Chapter Title: On Sticky Prices: Academic Theories Meet the Real World

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4   On Sticky Prices: Academic Theories Meet the Real World

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Any theory of how nominal money affects the real economy must face up to the following conundrum: Demand or supply functions derived—whether precisely or heuristically—from basic micro principles have money, M, as an argument only in ratio to P, the general price level. Hence, if monetary policy is to have real effects, there must be some reason why changes in M are *not* followed promptly by equiproportionate changes in P. This is the sense in which some kind of "price stickiness" is essential to virtually any story of how monetary policy works.[1]

Keynes (1936) offered one of the first intellectually coherent (or was it?) explanations for price stickiness by positing that money *wages* are sticky, and perhaps even rigid—at least in the downward direction. In that case, what Keynes called "the money supply in wage units," M/W, moves in the same direction as nominal money, thereby stimulating the economy. In the basic Keynesian model,[2] prices are not sticky *relative to wages*. It is wage rigidity that makes P respond less than one-for-one to M.

In recent years, macroeconomists have focused more on *price* rigidity than on *wage* rigidity. This shift of emphasis appears to have two roots. The first is the well-known fact that real wages do not display the pronounced counter-cyclical pattern that is implied by rigid nominal wages and flexible (and presumably procyclical) prices. Hence *price* rigidity must be part of the story.

1. There are some exceptions. For example, changes in the money-growth rate can affect real interest rates via their effects on inflationary expectations. However, no one seems to think such effects are quantitatively large. And in some models these effects are absent entirely.

2. By this I mean an IS-LM system with an aggregate supply side consisting of a standard marginal productivity condition for labor demand and a labor supply function with money illusion. As a limiting case, the latter could simply be a fixed money wage.

The second is the suggestion, made by Barro (1977) and Hall (1980), that the observed market wage rate might not be allocative—that is, it might equal neither the marginal product of labor nor the marginal utility of leisure. Instead, they suggested, the current real wage might simply be an installment payment on a long-term contract. In this view of the labor market, which derives from Azariadis (1975), Baily (1974), and Gordon (1974), labor and management enter into long-term agreements under which the firm offers stable real wages but variable employment. In present-value (or expected-value) terms, the wage matches both the marginal product of labor and the marginal utility of leisure. But these equalities need not hold period by period.

It is unclear how important this view of the labor market is in practice.[3] But the idea has nonetheless helped shift theorists' attention from wage rigidity to price rigidity. After all, it seems clear that however important long-term implicit contracts are in the labor market, they must be vastly *less* important in the product market. The research reported on here follows this recent tradition by focusing exclusively on theories of sticky *prices*, not sticky wages.[4]

There is no shortage of theories of either price or wage rigidity. The supply is limited only by the imagination of economic theorists; and since theorists can be produced at roughly constant cost, the supply of theories is quite elastic. In fact, it appears that new theories are generated far more frequently than old ones are rejected. Worse yet, the ebb and flow of competing theories seems to have more in common with hemlines than with science—based more on fad and fashion than on fact. Try to think of even a single case in which a theory of sticky prices has been rejected econometrically.

What is the reason for this sorry state of affairs? I believe it is that many, if not most, of the theories are epistemologically empty in the following concrete sense: (*a*) they predict that prices adjust less rapidly than some unmeasured Walrasian benchmark, (*b*) many of the theories predict nothing other than this, and (*c*) often the theories rely on variables which are either unmeasurable in principle or unmeasured in practice. Under such conditions, it is no wonder that conventional econometric methods get us nowhere.

Is there a better way? The research project reported on here is based on the notion that there is. Specifically, almost all of the theories share one aspect in common: each traces a chain of reasoning that allegedly leads the decision maker to conclude that rapid price changes are against the firm's best interests. It struck me that if a particular price setter actually follows such a chain of reasoning, *he just might know it!*[5] He might not be able to give an intellectually coherent explanation of his behavior, just as a pool player cannot explain mechanics. But if the idea is explained to him in plain English, he should recognize and agree with it. At least that was the belief that motivated this research.

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3. See, for example, Brown and Ashenfelter (1986).

4. For some related work on theories of sticky wages, see Blinder and Choi (1990).

5. Pardon the chauvinistic use of the male pronoun throughout, but it turned out that virtually 100 percent of the respondents were men.

With a team of graduate students, I interviewed two hundred randomly se-lected firms about their pricing behavior. The results are now being written up in a book, and full details on the survey methodology will be found there. But a few points need to be mentioned:

- The sampling frame represented the private, nonfarm, for-profit, unregulated gross domestic product (GDP)—about 85 percent of the total. Since sample selection probabilities were proportional to value added, further weighting of responses is neither necessary nor appropriate.
- At 61 percent, the response rate was gratifyingly high—which eases, but does not entirely eliminate, worries that the sample might be unrepresen-tative.
- The interviews were conducted in waves between April 1990 and March 1992, and most of the interviewers were Princeton graduate students trained by me. The interviews were done face-to-face, almost always in the respon-dent's place of business.
- Questions were usually read verbatim from the questionnaire, but were sometimes paraphrased. Respondents always answered in their own words, and interviewers coded the responses on prescribed scales.

This paper focuses on a small but interesting subset of the results: what we learned about the nature and validity of five theories of sticky prices that were prominent in the academic discussions of the 1980s. The five theories were selected by me on a purely subjective basis. But my intent was to pick the theories that were most discussed, say, at NBER conferences and in graduate classrooms during the decade.

On average, as we shall see, these five theories get lower marks than the seven theories that were not "hot" in the 1980s. But the point of this paper is not to argue that economic theorists have been barking up the wrong trees, but to see what we can learn about the trees up which they have been barking. For example, the following question is often thought to be a showstopper in discussions of the "menu cost" theory: Aren't there even bigger fixed costs of adjusting quantities? Or another example: How common is judging quality by price? Answers to questions like these are available from the survey data—and apparently nowhere else.

The paper is organized into eight sections. Section 4.1 offers evidence that prices are indeed sticky. Section 4.2 looks briefly at some of the reasons why, including a list of the twelve theories "tested" by the survey. Then sections 4.3–4.7 examine in turn the evidence on each of the five recently popular theo-ries. Section 4.8 is a brief summary.

4.1   Evidence That Prices Are Sticky

I suggested earlier that there is no such thing as definitive evidence that prices are "sticky" because (a) the Walrasian adjustment norm is unmeasured, and (b) the prices that actually clear markets might be long-run contract prices

Table 4.1	Frequency of Price Adjustment (number of times per year)	
	Frequency	Percentage of Firms
	Less than once	10.2%
	Once	39.3%
	1.01 to 2	15.6%
	2.10 to 4	12.9%
	4.01 to 12	7.5%
	12.01 to 52	4.3%
	52.01 to 365	8.6%
	More than 365	1.6%

and (b) the prices that actually clear markets might be long-run contract prices rather than spot prices. Nonetheless, empirical evidence on the frequency of price adjustment makes it hard for many economists—including me—to believe that the macroeconomy is an auction hall writ large.

Our survey collected what may be the first evidence on price stickiness ever derived from a random sample of the whole economy. To begin with, we asked:

A10.[6] How often do the prices of your most important products change in a typical year?

Of our 200 firms, 186 answered this question, giving a median response of 1.3 times per year. As table 4.1 shows, there is a strong mode at 1.0, meaning that *annual* price changes are most typical. Perhaps more pertinent for macro models, fully 78 percent of GDP is apparently repriced quarterly or less often. That certainly seems like enough price stickiness to matter.

The most fascinating numbers in the table may be in the tails of the distribution, however. About 10 percent of GDP is apparently repriced less frequently than once a year; and an equal amount is repriced more than once a week. Indeed, 1.6 percent is repriced more than once a day. Yes, Virginia, there is an auction-market sector. But it is pretty small.

From a theoretical point of view, table 4.1 does not really offer the right measure of price stickiness. For example, if cost and demand shocks occur infrequently, Walrasian prices might change only once or twice a year. We would not want to call that price rigidity. Therefore, another series of questions inquired about what seem to me the conceptually correct measures of price stickiness: how much time elapses between a shock to either demand or cost and the corresponding price adjustment?

There were actually four such questions, corresponding to positive and negative shocks to both demand and cost. For example:

A13(a). Firms and industries differ in how rapidly their prices respond to changes in demand and costs. How much time normally elapses after a significant increase in demand before you raise your prices?

6. Numbers like this throughout the paper correspond to question numbers on the questionnaire.

The other three questions were similar, with "decrease" replacing "increase" and/or "cost" replacing "demand." Respondents were left to decide for themselves the meaning of the adjective "significant." Table 4.2 summarizes the results.

Much detail lies behind this small table. (The meaning of the last column will be explained below). But the main observations seem to be the following:

1. Lags in price adjustment are long. In round numbers, about three months typically elapses between the shock that would change prices in a Walrasian world and the firms' price response. This is good news for simple macro models that assume a "one period" lag in adjusting prices.

2. There is essentially no evidence for the common belief that prices adjust more rapidly upward than downward. For demand shocks, the data suggest equal response times. For cost shocks, price increases do appear to come about a half-month faster than price decreases—a difference which is highly significant in a statistical sense but not very large economically.

3. There is no evidence for the commonly held view that firms respond more rapidly to cost shocks than to demand shocks. Rather, the data suggest strikingly similar response times.

4. The cross-sectional variances in adjustment lags are huge—larger than the mean itself in all four cases. It would be a mistake, I believe, to interpret these large standard deviations as reflecting mostly sampling variance, though some is surely present. Rather, these numbers testify to the enormous heterogeneity across firms in the speed of adjusting prices.

An important technical point about item nonresponse must be made at this point. The response rates to the four variants of this question were among the lowest in the survey. Thus the bad news is that we got fewer responses to the more appropriate (but harder) question about price stickiness. About three quarters of the nonresponse stems from the fact that the question was inappropriate for some firms. (See the final column in table 4.2) For example, when we inquired about delays in cutting prices following a decline in cost, seventy-one firms told us that they never experience cost reductions. As you can see in the table, many more firms experience positive than negative cost shocks.

