takes for a constant price to decrease from SP^* to sP^* , that is, the time during which the price is fixed, is constant as well.

17. Indeed, Gertner (1986) shows that in a simple dynamic model with fixed costs of changing prices the multiplicity of equilibria makes it possible to support the collusive outcome in an oligopoly. Deviations from this equilibrium are deterred with the credible threat of moving to another equilibrium.

Now suppose that the money supply grows at the rate g and that the price level also happens to grow at the rate g . Then a firm with a demand function given by (6) would see its desired price growing at the rate g . It would thus choose to keep its price constant for a period of length T and change its price by gT every time it changes its price. Rotemberg (1983) assumes that there is a continuum of firms with such demand functions and that they start off uniformly distributed over the time of their last price change. It is shown that in this case, there is an equilibrium in which the price level (defined as the unweighted geometric average of the prices of different firms) does indeed grow at the rate g . The reason for this is that over any interval of length τ , a fraction τ/T of firms change their prices by gT so that prices rise on average by $g\tau$. Thus aggregate output does not vary over time.

In this equilibrium the distribution of the ratio of actual prices to P_t^* is uniform with boundaries S and s . As Caplin and Spulber (1986) have emphasized, the distribution of real prices is time-invariant as well. This fact permits a different interpretation (which I will refer to as interpretation B) of the constancy of the rate of growth of the price level. In an interval of length τ , P^* rises by τg . This means that a fraction $\tau g/(S - s)$ of firms find their price out of line and adjust their price by $(S - s)$. Thus the average price rises by τg .¹⁸

It is worth noting that if, as in Sheshinski and Weiss (1977) or Rotemberg (1983), the Ss rule is due to fixed costs of changing prices, an increase in the rate of inflation increases the size of individual price changes. More generally, the only case for which it is known that a constant Ss rule is optimal is the case of a constant rate of increase of P^* .

Nonetheless, Caplin and Spulber (1986) assume that S and s are fixed independently of the stochastic nature of the economy. One defense for this approach is that, once again, the costs to firms from fixing these variables is small in a wide range of circumstances. Still, it is difficult to see exactly what form of "near-rationality" leads to a constant Ss rule.¹⁹ One implication of constant Ss rules is that price changes are all of equal size when measured in percentage terms. This obviously rules out the possibility that the prices of some products sometimes fall and other times rise. This is counterfactual for many products. Nonetheless Cecchetti's (1986) study shows that in the United States the average percent change

18. This is the same interpretation as above once it is recognized that with constant inflation $(S-s)$ equals gT .

19. One possibility is that, for costs of changing prices alone, firms would keep their prices fixed for even longer intervals and thus have even bigger price changes whenever they choose to change their price. The firms might then be forced into a constant S, s rule if customers found nominal increases of more than $(S-s)$ percent intolerable.

of individual magazines' prices in the more inflationary 1970s is not appreciably different from the average for the 1960s. On the other hand, in their study of the prices of Israeli noodles, Sheshinski, Tischler, and Weiss (1981) report price increases that are on average more than twice as high in the more inflationary period 1974–1978 than in the period 1965–1973.

The implications of a constant Ss rule together with what appears like a technical assumption that P^* have a monotone continuous sample path are nothing short of startling. Suppose that firms start with their prices uniformly distributed over the interval $[SP^*, sP^*]$. Assume that P^* rises, over some interval τ by v . Then a fraction $v/(S - s)$ of firms adjust their price by $(S - s)$ so that the price level rises by v . This is just a rewording of the interpretation B given above. It means that, if the money supply rises by v over an interval, there is an equilibrium in which the price level does as well even though individual prices are rigid. The requirement that sample paths be monotone and continuous is necessary to ensure that the distribution of prices remains uniform after all changes in the money supply.

What the Caplin and Spulber theory obviously misses is the effect of a monetary "shock." By a shock, I mean a very short period of time during which money goes up very fast. If it happens so fast that no prices adjust in the interim, the adjustment of prices after the monetary shock leaves a distribution of prices with an atom at SP^* ; it is no longer uniform. A different possibility is that while the money supply is temporarily growing fast some firms are adjusting their prices as well. These firms, if they are rational, ought to anticipate that prices will grow relatively fast in the near future. They should thus increase their own prices by more than $(S - s)$ and thus abandon the Ss rule. This is likely to reintroduce nonneutralities.

Another way of seeing how optimal price adjustment rules create non-neutralities is to consider the partial equilibrium story of Tsiddon (1986). Tsiddon assumes that the economy is initially in a steady state in which P^* is growing at the rate g and firms are following the optimal Ss rule. He then assumes that, without any jump in P^* , the rate of growth of P^* unexpectedly shifts to a lower level g' . This lower rate of growth is then expected to prevail forever. Firms must therefore revise their optimal Ss rule according to the formulae in Sheshinski and Weiss. It is now optimal to have a smaller band of prices, a higher value, s' , for the low relative price that triggers a price change and a lower value, S' , for the price that is set when prices are changed.

The question is how the firms whose prices are between s and s' or between S and S' should respond. Clearly those who find themselves

with too low a price, that is, those between sP^* and $s'P^*$, are beyond the price that triggers a change; they should raise their price to $S'P^*$. Instead, those who find themselves with too high a price, that is, those between $S'P^*$ and SP^* , may choose to let their price be eroded by inflation. Even if they choose to adjust their price downward the downward adjustment ($S' - S$) is much smaller than the upward adjustment ($S' - s$) so that the price index rises on the instant in which the rate of growth of P^* falls. It is tempting to interpret this to mean that policies of disinflation promote upward revisions in prices that contribute to recessions. This interpretation is somewhat premature because P^* is not being determined in equilibrium. In particular, disinflationary episodes are likely to be accompanied by increases in the demand for money. Suppose the disinflation is attempted by simply reducing the rate of money growth. Then P^* would jump down as soon as money growth slows down, thereby reducing the incentive for prices to rise.²⁰

The great insight of Tsiddon's paper is that the optimal change of Ss rules that accompanies changes in monetary policy has, itself, aggregate consequences that are missed when the Ss rule is assumed to be constant. In spite of this, the paper by Caplin and Spulber (1986) is valuable for stressing that the pervasive rigidity of individual prices does not automatically imply that monetary injections raise output.

Another way of making this point is the following. With fixed costs of changing prices very small changes in the external environment can trigger very large price changes. This occurs because at the instant of price change the firm is almost indifferent between maintaining its current price for a while longer or incurring the cost of price change now and moving to a quite different price. This indifference can easily be broken. This means that it is possible to imagine distributions of prices such that a small increase in the money supply makes many firms increase their price a lot so that an increase in the money supply triggers a recession.²¹ The message from this line of thinking is that the *distribution* of prices is all-important when deciding whether an increase in the money supply is

20. See Dornbusch and Fischer (1986) for evidence that the major episodes of disinflation are accompanied by great surges in money demand. That these increases in money demand are generally not fully accommodated by expanded money supply can be seen from the high interest rates that tend to accompany disinflations.

