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Learning By Asking Those Who Are Doing

Alan S. Blinder*

1. Methods of Economic Inquiry

Research economists normally rely on two principal methods of economic inquiry: theory and econometrics. Other methods are viewed as vaguely unscientific. Perhaps this is as it should be. But I want to argue here that we might be able to learn something more if we opened our eyes and ears and listened a little more to the subjects who populate the economies we study, the people who actually *do* the things we *theorize* about. And I want to support this vague methodological position by reference to a concrete and important issue on which I am now doing extensive interview research: the microtheoretic foundations of price stickiness.

In the other social sciences, the notion that you can learn things by asking people would hardly be a revolutionary message. Indeed, it would hardly be a message at all, for asking people is a fundamental tool of inquiry in anthropology, sociology, psychology, and even, to some extent, political science. Yet it is something that economists not only rarely do, but often actually sneer at. After all, we don't want to act like sociologists and political scientists. Physicists and chemists do not ask their subjects why they behave as they do, so we shouldn't either—or so we think. But is that a scientific attitude? If molecules could talk, would chemists refuse to listen?

The last question is polemical, I admit. There *are* a number of valid objections to interview studies of economic behavior; and there are certainly good reasons for interpreting the results with care. For example, critics will argue that, because subjects of interviews have no incentive to respond truthfully or thoughtfully, *homo economicus* might refuse to cooperate or give misleading answers.

Where the subject has reason to conceal the truth or mislead the interviewer, this objection is a show stopper. In such cases, the interview method is simply not a promising mode of inquiry. Thus, for example, interviews may be a poor way to estimate the extent of tax evasion or the prevalence of collusion among businesses. But there are many interesting and important questions about which people have no particular reason to conceal the truth. As long as people are not pathological liars, there are many issues where the interview method might help.

The thoughtfulness problem goes deeper. We all know the billiard-ball analogy: a good pool player makes excellent use of the laws of physics without understanding them, and certainly without being able to articulate them properly. If you ask an expert pool player to explain how he shoots so well, he may not give you a coherent answer—and almost certainly will not give you the correct answer. For this reason, I think, many economists are skeptical that we can learn anything by asking "economic players" about how they play the game.

In part, I agree. I do not, for example, think we can learn much by asking corporate executives about the goals of their companies. But more pointed questions, posed in plain English, can elicit more

*Dept. of Economics, Princeton University, Princeton, NJ 08544.

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useful responses. For example, if you ask a skilled billiards player whether he bases his shots on the principle that the angle of incidence equals the angle of reflection, he will probably think you strange. But, if you take him to the table and ask (pointing to the proper angles), "Do you try to make this angle the same as that angle?," I imagine he would respond in the affirmative.[1]

Legitimate questions can also be raised about the size and representativeness of interview samples. Detailed case studies of two or three companies may suggest promising avenues for theoretical or empirical research, but they cannot produce useful statistical generalizations. And samples that are not representative of the underlying population give no basis for drawing inferences about population statistics. These are familiar problems, well known to any user of data, no matter how those data are generated.

So the role of the interview method in economic inquiry is surely limited. But we should not lose sight of two facts. First, most of the data we get from standard sources comes, in the first instance, from interviews or mail questionnaires. How else do you think the Labor Department measures the unemployment rate or the Commerce Department estimates the GNP?

Second, theory and econometrics have their limitations, too—limitations which are sometimes more severe than we like to admit. Econometric evidence is often equivocal and/or subject to methodological dispute. Theoretical deductions are often untested and/or based on untested premises. Worse yet, either the conclusions or the assumptions may be *untestable.*

As economists, we should evaluate the usefulness of the interview method by posing the classic question, "Relative to what?" The imperfect knowledge we pick up from interviews and questionnaires should not be compared to some Platonic ideal, but to the imperfect knowledge that nonexperimental scientists can deduce theoretically or glean from econometric studies. By this more reasonable standard of evidence, data culled from interviews certainly looks admissible.

To make this methodological discussion more concrete, think about the reasons for wage-price rigidity or stickiness in the macroeconomic sense. This is clearly an important issue; indeed, some would say it is *the* central theoretical question of macroeconomics. For example, it is critical to understanding why changes in nominal money have real effects. Yet what have we learned about this question from decades of theoretical and econometric research?