In sum, looking at a random sample of GDP, rather than a selected corner of the economy, reveals a substantial degree of price stickiness—certainly enough to give monetary policy a handle on real activity. But this stickiness does not appear to be asymmetric in the two ways commonly assumed by econ-

Table 4.2   **Lags in Price Adjustments (in months)**

Type of Shock	Mean Lag	Standard Deviation	Number of Responses	Number of "Never Happens"
Increase in demand	2.88	3.21	128	52
Increase in cost	2.76	3.00	163	23
Decrease in demand	2.90	3.70	132	50
Decrease in cost	3.27	3.92	101	71

Table 4.3   The Twelve Theories

Theory Number and Name	Brief Description
B1 Nominal contracts	Prices are fixed by contracts
B2 Implicit contracts	Firms tacitly agree to stabilize prices, perhaps out of "fairness" to customers (Okun 1981)
*B3 Judging quality by price	Firms fear customers will mistake price cuts for reductions in quality (Allen 1988)
B4 Pricing points	Certain prices (like $9.99) have special psychological significance (Kashyap 1992)
*B5 Procyclical elasticity	Demand curves become less elastic as they shift in (Bils 1989; Shapiro 1988)
B6 Cost-based pricing with lags	Price rises are delayed until costs rise (Gordon 1981; Blanchard 1983)
*B7 Constant marginal cost	Marginal cost is flat and markups are constant (Hall 1986)
*B8 Costs of price adjustment	Firms incur costs of changing prices (Rotemberg 1982; Mankiw 1985)
B9 Hierarchical delays	Bureaucratic delays slow down decisions
*B10 Coordination failure	Firms hold back on price changes, waiting for other firms to go first (Ball and Romer 1991)
B11 Inventories	Firms vary inventory stocks instead of prices (Blinder 1982)
B12 Delivery lags, service, etc.	Firms prefer to vary other elcmcnts of the "vector," such as delivery lags, service, or product quality (Carlton 1990)

omists. Firms tell us that they do not typically respond faster to demand shocks than to cost shocks; nor do they raise prices faster than they lower them. It pays to ask![7]

4.2   Reasons for Price Stickiness

This survey sought to find out which theories of sticky prices hold most appeal for real-world decision makers. Table 4.3 names the twelve theories that were evaluated in the survey and offers brief explanations (and citations) for each. Asterisks indicate the five that I subjectively selected for attention in this paper on the grounds that they received the most academic attention in the 1980s.

Most of the entries in table 4.3 are self-explanatory, but a few are not. The procyclical elasticity theory represents a class of models of *countercyclical markups*. If new customers are more price-elastic than old ones, then the price elasticity of demand will be procyclical, making the optimal markup count-

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7. In this case, asking bolsters the answers given by the paltry econometric evidence that exists. As early as 1981, Okun (1981, 165) noted that empirical Phillips curves do *not* offer evidence of asymmetry.

ercyclical. No one doubts that prices depend on costs. What makes cost-based pricing with lags a macroeconomic theory of price stickiness, rather than a tautology, is the recognition that most goods and services pass through several stages of processing (each with its own lag) on their way to ultimate users. Hierarchical delays is the one theory on the list that did *not* come from the academic literature; it was suggested, instead, by a top executive of a large manufacturing company during pretesting of the questionnaire. Some (including me) may prefer to think of it as a particular kind of menu cost.

The popularity poll among the twelve contending theories was run as follows. I translated the basic idea of each theory into plain English, and then asked price setters how important it was in affecting the speed of price adjustment *in their own company.* For example, the question about nominal contracting was

> B1. One idea is that many goods are sold under explicit contractual agreements that set prices in advance, so firms are not free to raise prices while contracts remain in force.
>
> How important is this idea in slowing down price adjustments in your company?

And there was a similar question for each of the twelve theories. Respondents answered in their own words, and interviewers coded the responses on the following four-point scale:

$$1 = \text{totally unimportant}$$
$$2 = \text{of minor importance}$$
$$3 = \text{moderately important}$$
$$4 = \text{very important}$$

It is straightforward to compare the average ratings given to each of the twelve theories, as I shall do shortly. But first a few important caveats.

First, the reader should be cautioned against identifying the above mentioned scale with the standard four-point scale used to grade college students. For example, an average grade of 4.0 would not connote anything as mundane as a straight-A average. Rather, it would mean that *every single respondent* considered the theory "very important," that is, we had stumbled upon God's truth! Plainly, this is not going to happen. A more plausible standard of excellence would be an average rating of, say, 3.0. On the low end, an average score of 1.0 would mean that *every single respondent* totally rejected the theory—which is more like brain death than a D. So it is perhaps more useful to think of the likely range of survey results as going from a top score of 3.0 for a wonderful theory to, say, 1.5 for a disastrous one.

Second, with very few exceptions, the theories are not mutually exclusive. Firms can, and often did, agree with several.

Third, the survey appraises *microeconomic* importance, not *macroeconomic*

Table 4.4   Ratings of the Twelve Theories

(1)	(2)	(3)	(4)	(5)	(6)	(7)
Rank	Theory Number[a]	Mean Score	Standard Deviation	t-statistic[b]	Accept Rate	Premise?
1	*B10	2.77	1.25	1.0	60.6%	100.0%
2	B6	2.66	1.26	0.6	55.5	100.0
3	B12	2.58	1.20	1.6	54.8	77.0
4	B2	2.40	1.26	2.1**	50.5	68.3
5	B1	2.11	1.25	1.8*	35.7	62.2
6	*B8	1.89	1.18	0.4	30.0	64.3
7	*B5	1.85	1.07	0.8	29.7	58.5
8	B4	1.76	1.04	1.8*	24.0	50.8
9	*B7	1.57	1.03	0.1	19.7	48.4
10	B11	1.56	0.97	1.9*	20.9	85.6[c]
11	B9	1.41	0.87	1.2	13.6	100.0
12	*B3	1.33	0.77	—	10.0	21.0

[a]Refer to table 4.3 for descriptions of the theories.

[b]For the hypothesis that the mean score is significantly above that in the next row.

[c]The question was not asked of firms producing services. Thus 85.6 percent is the percentage of these firms that report holding inventories of finished goods.

*Significant at the 10 percent level.

**Significant at the 5 percent level.

importance; and the aggregation process is complex. To illustrate, imagine that ten firms selling intermediate goods followed the menu-cost theory while thirty firms selling final goods simply marked up costs *immediately,* with no lags. Then menu costs would be *the* macroeconomic source of price stickiness even though three-quarters of all firms reject the theory entirely.

With this understood, I turn to the results of the beauty contest. Table 4.4 ranks the theories by mean scores (column 3) and also shows the standard deviation across firms (column 4).[8] Asterisks in column 2 once again indicate the five recently popular theories. The other columns require some explanation.

- The *t*-statistic in column 5 is the test statistic for the hypothesis that the theory's mean score significantly exceeds that of the theory ranked *just below* it. As you can see, a single-rank difference is statistically significant in only four cases. However, eight of the ten two-rank differences are significant at the 10 percent level, and all nine three-rank differences are significant at the

8. To put the estimated standard errors into perspective, some benchmarks may be useful. The standard deviation of a multinomial distribution with four equally likely outcomes—1, 2, 3, and 4—is 1.12. If the four probabilities are (1/8, 3/8, 3/8, 1/8) instead, the standard deviation drops to 0.87; if they are (3/8, 1/8, 1/8, 3/8), the standard deviation rises to 1.32.

5 percent level. Thus the rankings shown in table 4.4 are sharper than they may seem.

- The Accept Rate in column 6 offers an alternative way to rank the theories, offered because our numerical scale does not really have the *cardinal* significance that the rankings in column 1 pretend it has. Column 6 therefore reports a simple measure of how many firms accept each theory—the fraction of respondents rating the theory as "moderately important" or higher. As you can see, the two rankings hardly differ.
- I come now to the puzzling column 7. Nine of the twelve theories apply only to firms that have some particular characteristic. For example, inventories cannot explain price stickiness in firms that have no inventories. In each of these nine cases, we first pose a preliminary factual question (for example, "Does your firm hold inventories?"), and proceed to inquire about the theory only if the factual question is answered in the affirmative. If not, we skip over the theory and score it as "totally unimportant" by definition. The column marked Premise? shows the percentage of the sample to which the theory applies *in principle*—that is, for which the factual premise is true. For example, 68.3 percent of the firms in our sample say they enter into implicit contracts with their customers (as assumed by theory B2).

The theories naturally group themselves into three tiers, each with four members.

The top group scores well—if we are not too fussy about grading standards! If these results are believed, economists interested in the microfoundations of sticky prices should be focusing their attention on these four theories. Three of them are part of the modern Keynesian tradition: implicit contracts a la Okun (B2), cost-based pricing (B6), and coordination failure (B10). And nominal contracts (B1) finishes in fifth place. It is worth noting that two of the theories in the top group (implicit contracts and delivery lags) have especially high acceptance rates within their spheres of applicability: 74 percent (that is, .505/.683 = .74) and 71 percent, respectively. Thus, while there are sectors of the economy to which these theories do not apply, they appear to offer quite good explanations of price stickiness for the majority of firms.

The bottom tier of four theories scores amazingly poorly. Remember, a mean score of 1.5 is equivalent to half the firms rejecting the theory outright and the other half attaching only "minor importance" to it. That two theories (judging quality by price [B3] and hierarchical delays [B9]) actually rate lower than this minimum score is remarkable.[9] Unless my results are way off the mark, these theories deserve to be eliminated from further consideration. The inventory-based theory (B11) is the only one on the list of twelve in which I had a proprietary interest; I offer this as evidence that the election was not

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9. Remember, there was some preselection: I rejected theories that in my judgment seemed fanciful.

rigged. Two of the four theories in the bottom group (constant marginal cost [B7] and judging quality by price [B3]) are among the five I selected as having garnered the most scholarly attention in the past decade, and so merit more extensive discussion.

About half of the private for-profit economy apparently has the cost structure assumed by Hall (1986): marginal cost curves that are flat within the relevant range. Obviously, the theory does not apply to the other half. But even within the half of GDP produced under constant marginal cost (MC), the theory's acceptance rate is a mere 41 percent. Thus it seems most unlikely that constant marginal cost is a major factor behind *aggregate* price stickiness.[10] The adverse selection theory fares much worse. The premise that customers judge quality by price apparently applies to just one-fifth of GDP; and only half of these firms rate it an important cause of price rigidity. Apparently, adverse selection is an important source of price rigidity in only a corner of the economy.