21. It is actually quite easy to construct fully worked out examples in which monetary expansions reduce output in these models. Suppose the economy starts in a boom. Now consider a sudden increase in the money supply after which money will be constant. This tends to make all firms adjust their price to the new long-run optimal level. This brings output back to its normal level.

contractionary or expansionary.²² This is not the sort of variable that finds a natural home in popular Keynesian discussions.

Staggering The equilibrium with constant inflation considered in Rotemberg (1982) has both a constant S_s rule and intervals of constant length between price changes. Indeed, just as with constant S_s rules, keeping the interval between price changes constant is generally optimal only when P^* grows at a constant rate.²³ Yet there is a literature (Blanchard 1983, 1986a, Ball and Romer 1986a, Ball and Cecchetti 1986, Parkin 1986) which assumes that prices are set for intervals of constant length. This literature probably spawned under the influence of the earlier literature on rigid labor markets (Fischer 1977, Taylor 1980) in which contracts of constant length have descriptive appeal.

For prices, the notion that the interval between price changes is constant is clearly counterfactual. There is much anecdotal evidence and some hard evidence such as Cecchetti's that price changes are more frequent when inflation is high.²⁴ Thus *Reader's Digest* changed its newsstand price in both January 1974 and January 1975, while it kept its price constant between September 1957 and January 1967.

It is worth pointing out that the assumption of a constant interval between price changes makes the degree of price rigidity difficult to estimate with aggregate data. One possibility (Taylor 1980b) is to impose a certain length for this interval before estimation. In this case, knowledge of the discount rate used by firms completely determines the path of prices—there is nothing else to estimate. Alternatively, one can use the procedure of Christiano (1985) who estimates the model repeatedly, letting the period for which prices are fixed be different multiples of the sampling interval. Comparison of these different estimates is of course difficult.

If in addition to maintaining the assumption of constant intervals one imposes the condition that the number of firms that change their price in

22. This result is reminiscent of the importance of the cross-sectional distribution of money balances in general equilibrium models in which money is held because there is a fixed cost of converting bonds into money. See Grossman and Weiss (1983), Rotemberg (1984), Fusselman and Grossman (1986), Romer (1986).
23. It is possible to have i.i.d. demand shocks as well (as in Parkin 1986) only if price must be set before demand is observed. Conversely, if there is any stochastic element to demand that is observed before prices are set it will be generally optimal, at least for some realizations of demand, to incur the cost and change price.
24. Additional evidence against the hypothesis that the interval between price changes is constant is provided by Lieberman and Zilberfarb (1985) and Sheshinski, Tischler, and Weiss (1985).

any interval of length τ is constant, that is, that firms are uniformly distributed over the time they last changed their prices, some interesting dynamic responses of output to money emerge (see Blanchard 1983). In particular, under this "staggering" assumption, Blanchard shows that the length of time over which a particular once-and-for-all increase in money affects output can exceed the length of time for which individual prices are fixed.

One question that has emerged here is the following. Assuming firms maintain constant the intervals for which their prices are fixed, will their price changes be staggered or will firms change their prices at the same time, that is, will they be synchronized?²⁵ This question has been asked both of models of labor contracts (Fethke and Policano 1984, Matsukawa 1985) and of price setters (Parkin 1986, Ball and Romer 1986a). While the results differ in their specifics, there is a tendency for staggered and synchronized equilibria to coexist.

This coexistence can be seen most easily for the case of constant money growth whose staggered equilibrium is discussed in the previous section. I now consider synchronized equilibria. We saw in section 6 that if other firms raise their prices at a point in time the demand for all other firms rises. This increase in demand, in turn, raises the incentive to raise prices. This means that if all other firms are changing their prices together very often, each individual firm will want to do so as well. At this equilibrium the only reason a firm changes its price is because others do so. The firm thus has no desire to change its price between the price changes of others. This lack of desire to deviate from the equilibrium is what brings about its existence. Moreover, the logic of these synchronized equilibria is such that there are several of these equilibria. These differ by the length of time during which all prices are constant.²⁶ This means in particular that there is no reason for synchronized equilibria to be periodic.

The key difference between synchronized and staggered equilibria is that in the former both the price level and output jump around discontinuously. This lacks descriptive appeal. Such jumps would presumably be reported by newspapers. Still, in both of these equilibria, as long as the interval between price changes is constant, monetary changes generally affect output. In the staggered setting they do so because only some firms change prices. In synchronized equilibria they do so because most of the time all prices are constant.

The authors mentioned above have tried to derive conditions under

25. This synchronization is called *bunching* by Parkin (1986).

26. See Ball and Romer (1987).

which either the staggered or the synchronized equilibria disappear. One possible criticism of the staggered equilibrium is that it is unstable. Nothing brings back the uniform distribution if it is ever disturbed. Yet there are a myriad modifications of the model that ensure stability. These include the presence of idiosyncratic shocks (Ball and Romer 1986a), small differences in the S_s policies of different firms (Caplin and Spulber 1986), as well as the randomization that is optimal to foil speculators (Benabou 1986a).

There are several ways of ruling out perfectly synchronized equilibria. The first is to assume that there are firm-specific shocks (Ball and Romer 1986a). Yet even in the presence of such shocks there can be differing degrees of bunching after a monetary shock. The second is to assume the government would rapidly reduce the money supply if all prices were to increase together (Parkin 1986). The third is to assume that when another firm raises its price the demand for one's own product actually falls. This is impossible with the cash-in-advance constraint used here. However, Blanchard (1987) proves it to be possible if the elasticity of a firm's demand with respect to real money balances exceeds that with respect to the firm's price. Finally, Ball and Cecchetti (1986) argue that firms want to learn about their optimal price from their competitors' price. They may thus wait until others have changed their price. In continuous time this would lead to a classic war of attrition.²⁷ Moreover, unless learning about prices charged by others takes time, price changes would still occur in spurts.

Quadratic Costs of Changing Prices The difficulties with the fixed costs of changing prices model in a dynamic setting are akin to those of investment models with fixed costs of investment. If anything they are more severe because of the induced multiplicity of equilibria. The standard solution in the investment literature is to pretend that the costs of investment are convex. The justification for this, that it is easier to absorb new capacity into the firm at a slow rate, flies in the face of the lumpiness of actual investment projects. The reason such models survive professional scrutiny is that whatever the weakness of the assumption one can actually solve them and obtain aggregate investment equations in a form suitable for estimation.

A similar reasoning might apply to costs of changing prices. Here too one could in principle argue that the cost of increasing prices is the cost of upset customers and that customers might be more than propor-

27. See Fudenberg and Tirole (1986) and the references cited therein.

tionately upset with larger price changes.²⁸ Again one would have to confront the fact that individual price adjustment is lumpy. Here this observation is more damaging, because the main reason for taking models of costly price adjustment seriously is the fact that individual prices seem sticky.