We have, of course, generated many theories of wage or price stickiness; indeed, new ones appear each year. But ask yourself what these theories predict. Often the answer is nothing more than that wages or prices move "slowly" relative to some unmeasured norm, such as the Walrasian equilibrium. In a distressing number of cases, the theories seem to make *no other prediction,* which leaves them rather empty. Furthermore, since they all share the *same* prediction—that prices are "sticky"—how are we to discriminate among them? It seems difficult even to imagine what a decisive test would look like, much less to carry one out. Yet theories continue to proliferate. Is that doing science, or solving crossword puzzles?

Normally, we rely on econometrics to discriminate among competing theories. What light has econometric evidence shed on this issue? Econometric studies *have* established to *my* satisfaction that most wages and prices move too slowly to be explained by market-clearing theories. But, since the Walrasian benchmark is unmeasured, many neoclassical economists are not similarly persuaded.

Furthermore, I think almost everyone agrees that we know next to nothing about which of the several dozen theories of wage-price stickiness are valid and which are not. By now, we might have expected statistical tests to have rejected the weaker theories, especially since many theories have been around a long time. But that does not seem to have happened. The main reason, I think, is the one I just mentioned: The theories make so few testable predictions that econometric study is a blunt investigative tool. On top of this comes the litany of standard objections to time series econometric evidence. You can recite it as well as I: results are fragile due to small samples and multicollinearity, there may have been "regime changes" during the sample period, appropriate instruments are scarce or nonexistent, computers make data mining too easy, and so on and on and on.

Stacked up against competition of this caliber from theory and econometrics, the interview method doesn't look so bad after all—especially if viewed as a supplement to, rather than a replacement for, more conventional modes of economic inquiry.

2. *Some Examples*

Let me illustrate with two concrete examples taken from a study of theories of wage rigidity (Blinder and Choi, 1990).

Theories based on asymmetric information were a major growth industry within economic theory in the 1980s. Stiglitz (1987), Weiss (1980) and others argued that adverse selection in the labor market might explain why firms do not cut wages when employment demand declines. The idea is that businesses fear they might lose their best workers and retain their worst ones. The theory is logically coherent,[2] sounds plausible on *a priori* grounds, and can be expressed with some mathematical/logical elegance. So it satisfies the three chief criteria our profession uses to judge theories these days.

But there is a more important criterion, in my view: empirical validity. And we know precious little about that because the adverse selection theory rests on the existence of *unobservable* productivity differences among workers; and it is pretty hard to bring econometric evidence to bear on that issue. So conventional econometrics may *never* give a definitive verdict on the validity of this theory. Is there another way?

I think there is. As part of a small-scale interview study of wage setting behavior, Don Choi and I (1990) asked either the personnel director or compensation manager of 19 companies the following hypothetical question:

> There are two workers who are being considered for the same job. As far as you can tell, based on interviews, experience, education, and so forth, both workers are equally well qualified. One of the workers agrees to work for the wage you offer him. The other one says he needs more money to work for you. Based on this difference, do you think one of these workers is likely to be an inherently more productive worker?

Not a single manager answered yes. Because of the small size and non-random nature of the sample, this is hardly definitive evidence against the theory. But, in my view, it is damaging evidence. And, given the paucity of empirical evidence of *any* kind on adverse selection, it cannot be ignored. Furthermore, if we had obtained the same results from a sample of several hundred firms selected randomly, the theory would be in deep trouble.

Of course, data from interviews are not always so unequivocal. Consider the implicit contract theory, which views long-term labor contracts as a way to provide real wage insurance to workers.[3] This is a beautiful theory that has been around a long time and has been extended, and disputed, in a number of theoretical ways. But how many econometric tests of the theory can you think of? Why, after 16 years, do we know no more about its empirical validity than we did in 1974? Maybe the reason is that the theory has few if any implications that can be tested by conventional methods.

Choi and I "tested" the theory in an unconventional way by asking our managers the following question:

> One theory on why wages do not fall states that workers do not like unpredictable changes in income. Therefore, workers and employers negotiate a stable wage that does not tend to fall during recessions or rise during booms. This steady wage acts as a type of wage insurance for the worker. How plausible or relevant does this seem as one reason why wages do not fall?