In the middle comes a group of four theories that earn "average" grades:[11] nominal contracts (B1), costs of price adjustment (B8), procyclical elasticity (B5), and pricing points (B4). These theoretical bottles are either half full or half empty, depending on your tastes. The survey results for these four theories will neither persuade a skeptic nor dissuade a believer. In each case, roughly half of the firms to which the theory applies rate it as "moderately important" or higher. The differences in mean scores stem from the 51 percent who report the existence of psychological pricing points to the 69 percent who have a meaningful volume of nominal contracts.

The way we posed the questions underlying table 4.4 has two virtues:

1. It asks each price setter to describe only *his own* firm's *actual* behavior, rather than deal in hypotheticals.

2. It does not ask respondents to think or talk abstractly about the reasons for sticky prices, but only to react to concrete suggestions. While I consider this second virtue extremely important, it does have one drawback: in outlining the theories, we may have put ideas into people's heads. Based on my experience in the field, I doubt that this was a major problem. But for those who are worried about it, the survey offers an alternative.

Prior to mentioning any of the twelve theories, we asked each respondent the factual question given earlier:

A10. How often do the prices of your most important products change in a typical year?

10. Even ignoring the difference between *real* marginal costs (to which the theory applies) and *nominal* marginal costs.

11. The average ranking across all theories and all respondents is exactly 2.0. Economists have a way to go! On the positive side, however, 89 percent of all firms rated at least one theory a 4 and every firm rated at least one theory a 3.

Immediately after, we followed up with the question

A10(a). Why don't you change prices more frequently than that?

This is not the sort of question I like to ask, for it is too abstract and requires a great deal of introspection. It is a bit like asking the proverbial pool player, "Why didn't you shoot that shot differently?" Nonetheless, the question did give respondents a chance to choose their favorite explanation for price stickiness before their minds were contaminated by our suggestions.

Because the question is open-ended, the answers defy neat categorization. One hundred seventy-one firms answered; but some gave us two or three replies, and others answered with some variant of "We change prices as often as we please"—suggesting that they should not have been asked the question in the first place. This left 217 usable responses, and table 4.5 is my heroic attempt to tabulate them in some coherent, albeit admittedly subjective, way.

The explanation that tops the list, antagonizing customers, is open to various interpretations and does not obviously correspond to any of our theories—though it does evoke implicit contracts. Much the same can be said of the catch-all "competitive pressures," which reminds one of coordination failure but is also open to other interpretations. After that, however, we can clearly recognize several of the theories listed in table 4.3. Twenty-eight firms cite costs of price adjustment. The twenty-seven firms that said they change prices infrequently because their costs change infrequently are implicitly agreeing with cost-based pricing. An additional fifteen firms gave answers that evoked coordination failure. And nineteen companies mentioned either explicit or implicit contracts.

The remainder of the paper looks in more detail at the five theories of greatest recent interest.

Table 4.5	Why Don't You Change Prices More Frequently Than That? (n = 217 responses from 171 firms)	
Response		Number of Firms
It would antagonize or cause difficulties for our customers		41
Competitive pressures		28
Costs of changing prices (**B8**)		28
Our costs do not change more often (**B6**)		27
Coordination failure, price followership (**B10**)		15
Explicit contracts fix prices (**B1**)		14
Custom or habit		11
Regulations		7
Implicit contracts with regular customers (**B2**)		5
Miscellaneous other reasons		20

Note: Bold-faced numbers in the table refer to the theories listed in table 4.3.

4.3   What Price Setters Think about Coordination Failure

Coordination failure, the top-rated idea in table 4.4, is old wine in new bottles. The notion that some industries consist of a "price leader" and several "price followers" is ancient, and has long been used to explain sticky oligopoly prices. But it requires a characteristic skewness in the industrial structure and/or begs the question of who becomes the price leader and why.

Ball and Romer (1991) developed a modern version of this old idea—one that is applicable to symmetric and *competitive* market structures—by combining menu costs with some theoretical ideas set forth by Cooper and John (1988). The basic notion is that firm i's optimal degree of nominal price flexibility depends positively on firm j's.[12] So, if other firms select high price flexibility, then firm i will, too. Conversely, firm i will choose sticky prices if other firms do so. There are, in a word, multiple equilibria with varying degrees of price rigidity.

We enquired about this theory by posing the following question:

> B 10. The next idea is that firms would often like to raise their prices, but are afraid to get out of line with what they expect competitors to charge. They do not want to be the first ones to raise prices. But when competing goods rise in price, firms raise their own prices promptly.
>
> How important is this idea in explaining the speed of price adjustment in your company?

Notice that the question is phrased to sound like Ball and Romer's symmetric version of coordination failure, not the classic asymmetric leader/follower pattern. Nonetheless, a number of firms answered with the latter in mind. The distribution of responses in the full sample is shown in table 4.6. If we divide the sample into five major industrial sectors, the theory ranks first in trade and services, second in manufacturing and the transportation-communications-utilities composite, and third in construction. If we divide the sample according to (self-reported) cyclical sensitivity, the coordination failure theory again ranks first in each subsample. It seems clear that this is the theory that practitioners regard most highly.[13]

When we asked those respondents who agreed at least somewhat with the theory *why* they did not want to go first, the overwhelmingly most frequent reply was fear of a large loss of sales if rivals did not match the price increase. This fear, by the way, mainly deters price *increases*, not price *decreases*, as the following question in table 4.7 attests. There is thus strong evidence of asymmetry in this theory, but it is in the direction opposite from that usually assumed: prices are more rigid upward than downward!

One thing we would like to understand is the cross-sectional variation in

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12. This is what Cooper and John (1988) call "strategic complementarity."
13. One caveat: table 4.4 shows that its overall rating is not significantly higher than the second-ranked theory, cost-based pricing with lags.

Table 4.6   **Distribution of Ratings of the Coordination Failure Theory (n = 198)**

Code	Response	Percentage of Firms
1	Totally unimportant	27.5%
2	Of minor importance	10.6%
3	Moderately important	19.4%
4	Very important	42.4%

Note: Mean response = 2.77. Mean response among those not answering 1 = 3.41.

Table 4.7   **B10(b). Do You Also Delay Price Cuts Because You Do Not Want to Be among the First Firms in the Industry to Cut Prices? (n = 183)[a]**

Code	Response	Percentage of Firms
1	Rarely or never	61.3%
2	Sometimes	13.8%
3	Usually or always	24.8%

Note: Mean response = 1.63.

[a]Twenty firms answered, "We never cut prices"; hence we have only 163 *numerical* responses.

how firms rate the various theories. This can be done by looking at cross tabulations. But, in a data set with twelve theories and more than 125 variables, the number of possible cross tabulations is enormous. A more efficient way of summarizing the correlations is by estimating regression models with the rating of each theory on the left and a variety of explanatory variables on the right. The reader should understand that, in doing so, I am not testing any hypotheses. The estimate models are intended merely as convenient summaries of the *partial* correlations found in the data.

What is the most appropriate statistical model? In this paper, I estimate an *ordered probit* model for each theory. So, before presenting any results, I ought to explain what that model is.

Suppose we have a categorical variable (like the scores respondents give to some theory) which can take on one of k discrete values: 1, 2, 3, . . . , k. These responses are *ordinal* rather than cardinal—that is, 4 is bigger than 3, which is bigger than 2; but there is no sense in which 4 is twice as big as 2. the ordered probit or logit model postulates the existence of a continuous latent variable, call it z, which indicates the firm's evaluation of the theory. The value of variable z for firm i is assumed to be linearly related to a set of regressors, X_i:

(1) $$z_i = X_i b + c + u_i,$$

and the respondent is presumed to give response j ($j = 1, . . . ,k$) if

(2) $$a_{j-1} < z_i < a_j.$$

In most applications, the lowest bound, a_0, is taken to be zero and the highest bound, a_k, is taken to be infinity.

The model is completed by the assumption that the cumulative distribution function (cdf) of the random error, u, is either normal (the ordered *probit* model) or logistic (the ordered *logit* model). I chose the former for no other reason than that it makes it easier to think of (1) as a linear regression. But nothing hinges on this decision; ordered *logit* models yielded substantially identical results.

The probability of observing response j from firm i is the probability that

$$a_{j-1} - X_i b - c < u_i < a_j - X_i b - c,$$

evaluated according to the normal cdf. In what follows, I adopt the notation OP(B1) = Xb to connote the ordered probit model explaining the scores of theory B1 by regressors X with coefficients b.[14]

Estimation is by maximum likelihood, as described in Greene (1990, chap. 20).[15] If there are m regressors, the model has $m + k$ parameters: c, the m-vector b, and the $(k - 1)$-vector a. In most of my applications, k can be as large as seven—allowing for intermediate responses like 3.5. The algorithm usually converged rapidly if m was no larger than, say, ten or twelve. But with larger numbers of regressors and smaller numbers of observations,[16] we began pushing the limits of computability.

The best fitting ordered probit model for the coordination-failure theory (B10) was (with asymptotic standard errors in parentheses):[17]

OP(B10) = .0074 REGULARS + 1.01 REG − .28 INFL + .13 MC
(n = 169)   (.0043)   (0.25)   (.11)   (.07)

−.15 INTERVAL − .35 ACCURACY − 1.18 UNION −.63 IMPLICIT
(.11)   (.14)   (0.61)   (.21)

$$\log L = -210.7, \text{ pseudo-}R^2 = .122, \text{ linear } R^2 = .353.$$

The variables will be defined momentarily. In addition to the log likelihood, two measures of goodness of fit are offered:[18]

1. The pseudo-R^2 is McFadden's statistic, defined as $1 - L_1/L_0$; where

L_1 = the log likelihood for the estimated model, and
L_0 = the log likelihood for a model with only a constant.