In a series of papers (Rotemberg 1982a, 1982b) I have nonetheless pursued the implications of quadratic costs of changing prices. I give some further justifications for their use after I draw out their implications.

These models start from the maximization of firm profits subject to the demand given by (6) and the technology (8). This leads to a path of prices P^* (given by (9)) which the firm would charge in the absence of costs of changing prices. As argued above, it is then possible to take a quadratic approximation of the firm's profits around P^* . Rotemberg (1982a) then postulates that firm i minimizes:

$$E_t \sum_{j \geq 0} \rho^j [(p_{it+j} - p_{it+j}^*)^2 + c(p_{it+j} - p_{it+j-1})^2] \quad (14)$$

where E_t takes expectations at t , lower-case letters are the logarithms of the respective upper-case letters and c is a parameter which is high if the cost of changing prices is high. The solution to this problem is

$$p_{it} = \alpha p_{it-1} + [(1 - \alpha)(\delta - 1)/\delta] \sum_{j \geq 0} (1/\delta)^j E_t p_{it+j}^* \quad (15)$$

where α and δ are related to ρ and c ; the former is smaller than one while the latter is larger than one. This solution has a natural interpretation. Because the costs of increasing price are convex, firms opt to change their prices slowly. Prices are a weighted average of past prices and p^* s. Because it will also be costly to change prices in the future, future p^* s are taken into account as well.

Aggregation of (15) across firms, which is trivial once symmetry is recognized, gives rise to an equation analogous to (15) in which the current price level is a weighted average of the past price level and present and future levels of the money supply. This dependence of current prices on the future is, of course, common to all models in which rational firms have rigid prices.

The main advantage of this quadratic approach is that it leads to equations that can easily be estimated. Rotemberg (1982b) applies it to aggregate U.S. data while Rotemberg and Giovannini (1986) apply it si-

28. As Cecchetti points out, this may be responsible for the fact that the size of magazine price changes does not rise appreciably in the 1970s.

multaneously to German prices and the dollar/DM exchange rate. The model tracks fairly well the behavior of prices and exchange rates.²⁹

The obvious implication of this model is that sudden increases in the money supply translate themselves rather slowly into price increases, so they raise output. This brings back the issue of how such a model, even when only applied to aggregate data, can be consistent with the sharp and sporadic price increases observed in micro data. The obvious answer to this is that models of this type capture the existence of some firms that delay their price changes relative to the underlying fundamentals.

One model of delay whose equilibrium looks identical to the aggregate version of (15) is presented in Calvo (1983). He assumes that firms have an exogenously given probability π of changing their price in any particular time period.³⁰ This probability is presumably chosen optimally as well but Calvo does not allow it to vary over time. An optimizing firm that changes its price and minimizes (14) with c set equal to zero will thus charge a price Z_t which is a weighted sum of current and expected future p^* s:

$$Z_t = (1 - (1 - \pi)\rho) \left\{ \sum_{j=0}^{\infty} [(1 - \pi)\rho]^j p_{t+j}^* \right\}$$

The future p^* s are less important the higher π is and the higher the rate at which the future is discounted. The logarithm of the price level is then a weighted average of the logarithm of the past price level (with weight $(1 - \pi)$) and Z (with weight π):

$$p_t = (1 - \pi)p_{t-1} + \pi(1 - (1 - \pi)\rho) \left\{ \sum_{j=0}^{\infty} [1 - \pi]^j p_{t+j}^* \right\}$$

which is indistinguishable from the aggregate version of (15).³¹ Thus the estimates in Rotemberg's (1982) favorite specification imply that in the postwar United States about 8 percent of prices are adjusted every quarter; the mean time between price adjustments is about three years. Instead, the estimates of Rotemberg and Giovannini (1986) imply that between 1974 and 1982 the average time between price adjustments in Germany was only about twelve months.

29. It is statistically rejected when required to fit all aggregate output dynamics; the demand equation given by aggregating (6) is too simple.

30. Calvo's model is in continuous time but the basic structure is the same as the discrete time model presented below.

31. This argument may imply that other solutions to quadratic optimization problems (such as those for investment, labor demand, or inventory accumulation) are also intimately linked to the solution of problems with random delays.

This probabilistic interpretation of an equation such as (15) has both strengths and weaknesses. The Calvo model has the advantage that individual price changes are large while the price level adjusts sluggishly. Yet the observation that price changes are more common in periods of high inflation is inconsistent with a constant probability of changing prices. The model also fails to explain why price changes are stochastic.

A partial equilibrium reason for randomizing the timing of price changes is given in Benabou (1986a). He considers demand functions for goods that are storable. Firms for whom the date of price change is known will then be subject to speculative attacks; individuals will hoard their goods just before the price rises. There is then no pure strategy equilibrium to the game between price setters with constant costs of changing prices and speculators. The solution is for the price change to occur at a random date.

A different reason for delay and possibly even for somewhat random delay is that there are costs to gathering information and costs of changing prices. By costs of gathering information I have in mind, as do Rotemberg and Saloner (1986), that the firm does not know its optimal price without spending resources. If only these costs are present, prices should change often since firms would charge their best guess of the optimal price. Suppose, however, that these costs coexist with costs of changing prices. Then firms will keep prices constant for some time, occasionally investigate the optimal price, and only then change prices. Suppose then that the money supply rises by a known amount. Not all firms will respond by investigating their optimal price and some firms will react only with delay. This delay would obviously become somewhat stochastic if information of varying quality is observed randomly. It is worth stressing, however, that at this point we lack even partial equilibrium models in which costs of gathering information and of price adjustment are important.

8. *Multiple Equilibria*

As we have seen, small costs of changing prices generate multiple equilibria. Yet, at least in the two-period model and for a specified range of monetary growth, all the equilibria feature price rigidity, and this price rigidity is responsible for the Keynesian outcomes. In this section I review a literature in which the multiplicity of equilibria can, by itself, be viewed as Keynesian. The connection between multiple equilibria and Keynesian economics is clearly articulated in Bryant's (1983) model of effort in teams and in Woodford (1986). The connection is apparent once one recognizes that the equilibria differ by the (correct) beliefs agents

have about their environment. These beliefs can be thought of as animal spirits.

The great strength of these models is that they illustrate the possibility that employment can fall below full employment without any adverse shift in technology or tastes. At least some of these models find two other features of Keynesian thinking more difficult to accommodate. The first is that large fluctuations in output are accompanied by long periods in which individual prices are rigid. The second is that money affects output predictably. In these models, for changes in money to have an effect they must switch the economy from one equilibrium to another. They must thus affect agents' beliefs about their environment in pre-specified ways. Another way of posing this problem is that one can only know how money will affect output once a particular equilibrium is selected. Yet the models provide little guidance for this selection.