Here the results were much less decisive. Ten of the 19 managers said they found the idea "plausible or relevant," leaving the bottle half full or half empty, depending on your predisposition. With a larger sample, we might have pursued the question further by looking into the differences between firms that do and do not have implicit contracts with their workers. But, with only 19 observations, that option was not open to us.

Blinder and Choi (1990) turned up a number of other interesting results, which I will not dwell on here—except to note that perceptions of fairness were found to be very important. Instead, I want to turn to a large-scale study of price stickiness which I am currently directing at Princeton.

3. *An Interview Study of Price Stickiness*

Many traditional economic models of wage-price rigidity treat prices as flexible relative to money wages and see the labor market as the source of nominal rigidity. The aggregate demand-aggregate supply model inscribed in many textbooks is a case in point; so is any empirical model based on a Phillips curve and a markup equation.

More recent theorizing on the microfoundations of macroeconomics has abandoned this approach, however, and sought to explain nominal rigidities directly in the product market. Why? Because of worries that the money wage paid this period may simply be an installment payment on a long-term labor contract—as suggested by the implicit contract theory—and therefore play little or no allocative role.[4] I have just suggested that no one knows how important this problem is in practice. But almost everyone believes it is a bigger problem in labor markets than in product markets—which is why recent theoretical work on nominal rigidities has focused on prices rather than wages. Since I want at least *some* economists to pay attention to my interview results, I follow this newer tradition and focus directly on prices.

The first step was to draw up a list of theories of price stickiness which seemed to be prime candidates for "testing" by this methodology. While reasonably comprehensive, my list was not exhaustive for several reasons. Some theories, while logically coherent, seemed rather too obscure or too complicated to be explained in plain English to practical businesspeople. That does not mean that the theories are false, only that the interview method is not a good way to assess their validity. Other theories struck me as too fanciful to describe to real businesspeople with a straight face. Still others may have escaped my attention entirely. Despite these omissions, my original list of 12 theories covered the intellectual waterfront pretty thoroughly.

It would take too long to describe all the theories. But they share three attributes to which I want to call your attention.

First, if we restrict ourselves to conventional modes of economic inquiry, most of the theories are *empty* in the following sense: either they involve unobservable variables in an essential way, or they have no real implications other than that prices are "sluggish" in some unmeasurable sense, or both. Thus econometric modeling does not seem a promising way to discriminate among the theories. In particular, it seems capable of rejecting few, if any, of them.

Second, despite this observational (nonobservational?) equivalence, it really *matters* which theory is correct. It matters for the validity of economics as a descriptive science; and it matters for the conduct of macroeconomic policy because only some sources of price rigidity open the door to welfare-improving policies. For example, an increase in aggregate demand will probably raise social welfare if a coordination failure is keeping the price level too high (relative to the money stock, say). But the same policy might lower social welfare if prices are rigid because of sizable real costs of price adjustment.

Together, these two attributes are bad news for conventional modes of economic inquiry.

Third, however, each of the 12 theories describes a chain of reasoning which allegedly leads the firm to conclude that a change in price would be unadvisable. This third attribute gave me the idea for an interview study.

If people actually think the way one of these theories says, they ought to *know* that they do. If you just ask them an open-ended question like, "Why don't you cut your prices when sales decline?", you might get shrugs or incoherent answers. But, if you confront them with the chain of reasoning they actually follow, they ought to recognize and agree with it. Conversely, if they explicitly deny the relevance or validity of a particular argument, then it is probably not governing their behavior. At least that was my methodological precept. If the true reasons for price stickiness are buried deep in the subconscious of decisionmakers, interviews will probably miss them.

Let me illustrate what I mean with two concrete examples.

An old theory of macroeconomic price rigidity which enjoyed a revival in the 1980s holds that profit-maximizing firms with market power set price (p) equal to marginal cost (MC) times a markup factor, which depends on the elasticity of demand ($e < -1$):

$$p = MC(e/(1 + e)).$$

If demand curves become less elastic when they shift in, then rising markups may offset falling marginal costs, leaving the profit-maximizing price unchanged. *In principle,* this theory can be tested by conventional econometric means. All we need do is measure the cyclical sensitivity of demand elasticities in many industries. In practice, of course, that is a tall order, unlikely to be filled with the data we have.