14. Again, see table 4.3 for brief descriptions of the theories.
15. The precise model fit is known as the proportional odds model. It is programmed into the STATA package.
16. Owing to missing data, the sample size often shrinks when a new independent variable is added.
17. Several interviewer dummies were also included in this and subsequent regressions, but their coefficients are not shown separately.
18. Here, and in what follows, I do not bother to report p-values for the chi-squared tests of the overall significance of the models; they are always below .0001.

2. The linear R^2 is a concept I invented as a more intuitive measure. It is interpreted simply as the R^2 we would get if the latent variable z were actually measured and we ran the linear regression (1). It is a transformation of the McFadden measure calibrated by Monte Carlo methods.

The variables found "significant" in regression (3) are:

REGULARS = the fraction of sales going to regular customers. Apparently, coordination failure is a less important source of price stickiness when you have off-the-street customers. One interpretation is that it is the regular customers who are angered by unmatched price increases.

REG = a dummy equal to 1 if the firm has any regulatory restrictions on pricing.[19] The coefficient means that such firms are much more likely to deem coordination failure important.

INFL = a categorical variable indicating how often the firm uses economy wide inflation forecasts in setting prices. I interpret this as an indicator of economic rationality, since *any* price setter concerned about his desired *relative* price must take forecasts of future inflation into account. The negative coefficient means that "smarter" firms are less constrained by coordination failure.

MC = a categorical variable indicating the shape of the firm's marginal cost function (higher values of MC connote more increasing marginal cost). Apparently, the coordination failure theory rates better with firms that have rising MC curves.

INTERVAL = a dummy variable indicating that the firm follows a time-dependent (rather than state-dependent) policy for price reviews. It seems natural that firms with regular periodic price reviews should be less concerned about coordination problems; but the statistical association is weak.

ACCURACY = a categorical variable indicating how accurately the firm can estimate its marginal costs. Those that can do this well apparently worry less about coordination.

UNION = the unionization rate in the firm's three-digit industry. Thus firms in more unionized industries view the coordination theory less favorably.

IMPLICIT = a dummy variable indicating the presence of implicit contracts of the Okun variety. An Okunesque interpretation of the negative coefficient would probably say that implicit contracts permit price increases when (and only when) costs increase—regardless of whether other firms are raising their prices at the same time.

4.4   What Price Setters Think about Adjustment Costs

Perhaps the simplest explanation for price stickiness is that firms face explicit costs of changing prices. This theory comes in two variants. In the first (e.g., Rotemberg 1982), adjustment costs are convex and, where explicit solu-

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19. Truly regulated firms were excluded from the sample. But, for example, firms in the sample might sell some regulated products or face restrictions such as prenotification.

Table 4.8		A10(b). When You Do Raise or Lower Prices, Do You Normally Do It All at Once or in a Series of Smaller Changes? (n = 198)	
	Code	Response	Percentage of Firms
	1	Normally all at once	74.0%
	2	It varies	9.6%
	3	Normally in small steps	16.4%

tions are needed, quadratic. More recent theoretical efforts (e.g., Mankiw 1985) focus on models in which a lump sum ("menu cost") must be paid any time the price is changed.

The two different types of adjustment costs carry starkly different implications at the micro level. If adjustment costs are convex, a firm wishing to raise its price will do so gradually, in a series of small steps, rather than all at once. But if the costs of price adjustment are lump sum, we should observe infrequent price changes of sizable magnitude. At the macro level, aggregation smooths the adjustment, thereby blurring the distinction between the two models. But firm-level micro data offer a rare opportunity for a sharp test.

We have several such tests in the survey, and they all point strongly toward menu costs. First—before even mentioning adjustment costs—we asked all two hundred firms about their pricing practices. Do they normally adjust prices all at once or in a series of small steps? The answers are shown in table 4.8. The vote points overwhelmingly toward menu costs—*if* adjustment cost is the reason behind these responses.

Later in the interview we turned to adjustment costs explicitly, asking respondents if their firms really have such costs. Specifically:

> B8(a). Another idea is that the act of changing prices entails special costs in itself, so firms hesitate to change prices too frequently or by too much. The costs we have in mind are not production costs, but costs like printing new catalogs, price lists, etc., or hidden costs like loss of future sales by antagonizing customers, decision-making time of executives, problems with salespeople, and so on.
>   Does your firm incur such costs when it changes prices?

Notice that there is at least one item on this list that economists would not normally classify as an adjustment cost: the notion that *today's* price change might affect *tomorrow's* demand curve by "antagonizing customers." There is certainly a danger that respondents might have confused the idea that q_{t+1} depends on $p_{t+1} - p_t$ given p_{t+1}—which *would* constitute a genuine adjustment cost—with the simpler idea that higher p_t probably means higher p_{t+1} and therefore lower q_{t+1}. Unfortunately, there seemed to be no way around this problem in the context of an interview.[20]

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20. One reader asked why we included "antagonizing customers" on the list at all. The answer is simple: in pretesting, respondents kept bringing it up.

Table 4.9   **B8(a). Does Your Company Have Explicit Costs of Price Adjustment?
(n = 200)**

Code	Response	Percentage of Firms
1	No	35.8%
2	Yes, but trivial	21.3%
3	Yes	43.0%

Note: Mean response = 2.07.

With this caveat in mind, I proceed to the answers to the question, which are shown in table 4.9. What you make of these results depends on how you interpret the intermediate response, "yes, but trivial." If it is viewed as affirming the existence of adjustment costs, then firms representing almost two-thirds of GDP report having such costs. If, on the other hand, we treat trivial adjustment costs as, well, trivial, then the adjustment-cost theory applies to only 43 percent of the economy.

The impression that menu costs are the dominant form of adjustment costs was strongly confirmed by a follow-up question, asked only of firms that reported having adjustment costs (see table 4.10). By a margin of more than five to one, respondents gave the answer associated with menu costs rather than the one suggested by convex costs.

Note, by the way, that the evidence presented by Carlton (1986) and Kashyap (1992) in no way contradicts my finding that menu costs are dominant. Tables 4.8 and 4.10 suggest that convex adjustment costs apply to around 20 percent of GDP. Thus it should not be hard to find examples of very small price changes. But they are the exception, not the rule.

I come now to the main question about the importance of the adjustment-cost theory. The answers are shown in table 4.11. The theory scores fairly well (an average rating of 2.97) among the eighty-five firms that report nontrivial adjustment cost. But, since so many companies have either zero or trivial adjustment costs, its overall rating is quite low. The average score of 1.89 (a bit below "of minor importance") places it sixth among the twelve theories tested.

4.4.1 Adjustment Costs for Quantities?

Proponents of the adjustment-cost theory of price rigidity are often confronted with the following theoretical question. While there may be adjustment costs for changing *prices*, there surely are adjustment costs for changing *quantities*. Why should we believe that the former are greater than the latter? In some quarters, this question is thought to be a showstopper: adjustment costs for quantities are *assumed* to be greater than adjustment costs for prices, and so the theory is dismissed as an implausible explanation of sticky prices.[21]

While the relative size of the two types of adjustment costs is surely an

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21. For an example, see Gordon (1990, 1146).

Table 4.10 **B8(c). Do These Costs of Changing Prices Come Mainly from Changing Prices Often or Mainly from Changing Them by Large Amounts? (n = 81)**

Code	Response	Percentage of Firms
1	Mainly from often	69.1%
2	Mainly from large amounts	13.6%
3	Both	17.3%

Table 4.11 **B8. How Important Are Costs of Changing Prices in Slowing Down Price Adjustments in Your Company? (n = 200)**

Code	Response	Percentage of Firms
1	Totally unimportant	59.3%*
2	Of minor importance	9.8%
3	Moderately important	14.3%
4	Very important	16.8%

Note: Mean response = 1.89. Mean among those reporting nontrivial costs = 2.97. Rank = sixth.
*This includes seventy-one firms that said they have no adjustment costs.

empirical issue, it cannot be resolved with standard data since we have no direct measurements of these costs. The survey offered a unique way of approaching this issue, and we did so through a series of questions. First, we asked firms whether they prefer to adjust price or output when demand changes. Most prefer output adjustments, as table 4.12 shows. Notice once again the indication that prices are more sticky upward than downward: the vast majority prefer to raise production rather than price when demand rises, but only a slim majority prefer to cut volume rather than price when demand falls.

We followed each of these questions with the simple query "Why is that?" Since the inquiry was open-ended, the answers are not easy to summarize, and doing so would take far too long. Suffice it to say that relatively few firms cited costs of price adjustment as the reason for their preference.

Finally, we asked directly whether the firm also has explicit costs of changing *output:*

B8(j). Do you incur special costs from the act of changing your production—analogous to the special costs you incur when you change prices? We have in mind things like temporary halts in production, expenses in recruiting new workers, etc. ($n = 77$)

The answers were as follows: no—34.4 percent; yes, but minor—23.4 percent; yes—42.2 percent.

It is surprising, I think, that about one-third of all firms deny having *any*

adjustment costs for changing production. While the sample distributes itself somewhat evenly across the three categories, one should remember that this question was asked *only* of firms that reported meaningful costs of adjusting prices. Thus I read the responses as suggesting that, contrary to intuition, adjustment costs for *quantities* may well be *smaller* than adjustment costs for *prices*.

4.4.2   Correlates of Adjustment Costs

What kinds of firms believe that costs of changing prices are a significant factor behind price stickiness? I approach this question by estimating *two* ordered probit models. The first explains ADJ, a categorical variable indicating the presence or absence of adjustment costs, that is, the response to question B8(a) above, coded as follows:

$$1 = \text{no}$$
$$2 = \text{yes, but trivial}$$
$$3 = \text{yes}$$

Then, *only* for firms that answer either 2 or 3 to this question, I estimate an ordered probit model for the answer to question B8—the firm's evaluation of the adjustment-cost theory.