Somewhat arbitrarily, I divide this literature on multiple equilibria in three strands. The first strand (embodied in Stiglitz 1979, 1984 and Woglom 1982) is closely connected to price rigidity and to the models of sections 4–7. It starts from partial equilibrium models with multiple equilibria. Then a change in the money supply can be consistent with unchanged nominal prices. The second strand considers fairly classical models but modifies the institutions of price setting, wage setting, hiring of labor and purchasing of commodities and thereby achieves multiple equilibria. Models of this type include those of Diamond (1982), Weitzman (1982), Cooper and John (1985) and Roberts (1986). The third strand focuses on the overlapping generations model. As Kehoe and Levine (1985) demonstrate, this model can possess a great many equilibria. These have been given Keynesian interpretations by Geanakoplos and Polemarchakis (1986) and Woodford (1986). I consider these ideas in turn.

Partial Equilibrium Multiplicities Consider an oligopoly whose member firms live forever and produce a homogeneous good subject to constant marginal costs. One equilibrium is the usual Bertrand one of charging marginal cost. Yet there are also equilibria with higher prices. These occur, as in Friedman (1971), because each firm knows that any undercutting of the implicitly agreed-upon price will lead to a price war in which price reverts to marginal cost. Indeed, if there are not too many firms, any price between the monopoly price and marginal cost constitutes such an equilibrium. Thus, as Stiglitz (1984) notes, there exists in particular an equilibrium in which nominal prices stay constant over some period of time. This equilibrium is not particularly desirable to the oligopolists except insofar as it facilitates coordination. Rotemberg and

Saloner (1986b) argue that when a collusive equilibrium of this type is maintained through price leadership (that is, by letting one firm choose the price for all firms) then the rigidity of prices is actually useful to the oligopoly. The advantage of rigidity is that it prevents the leader from exploiting the follower by varying the price to its own advantage in response to relative shifts in demand.

Similar multiplicities arise in Stiglitz's (1979, 1985) model of search. He considers equilibria in which all firms charge the same price. Suppose one firm decides to charge a different price. A firm that chooses to lower its price attracts very few customers because search is costly. (Indeed, it attracts only those people with very low search costs who are willing to search repeatedly in order to find the one cheap store.) If the firm chooses instead to raise its price it will lose many more customers since they are all sure that they can obtain a lower price by going to one additional store. Thus the demand curve facing an individual firm has a kink at the price charged by all the other firms. This tends to make this price optimal. Hence there are multiple equilibria and it is possible (and perhaps even natural) to keep nominal prices unchanged even when the money supply rises.³²

It is worth briefly drawing the contrast between this search story and costs of changing prices, particularly when these are viewed as the cost of upset customers.³³ The two are obviously related since the search model implies that it is costly to charge a price different from that of other firms. In the equilibria in which others keep their prices constant, this is the same as making prices costly to change. The key difference between the two models is that in the search model it is worth matching other firms' prices even if these are extremely volatile. Instead, the models with costs of changing prices assume that in an environment with very volatile prices, a single firm would benefit, that is, it would please its customers, if it stabilizes its own.

Two observations should be made about the multiplicities in these models. The first is that these models are as consistent with excessively rigid as with excessively volatile prices. In these models, if people believe prices will change, such a change will take place even if nothing fundamental changes. The second is that these stories rely critically on

32. The macroeconomic multiplicities that result from the story in Stiglitz (1979) can be found in Woglom (1982).

33. The model of Benabou (1986b) has both search costs and costs of changing prices although here these are best interpreted as administrative costs. With both these costs and positive inflation he shows that there exists an equilibrium in which prices are dispersed even when the corresponding static model has as its unique equilibrium all firms charging the monopoly price.

competition among firms to generate the multiplicities that are associated with price rigidity. Thus they are hard to reconcile with the evidence that monopolies keep their prices more rigid than oligopolies.

Multiplicities Due to Non-Walrasian Institutions In this section, I survey models that abandon the Walrasian auctioneer in both goods and labor markets. In these models the institutional framework as well as the timing of production, pricing, and exchange are modeled explicitly. Diamond (1982) presents a search model in which opportunities to produce arrive randomly. These opportunities differ in the amount of effort needed to obtain one unit of output. Having produced, individuals must then search for a trading partner, that is, another person who has also produced. Only when they trade the fruits of their production with one another can they consume. In this model there are different equilibria corresponding to different effort levels individuals are willing to incur. If others are willing to incur much effort, there are many trading opportunities (search for trading partners will be short) and expending much effort becomes worthwhile. One interpretation for the equilibria with low levels of output is that individuals work little because, even though measured real wages are high, individuals fear that they will find it difficult to purchase useful goods and services. This lacks descriptive power for Western countries. Another interpretation is that firms are not employing workers because they fear it will be difficult to sell the output. Under this interpretation the lack of a mechanism whereby the unemployed workers might be able to bid down wages is a drawback.

Such a mechanism is considered in the related model of Roberts (1986). This model deviates less from the Arrow-Debreu framework in that transactions are not time-consuming. It differs from that framework only in the sequence of pricing and purchasing decisions. Initially, firms announce prices and wages. Workers then offer their labor. Next, firms decide whom to hire and, finally, output is sold.³⁴ Even when Roberts restricts himself to equilibria in which prices and wages equal their Walrasian levels, output can be anywhere between zero and the Walrasian level. What is most remarkable about this model is that equilibria with low output have true involuntary unemployment; workers who do not work envy those who do. These equilibria can be understood as follows. As in the second interpretation of the Diamond model, if other firms hire

34. This describes timing in the second of Roberts's models. In his first model workers must simultaneously announce their offers of labor and purchases of goods. As a result individuals refuse to purchase from low-priced firms if they fear that these will be unable to attract the necessary workers.

few workers, few individuals will be able to afford one's own product and therefore one's demand will be low. The natural response to such low expected demand is to hire few workers oneself.

The key question in the model is why, if there are unemployed workers, there is no pressure for wages and prices to fall. After all, a firm that lowers its price will attract a great many customers, while one that lowers its wage will still be offering appealing positions to the otherwise unemployed. This rather old question is given a theoretically very ingenious answer by Roberts. A firm that contemplates deviating by lowering its price and wage is "threatened" with a drastic change in the equilibrium if it does so.

As I mentioned, in the Roberts model there is also an equilibrium with full employment. Moreover, since several firms produce any given good with a constant-returns-to-scale technology, the full employment equilibrium is feasible, even leaving out one of the firms. Thus Roberts (1986) assumes that if any firm lowers its wage, the equilibrium immediately shifts to the one with full employment where this particular firm is inactive. This leaves firms indifferent between lowering their wage (and thereby eliminating all unemployment) and keeping the same prices and wages as all the other firms.