And there is another difficulty, which points directly to the interview method. The theory quite explicitly pertains more to management's *perception* of how the elasticity of demand behaves over the business cycle than to objective realities.[5] It is therefore not enough to learn that estimated values of e move procyclically (in absolute value). For the theory to be valid, managers must *believe* that demand becomes less elastic in slumps *and act on that belief.* If managers actually hold such beliefs, it seems to me, they should know that they do—even if they have never heard of the concept of elasticity.

To test this theory, we pose the following plain-English question:

> It has been suggested that, when business turns down, a company tends to lose its least loyal customers and retain its most loyal ones. Since the remaining customers are not very sensitive to price, a price reduction will not raise sales very much. Is this idea true in your company?

If the respondent answers yes, we then ask, "How important is it in explaining the speed of price adjustment in your company?" So far, our small sample of firms is somewhat negative on this idea.

As a second example, consider the "invisible handshake" theory of Okum (1981), which is the product-market counterpart of the implicit contract theory of labor markets. Here the idea is that firms have implicit understandings with their regular customers which proscribe price increases when markets are tight (and, presumably, permit prices to be maintained in slack markets). It seems to me that, if such understandings exist, firms should know that they do. To assess the validity of this theory we pose the following question:

> Another idea has been suggested for cases in which price increases are *not* prohibited by explicit contracts. The idea is that firms have understandings with their regular customers—who expect the firm not to take advantage of the situation by raising prices when business booms. Is this idea true in your company?

If the respondent answers yes, we again follow with the question about importance. Our early responses to this question are bifurcated in an interesting way: half the firms see the invisible handshake as a very important factor while half see it as totally unimportant.

By posing questions like these for each of the 12 theories to each of about 200 firms, I hope to get a good idea of the empirical relevance of each theory in the U.S. economy. The questionnaire also includes followup questions, specific to each theory, that probe more deeply. For example, respondents who agree with the invisible handshake theory are asked whether it also slows down price increases that emanate from the cost side. In addition, the survey collects a variety of factual information about each firm. This will be useful both for cross-tabulations (which type of firm follows which theory?) and for econometric equations using agreement with the theory as the dependent variable and factual information about the firm as the independent variables.

4. *Some Issues in Research Design*

When you start thinking about designing a study like this, at least four major questions spring to mind.

1. Can economists' theories be made intelligible to practical business people who, while generally intelligent, understand neither mathematics nor economic jargon?

2. Can you get your foot in the door? —which means both getting the firm to agree to the interview and getting to see the right person.
3. Once in the door, can you get people to answer your questions? Or will they clam up or dissemble rather than reveal trade secrets?
4. When the study is over, can you get economists to believe the results?

4.1. Putting the Theories in Plain English

I addressed the first question by using the experimental method: I sat down at my word processor and tried to do it! Much to my delight, the task was much easier than I had anticipated. After a few evenings, I had a working draft. I then showed it to a few people to elicit comments, convinced the Russell Sage Foundation to fund a pilot study,[6] and tried it out on a few companies, large and small, which volunteered to be guinea pigs. At every stage, the questionnaire was redrafted, refined, and hopefully improved.

We learned a lot from the trial-and-error process. Literally hundreds of small changes were made in the questionnaire, one of the original 12 theories was dropped from consideration, and one new one was added. In general, however, the current version of the questionnaire bears a strong resemblance to the one I started with, for translating from technical journalese into English simply proved not to be very difficult. I suspect this would be true of most interview studies that economists might contemplate.

However, one important caveat should be entered. Not infrequently, a question had to be rephrased on the spot to make the respondent understand it.[7] To do that successfully, an interviewer must be reasonably articulate, must be able to think on his or her feet, and, most importantly, must understand the economics well enough to paraphrase a question without changing its meaning. Thus professional polltakers obviously will not do. The interviewers in my study are carefully selected Princeton graduate students.

I should perhaps explain why one theory was deleted from the list and another was added.