The best-fitting equations are

(4) $\text{OP(ADJ)} = -1.47 \text{ RET} -1.03 \text{ WHOLE} -1.51 \text{ CON}$
        $(n = 169)$ $(.47)$ $(.36)$ $(.38)$

$+.010 \text{ CONSUMER} -.29 \text{ CYCLICAL} + .36 \text{ INFL} -.13 \text{ CONTRACTS}$
$(.004)$ $(.12)$ $(.12)$ $(.07)$

$+ .17 \text{ INTERVAL} -.34 \text{ LOYAL} -.17 \text{ MC} + .23 \text{ INVENTORY;}$
    $(.11)$ $(.21)$ $(.08)$ $(.13)$

        $\log L = -154.1; \text{pseudo-}R^2 = .198; \text{linear } R^2 = .508.$

(5) $\text{OP(B8)} = 0.76 \text{ MFG} + 1.41 \text{ SERV} + .91 \text{ TCU}$
        $(n = 112) \, (.33)$ $(.45)$ $(.54)$

Table 4.12   **Reactions to Changes in Demand (B8(f), [h]. When your demand rises [falls], do you normally prefer to raise [decrease] your production, increase [cut] your prices, or both? [n = 78 (76)])**

		Percentage of Firms when	
Code	Response	Demand Rises	Demand Falls
1	Level of production	61.5%	36.8%
2	Prices	4.5%	27.0%
3	Both	34.0%	36.2%

$$-.44 \text{ CYCLICAL } -.15 \text{ CONTRACTS}$$
$$(.13) \qquad\qquad (.08)$$

$$-.64 \text{ LOYAL } +.28 \text{ MC } + .37 \text{ INVENTORY};$$
$$(.23) \qquad\quad (.09) \qquad (.17)$$

$$\log L = -151.9; \text{ pseudo-}R^2 = .143; \text{ linear } R^2 = .401.$$

As earlier, asymptotic standard errors are in parentheses.

The fit of these regressions is good. Indeed, the first is extraordinary for a model with categorical data—equivalent to an R^2 of .51 in a linear regression of the latent variable on the regressors. The significant righthand variables are

RET, WHOLE, CON, MFG, SERV, TCU = a set of dummy variables for retailing, wholesaling, construction, manufacturing, service industries, and a composite of transportation, communications, and utilities.

Thus the coefficients in the two equations mean that the trade (especially retail trade) and construction sectors are considerably less likely than the others (a) to have significant adjustment costs, and (b) to view such costs as an important source of sticky prices, even if they have them. In the case of construction, this is to be expected: for the most part, each sale has its own unique price. In the case of retail trade, however, this may be a bit surprising. After all, the phrase "menu costs" derives from a retailing application. However, what made Mankiw's (1985) paper so interesting was his demonstration that *presumably small* adjustment costs could account for sizable rigidities.

CONSUMER = the fraction of output sold to consumers. The strong positive coefficient in equation (4) is as expected; it means that adjustment costs are larger when you sell to consumers rather than to businesses. However, this does not carry over to equation (5). The importance of adjustment costs as a cause of price rigidity is apparently independent of whether the firm sells mainly to consumers or businesses.

CYCLICAL = a categorical variable indicating whether the firm is cyclically sensitive or insensitive (self-reported). The strongly negative coefficients in both regressions mean that cyclically sensitive firms both report smaller adjustment costs and see them as a less important source of sticky prices. Perhaps this is because firms with volatile demand curves arrange their affairs this way.

CONTRACTS = a categorical variable indicating the fraction of sales made under written contracts. The negative coefficients in both equations mean that firms that sell more under written contracts worry less about adjustment costs, as is to be expected.

INTERVAL, defined earlier, gets a weak positive coefficient in equation (4), which is puzzling; but it is not significant in equation (5). Perhaps regular periodic price reviews are costly even though they do not rigidify prices.

LOYAL = a dummy variable equal to 1 for firms that lose their less loyal customers (and retain their most loyal ones) when demand falls (see the next

theory). The source of the correlation here is far from obvious, but it is quite strong. Firms that accept this premise are much less likely to see adjustment costs as important.

MC was defined earlier: higher values connote more steeply sloped MC curves. Curiously, the coefficients are of opposite signs in the two equations. The negative coefficient in equation (4) and positive coefficient in equation (5) mean that firms with more upward-sloping MC curves report *smaller* adjustment costs for changing prices but nonetheless attribute *more* importance to the theory.

INVENTORY = a dummy variable indicating whether the firm holds inventories of finished goods.[22] Such firms apparently have larger costs of price adjustment and view these costs as a more important source of price rigidity. This finding may reflect conscious decisions in firms' choices of pricing and inventory technologies. A flexible inventory policy makes it easier to vary *sales* instead of *prices* when demand fluctuates—which is just what you would want to do if it was costly to adjust prices but not very costly to adjust sales.

4.5   What Price Setters Think about Countercyclical Markups

There are many theories of countercyclical markups, but some are uniquely unsuited to this type of investigation. For example, we cannot very well ask firms whether oligopolistic collusion is more effective at cyclical peaks or troughs. The version we did try to test assumes that countercyclical markups derive from procyclical elasticity of demand. This is an old idea, dating back at least to Pigou (1927) and recently revived by Bils (1989) and Shapiro (1988).

The specific version tested in the survey posits that firms have different classes of customers who vary systematically in their price and income elasticities of demand. More loyal customers have lower elasticities and less loyal customers have higher ones. Thus, as the firm's demand curve shifts inward during a recession, its customer base becomes less price-elastic.

For purposes of the questionnaire, the idea was translated as follows:

> B5(a). It has been suggested that, when business turns down, a company loses its least loyal customers first and retains its most loyal ones. Since the remaining customers are not very sensitive to price, reducing markups will not stimulate sales very much.
>   Is this idea true in your company?

The sample divided roughly 60/40 on this question, with 114 firms answering "yes" and 81 answering "no."

Notice an ambiguity in the way the question is worded—one which I deliberately let stand. The change in a firm's customer base when its sales decline

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22. This question is not asked of service firms; instead they are automatically coded as holding zero inventory.

presumably depends on whether the drop in sales is economywide, indus-trywide, or specific to the firm. In addition, a firm's elasticity of demand should depend on how its competitors react when it changes its price. Rightly or wrongly, I judged these matters too complex—and too wordy—to be embod-ied in the question, and so left it to each firm to answer under whatever blend of ceteris paribus and mutatis mutandis conditions seemed most relevant. That, of course, leaves significant ambiguity over what respondents were assuming.

We followed this question by asking the 114 firms that accepted the theoreti-cal premise what they thought of it as an explanation for sticky prices:

B5. How important is it in explaining the speed of price adjustment in your company?

The answers, which are displayed in table 4.13, fall somewhat short of a ring-ing endorsement of the theory. Taken at face value, they imply that the notion that cyclical shifts in customer loyalty account for price stickiness has no em-pirical relevance at all for 56 percent of American industry and substantial relevance for only 31 percent.

When we disaggregate the responses by industry, manufacturing and retail-ing stand out as the two sectors of the economy in which this theory has the most validity.

4.5.1   Correlates of Agreement with the Theory

It is of interest to know, first, what kinds of firms believe they can distinguish among customer groups identified by loyalty, for only such firms can possibly find this theory of any importance. One way to summarize these cross-sectional differences is in the following ordered probit regression (all variable names but one have been previously defined):

(6)   OP(LOYAL) = .54 IMPLICIT − .33 CYCLICAL − .26 ADJ
       $(n = 179)$ $(.22)$ $(.12)$ $(.13)$

           − .90 CON − .00002 SIZE;
           $(.39)$ $(.00001)$

       $\log L = -101.6$, pseudo-$R^2 = .163$, linear $R^2 = .443$.

Table 4.13	**B5. How Important is Procyclical Elasticity in Slowing Down Price Adjustments in Your Company? (n = 195)**		
	Code	Response	Percentage of Firms
	1	Totally unimportant*	55.9%
	2	Of minor importance	12.8%
	3	Moderately important	21.5%
	4	Very important	9.7%

Note: Mean response = 1.85. Mean among those accepting the premise: 2.46 (n = 114). Rank = seventh.

*This category includes firms that denied the premise by answering "no" to question B5(a).

The fit of the model is excellent as these things go. It makes sense that firms with implicit contracts draw a clear distinction between "loyal" and other customers, and *perhaps* it makes sense that larger firms do not (the variable SIZE measures annual sales). But I see no obvious intuitive explanations for the other three coefficients.

I turn now to the evaluation of the theory. What kinds of firms find the procyclical elasticity theory an important factor behind sticky prices? The best ordered-probit model explaining the answers to the main theory question was[23]

$$(7) \quad OP(B5) = .44 \text{ CYCLICAL} + 1.15 \text{ NONPRICE} - .12 \text{ MC}$$
$$(n = 100) \, (.14) \qquad\qquad (.34) \qquad\qquad (.09)$$

$$+ \, .018 \text{ FIXED} + .50 \text{ RATION} + 1.37 \text{ RETAIL} + .47 \text{ MFG}$$
$$(.006) \qquad\quad (.14) \qquad\qquad (.53) \qquad\qquad (.32)$$

$$+ \, .27 \text{ ACCURACY} - .016 \text{ GOVT} - .56 \text{ POINTS} - .24 \text{ INVENTORY}$$
$$(.19) \qquad\qquad (.009) \qquad (.29) \qquad\qquad (.18)$$

$$\log L = -108.4, \text{ pseudo-}R^2 = .221, \text{ linear } R^2 = .548.$$

These results seem very erratic and hard to explain.[24] The only regressor common to both equations—CYCLICAL—has opposite signs in each.[25] Firms with rising marginal costs are actually *less* likely to agree with the procyclical elasticity theory, which is unfortunate since the idea is meant to explain constant price in the face of rising MC. But the coefficient is not significant at conventional standards. The new variables appearing in (7) are

NONPRICE = a dummy for competing on nonprice elements instead of price. The highly significant positive coefficient may indicate that these firms elect not to compete on price because of elasticity pessimism.

FIXED = the percentage of costs that are fixed. Since relatively high fixed costs mean relatively low marginal costs, these two cost variables (MC and FIXED) may be picking out firms that base prices more on costs and less on demand. However, the positive coefficient on ACCURACY—a categorical variable indicating how well firms think they know their marginal costs—argues against this interpretation.

RATION = a categorical variable indicating the frequency with which customers are rationed in what they may buy. Firms that do so frequently apparently place less faith in procyclical elasticity.

POINTS = a categorical variable indicating the presence or absence of psychological pricing points—as suggested by Kashyap (1992). For some reason, firms with such pricing points are less likely to attribute importance to procyclical elasticity.