Several features of this construction deserve to be noted. First, to support the Walrasian equilibrium in a non-Walrasian model it is obviously necessary that, if a firm producing good j lowers its wages below the Walrasian level, those employed by the firm be employable (at Walrasian wages) in another firm producing good j . Roberts, however, requires one order of magnitude more information and coordination. When a firm producing j lowers its wage, firms producing goods other than j must react dramatically as well. Second, the unemployment equilibria are not robust to the introduction of a single civic-minded firm.³⁵ Such a firm would prefer to eliminate unemployment if it could do so without affecting profits. In the model, this can be achieved by lowering wages and prices. These criticisms are a little off the mark—there may be other ways of supporting equilibria with low output. The model should be seen as an important warning. Once the Walrasian auctioneer is abandoned, care must be taken to ensure that equilibria are unique if one seeks to perform standard comparative statics. This warning seems particularly pertinent for the models surveyed in section 4 which lack auctioneers in both goods and labor markets.³⁶

35. Joe Kennedy's Citizens Energy Corporation is ruled out.

36. The uniqueness of the model fleshed out in section 4 is due to the presence of a Walrasian auctioneer who clears the labor market.

Multiplicities in the Overlapping Generation Model The standard overlapping generations models differs considerably from the other models considered in this survey. In particular, prices are set by a Walrasian auctioneer and dynamics are of paramount importance. Still, as Kehoe and Levine (1985) have shown, if there are several goods (or if agents live for more than two periods), the model can have continua of perfect foresight equilibria all of which converge to a steady state. This multiplicity can be thought of as follows. In each period, there is a Walrasian auctioneer who clears current markets for goods and labor. If agents have different expectations about prices tomorrow, the prices that clear markets today will differ as well. There are multiple equilibria because different prices today are supported by different (correct) beliefs about prices in the future. It is thus natural to index these equilibria by the beliefs about the future that they require.

Geanakoplos and Polemarchakis (1986) consider a model whose multiplicities can be indexed by two numbers. They associate one number with expected future output and the other with the *current* nominal wage. Both these numbers are picked outside the model. If an increase in the money supply leaves these two variables unaffected, it has the usual Keynesian effects. One great advantage of their model is that it is amenable to analysis using the standard IS-LM graphical apparatus. Note that in this model, prices are not rigid per se. Nominal wages stay constant when the money supply changes because expectations of future variables, including future wages, have changed in just the right way. For this story of price rigidity to be convincing, an intuitive justification for this response of expectations about the future will have to be provided.

9. Conclusions

In these conclusions, I give my own views about the strengths and weaknesses of the recent crop of Keynesian microfoundations. I must start by noting that I view the existence of multiple equilibria as a weakness in any economic model. First, if many things can happen the models are much more difficult to reject. Indeed whether it is even possible to reject models like those in section 8 is an open question. Second, and perhaps more important, when there are multiple equilibria it is impossible to know how the economy will react to any particular government policy.

Therefore I view the models surveyed in section 8 as incomplete. All three types of models suggest literally that any level of output is an equilibrium. My hope is that ways will be found to rule out all but a set of locally unique equilibria in these models. For instance, in the case of supergames the equilibrium with the highest profits for the oligopoly,

at least when the firms are symmetric, appears natural. If this locally unique set of equilibria turns out to involve expansions in GNP in response to increases in the money supply, the models would obviously provide more solid Keynesian microfoundations.

Unfortunately this criticism also applies at least to some extent to models in which there are costs of changing prices. This is true even though the obvious purpose of these models is to provide unique Keynesian equilibria. The reason, as I showed, is that any increase in prices by other firms creates an additional incentive to raise one's own price. There thus tend to be equilibria with varying degrees of price rigidity. Moreover these equilibria can be qualitatively quite different; in some the price level evolves smoothly, in others it is subject to large jumps. Perhaps most dramatically, whenever prices are different from optimal prices there exists the possibility that prices will change simply because they are expected to change. Here too it would be good to know how to select equilibria.

Yet there is perhaps another way of thinking of these models. Suppose the evolution of money can be characterized by a stationary stochastic process. Then an equilibrium can be thought of as a stochastic process for prices that dictates the prices to every conceivable eventuality. As mentioned above, there are many such equilibria. In spite of this multiplicity, the model would still provide rather strong Keynesian microfoundations if every such equilibrium exhibited positive correlation between monetary surprises and output. This would still leave open the question of how to validate the model empirically. Tests would have to be designed that examine only the features which are common across equilibria.

Even this is not enough. There must also be some equilibrium consistent with both the aggregate price data (prices respond slowly to money) and the individual price data (inflation makes price changes larger and more frequent). As is clear from the discussion in section 7, no existing model satisfies these dual requirements. While some of the existing models can account for some features of the aggregate data, they do not explain the individual data.

The task proposed in the previous paragraphs is daunting. Yet, there are two grounds for feeling it will ultimately be completed. The first is the pervasive microeconomic evidence that firms perceive price changes as costly. In particular, the fact uncovered by Stigler that prices of monopolies are more rigid than prices of duopolies is, as Rotemberg and Saloner (1986) show, an almost natural consequence of fixed costs of changing prices. This is most easily seen when a decrease in cost occurs.

Then, one reason for a single firm in an oligopoly to lower its prices is its desire to undercut its competitors and take customers from them. This is an incentive to lower prices which monopolists obviously do not perceive. So the equilibrium with rigid prices is more easily disturbed in an oligopoly. Oligopolists are more likely to change prices when costs change or, as Rotemberg and Saloner (1986) show, even when there is aggregate inflation.

The second reason for optimism is that the papers I have surveyed in sections 5–7, while not the final word, have clarified greatly which features of price rigidity lead to Keynesian effects and which do not. Thus we have learned from Caplin and Spulber (1986) and Tsiddon (1986) that it is the fact that firms will optimally change both the lowest relative price they are willing to tolerate and the relative price they set when they change their price that is responsible for monetary nonneutralities. Similarly, the staggering literature teaches us one important form the multiplicities of the static model take when firms are embedded in a dynamic context. It also shows how these multiplicities can be consistent with having each equilibrium be subject to monetary nonneutralities. Finally the models of Rotemberg and Calvo show how aggregate dynamics may be easier to understand and estimate when individual decisions involve some stochastic elements.

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Comment

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When I was growing up, I was taught that the inflexibility of nominal wages is the root of all evil, or at least the cause of most business cycles. A contraction in the money supply leads to a reduction in aggregate demand and thus in prices. But since nominal wages are sticky, the real wage rises, moving the economy along the labor demand curve to lower employment and lower output.

One of the difficult things about growing up is finding out that not everyone shares our beliefs. That realization naturally leads to the next: perhaps the beliefs we are taught by our parents (in my case, Alan Blinder and Stanley Fischer) are not correct. It is this loss of innocence that led to my interest in the topic surveyed so astutely here by Julio Rotemberg, "The New Keynesian Microfoundations."

Problems with the Old Keynesian View There are at least three major, well-known problems with the view that nominal wage inflexibility is crucial to understanding economic fluctuations. The first is that if nominal wage contracts are responsible for large and inefficient fluctuations in employment, then rational workers should not agree to them. No amount of high-powered theory is likely to explain the sort of nominal wage rigidity on which textbook Keynesian models rely. Ultimately, assuming such nominal wage rigidity is tantamount to assuming substantial departures from rationality.