Greenwald and Stiglitz (1989) offered an ingenious theory of price stickiness. They argued that prices vary less than quantities over business cycles because risk-averse firms are *more uncertain* about the consequences (for profits) of changing prices than about the consequences of changing quantities. To me, this idea made a good deal of sense; and, even though it pertains to second-derivative properties of profit functions, I thought I could explain it in plain English. I was wrong. Several variants of a question which seemed quite clear to me simply did not register with the executives we interviewed. Sometimes they just furrowed their brows. More often, they gave an answer, but not to the question we thought we were asking. After several unsuccessful attempts, I concluded that the interview method is simply not capable of dealing with this particular theory.

I tell this story to underscore the point that not all theories are testable by interviews. That does not make them bad theories; it just means that some other method of inquiry must be used. On the other hand, one might wonder about the validity of a theory (of *their* behavior) that is incomprehensible to actual practitioners.

The one new theory added to the list also illustrates a general point. At the end of each interview, we ask the respondent whether we have ignored any important source of price stickiness. A top executive of one giant corporation suggested an interesting one: the difficulty of getting a large, hierarchical organization to act. This is not a factor that economic theorists normally think about, though maybe they should.[8] By asking 200 firms about this idea, our interview study should tell us whether economists are overlooking something important here.

There are, in addition, many places in the questionnaire where subsidiary questions were added because of ideas we picked up in the early interviews. For example, some firms have trouble raising prices because the sales force does not want to cooperate, or at least some executives think so. I cannot remember ever seeing this notion incorporated in an economic model.[9]

In general, it seems to me that asking practictioners is an excellent and underutilized way to generate hypotheses that can subsequently be tested by conventional econometric methods.

4.2 Eliciting Cooperation from Businesses

Whether or not firms would agree to be interviewed was the second major worry, for a very low response rate would raise fears of serious selectivity bias.[10]

My first approach was to look for other interview studies and see what response rates they obtained. But there were precious few, and none that were designed to generate a random sample of the GNP. Then I turned to one of the economist's favorite tools: armchair introspection, in this case by both researchers and business executives. That proved to be a dead end as well; different introspectors gave me "guesstimates" ranging from 10% to 66%.

So I decided to run a small-scale pilot study whose primary objective was to estimate the likely response rate in a large-scale study.[11] We drew a stratified (by firm size) random sample of 16 firms from the northeastern United States, wrote introductory letters to each of them, and then followed up with phone calls and/or further mailings as necessary. After considerable effort, eight companies agreed to be interviewed, seven declined, and we gave up on the last one after numerous attempts to get an answer. Counting this last firm as a no, our estimated response rate was therefore 50% with a standard error of 12.5%.[12] In addition, we had no trouble at all getting hooked up with the person or persons in each company who could answer our questions. The number of "I don't know" responses was small.

These results seemed good enough to merit proceeding to a large-scale study. So we have just sent out the first of 400 letters in the hope of completing about 200 interviews.

4.3 Getting Answers to the Questions

In designing the questionnaire, my position from the outset was that sensitive questions had to be avoided, no matter how interesting economists might find them. In particular, I banned any question that might conjure up images of the Antitrust Division of the U.S. Department of Justice. Thus there are no questions about limit pricing, oligopolistic interdependence, industrial structure, and a variety of other factors that a specialist in industrial organization might think relevant to pricing. It is not that I think that the ideas of industrial organization are wrong; I just did not want my respondents to worry about confidentiality (which we guaranteed). Fortunately, since my focus is on macroeconomic phenomena—such as responses to changes in demand and costs that are largely beyond the control of the firm—this self-imposed constraint was not particularly constraining.

Still the only way to know whether we had succeeded in avoiding sensitive areas was to try out the questionnaire. The results here were unambiguously favorable. Of the 14 interviews we completed,[13] only one person thought that even a single question asked for proprietary information. That individual refused to answer two seemingly innocuous questions (out of a 30-page questionnaire!), but inadvertently answered one of them later in the interview.

Thus, given our self-imposed taboos, getting people to talk proved to be no problem. Our experience, in fact, was not that people were tight-lipped, but rather that they were often eager to reveal to us details and specific incidents about which we would never dare ask. Some of the interviews lasted much longer than necessary, and several respondents told us that they found the discussion stimulating, enjoyable, and even informative.