23. This equation was estimated *only* for firms that answered 2 ("yes") to equation B5(a).
24. The small sample size for equation (7) may be playing a role here.
25. However, the industry results are consistent in that CON gets a negative coefficient in (6) and (implicitly) a negative one in (7). Similarly for MFG and RETAIL.

The dummy variables RETAIL and MFG show that retailers are much more likely, and manufacturers slightly more likely, to see their customer base as divided into loyal and disloyal segments. I had no particular priors on these industry dummies.

Finally, firms that hold inventories (INVENTORY) or sell more to the government (GOVT) are less likely to believe in procyclical elasticity.

4.6   What Price Setters Think about Constant Marginal Cost

Go back to thinking of the elasticity of demand as a constant. If marginal cost is also constant, then price should be, too. That simple idea is *almost* a theory of price rigidity over the business cycle, but it leaves one important loose end. The demand and cost functions of micro theory implicitly apply to *relative* prices and *real* marginal costs. The theory therefore really says that *markups* are constant over the business cycle. Nominal prices are sticky only if nominal input costs are.

Although neither price stickiness nor constancy of MC was his primary concern, Robert Hall (1986) revived interest in this very old idea. Hall's goal was to explain procyclical productivity by appealing to market power—markups of price over marginal cost. Constant MC entered the picture to resolve a paradox that Hall pointed out: the high degree of market power that he estimated on U.S. data is hard to reconcile with the comparatively low level of business profits.

Hall's explanation was that fixed costs ("excess capacity") are substantial. Thus he envisions a world in which the typical marginal cost curve is flat up to capacity and then nearly vertical. A competitive firm producing on the flat portion of its MC curve with $P = $ MC is not making any contribution to its fixed costs. Therefore, to cover fixed costs in the long run, it must spend a reasonable amount of time on the vertical portion of its MC curve—with high prices in periods of peak demand. Hence prices should be cyclically sensitive. But a firm with substantial market power need do no such thing. Since it maintains P above MC all the time, it is constantly earning a contribution toward its fixed costs. That is why, in Hall's (1986, 315) words, "a finding of market power points in the direction of constant marginal cost."

Notice the highly qualified phrase "points in the direction of." High fixed costs do not imply that marginal cost must be flat, and Hall makes no such claim. Instead, he uses a kind of "menu cost" argument to suggest that MC curves are flat: if they are, then the cost of price rigidity is small because profits drop only slightly when output deviates from its profit-maximizing level.

To believe in constant marginal cost as a theory of price rigidity, you must accept two premises: first, that marginal costs are constant, and second, that this constancy is an important source of sticky prices. Hence, I again evaluate the theory in two stages.

First, how are firms' marginal cost curves really shaped? This turned out to be a tricky question because the term *marginal cost* is not in the lexicons of most businesspeople; the concept itself may not even be a natural one. For purposes of the survey, we translated "marginal cost" into "variable costs of producing additional units," and posed the following question:

> B7(a). Some companies find that their variable costs per unit are roughly constant when production rises. Others incur either higher or lower variable costs of producing additional units when they raise production.
>   How would you characterize the behavior of your own variable costs of producing additional units as production rises?

This proved a difficult question. It often had to be repeated, rephrased, or explained. Even so, 10 of our 200 respondents were unable to answer it. The other 190 executives answered in their own words, sometimes at great length, and interviewers classified the responses into one of five categories offered on the questionnaire. The simplest way of summarizing the answers is graphically. Figure 4.1 depicts five possible shapes for the MC curve and the fraction of firms selecting each. The spikes in panels b and d indicate cases in which costs jump discretely when output crosses certain critical thresholds—as when a bank opens a new branch office or a railroad adds an additional car.

When juxtaposed against the standard neoclassical assumption that panel e is the rule, the answers are stunning. Only 11 percent of firms report that their MC curves are rising (panel e). By contrast, 40.5 percent claim that their MC curves are falling, presumably *globally* (panels a and b).[26] The good news for the constant-marginal-cost theory is that approximate constancy of MC (panels c and d) is the modal case—encompassing 48.4 percent of GDP. The bad news is that this group accounts for less than half of GDP and that almost as many firms say they have *falling* MC. If anything, it appears that Hall did not go far enough in arguing against the standard neoclassical view that the MC curve is upward sloping.

My own experience as an interviewer leads me to discount these results somewhat because many executives had difficulty understanding the question. Some may have confused marginal cost with average cost (AC), and it is surely not surprising that many firms have falling AC curves. Nonetheless, the discount would have to be pretty severe before we read figure 4.1 as saying that rising MC is the norm. In any case, we certainly could not ask firms that denied having constant MC whether constant MC is an important source of sticky prices. So the main question about the theory was posed only to firms that classified themselves in panels c or d of figure 4.1—a total of ninety-two companies. The others were automatically coded as answering "totally unimportant" to the following question:

26. Ramey (1991) offers econometric evidence for declining MC in seven industries, six of them in manufacturing.
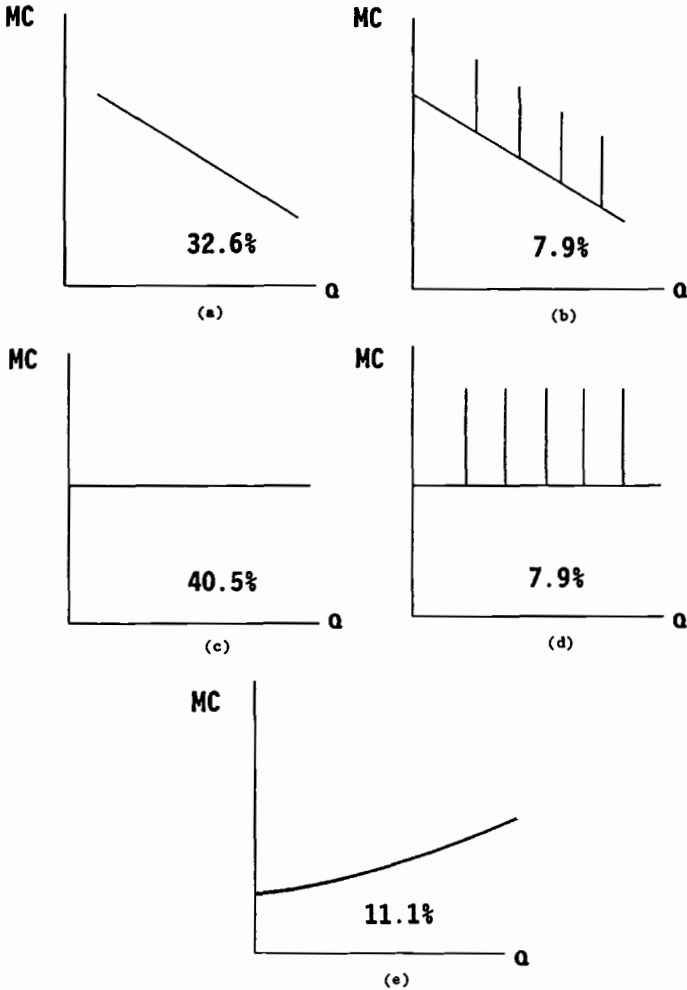
Fig. 4.1   Possible shapes of marginal cost curves

B7. It has been suggested that many firms base prices on costs. Hence firms with constant variable costs per unit have no reason to change prices when production changes.

   How important is this idea in explaining the speed of price adjustment in your company? (n = 90)

Table 4.14 summarizes the answers; they give the theory a pretty poor rating. Firms representing almost three-quarters of GDP reject the theory outright,

either because their MC curves are not flat (52 percent) or because they do not see constant MC as an important cause of price rigidity (21 percent). Only about 20 percent of respondents accept the theory in the sense that they give it a rating of 3 or higher. The average score of 1.57 on the four-point scale is low in an absolute sense and ranks the theory ninth of the twelve evaluated in the survey. Even within the population of firms that report flat MC curves, the theory's average rating is only 2.19. Simply put, it seems most unlikely that constant marginal cost is an important factor behind macroeconomic price rigidity.

4.6.1   Correlates of the Theory

Few variables carry any predictive power for the shape of the firm's marginal cost curve, that is, for the appropriate panel in figure 4.1. Even the ordered probit models that best predict the answers have exceedingly modest fits:

(8) $$OP(MC) = .40 \text{ POINTS} - .24 \text{ ADJ} - .23 \text{ MFG}$$
        $$(n = 190) \ (.17) \qquad\qquad (.10) \qquad (.18)$$

        $$\log L = -248.5, \text{ pseudo-}R^2 = .049, \text{ linear } R^2 = .17,$$

(9) $$OP(MC) = .40 \text{ POINTS} - .25 \text{ ADJ} - .39 \text{ MFG}$$
        $$(n = 154) \ (.19) \qquad\qquad (.11) \qquad (.21)$$

        $$- .0034 \text{ ELAST} - .41 \text{ REG};$$
        $$(.0024) \qquad\qquad (.23)$$

        $$\log L = -195.0, \text{ pseudo-}R^2 = .089, \text{ linear } R^2 = .274.$$

The sample size is much smaller in the second regression because the variable ELAST (the elasticity of demand) is missing in almost 20 percent of the observations. It gets a not-very-significant negative coefficient, indicating that steeper MC functions are associated with less elastic demand curves.

The two regressions show that manufacturers (MFG) are less likely than other firms to have increasing marginal costs, as are firms with substantial costs

Table 4.14	B7. How Important Is Constant Marginal Cost in Explaining the Speed of Price Adjustment in Your Company? (n = 190)		
	Code	Response	Percentage of Firms
	1	Totally unimportant*	73.1%
	2	Of minor importance	7.2%
	3	Moderately important	9.0%
	4	Very important	10.6%

Note: Mean response = 1.57. Mean among those with constant marginal cost: 2.19 (n = 90). Rank = ninth.
*This category includes firms that said marginal cost was not constant.

of adjusting prices (ADJ). The variable REG indicates that (partially) regulated firms are more likely to have *falling* MC curves, as is to be expected.

The other significant variable is POINTS. For some reason, firms that have pricing points are also more likely to report rising MC.