The second problem with relying on the inflexibility of nominal wages is that it is not obvious that observed nominal wages directly affect employment decisions. If these wages are merely installment payments within the context of a long-term employment relationship, then the current level of wages need not be a meaningful relative price. In other words, the reason rational workers agree to fixing nominal wages may be that these wages are not important for determining employment.

The final, and perhaps most serious, problem with the unadorned nominal wage story is that real wages do not move over the business cycle as the theory predicts. According to this view, firms lay off workers in recessions because labor costs are too high; prices have fallen but wages have not. In practice, however, real wages do not seem higher in

I am grateful to David Weil for research assistance.

recessions. To the extent that real wages appear cyclical at all, they seem procyclical.

The New Keynesian View Growing dissatisfaction with the textbook Keynesian view and the emphasis on wages has turned attention to the goods market. According to the new Keynesian view, firms lay off workers in recessions not because labor costs are too high, but because sales are too low. This new view seems to avoid the three problems that plagued the textbook Keynesian story.

First, it does not require gross departures from rationality. Firms can of course increase sales by cutting prices. But the benefit of doing so is second-order; a small cost of price adjustment (that is, a small “menu” cost) can make it privately rational for a firm not to change its price. Nonetheless, the cost to society of this price rigidity is first-order. This disparity between private and social objectives arises because of the pre-existing distortion of noncompetitive pricing.

Second, one cannot argue that the rigidity of observed prices is irrelevant to the allocation of resources. As Rotemberg points out, the fact that magazine prices move infrequently cannot be dismissed. These prices represent transactions prices, not installment payments.

Finally, the cyclical behavior of the real wage is not a problem for the new Keynesian view. Once aggregate supply is derived from inflexible goods prices rather than from inflexible nominal wages, real wages can easily be acyclical or procyclical (Blinder and Mankiw 1984).

Most of the work on the new Keynesian microfoundations has been theoretical. I share Rotemberg’s ambivalent assessment of the current state of this literature. While we have made substantial progress in the past few years, the models remain highly stylized. We are still far from having solved for the general equilibrium of a dynamic economy in which the money supply is stochastic and in which firms face menu costs when they increase or decrease their posted prices. Yet the models that we have solved are useful—they can give us insight into what the solution to the more general model will look like.

Some Evidence Since I agree with most of what Rotemberg writes in this article, I will turn my attention to an issue Rotemberg does not address: testing. If the new Keynesian economics is to succeed, it must persuade nonbelievers, perhaps even those not brought up as Keynesians. It must have implications that are different from other prominent theories of the business cycle, and these implications must be empirically supported.

One implication of menu cost theories is that aggregate demand should

have smaller real effects in high inflation economies. There are two possible arguments for this claim.

The first and more obvious argument is that the higher the rate of inflation, the more frequent are price changes. In a high-inflation economy, a shock to aggregate demand can be quickly incorporated into prices, since prices would be changing anyway. In a low-inflation economy, prices adjust less often, so changes in aggregate demand have larger real effects.

The second argument is based on the neutrality proposition of Caplin and Spulber (1987). Caplin and Spulber show that money can be neutral even in the presence of menu costs. In response to a monetary shock, some firms do not adjust their prices, but other firms adjust their prices by a large amount. The net effect can be a proportionate change in the aggregate price level, resulting in monetary neutrality.

As Rotemberg makes clear, the Caplin-Spulber result requires that prices move monotonically over time.¹ If both price increases and price decreases are possible, then a simple (*s*,*S*) rule is not appropriate. In a high-inflation economy, this qualification may not be important, and Caplin-Spulber neutrality may be approximately correct. But in a low-inflation economy, price decreases on some products will be common, so Caplin-Spulber neutrality will not apply.

Regardless of the precise argument, the empirical claim is probably a robust implication of menu cost models: the higher the rate of inflation, the smaller the real effects of aggregate demand. As a natural test of this claim, let me suggest a procedure that is analogous to that followed by Lucas in his famous "International Evidence" paper. The reduced-form model Lucas estimated for each country is

$$y_t = \text{constant} + \pi \Delta x_t + \lambda y_{t-1}$$

where y_t is detrended log real GNP, and Δx_t is the change in the log of nominal GNP.

Lucas's parameter π is a natural measure of the real effects of aggregate demand. If $\pi = 1$, then a shock to aggregate demand has a proportionate impact on output. If $\pi = 0$, then a shock to aggregate demand has no impact on output; it is fully reflected in prices.

Column 1 of table 1 presents a cross-sectional regression of Lucas's estimate for π for eighteen countries on each country's mean level of inflation. The results show a clear negative relationship, as predicted. If the

1. See also the discussion in Blanchard (1987).

Table 1 THE IMPACT OF INFLATION ON THE OUTPUT-INFLATION TRADEOFF

| Sample | (Dependent variable: Estimated tradeoff (π)) | | | | | | | |
|---|--|----------------|-------------------------|-------------------------|----------------|----------------|----------------|----------------|
| | (1) Lucas | (2) Alberro | (3) Lucas | (4) Alberro | (5) Lucas | (6) Alberro | (7) Lucas | (8) Alberro |
| | All countries | All countries | Low-inflation countries | Low-inflation countries | All countries | All countries | All countries | All countries |
| Number of observations | 18 | 49 | 16 | 42 | 18 | 49 | 18 | 49 |
| Constant | 0.66 (0.05) | 0.58 (0.05) | 0.69 (0.13) | 0.76 (0.07) | 0.68 (0.06) | 0.57 (0.04) | 0.64 (0.06) | 0.63 (0.05) |
| Mean inflation (Δp) | -3.3 (0.8) | -1.7 (0.4) | -4.4 (4.4) | -6.5 (1.8) | -2.1 (1.6) | -3.3 (0.8) | -2.5 (1.4) | -2.8 (0.6) |
| Standard deviation of aggregate demand ($\sigma(\Delta x)$) | | | | | -1.8 (2.0) | 1.9 (0.9) | | |
| Variance of aggregate demand ($\sigma^2(\Delta x)$) | | | | | | | -6.6 (8.7) | 2.5 (1.0) |
| \bar{R}^2 | 0.50 | 0.32 | 0.00 | 0.24 | 0.49 | 0.37 | 0.49 | 0.39 |
| s.e.e. | 0.18 | 0.26 | 0.18 | 0.25 | 0.18 | 0.25 | 0.18 | 0.25 |

Note: Standard errors are in parentheses.

mean level of inflation is zero, two-thirds of a shock to nominal GNP is in the first year reflected in real GNP and one-third is reflected in prices. If the mean level of inflation is 20 percent per year ($\Delta p = 0.2$), the predicted value of π is zero, implying money is neutral.