4.4 Getting Economists to Pay Attention

I come now to the hardest question of all: Can we get economists, who are accustomed to sneering at "anecdotal evidence," to pay attention to the survey results? I do not delude myself into thinking that I have a perfect solution. There are those in our profession whose beliefs cannot be shaken by this sort of evidence, maybe not by any evidence. But I think they are a minority and focus my efforts on the persuadable, but not gullible, majority.

I have taken several concrete steps to increase the persuasiveness of the ultimate findings, whatever they might be. First, I wrote letters to those I think of as either the originators or major proponents of each theory. Each letter explained the nature of the study, included a copy of the relevant portion of the questionnaire, asked for suggested improvements in the questions, and asked the "theorists" to suggest other testable implications of their theory. Most responded.[14] Several offered good suggestions for modifying the questionnaire, which I adopted. Interestingly, however, not a single person suggested any further implications that could be tested in the questionnaire.

Second, and much more importantly, we have taken great pains to ensure that the sample of firms is (a) large enough to generate a database suitable for serious statistical analysis, (b) randomly selected, and (c) representative of the private, for-profit GNP. (An appendix describes briefly how this was done.) To my knowledge, this is the first time this has ever been attempted in an interview study. It is what makes the study unique; I am after statistical generalizations, not anecdotes.

When all is said and done, however, the biggest factor affecting the persuasiveness of the results is one that is beyond my control. As I illustrated earlier with some examples, interview studies may yield definitive results on some questions but murky results on others. Given the skepticism that many economists have about interviews, only truly striking results are likely to be persuasive. This is risky research, not for the fainthearted—nor the untenured.

5. *Some Very Preliminary Results*

Let me conclude by offering a sneak preview of the results. First, two cautions. When this paper was drafted, we had conducted only 14 interviews; and, of these, only eight were with firms selected randomly. So the sample is not only small but nonrandom. Furthermore, we made changes in the questionnaire while the interviews were in progress. So what I am about to report should be thought of as anecdotal, not statistical, evidence. Still, some of the findings are strong enough to be of interest.

First, one theory stood out from the rest in that *every one* of our 14 firms rejected it as "totally unimportant." That was the adverse selection theory that holds that firms worry that customers will interpret a price reduction as a reduction in product quality. On the following four-point scale:

1 = totally unimportant
2 = of minor importance
3 = moderately important
4 = very important,

it received a perfect 1.0 score. Interestingly, this idea and its close relatives have attracted great interest from economic theorists in the last decade.[15]

Two other theories stood out as unusually unpopular, with average scores of 1.6. One, I must confess, was the simple Keynesian notion that nominal contracts prohibit price increases in the short run; 10 of the 14 firms rejected that hypothesis out of hand. The other was the new theory we added: that large hierarchical organizations have a hard time changing their prices. However, this is hardly surprising since most of the respondents were from small companies. We must try the idea out on a random sample of the GNP, which includes many giant corporations, before making a judgment.

On the other side of the ledger, three theories received ratings of 3.0 or 3.1 from our respondents, the highest scores of any. One was my rough translation of the coordination failure theory: that firms may want to raise prices, but wait until their competitors move first.[16] Nine of our 14 firms called that a "very important" factor in delaying price increases. The second was a simple idea based on asynchronization and lags: that prices are based on costs, so that price increases must wait for cost increases, which take a while.[17] The third—which we asked only of firms that hold inventories—is that firms react to lower demand by letting their inventories rise rather than by cutting prices.[18]

I would not, however, want to go out on a limb and predict that these results will hold up in the larger sample. That, after all, is why we are going through the pain and effort of interviewing 200 randomly-selected companies. Ask me for the results two years from now.

APPENDIX: CREATING A REPRESENTATIVE SAMPLE

We purchased from a commercial source a tape listing firms in the northeastern United States with annual sales over $10 million.[19] From this, we excluded government enterprises and nonprofits on the grounds that the theories we wanted to test were all about profit-seeking firms. There were 24,494 such firms, excluding (as best we could) subsidiaries. We assigned a sampling weight to each firm proportional to its value added,[20] and then drew—by computer—a random sample of 400 firms.[21]

Notice that we exclude companies with annual sales under $10 million. Why? They do, after all, employ nearly half the labor force.[22] The reason is that there are so many of them, and the expense of reaching any sizable portion would be prohibitive. Clearly, any optimal sample design would balance the value of the information obtained against the costs of obtaining it and would, therefore, assign very small sampling weights to very small companies. We approximated this crudely by putting zero weight on any company below the $10 million threshold. Analogous cost considerations motivated the geographical truncation: costs depend on distance from Princeton, N.J.; benefits do not.