Many more variables correlate with a firm's evaluation of the importance of constant MC as an explanation of sticky prices, given that they have constant MC. But here statistical inference starts to get thin because the sample is so small. (Only ninety-two firms report constant MC curves.) The following equation fits (overfits?) the data extremely well:

$$
\begin{aligned}
(10) \qquad \text{OP(B7)} = &-3.36 \, \text{MFG} + 5.78 \, \text{UNION} \\
(n = 77) \quad &(0.55) \qquad\quad (1.39) \\[4pt]
&- .22 \, \text{CONTRACTS} - .87 \, \text{INTERVAL} \\
&\quad (.13) \qquad\qquad\quad (.23) \\[4pt]
+ .70 \, \text{IMPLICIT} - &1.06 \, \text{JUDGE} - 1.21 \, \text{POINTS} + 1.25 \, \text{LOYAL} \\
(.40) \qquad\quad &(0.44) \qquad\quad (.37) \qquad\qquad (0.37) \\[4pt]
+ 1.61 \, \text{INVENTORY} + &.77 \, \text{ADJ} + .033 \, \text{GOVT} \\
(0.29) \qquad\qquad &(.22) \qquad (.012)
\end{aligned}
$$

$$
\log L = -65.9, \ \text{pseudo-}R^2 = .407, \ \text{linear } R^2 = .737.
$$

All of these variables have appeared previously, except for JUDGE—a dummy variable for whether the firm's customers judge quality by price (see the next theory).

Given the tenuous statistical foothold of these estimates, it is probably inadvisable to make much of them. Taken at face value, they say that firms are *more* likely to see constant marginal cost as an important source of sticky prices if they have adjustment costs for changing prices (ADJ), enter into implicit contracts with their customers (IMPLICIT), and have the kind of customer loyalty assumed in the previous theory (LOYAL). These all make intuitive sense, although ADJ gets the opposite sign in equations (8) and (9). But firms also rate the constant-MC theory higher if they are in more unionized industries (UNION), sell more to government (GOVT), and hold inventories of finished goods (INVENTORY); there is no clear explanation for these correlations.

On the negative side, firms attribute *less* importance to the constant-MC theory if they are in manufacturing (which reinforces equations [8] and [9]), if they sell more under written contracts (CONTRACTS), and if they have periodic price reviews (INTERVAL). These findings seem intuitive. But it is far from clear why constant MC should be less important when customers judge quality by price (JUDGE) or have pricing points (POINTS).

4.7   What Price Setters Think about Judging Quality by Price

Few topics in economic theory were hotter in the 1980s than imperfect information, especially *asymmetrically* imperfect information. Literally hun-

dreds of papers were written exploring the implications of adverse selection and moral hazard in such markets. It is only a slight exaggeration to say that the rule book of economic theory was substantially rewritten. Joseph Stiglitz (1987), for example, referred to the repeal of the law of supply and demand.

Adverse selection, in particular, has been offered by Stiglitz and several co-authors as an explanation of why wages, prices, and interest rates do not fall promptly to clear markets. The application to price rigidity is straightforward: firms may hesitate to cut prices in slack markets out of fear that their customers will infer (incorrectly) that product quality has been reduced. Allen (1988) developed an explicit formal model of a related idea in which unobservable quality differences make prices sticky as long as demand shocks are sufficiently serially correlated. I think it fair to say that the notion that judging quality by price might inhibit price reductions has come into common currency.

Unfortunately, no one knows whether this theory has any empirical validity. Do firms really believe that their customers judge quality by price? Is that why prices do not fall in slumps? In a sense, the adverse selection theory is the quintessential example of why I chose the survey method to study price stickiness. The theory is not just *untested*, it is virtually *untestable* by conventional methods. After all, the basic premise is that certain critical dimensions of quality are *unobservable*—even by the people who buy the product. How, then, can any objective data tell the econometrician whether or not the theory is true?

If their own words are to be believed, actual price setters do not believe that judging quality by price inhibits price reductions. This theory was assessed by asking the following question:

> B3(a). Another idea is that firms hesitate to reduce their prices because they fear that customers will interpret a price cut as a signal that the quality of the product has been reduced.
>   (a) Is this idea true in your company?

If (a) was answered in the affirmative, we followed with:

> B3. How important is it in discouraging or delaying price decreases in your company?

As table 4.15 shows, this theory rates flat last among the twelve theories tested, with a mean score of just 1.33 on the one-to-four scale. This is an amazingly negative evaluation. The main factor behind this dismal showing is that the vast majority of firms—78.5 percent to be precise—simply do not believe that their customers would "interpret a price cut as a signal that the *quality* of the product has been reduced." Among those that do, the theory attains a respectable average score of 2.56. But there are simply too few such firms to have much of an impact in the aggregate.

These negative results hold more or less across the board. The theory ranks last in manufacturing, utilities, and construction; second to last (with a mean score of only 1.20!) in trade; and third from last in the service sector. The

Table 4.15   **B3. How Important Is Judging Quality by Price in Discouraging or Delaying Price Decreases in Your Company? (n = 200 responses)**

Code	Response	Percentage of Firms
1	Totally unimportant*	81.5%
2	Of minor importance	8.0%
3	Moderately important	6.3%
4	Very important	4.3%

Note: Mean response = 1.33. Mean if quality is judged by price: 2.56 (n = 42). Rank = twelfth.
*This category includes firms that answer "no" to question B3(a).

theory's few boosters seem to be concentrated in the service sector. Of the twenty companies giving the theory a score of 3 or better, eleven sell services.

4.7.1   Correlates of Judging Quality by Price

As these things go, our ability to model econometrically which firms find judging quality by price an important source of price rigidity is reasonable. The best ordered-probit model was (with asymptotic standard errors in parentheses):

(11)   OP(JUDGE) = $-.51$ TRADE -1.14 CON $- .0055$ BUSINESS
       (n = 190)   (.36)   (0.61)   (.0041)

$\qquad + .32$ INFL $+ .85$ POINTS $-.21$ RATION $+ .39$ REG;
$\qquad$ (.15)   (.27)   (.14)   (.26)

$\qquad$ Log$L = -74.9$; pseudo-R^2 = .200, linear R^2 = .513

The one new variable in this regression is BUSINESS—the fraction of output sold to other businesses. Its negative coefficient indicates that judging quality by price is less prevalent in business-to-business transactions, as is to be expected. The other right-hand variables have appeared before.

- Surprisingly, especially given the negative coefficient on BUSINESS, companies in wholesale and retail trade are *less* likely to believe that their customers judge quality by price.
- There is apparently less judging of quality by price in construction (CON), which may or may not be surprising.
- Firms that believe that their customers have psychological pricing points (POINTS) also tend to believe that their customers judge quality by price. Perhaps psychological manipulability is a generic trait.
- Firms which ration their customers more frequently (RATION) are less likely to report that their customers judge quality by price, perhaps because their products tend to be standardized.
- Judging quality by price is more common among firms that are subject to some sort of regulatory restrictions on pricing.

- INFL is a puzzling variable. The positive coefficient means that "smart" firms are more likely to report that their customers judge quality by price. Have they been reading Joe Stiglitz?

4.8   Summary

Direct survey evidence supports the common macroeconometric finding that changes in P lag behind changes in M. Indeed, almost 80 percent of GDP is repriced quarterly or less frequently. And the typical lag of a price change behind a shock to either demand or cost is about three months. That much price stickiness appears to be enough to give monetary policy a handle on the real economy.

Or is it? The answer depends on the *sources* of all this stickiness. Five theories of sticky prices were popular in the academy during the 1980s, but only one of them is also popular with actual decision makers: coordination failure.

Apparently, many firms hesitate to change their prices for fear that competitors will not follow suit. In fact, this is the most popular theory among the twelve tested in the survey. However, our respondents tell us that the resulting price rigidity is asymmetric in the direction *opposite* from that usually assumed. Firms are more likely to delay price *increases* out of fear that competitors might not match them than they are to delay price *decreases*. Thus, if coordination failure were the *only* source of sticky prices (which it is not), the real effects of monetary expansions would be greater than those of monetary contractions. As Ball and Romer (1991) note, the most obvious policy implication of the model is that more coordinated wage and price setting—somehow achieved—could improve welfare. But if this proves difficult or impossible, the door is opened to activist monetary policy to cure recessions.

The worst of the five theories, according to our respondents, is the notion that firms hesitate to cut prices in slumps because they fear that customers will misconstrue price cuts as signals of reductions in quality. Hardly any of the two hundred firms interviewed believe their customers behave this way; those that do are mostly in services. Although this idea has attracted an enormous following in academia, it seems to have virtually no following in the real world.[27]

The simple theory that flat marginal cost curves underlie price rigidity fares only slightly better. About half of the GDP is apparently produced under conditions of constant MC. But even this group does not see constant MC as a terribly important source of sticky prices. This means that, for many such firms, the markup between price and marginal cost is cyclically variable, not constant. One fascinating finding from the survey is that about 40 percent of GDP is produced under *falling* marginal costs, though it is certainly possible that

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27. The basic idea of adverse selection may well be important in other contexts. But Blinder and Choi (1990) found similarly negative evidence regarding wage setting.

many respondents confused average with marginal cost in answering this question.

Most modern theories of cyclically sensitive markups make markups *countercyclical*, not *procyclical*. One such theory was tested in the survey and found wanting: the idea that the price elasticity of demand falls in slumps and rises in booms. Our respondents favored the premise by a margin of 58.5 percent to 41.5 percent, but did not view it as a fundamental source of price stickiness.[28]

Last, but not least, the survey taught us a great deal about the nature and validity of the theory that price rigidity stems from adjustment costs.

First, there are strong indications that adjustment costs for changing prices are more often fixed (menu costs) than convex. But since about 20 percent of the economy may have convex adjustment costs, empirical findings that some firms make *very small* price changes should not be construed as rejection of the menu-cost theory.

Second, while the menu-cost idea is symmetric in theory, it appears to be strongly asymmetric in practice. Firms tell us that adjustment costs more often deter price increases than price decreases. This makes it most unlikely that adjustment costs have much to do with printing new price lists, etc.

Third, adjustment costs are an important source of price rigidity within the approximately 40 to 45 percent of the economy that reports nontrivial adjustment costs.