Column 2 presents the same cross-sectional regression using the estimates in a paper by Alberro (1981), who replicated Lucas's study using data from many more countries. The results again imply that aggregate demand has a smaller impact in high-inflation economies.

These results are not fully attributable to the inclusion of a few very high-inflation countries. The regressions in columns 3 and 4 include only those countries with mean inflation below 10 percent per year. In Lucas's sample, the estimate stays about the same, but the standard error increases substantially. In Alberro's sample, the coefficient on inflation remains statistically significant. The coefficient in column 4 is very different from the coefficient in column 2, however. This change in the coefficient is evidence against the linear specification, which of course cannot hold over all rates of inflation.²

The next step is to compare the "menu cost" explanation of the output-inflation tradeoff to Lucas's. Lucas emphasized the signal-extraction problem of private agents when they attempt to infer relative prices from observed nominal prices. In his model, a higher variability of aggregate demand implies a lower value of the tradeoff parameter π . Both Lucas and Alberro reported that more variability in aggregate demand is indeed associated with a lower value of π .

The regressions in columns 5 through 8 nest both hypotheses. In Lucas's sample, it is difficult to distinguish the two explanations. Neither the inflation coefficient nor the variability coefficient is individually significant, although the t-statistic on inflation is somewhat larger. In Alberro's sample, the inflation coefficient is highly significant, and the variability coefficient has the opposite sign from that predicted by Lucas.³ Once we correct for the mean level of inflation, there appears to be no evidence for Lucas's hypothesis that greater variability in aggregate demand lowers the impact of aggregate demand on output. Yet the output-inflation tradeoff is substantially affected by the mean level of inflation.⁴

I interpret the evidence in table 1 as supportive of many of the new

2. The square of the rate of inflation is statistically significant when added as an additional regressor to the equation including all countries.
3. If the square of the rate of inflation is included as an additional regressor, or if the sample is restricted to the low-inflation countries, the variability coefficient becomes negative and statistically insignificant while the inflation coefficient remains highly significant.
4. Kormendi and Mequire (1984) report that the real impact of monetary disturbances is not significantly related to the mean of monetary growth. Their result is not necessarily

Keynesian models surveyed by Rotemberg. This test is not without flaws, however. Lucas's procedure for estimating the key parameter π suffers from all of the problems of Phillips curve estimation, one of the foremost being the issue of simultaneity. Moreover, there are undoubtedly other explanations for the role of mean inflation in determining the output-inflation tradeoff.⁵ I hope this crude and preliminary evidence will stimulate others to do more definitive empirical work.

Let me close by thanking Rotemberg for a thorough and penetrating survey. I expect he will continue to see much work in this field, and Rotemberg has done an excellent job of identifying where the problems lie. I am among the shrinking majority who believe that monetary policy has real effects. For us, the new Keynesian microfoundations currently provide one of the most promising areas of research.

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Comment

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Julio Rotemberg defines Keynesian models to be ones for which nominal prices of goods and factors respond slowly to unexpected changes in the money supply and as a result there are associated changes in real output and employment. He defines microfoundations to mean models with

inconsistent with the finding reported here. Since the trend growth rates of output and velocity vary across countries, mean monetary growth is an imperfect proxy for mean inflation.

5. See, for example, Hercowitz (1983).

maximizing agents and consistent behavior of these economic actors—in other words, general equilibrium underpinnings.

His definition of a Keynesian model does not fit well with conventional labels. It implies that not only Klein, Modigliani, and Tobin are Keynesians but also Brunner, Meltzer, and Friedman. The latter do not consider themselves to be Keynesian economists, but perhaps they should. Their models are close in structure to the Keynesian macroeconometric models with the principal differences being the magnitudes of certain parameters and the number of equations. But are Lucas and Sargent Keynesians? They would be by Rotemberg's definition. Both have models in which unexpected changes in the money supply have real consequences. Lucas and Sargent (1981) have argued that their general equilibrium approach is fundamentally different and incompatible with the Keynesian macroeconometric model tradition that preceded it.

Rotemberg is not providing nor claiming to provide microfoundations for the Keynesian macroeconometric models. He, like Lucas and Sargent, is operating within the general equilibrium tradition as are Kydland, with real cash balances as an input into the household's production function and both Fischer and Taylor with their nominal wage contracting models. All have equilibrium models with the property that the operating characteristics of real output and employment are not invariant to the rules by which monetary policy is selected.

To summarize, I think that Rotemberg should not be using Keynesian models to mean any model for which monetary policy matters. Rather he should simply state that he is reviewing models in which resources are required to change prices. The reviewed research program is at a very early stage and thus my comments are directed at what needs to be done and not at what has been accomplished.

The Static Model A monopolistic competitive model is reviewed. My general equilibrium interpretation of the sometimes implicit underlying economic environment is as follows. There are two types of agents—entrepreneurs and workers. There is but one period with a sequencing of decisions within it. At the first stage the entrepreneurs, who each manage a technology having labor as the only input and a technology-specific output good, set a nominal price for their product. At the second stage the workers go to some central authority and receive a currency allocation. The amount received is a random variable. Here my interpretation may differ from that of Rotemberg. An alternative interpretation is that all workers expect this distribution to be a certain amount and are sure of this. In fact, the amount distributed may differ from the expected. I reject this interpretation because it is inconsistent with the gen-

eral equilibrium modeling paradigm espoused. I therefore assume the amount of currency distributed is a random variable and workers maximize expected utility.

One problem that is not explicitly addressed is the objective of the firm. Rotemberg does not specify who owns the technology. This matters if there are profits in equilibrium and with monopolistic competition typically there are profits. I assume the entrepreneur-managers of the technologies are the residual claimants and like the workers maximize expected utility and make purchases of goods.

At the third stage, the managers may at a resource cost alter their price, as the result of the currency distribution being different than expected. With this model there is a cash-in-advance as well as budget constraint facing the agents. Consequently, workers cannot use their wage payments to purchase consumption goods. They use their wage payments, which are in units of currency, to pay back the central authority at the final stage.

With this particular arrangement, multiple equilibria are possible. If no other entrepreneur changes its price, then it is sometimes optimal for a given firm to keep its price fixed. If on the other hand other firms all change their price by a certain nonzero amount, it may be optimal for this firm to change its price by the same amount. Provided the currency distribution to workers differ by a sufficiently large but not too large amount, there will be these two equilibria.

What Are the Facts Being Mimicked? The key aggregate fact reported is the response of a monetary innovation of a vector autoregression. Does this result deserve the status of a macroeconomic fact? I think not. The standard errors of the estimated responses are huge (see Runkle 1987) given the sample size employed. The results have not been shown to hold up over time and across countries. To name a few measurement problems the results are sensitive to the detrending procedure, the seasonal adjustment method employed, the time interval used, the smoothing coefficients selected, and the set of variables employed. Further, the validity of this measuring procedure has not been established. I suspect that virtually any monetary response pattern for money, GNP, and the price level can be generated as equilibrium responses for models with output determined by an exogenous endowment process.