These two exclusions, however, might compromise the representativeness of the sample, so we "remedied" them in the following way. We hypothesize that, if pricing behavior differs from state to state, the main reason is that *pricing* differs across *industries* and the *industrial structure* differs across *states.* That is, we suppose that if the pricing policies of a California firm differ from those of a New Jersey firm, it is not because its head office is in California, but because it may be in a different industry. Under this hypothesis, we can (and did) create a *synthetic national sample* by adjusting the sampling weight of each firm in the Northeast to reflect national, rather than regional, shares in value added.[23] Firms in industries that are over-(under) represented in the Northeast were given appropriately lower (higher) sampling weights.

Once we were doing that, it was a simple matter to eliminate (part of) the potential bias from omitting small firms. For example, any sample that excludes firms with annual sales under $10 million will underweight retailing. We simply raised the sampling weight of each remaining retail firm enough to assign retailing its proper national weight. And we did the same for every industry.

Thus, while our sampling frame is geographically restricted, its industrial structure is the same as that of the nation as a whole. To the extent that any "large firm bias" that might exist stems from the different industrial structures of small versus large firms, we have therefore eliminated it from our sample. However, if small firms really differ from large firms *in the same industry,* then the bias remains.

NOTES

1. This is itself a testable hypothesis—using the interview method!
2. There is one big weakness, however. If productivity differences across workers really are unobservable to employers, then it is not clear why those with higher productivity should have better opportunities elsewhere in the labor market. The standard answer is that such people are more productive in home production; but that seems a weak reed in a modern industrial economy.
3. Azariadis (1975), Baily (1974), Gordon (1974).
4. See, for example, Barro (1977).
5. Of course, under rational expectations, these should be the same. But our interviews have uncovered sharp differences in estimates of the elasticity of demand even within the same firm!
6. This statement vastly understates the role of Eric Wanner, who encouraged me from the beginning—well before I was convinced to go ahead.
7. Such misunderstandings have been reduced in the many redraftings of the questionnaire.
8. However, in my view, it may be one important source of "menu costs."
9. However, principal-agent theory would seem a natural vehicle for doing so.
10. However, a low response rate does not prove that there is selectivity bias.
11. Eric Wanner suggested this route.
12. The response rate in Blinder and Choi (1990) was also about 50%. A coincidence?
13. I mentioned eight interviews in the pilot study. There were also six "guinea pig" interviews conducted before the pilot.
14. In this context, thanks go to George Akerlof, Lawrence Ball, Olivier Blanchard, Dennis Carlton, Robert Gordon, Bruce Greenwald, Anil Kashyap, David Romer, Julio Rotemberg, Andrew Weiss, and Janet Yellen.

15. See, for example, Stiglitz (1987). Equally interesting, I think, is the fact that the managers in Blinder and Choi (1990) strongly rejected the adverse selection theory for the labor market.
16. Cooper and John (1988).
17. Gordon (1981), Blanchard (1983).
18. Blinder (1982).
19. We compared several alternative databases; the one we selected was about as comprehensive as any. The "northeast" is defined as the following states (plus the District of Columbia): New Jersey, New York, Connecticut, Massachusetts, Rhode Island, Pennsylvania, Delaware, Maine, Vermont, Maryland, New Hampshire, Virginia, Ohio, Indiana, Illinois and Michigan.
20. We actually had estimated employment for each firm and the ratio of value added to employment for each two-digit industry.
21. Actually, we had to draw more because we know from experience that some of the individuals or companies drawn into the sample will be dead, or will have vanished with no forwarding address.
22. For example, the firms in Ward's directory of companies with more than $11 million in annual sales employ 55.6 million workers.
23. This was done at the two-digit SIC level. Thus all firms in the same two-digit industry received the same reweighting, even if they were in different four-digit industries.

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