Fourth, the common objection that costs of changing *quantities* must dwarf any costs of changing *prices* has much less validity than is commonly supposed. In fact, many of our decision makers seem to be saying that the former are smaller than the latter.

I cannot resist closing with a conclusion about methodology rather than substance. I think this survey demonstrates that we can learn things of interest by asking actual decision makers to tell us about their behavior. If the survey approach is right, people will cooperate. If the questions are well-posed, people will give thoughtful and coherent answers. Some of the information we can learn through attitudinal surveys can apparently be obtained in no other way.

I would be the last to argue that other more conventional modes of economic inquiry should be abandoned. But the law of diminishing returns suggests that learning by asking, the most underutilized of all economic research tools, may now offer high returns. The total cost of this research project was the equivalent of about two standard National Science Foundation grants to senior researchers. Who doubts that the survey described here added more to our knowledge than two such typical grants?

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28. I should not overstate the case. Among the 58.5 percent that accept the premise, the theory received modestly good grades. See table 4.13.

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Comment   Olivier Jean Blanchard

On Methodology

In his conclusion, Alan Blinder challenges us: "Who doubts that the survey described here added more to our knowledge than two such typical [National Science Foundation] grants?" As Blinder was embarking on his study, I was sure I would not be among the doubters. I believed that we would get fairly sharp answers as to what firms viewed as important or unimportant, and that this would help focus further theorizing. I must admit to being disappointed by the results.

Look at table 4.4, and take the ratio of column 6, the acceptance rate, to column 7, the proportion of firms for which the premise of the theory is at all applicable. For nine out of twelve theories, the ratio is above 46 percent. For seven out of twelve, the ratio is between 46 percent and 60 percent. The image this evokes and that recurs throughout my reading of the results is, that confronted with the twelve statements, the firms often had the reaction: "Now that you say it, yes, maybe that *is* kind of what we do."

There is, I suspect, a lesson here about the limits of the approach that Blinder has taken in the survey. Firms do think about pricing in their own way. Presented with short summaries of alternative academic theories, they find that most capture something, but that none is quite right. Role reversal may be useful here. Suppose that a businessman decided to find out how economists thought about inflation. Having drawn a list of theories—inflation is due to money growth; inflation is due to changes in relative prices; inflation is due to budget deficits, inflation is due to union militancy, inflation come from depreciation, and so on—he came to Blinder and asked him to rank the theories from 1 to 4. Blinder would boil at the idea of being so constrained in his answers, but would see most statements as having a grain of truth, and would give a lot of 2s and 3s. Being an academic, he would then qualify his answers at length. But businessmen are not academics. They may not have a clear, explicit view of how they set prices, and may not want to spend the time needed to qualify their answers. Thus, they give the 2s and the 3s, do not bother qualifying very much, but it does not quite capture what they do.

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It is interesting to contrast the approach taken by Blinder to that recently taken by Truman Bewley and Bill Brainard in asking firms about wage setting (Bewley and Brainard 1993). Bewley and Brainard also start from the idea that we can learn a lot from listening to firms. But in sharp contrast to Blinder, Bewley and Brainard act like psychoanalysts, remaining mostly silent as personnel officers explain what they do and why they do it. The picture which comes out is both rich and confusing. Whether and how the arguments used by firms relate to our theories of wage setting is often unclear. But what is clear is that the interviewees would not have felt at ease evaluating the relevance of our various theories of fairness, efficiency wages, and so on, had they been put to them as short statements.

On Price Stickiness

Macroeconomists have long believed in two stylized facts about price-setting by firms. The first is that, given factor costs, prices do not respond to demand shifts very much. The second is that most prices are not set continuously, leading to lags in the response of prices to their underlying determinants. In the 1980s, these two stylized facts have come to be known as "real rigidities" for the first, and "nominal rigidities" for the second. I am not sure the terminology is felicitous. But it is convenient, and I shall use it.

Both facts have been seen as contributing to the slow adjustment of the price level to movements in aggregate demand. But they have also been seen as requiring quite separate explanations. To explain the first, the weak effect of demand shifts on prices given factor prices, most of us have looked for explanations based on imperfect competition. The intellectual challenge here, and not a small one at that, has been perceived to be the development of a theory of imperfect competition which delivered such behavior. In contrast, explaining the second has appeared to most of us to be rather trivial: continuous price setting is obviously very costly. Rather, the challenge has been perceived to show how small lags at the microlevel may, through the interactions between the firms' decisions, lead to large price-level inertia and large macroeconomic costs.

Obviously the dichotomy I have drawn between "real" and "nominal" rigidities is too sharp. Rather trivially, one can only have discrete price setting if there are price setters; there can be price setters only if they have at least transient monopoly power, thus if there is imperfect competition. Also, if firms decide not to change prices in response to demand shifts, then everything else being equal, there is one less reason to change prices, and thus prices will be changed less often. Or, to take yet another interaction, if fluctuations in demand are mean-reverting, then less frequent changes in prices will also mean a smaller average response of prices to demand. But while they interact, they are about two different phenomena.

This distinction is not made explicitly in the survey. Some of the theories are clearly about why prices may not respond to demand given factor prices,

and some are about why there may be lags in the response of nominal prices to their determinants. Roughly, theories B1, B4, B8, and B9 are primarily about nominal rigidities, the others primarily about real rigidities. Is it because the distinction is one which economists may see but firms would have had a hard time grasping? I do not think so. Real rigidities correspond to questions such as When you sit down to change prices, how do you take into account changes in demand and changes in factor prices? Do you react differently to changes in demand if they are industrywide or specific to your firm? Nominal rigidities correspond instead to questions such as How often do you change prices? Does this happen on regular dates, or is it triggered by events, such as large changes in factor prices? Do you change most prices at once? Do you try to get the price right now, or later when it is still fixed and inflation has taken place?

I think the survey pays a large cost as a result of not making this distinction. At the presentation stage, organizing theories along those two lines would have helped at least this reader to map the results to macro implications. But the cost is higher. Not making the distinction leads Blinder to ask what I think are incorrectly phrased questions, and thus get what are quite possibly misleading answers. A number of theories which are designed to explain why prices may respond little to demand shifts are phrased in terms of explaining a slow response rather than a lack of response. I shall take one example. Question B7 reads:

> It has been suggested that many firms base prices on costs. Hence firms with constant variable costs per unit have no reason to change prices when production changes.
>
> How important is this idea in explaining *the speed of price adjustment* in your company? (emphasis added)

Take a firm which uses a fixed markup rule and has constant variable costs. It may interpret the question as "How important is this idea in explaining why you do not change prices in response to demand?" and answer "Very important." But it may instead interpret the question literally and conclude that flat marginal cost has nothing to do with the speed of adjustment, and thus answer "Totally unimportant."

This point is more than nitpicking. Very surprisingly—at least given my priors—of those firms which declared to have roughly constant variable cost per unit, 73 percent turn out to answer "Totally unimportant" to the question. What do they have in mind? That markups are not constant, or that markups are constant but this has nothing to do with slow adjustment? This takes me to my third set of points.

On the Picture of Price-Setting Which Emerges

Together, the three top-ranked theories point to imperfectly competitive markets, in which (*a*) firms worry about the reaction of other firms to price changes ("coordination failures," ranked first); (*b*) firms rely on simple cost-

plus pricing rules, perhaps as coordination devices ("cost-based pricing with lags," ranked second); and, (c) partly in contradiction to the rationale for using simple price rules as coordination devices, goods have many attributes other than price that are adjusted in response to demand ("delivery lags," ranked third). This is a very interesting picture. But in each case one would like to know more.

Blinder emphasizes that he is looking for a particular kind of "coordination failure," one which can emerge even in markets with large numbers of firms. As he himself acknowledges, however, what firms have in mind when they answer "Very important" to that question is in fact unclear. He indicates that a number of firms appear to be thinking of a leader-follower relations. Thus I see those answers as confirming the notion that most firms worry very much about other firms' reactions, but as telling us little beyond that. The black box of short-run movements in prices in response to demand shifts in imperfectly competitive markets remains closed.

What firms mean by cost-based pricing also remains unclear. Do firms have more or less flat marginal cost? The answer to that question is clear. Only 11 percent of the firms report upward sloping marginal cost; the others report either flat or decreasing marginal cost. Do firms use more or less constant markups? Here there are conflicting answers. Of those reporting constant marginal cost, 73 percent dismiss that as "totally unimportant" in explaining the speed of price adjustment. I discussed earlier how we might interpret the answer. One interpretation is that firms do have highly variable markups. But this interpretation is contradicted by the answers to question B6, which describes cost-plus pricing, and finds substantial support among firms, being ranked second overall.

Nominal rigidities do not score high. Costs of adjustment of prices come in in sixth place. But, as I suggested earlier, this is probably as it should be, even if nominal rigidities are important. The theme of the research on nominal rigidities is that they appear to be relatively unimportant to individual price setters, yet they may cumulate to have large macro effects. This may be a case where asking firms is indeed not the way to go. And for the same reasons, namely since they see these aspects of price-setting as minor, asking them to compare the costs of adjusting quantities to those of adjusting prices may not elicit very useful answers.

Where the survey could have been more useful here would have been in asking such questions about whether firms tended to adjust prices at regular intervals, or in response to specific changes in the environment, whether firms adjusted the price to a level which was right at the time of price setting, or took into account future inflation and set the price higher as a result. We have learned that the answers to the first set of questions are of much importance in determining how individual price rigidities get amplified or eliminated in the aggregate. I have come to the conclusion that the answers to the second have important macro implications. If there is positive inflation, and if firms cor-

rectly anticipate that their real price will decline throughout the period during which it is set in nominal terms, they will choose a price which is too high today, but right on average. Average profit margins will be roughly invariant to inflation. If instead firms set prices so that they are right at the time of price-setting, then there will be a systematic inverse relation between profit margins and inflation. Recent work I have carried out on the evolution of profits in France suggests to me that some of the large increase in profit margins in France in the 1980s has come from this effect combined with the decrease in inflation (Blanchard and Muet 1993). If I am right, this is an important implication of nominal price-setting. This is clearly a case where we would learn much from asking firms.

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