Need for and Difficulty in Developing a Dynamic Model Even if one takes these VAR results seriously, which apparently Rotemberg does or he would not have used them as the prime evidence for the importance of

monetary shocks as a source of real fluctuations, his theory had better be dynamic and produce time-invariant stochastic processes as equilibrium, which VARs assume. This is particularly important, for his VAR result indicates most of the induced fluctuations are the result of the propagation of the innovations and not the innovations themselves. This leads me to the conclusion that the model must be dynamic with important intertemporal links.

Making the model dynamic is not a simple task. First one probably should have the entrepreneur-firm being subject to an idiosyncratic shock. All prices do not move together and a costly price change model with this feature is likely to behave very differently than one without it. In particular the problem of multiplicity of equilibria may disappear if there are idiosyncratic shocks. The effects of nonconvexities often are smoothed out at the aggregate level when there is heterogeneity even though discontinuities remain in individual responses (see Caplin 1985).

In making the model dynamic, a prime candidate for inclusion as a firm's state variable is the price it charged in the previous period. If there is heterogeneity, the economy's state must include the entire distribution of these prices. To date, we have not been very successful in analyzing such economies. Still another crucial extension is the inclusion of capital accumulation. A key business cycle fact is that investment is highly volatile. Capital accumulation surely plays an essential part in any theory of the propagation of monetary shocks.

Inconsistency of Models with Findings in Labor Economics A discipline of applied general equilibrium analysis is that key assumptions of the model cannot be inconsistent with other findings. A crucial feature of the costly price change model reviewed is that the labor supply function has positive slope with respect to the real wage and this slope must be large. If so, countries with high real wages should have high hours of employment per capita. If anything, richer countries have a lower average per capita time allocation to market activities.

Measurement Implied by This Theory How can the cost of changing prices be measured? Perhaps they have become larger or smaller over time or differ across nations. As the amount of fluctuations predicted by this theory depends in an important way on this parameter, these numbers along with measures of economic volatility would make possible the assessment of the quantitative importance of this factor. I have no answer to the question of how to measure these menu change costs, but these theories will never be taken seriously until an answer is provided.

A feature of the nominal wage contracting models of Fischer and Taylor is that their theory admits to micro measurement. In the United States we do see labor contracts with nominal wage contracting and with employers having the right to choose employment. It seems possible to use applied general equilibrium theory to estimate the quantitative magnitude of fluctuations induced by monetary shocks in environments with wage contracting arrangements similar to those used in the United States. The theory could also be used to derive the aggregate consequence of alternative contractual arrangements such as the once-a-year coordinated contracting in Japan. If theory found that the arrangement matters in a quantitatively important way, one could compare the costs and benefits of alternative arrangements for wage contracting. Then one might seek micro foundations for the particular wage setting arrangement selected.

Concluding Comments Contractual elements seem crucial to any theory that can be used to assess the implications of alternative monetary policy for the operating characteristics of real output and employment. Introducing these features into an applied general equilibrium model is a challenging task that is beyond our current capabilities. Rotemberg's article makes clear how little we have accomplished to date. Given the rapid advance in theory, I am confident that ten years from now we will have the tools to incorporate contractual arrangements into applied dynamic general equilibrium models and that there will be models that can be used to evaluate monetary policy rules.

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Discussion

Martin Bailey expressed skepticism about the importance of menu cost theory. Price stickiness is not very important in the U.S. economy; what is important is the unresponsiveness of prices to demand. He also pointed out that only a small fraction of wages are determined by formal labor contracts. Similar skepticism was expressed by Robert Barro, who noted

that the evidence for price rigidity comes from newspaper or vending machine prices, which are not important parts of the economy.

Assar Lindbeck too doubted the importance of menu costs. This is not the best foundation for Keynesian economics. He thought that insider-outsider models are more promising for explaining involuntary unemployment, which is the key characteristic of Keynesian equilibria.

Arnold Harberger thought that Rotemberg and the discussion treated the theory too literally. We are always oversimplifying, and the problem is to see which oversimplifications are most useful. For example, the notion that marginal cost and marginal revenue are equalized is not realistic, as vacant seats in a train or a restaurant show. Nevertheless, the assumption that marginal cost is equalized to marginal revenue gives us good insights. Rather than stressing the reality of a theory, we should carefully characterize its implications.

Olivier Blanchard reported some stylized facts about wage and price adjustment from his recent work. Nominal wages adjust fast to nominal prices, while nominal prices adjust slowly to nominal wages. This result holds for many countries. Adjustment of prices to wages is faster in disaggregated than in aggregate price equations.

The problem of multiple equilibria was discussed by Laurence Ball, who argued that they are not discouraging for Keynesians. If there are multiple equilibria, then policy may be used to make the economy move between equilibria. He also stressed that very small menu costs can produce large effects.

Although near-rational theory is useful, we should be careful about what it means not to adjust, commented Andrew Abel. Does it mean that price is not adjusted, or that quantity is not adjusted? Since something must change when demand shifts, we need a reasonable explanation of exactly which variables adjust.

Rudiger Dornbusch pointed out the accelerating adjustment of prices and wages in hyperinflation. For example, prices and wages are initially adjusted every twelve months. As inflation accelerates, contract length becomes six months, three months, one month, and finally every price and wage comes to be quoted in dollars. This phenomenon bears on the theory discussed by Rotemberg.

Stanley Fischer pointed out one difference between price and output decisions. The decision to change price has to be communicated to all of a firm's potential customers, whereas decisions on quantity changes are made within the firm and do not have to be communicated to customers. This may make the cost of changing price larger than that of changing output.

Different adjustment speeds among different prices were pointed out

by Robert Gordon. Some prices adjust rapidly and some do not. It will be useful to ask how and whether labor market and goods market structures contribute to explaining the differential adjustment speeds.

Bennett McCallum commented on the need to explain why prices are preset in nominal terms. Since we usually postulate that people care about real variables, we have little explanation for the use of nominal pricing. One idea is that while sellers are indifferent between setting prices in nominal and real terms, buyers slightly prefer setting them in nominal terms. This may result in most prices being set in nominal terms.

The possibility of obtaining price inflexibility from small menu costs may be specific to Dixit and Stiglitz type models, commented Mark Bils. The results may not survive the effects of entry of other firms into markets.

Two other theories that could explain price inflexibility were mentioned by Maurice Obstfeld. One is customer market theory developed by Phelps and Winter, and the other is collusive industry theory developed by Green and Porter.

Herschel Grossman noticed the similarity of nominal price setting and nominal debt contracting. We do not have good theoretical explanations for either phenomenon; if we understood why debt contracts were made in nominal terms we might also understand why prices are set in nominal terms.

Concluding the discussion, Rotemberg clarified the welfare issues. The price stickiness modeled in his paper is not necessarily a bad thing; there are circumstances in such models where more price flexibility would actually reduce welfare.